

observations. The interruption of the blood-current so often seen in these cases did not occur. At one point a small coagulum was discernible, but with this exception no irregularity of the blood-flow occurred, nor were any of the small arteries white in outline, which sometimes occurs from lymph streaks following in their course.

The man was at once admitted as an in-patient for further observation. It was then discovered that he suffered from terrible cerebral neuralgia, which occurred in paroxysms after retiring to rest. Here, again, syphilis showed itself. Ten-grain doses of the iodide of potassium administered thrice daily proved of service, and the cerebral attacks became less frequent and less painful. On further testing vision, the central part of the retina was found to be but little affected. A few days after admission the disc became grey, hazy, and indistinct, as if from effusion, and this filmy condition gradually extended to the retina bounding the inner margin of the disc; this was followed by a diminution in the calibre of the vessels supplying the inner half of the retina, and ultimately paralysis resulted. Still a fair amount of central vision remained, but the field was exceedingly limited in extent. Then resulted a decided neuro-retinitis, which was well marked. After this the patient decidedly picked up in condition, lost his anæmic look, improved in appetite, and expressed himself as feeling, comparatively speaking, well. After the lapse of a week the effusion began to disappear, and the margin of the disc became more distinct, but still the peripheral portion of the retina showed no sign of returning health.

On Jan. 21st, the notes of the case say: "The vessels supplying the outer half of the retina are carrying more blood; there is now slight disturbance of the vitreous humour. On comparing the diseased with the healthy disc, it was found that the left disc appeared in every respect healthy, the vessels being of average size, and carrying a normal quantity of blood."

The yellow spot and its surroundings were frequently examined to ascertain if any effusion existed, or if there were minute blood extravasations, but its appearance never varied from that of health, as, indeed, might be expected from the fact of central vision being so good. Then a considerable time elapsed before any fresh symptoms occurred. On February 3rd vision began to improve, and the patient wished to leave the hospital, saying he felt much better in general health. It was now observed that the circulation in the retinal vessels was becoming more normal, but pressure on the vessels supplying the outer half of the retina produced no pulsation indicating that the clot was not all absorbed. Pressure still produced a modified degree of pulsation in the vessels supplying the inner half. It was suggested by Mr. Oglesby that the sudden paralysis of the inner half of the retina was probably due to the clot which blocked up the vessel to the outer half having become partly free, and so carried by the circulation into the adjoining vessel distributed to the opposite side.

The iodide of potassium was of great service in improving the general health, and was continued during his stay in hospital. Mr. Oglesby proposes, in the event of the man returning for further treatment, to put him through a gentle mercurial course.

MAISON MUNICIPALE, PARIS.

CASE OF GENERAL ARTICULAR RHEUMATISM RAPIDLY CURED BY PROPYLAMIN.

(Under the care of Dr. FÉREOL.)

THE patient, aged twenty-four, was suffering from a third attack of rheumatism. The first attack had occurred in 1869. Heart complications ensued, and were treated by means of the cupping-glass and scarificator and blisters. The rheumatic pains in the joints lasted three weeks. The patient was then sent to Aix to get the benefit of the waters. During the two following years he was free from rheumatism. A second attack then occurred. The pains lasted about two months, and slowly disappeared. The heart was again involved.

The third attack began on December 28th, 1873, with pain in the ankles, slowly extending to the larger joints. On January 2nd the pains were most intense, and heart symptoms were stated (rubbing, bruit de souffle, &c.); pulse

104; temperature 38° Cent.; thirst, anorexia, constipation, &c. Fifteen grains of propylamin to be administered.

Jan. 3rd.—Same state; no improvement. Twenty grains of propylamin ordered.

4th.—The condition of the heart has much improved under the influence of a blister; much less pain. Twenty-five grains of propylamin ordered.

5th.—Great improvement; the patient asks for food; can move his limbs; pulse less frequent.

6th.—Patient declares that the pains suddenly left him yesterday afternoon; walks about in the wards. Pulse and temperature normal. Treatment left off.

The rapid effects of this plan of treatment are especially important when compared with the two former attacks.

Medical Societies.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

TUESDAY, MARCH 24TH, 1874.

DR. C. J. B. WILLIAMS, F.R.S., PRESIDENT, IN THE CHAIR.

ON AN IMPROVED METHOD OF ABSCISSION OF THE ANTERIOR PORTION OF THE EYEBALL.

BY ROBERT BRUDENELL CARTER, F.R.C.S.,
OPHTHALMIC SURGEON AND LECTURER ON OPHTHALMIC SURGERY AT
ST. GEORGE'S HOSPITAL.

AFTER mentioning the conditions that call for abscission of the anterior portion of the eyeball, and the way in which the operation once practised was improved by Mr. Critchett, the author relates a case in which Critchett's operation was followed by sympathetic ophthalmia and loss of sight. He attributes this result either to traction upon the ciliary nerves in the cicatrix, or to laceration of one of them by one of the needles used to transfix the eye. In order to set aside these dangers, and at the same time to obtain a stump well calculated to carry an artificial eye, he has devised a plan of operating which is described. It consists of uniting the tendons of the recti muscles by catgut sutures, and then of uniting the conjunctival wound over them, no sutures being passed through the ocular tunics themselves. A patient who had been operated upon in this manner was exhibited to the Society.

Mr. HIGGINS said the usual plan at Guy's Hospital was to cut out the anterior portion of the eyeball, and then bring the conjunctival edge together; and they found this a better plan.

Mr. BRUDENELL CARTER had had no experience of this, and did not think it would give so good a stump as that formed by the tendons, &c.

RECENT EXPERIENCE OF CHOLERA IN INDIA.

BY JAS. MACKAY CUNNINGHAM, M.D.,
SANITARY COMMISSIONER WITH THE GOVERNMENT OF INDIA.

After some introductory observations on the importance of the cholera question, especially at the present time, Dr. Cunningham proceeded to remark on the special opportunities afforded by India for the study of cholera, and the great value of the information to be obtained there. He then entered into an examination of the evidence derived from the history of the epidemic of 1872 in Northern India. Two great points had to be determined—first, the influence of human intercourse in spreading the disease; and, secondly, the practical measures to be adopted for protection.

1. The evidence as regards human intercourse was considered with reference to the geographical distribution of cholera in India; the great areas of prevalence and exemption; the experience of the same tract in different epidemics; the endemic area, the seasonal and periodic rise and fall of cholera within this area; and the singular immunity of certain places. Further, with reference to this question, Dr. Cunningham dwelt upon the detailed evidence afforded by the history of 100 outbreaks in 1872. There was an entire absence of all evidence of communication of the disease, and the previous considerations were fatal, Dr. Cunningham believed, to this doctrine. The epidemic was not propagated along highways of communication, and did not travel any quicker in these days of railways than it did in olden times. Singular evidence against the contagiousness

of the disease was derived from the St. Peter's College outbreak, many cases having been sent thence into various localities without, in one single instance, disseminating the disease. The experience of attendants on the sick was against all suggestion of contagion, a small proportion of those only being attacked, and there was an absence of all evidence of contagion in regard to those who were attacked. As to indirect contagion, the facts bearing on the water theory, as illustrated by the outbreak at Peshawur and Mean Meer and other places, were wholly against its accuracy. Dr. Cunningham dwelt on the importance of local conditions in connexion with the singular localisation of the disease, as illustrated by the outbreaks at St. Peter's College, and amongst the troops at Kussoulie, in the camps at Mean Meer, &c.; and he urged the necessity for studying these local conditions much more closely. The incidence of the outbreak as regards time among different sections of the community next received attention.

2. In considering the practical measures to be adopted to afford protection from cholera, the impossibility of carrying out an efficient quarantine was considered, and the great evils attending any attempt at it shown by the experience of Upper India on this point. The primary importance of sanitary improvements was next urged, and a strong opinion was expressed on the tendency of the contagion views to interfere with progress in this respect.

Dr. HARDIE said he had had some experience of cholera in Mauritius during two epidemics, and in both cases it had been imported into the island by the ships bringing over coolies from Calcutta. In a previous epidemic it was introduced in the same way, through the quarantine rules being broken, so that the cholera had each time entered by the only port of the island. He did not think India was a good place to experiment upon cholera. In the epidemics he had seen if there was a cause of immunity he thought it was on the estates where the people were supplied with water from wells. He thought the water-streams were the great cause of its spread.

Sir WILLIAM GULL said he had gone over the whole story of cholera some years ago. He was interested to find in the paper that fresh statements added nothing to what Dr. Baly had already said. He thought it was to be regretted that the report of the Royal College of Physicians was so little known and referred to. The whole question of diffusion by water, human excreta, and of sporadic cases had been gone into. Occasionally water was a means of diffusion. When a ship was said to bring contagion to a place it should be remembered that a ship is a locality. The late Dr. Addison, speaking to him of the contagiousness of cholera, said, with regard to the admission of a patient with cholera into Guy's, that if you bring in a man from Tooley-street with it you bring in the locality, and so cholera. During the last epidemic a cholera hospital was established at Whitechapel, under the care of Dr. Sutton. The disease never spread to the nurses or the attendants, and he thought for this reason: as soon as a patient entered, his clothes were taken away, the patient sponged with Condy's fluid, and the hair cut; and this regulation he had helped to enforce lest the locality as well should be brought in by the patient. He did not believe in the evidence of the contagiousness of cholera. He thought the statement that it was spread by the evacuations was a good working theory, as it acted by frightening people, but proof of it as a scientific fact was wanting, and he would be glad to learn it. If this was one cause there were others. He believed it always came by ship, but sporadic cases were always met with before an outbreak. He referred to the outbreak of Asiatic cholera in Mr. Druit's farm in Surrey for poor children in the epidemic of 1848. About half of the children died, and it was at first supposed that their deaths had been hastened by bad food, poison, &c., and Mr. Druit died broken-hearted on account of the way he was judged by public opinion. After a time it was clearly seen that it was the beginning of the outbreak. And often isolated cases, not traceable to human intercourse, have preceded outbreaks. At present there was no scientific theory of the spread by contagion and evacuations. It was singular how heights above the sea-level had to do with the origin of cholera. Cholera did not descend streams, but ascended them. This was a fixed and certain law. As to sanitary improvements, there were two views, but only one mentioned in the paper. Sanitary improvement fortified the body

against epidemics by means of good air, ventilation, and cleanliness; then it removed telluric influences, as damp, dirt, &c., and so improved the health of the people.

Dr. BUCHANAN hoped Sir W. Gull would retract the expression that the water theory was useful though false; he would be sorry to advance a water theory if he thought it false. (Sir W. Gull then said he would say, an imperfect one.) He thought the remarks made about cholera in the first part of the paper were equally applicable to fever, and not alone to typhoid, but he would say even for typhus fever every assertion would hold good. He was sorry to hear many assertions which had been put forth about personal contagion. When Dr. Cunningham said he looked to India for evidence, and did not consider that brought forward in England, he would ask him to look afresh at our evidence. He did not think the circumstances in India were favourable for the investigation of cholera. If we wished to study measles or scarlet fever, would we go to a large place like London, or to a remote village, or to a place where its introduction is easily ascertained? Could outbreaks of yellow fever be as well worked out in the West Indies as the outbreak at Swansea was? It was from these out-of-the-way cases that we learn. With regard to the arrest of cholera, he thought by preventing filth being out of its place we did a great deal, as the poison is associated with excrementitious matter. He regarded atmospheric influence and telluric influence as mere words, and inoperative as causes.

Mr. NETTEN RADCLIFFE stated that he should have had some hesitation in taking part in the discussion, but, looking at the clock, he had begun to fear lest the discussion should come to an end without more definite reference having been made to the kind of facts upon which Dr. Cunningham had founded his opinions. Now, so far as the two points most prominently raised in the paper were concerned—those relating to the contagiousness of cholera, and the so-called water theory,—Dr. Cunningham's views were founded on fallacies. The "contagion" of which he spoke was not the "contagion" understood in this country; the "water theory" had hardly the faintest resemblance to what was meant here by the term. Dr. Cunningham judged of the contagion of cholera as if it were operative in the same way as the contagion of small-pox, and were, moreover, some self-operative agency acting irrespective of conditions. Such a doctrine of the contagiousness of cholera had no place in English teaching, so far as Mr. Radcliffe was aware. The very characteristic of etiological study in this country for many years, in respect to contagious diseases, had been, and still was, the determination of the conditions under which the particular contagions of the several diseases operated. Even small-pox was no exception to this study, and some of the most instructive data were furnished by inoculation. To argue that because the contagion of a disease did not operate unconditionally therefore contagion did not exist, was much the same thing as to argue against the germinative power potentially present in a potato or a grain of wheat because germination did not occur and growth follow except the potato or the grain of wheat were placed in certain well-understood conditions. The whole of Dr. Cunningham's inquiries as to the contagion of cholera had been governed by notions of this kind, and a perusal of the data he gave would prove that the results of the inquiry as to contagion were valueless to English research. Again, as to the water theory—the theory which had been broached here, and was used to explain certain facts of localisation alone. Dr. Cunningham spoke of the theory as inconsistent with the geographical distribution of the disease, as being negatived by the fact that bodies of troops widely separated from each other, and drinking from different sources of water, had been nevertheless severely attacked with cholera, and so forth. Such arguments proved that Dr. Cunningham was not dealing with the water theory understood here, but with something far different, and which had nothing in common with English notions. Even in one particular instance—the St. Peter's College, Agra—where an outbreak had occurred which would have led most English inquirers to make minute investigation into the sources of the water-supply, such inquiry, so far as the detailed report permitted a judgment to be made, had missed the very point to be inquired about. Mr. Radcliffe regretted that the time at the disposal of the meeting prevented him from illustrating the remarks he had made from

point to point in Dr. Cunningham's report. He referred briefly to Dr. Cunningham's observations as to the absence of any evidence in India that a quickened traffic had accelerated the movements of cholera, and pointed out that the data given by Dr. Cunningham did not contain the materials for a judgment. He asked, also, whether, if it were true in the comparatively narrow field of diffusion to which the data referred, they would set aside the facts obtained from the study of the great migrations of 1832, 1848, and 1865, in which, for example, the question of time of carriage from East Europe to America was a simple matter of observation. The first cargo of cholera carried to America in 1832 was carried in a sailing ship; the first cargo carried in 1866 was carried in a quick-sailing steamer. Dr. Cunningham maintained that the recent experience of India demonstrated the inaccuracy of the views now commonly entertained in this country as to the diffusion of cholera. He had illustrated, with respect to the water theory, particularly the outbreaks of cholera in Broad-street and East London. Mr. Radcliffe contrasted the circumstantial character of the inquiry into the two outbreaks referred to with the inquiries instituted by Dr. Cunningham, and concluded by saying that inquiries which were held to set aside the results of the English inquiries should produce evidence of having been conducted with an equal degree of minuteness.

Dr. FAYRER did not think that Dr. Cunningham had shown in anything a desire to underrate English labour. He was surprised to hear that India was not the place to study cholera. Dr. Cunningham had not dealt with opinions, but had mentioned facts. He believed cholera was due to some "influence," not a poison. With regard to contagion, he thought there was as much evidence to prove it as there was against it.

Dr. BURDON-SANDERSON said that Dr. Cunningham had come before them not bringing forward theories, but only stating facts. He did not agree with Dr. Buchanan that India was not the place to investigate cholera; for as to the question of etiology, it was better to proceed by the law of exceptions; but then cholera was so widely spread that its existence in a place was not an exception, so it was difficult to apply the rule. But Dr. Cunningham rested on the immunity of places; that was the exception. So those who asserted that personal communication was the cause influencing the spread of cholera in India, and those who maintained cholera spread up a stream, and got down by contaminating the water, both had to contend with the absence of cholera in places. He thought cholera was a disease of local origin, and, to go further, it attached itself to organic matter in a state of decomposition. That was the opinion Pettenkofer contended for, though obtained from a different source, and as expressed by Dr. Cunningham from the results of his observations in India.

Dr. CUNINGHAM, in reply, said that with regard to the etiology of cholera in India, there was an area in Lower Bengal where cholera was constantly present, and there were rises and falls of the disease, and beyond that area there were isolated cases, and epidemics prevailed. The supporters of contagion should take up these facts and grapple with them. As to Dr. Hardie's epidemics, supposed to be due to human intercourse, we had only to put against it that emigrants had gone from Calcutta to Mauritius for many years, and cholera was always present at Calcutta, and yet the epidemics Dr. Hardie spoke of were the only ones which had occurred. Because a ship arrived and cholera became epidemic over the island, should that be set down as the cause? The history of cholera in India was that it ascended streams, and did not descend them. The work of Sir W. Gull and Dr. Baly was well known in India, but he thought it was very little known in England. He thought that there was little more known of cholera here than there was twenty-five years ago. The doctrine of contagion was not a harmless one; he thought it a bad working theory as not being truthful; all that was said of the water theory showed that we had no evidence in support of it. Dr. Buchanan said the same arguments would apply to fever. But what if they did? The facts as to small-pox were as inapplicable as those of cholera. Then he said the circumstances in India were not favourable for the study of the disease; but he (Dr. Cunningham) thought that it was quite as easy to do this in India as in England, and that was no reason upon which to set aside the evidence. As to the meaning of the words contagion and water theory—whether

the water theory was the means of contagion, usually or rarely, was a matter of unimportance; the great question was, was there evidence of a single case in India or England that cholera spread by the evacuations of cholera patients? Anyone in India who had studied the question would throw aside the "Broad-street pump" evidence as valueless. The evidence he had brought forward with regard to the outbreak of cholera was as good as any evidence for any outbreak of cholera, or any other disease. Twelve or fifteen compounds had been attacked on or about the same day, and the people drew their water from different sources. With regard to the diffusion of cholera to-day as compared with that of former days, and depending upon the increased facility of communication, and that epidemics were gradually accelerating their rate of advance, he would only state that in 1817 a wave of cholera passed over India, and it took about seven months, in 1870 another wave passed over India, and it took three years, to travel from and to the same points, though the means of traffic by rail, good roads, &c., were multiplied manifold. With regard to the outbreak of cholera at St. Peter's School, Agra, it had been said that the whole thing was slurred over, and the evidence untrustworthy, and that it was due to contaminated water, which was used though it had been prohibited. He would only state that, of twenty-four day-scholars who drank the same water, all escaped except one, and he was the only one who resided in the same enclosure. Here, he thought, no water theory could be employed to explain the circumstances.

MEDICAL SOCIETY OF LONDON.

MONDAY, MARCH 2ND, 1874.

Dr. S. O. HABERSON, F.R.C.P., PRESIDENT, IN THE CHAIR.

MR. FRANCIS MASON showed four Polypi removed from the nostril of a boy aged twelve, at one operation. The largest was horseshoe-shaped, measuring two inches by one inch and a half; the next in size was two inches long and one inch wide, and resembled a large slug. Both these were removed by evulsion with the ordinary polypus forceps, through the fauces and mouth. The remaining tumours were flattened out, elongated, and measured nearly two inches. They lay in the cavity of the nostril and could be seen from the front, and were taken away with the forceps through the anterior nares. The pedicle of each polypus was extremely delicate. Mr. Mason spoke of the facility with which the operation was performed, and thought there was much less danger in adopting this method of removing polypi passing into the pharynx than is generally supposed. The application of a ligature to such tumours was a troublesome proceeding, and was really necessary in but few cases. He believed that severe hæmorrhage rarely accompanied the removal of the polypus; the bleeding was in most cases due to the attempt made by the operator to clear the nostril completely of the morbid growth, in doing which the mucous membrane was unavoidably injured. The tumours had existed as long as the boy could remember. There was but little hæmorrhage in this case, which was no doubt due to the slender attachments of the growth.

Mr. DE MÉRIC referred to the use of "Hilton's snare" in these cases, and spoke of the practice of slitting up the velum, and the necessity in some extreme cases of even removing the superior maxillary bone, in order to enable the operator to reach the base of the tumour.

Dr. PROSSER JAMES concurred in the opinion that there was little or no danger in removing polypi of this kind when the attachment was not broad; for cases thus manageable he deprecated the more grave operation mentioned by Mr. de Méric. That was only to be resorted to in very different cases, where neither ligature, forceps, nor *écraseur* could be applied. He was surprised to hear no mention made of the rhinoscope. Without ignoring the *tactus eruditus*, he thought it less certain than vision, and by the aid of the mirror a diagnosis could be formed at a much earlier period. True, in such a case as Mr. Mason's, the size of the growth would block up the passages, and it might be difficult to determine the extent of attachment; but the rhinoscope would enable the observer to see a polypus long before it attained such dimensions, when it was, in fact, very small. It would also enable us to remove polypi, by properly-shaped instruments, or to employ what was some-