

same. I did not measure either the femur only or the limb from the symphysis pubis. Reduction was effected readily enough by manipulation. Requesting the dresser to grasp the upper part of the thigh and make slight traction outwards, I flexed the thigh towards the abdomen, bringing the limb into a line with the trunk (adduction), and making traction of the limb forwards, with my arm under the patient's knee, for the purpose of preventing the head of the bone from rolling in the groove for the obturator externus below the acetabulum. Circumduction and extension were then practised, but when the limb was brought down, it was found that after all the "sciatic" displacement had been produced. Flexion, circumduction outwards, or rotation outwards (it is extremely difficult in practice to separate these two movements accurately when the head of the bone is displaced), and extension at once placed the head of the femur in the acetabulum.

Remarks.—I have not by me any surgical work which mentions the possibility of shortening of the limb in obturator dislocation. All the authorities speak absolutely of about two inches lengthening of the limb. Not having met with any mention of this condition, I thought I must have been mistaken in my measurement; but having produced the obturator displacement on two subjects, whose lower limbs were of equal length, I found that in neither was the limb longer when measured from the anterior superior spine to the malleolus. I apprehend, therefore, that at first, in the thyroid displacement, the head of the bone is not in close apposition to the obturator muscle over the thyroid foramen, but lies at a lower level, and that after a time it may either have its position altered by some movement of the body (such as my patient was subjected to in his removal from the ward to the theatre), or by the muscles which draw it well up against the pelvis. In the diagram in Mr. Erichsen's admirable work on "The Science and Art of Surgery," the head of the bone is placed low down on the margin of the obturator foramen near the tuberosity of the ischium, and it is evident at a glance that a higher position would materially shorten the lengthened skeletal limb. Mr. Luke has reported a case of dislocation of the head of the bone below the acetabulum, which is referred to by Mr. Holmes and Mr. Erichsen. I had the opportunity of witnessing it as it occurred—with two other dislocations of the hip, one into the thyroid foramen and the other on to the dorsum ilii—during a week in which I was officiating as dresser under Mr. Luke. The limb was lengthened for one inch only.

I am therefore inclined to believe that the description "dislocation into the thyroid foramen" does not express with accuracy the exact position of the head of the femur in the cases in which there are two inches of lengthening. In reporting cases of the kind it should be stated how the measurement is made, for when the head is in the oval foramen there is a marked difference between the measurement from the symphysis and from the anterior spine, when compared with similar measurements on the opposite side. It is scarcely necessary, perhaps, to add that care should be taken to ascertain whether the lower limbs are naturally of the same length. Since these remarks were written Mr. Holmes has published a clinical lecture in the *Medical Times and Gazette*, in which he doubts the possibility of lengthening in the true thyroid displacement.

In regard to the conversion of the "obturator" displacement into a "sciatic" displacement, that, as well as the reverse conversion of the "sciatic" into the "obturator" during manipulation, has been frequently noticed; and it is curious that reduction in either case may be facilitated by the change. Bigelow, indeed, recommends the conversion of the obturator into the dorsal displacement, which he considers easy of reduction. To what is this conversion to be attributed? Evidently to one of two causes: either to some error in manipulation—the surgeon, as Mr. Callender terms it, overacting his part by too much abduction or adduction, as the case may be,—or to the position of the rent in the capsular ligament. In some cases, no doubt, the capsule may fairly be described, with Mr. Callender, as being torn to pieces; in other cases the rent may be more or less located either to the lower, the inner, or the posterior segment, and unless the head be brought opposite to the rent during the manipulation I can understand that an untorn portion of the capsule might prove an impediment to reduction. I cannot, at present, follow Mr. Morris in his universal limitation of the position of the laceration of the capsule, but my mind is quite open to conviction on this as on any other subject.

(To be concluded.)

ON YELLOW FEVER.

BY ANDREW DUNLOP, M.D.

I HAVE already given some account of my small experience of yellow fever (THE LANCET, Feb. 15th, 1868), but after the recent papers of Mr. Cargill (THE LANCET, Oct. 27th, 1877) and Dr. Donnet (THE LANCET, May 11th, 1878) I am tempted to reproduce it in another form, and to add a contribution to the discussion of the still unsettled question of the contagious or non-contagious nature of the disease.

On the 26th of July, 1867, I arrived at Havana in medical charge of the steamer *Narva*, which was to lay a submarine telegraph cable between Cuba and Key West, and between the latter island and the west coast of Florida. Particulars are given in my former paper, and I need only say here that yellow fever broke out on board at the end of August, while the section between Key West and the mainland of Florida was being laid, and continued until we passed out of the Gulf Stream on our way to New York in the following month. The incidence of the disease on the ship's company is shown in the subjoined table:—

| | No. | Cases. | Deaths. | Remarks. |
|---|-----|-----------------|---------|-------------|
| Electrical staff ... | 5 | 3 | 2 | 1 doubtful. |
| Ship's officers ... | 4 | 2 | — | 1 doubtful. |
| Doctor ... | 1 | — | — | — |
| Engineers ... | 4 | 3 | 1 | — |
| Stewards ... | 3 | — | — | — |
| Cable hands ... | 19 | 14 | 10 | — |
| Crew, firemen, cook, carpenter, &c. ... | 24 | 2 | 1 | 1 doubtful. |
| Total ... | 60 | 24 ¹ | 14 | |

The most striking point here is the morbidity and mortality amongst the cable hands, compared with that amongst the crew. The crew slept in the fore-castle, the blue-jackets on the port and the firemen on the starboard side; their work was naturally almost entirely confined to the ship, except when they had to furnish boats' crews, &c. The cable hands slept between decks, on a level with, and close to, the openings of the two large tanks in which the cable was stowed. These tanks contained water covering the cable, and, as usual, gave off sulphuretted hydrogen in considerable quantity. These men were better fed than the crew, and their work in connexion with the cable took them a great deal on shore, especially on the beach when the shore ends were being laid.

If yellow fever is infectious in the ordinary sense of the word, it is impossible to imagine how it could have affected so unequally two classes of men living together in the close companionship of a ship of some 850 tons. In no case did I observe the slightest evidence of the disease spreading from the sick to those attending on them. One of the electrical engineers and myself constantly watched in turn another of the electrical staff during his fatal illness. He was in a small cabin, not well ventilated, and we were exposed to the emanations from the patient to such a degree that on my return home, about eight weeks afterwards, I could detect the peculiar odour of the disease in the clothes I had worn at the time; yet neither of us had a day's illness. Eleven of our sick were treated on shore, at the Naval Hospital at Key West, at the Hôtel, and elsewhere, yet in no instance did they communicate the disease to anyone. The clothes of the sick were washed ashore, but I never heard of the disease being spread in that way; in fact, everything tended to show that the disease did not spread from person to person, but arose in each case separately from a common local source.

With regard to the contagiousness or non-contagiousness of yellow fever, we find the most contradictory statements made, and the most directly opposite views strongly—not to say vehemently—insisted on. A commission of the General Board of Health made a careful inquiry into the subject in 1852, and came to the conclusion that the disease was a non-infectious one, and their report² contains a mass of

¹ One doubtful case is here included, which was not amongst the number given in my former paper.

² Second Report on Quarantine, Yellow Fever, 1832.

strong evidence in support of their conclusions. For example, the committee of physicians who reported on the epidemic at Barcelona in 1821 said: "The danger, so far from being in the direct ratio of exposure, was positively (in many instances) in the reverse ratio. In the marine lazarette, in which, from Aug. 7th to Sept. 13th, there entered seventy-nine sick (of whom fifty-five died), not one out of thirty-two of all classes of officers and attendants contracted the disease. In the lazarette of the Vice-Queen of Peru, which received fifty-six sick (of whom thirty-nine died), out of twenty-three persons of various classes who attended them, four only contracted the disease, and these had come out of Barcelona. In the Hospital of the Seminario, into which 1767 persons were admitted during the epidemic (of whom 1293 died), out of ninety attendants on the sick, three only contracted the disease, which is but at the rate of 1 in 30, constituting a far greater exemption from sickness than was enjoyed by any other portion of the community." "During the late epidemic of British Guinea," says Dr. Blair, "the yellow fever cases in their worst forms were never separated from other patients in our hospital wards. Our hospital nurses were not infected, although in the closest communication with the sick, and often smeared with their ejections, and these nurses were generally German and Portuguese immigrants. In December, 1843, the mate of the *Matilda Luckie* was admitted with the graver form of the disease, and of a low type, of which he died. His bed was in a sheltered corner of Ward 2, and had mosquito netting all round. Into this bed a seaman named Burton, who was admitted for disease other than yellow fever—slight indisposition,—was put for several days without any infection of any kind following. In 1849 yellow fever broke out in two coal ships in the harbour of Kingston, Jamaica. Dr. Milroy reports with regard to them: "Dr. Musgrave informed me that nearly twenty cases of aggravated yellow fever were received from these two vessels into the hospital. A very large proportion of them proved fatal. None of the other inmates or of the attendants of the hospital were affected." An outbreak took place on board H.M.S. *Bedford* at Gibraltar, there being no fever at the time in the garrison, and 130 sick were landed and sent to the hospital, of whom eleven died; others were left dangerously ill on the departure of the ship. The disease did not extend beyond the crew. Mr. Hartle, Deputy Inspector-General of Hospitals, who served in the West Indies during a period of more than thirty consecutive years, states that he has on several occasions witnessed the importation of yellow fever cases of the most malignant character, amounting in all to 107, but he never observed a single instance of the communication of the disease to any individual. Dr. Wood³ says:—"In the terrific epidemic which prevailed in Norfolk and Portsmouth (U.S.) in 1855, though great numbers of the population who fled from the pestilence were scattered through neighbouring and distant villages and cities, and many of them sickened and died, yet in no instance is the disease known to have been imparted to others. Attempts have been made to propagate the disease by inoculation with the blood and secretions of those affected, but without success, and even the black vomit has been swallowed with impunity."

There can be no doubt, however, that yellow fever has frequently been introduced into places previously free from it, and one of the best known instances will serve to show the usual phenomena of such an introduction, and aid in forming an idea of the nature of the disease.⁴ On the 25th of July, 1861, the *Anne Marie* arrived at Saint Nazaire from Havanna, having had nine cases of yellow fever and two deaths during the passage. On her arrival the captain, who was recovering from an attack, and all the crew returned to their homes, leaving the mate to superintend the unloading. This work was commenced on the 27th, seventeen labourers being employed. On the 29th a Government tug, the *Chastang*, which had lain close to the *Anne Marie*, left for Indret, about twenty-nine miles distant, arriving there the same day. Her crew, numbering five, on their return home, had resumed their ordinary work, when, on Aug. 1st, one of them was attacked with yellow fever, and between that date and the 5th the remaining four were also attacked. They were treated at their own homes at Indret. On Aug. 2nd the mate of the *Anne Marie* was taken ill, and shortly afterwards ten of the labourers, and thirteen other persons living at Saint Nazaire and its neighbourhood were attacked

in quick succession. They were treated at home, and fifteen died. The *Cormoran*, which lay for four days alongside the *Anne Marie*, and for six days more near her, left for Lorient, with a crew of six men, on Aug. 10th, arriving at her destination the same day. On the 14th two cases occurred amongst her crew, which were admitted into hospital, and terminated fatally. The steamboats running between Saint Nazaire and Lorient lay near the *Anne Marie*. One of them (No. 6), which left Saint Nazaire on Aug. 4th, had two of her crew attacked during the passage. One was taken into the hospital, where he died, and the other was treated at home, and recovered. Four persons on board two lighters lying near the *Anne Marie* were indisposed, and it was thought that their malady was a slight form of yellow fever. The *Arequipa* left Saint Nazaire (having also been lying near the *Anne Marie*) for Cayenne on Aug. 1st. On the 5th a case occurred, and from that date until Sept. 23rd seven others were seized, with a total of three deaths.

In considering this outbreak, it is to be observed—(1) That the convalescent captain and the crew returned to their homes, but in no case did they communicate the disease to anyone. (2) That in all the cases (forty-one, and five doubtful) except four, the patients had either been on board or in the immediate neighbourhood of the *Anne Marie*. (3) That of the four exceptions, one was an old-clothes-seller (*revendeuse*), a woman who bought clothing, bedding, old sails, &c., from sailors, and thus may have got into her possession articles from the *Anne Marie*. Another was a woman who consorted chiefly with sailors, and whose house was a place of call for them and men working on board ship. Besides it is quite possible that they also had been in the neighbourhood of the ship. The third exception was in the case of a shoemaker, whose assistant worked as a labourer on board the *Anne Marie*, and returned to his place beside his master, his "*vêtements encore tout mouillés*." (4) That all the patients were treated at home or in hospital, at Saint Nazaire, Indret, or Lorient (except those on the *Arequipa*), and that there was only one case in which the disease was apparently communicated to anyone attending on them. This was the fourth exceptional case alluded to above. The patient was a medical man living at Montoir, about five miles from Saint Nazaire, and who was attacked on the 13th, after paying a long visit to a yellow fever patient on the 11th, and died on the 17th.

The evidence of the occasional transmission of the disease by persons or things coming from an infected locality (ship or place), somewhat indistinctly shown in the *Anne Marie* case, was more strongly brought out in the epidemic at Lisbon in 1857.⁵ There the original focus of the disease was at the Custom-house, and fresh foci were formed by persons coming from the infected spot.

Sir W. Payne, who was one of the most ardent supporters of the doctrine of contagion, has brought forward a great deal of evidence in support of his views in his "Observations on Yellow Fever." He is strongly of opinion that the disease is personally contagious, like small-pox or scarlet fever.

The conclusions to be drawn from a consideration of the history of yellow fever and yellow fever epidemics seem to me to be as follows:—It is an intensely local disease, and local in more ways than one. It is confined chiefly within certain geographical limits, and requires a certain temperature for its existence, and is essentially a maritime disease, being found, with few exceptions, in ships, harbours, and in towns situated on the sea-coast or on the banks of navigable rivers. It is frequently confined to only one quarter of such town, and often to particular houses, or even to particular parts of a ship. Hirsch⁶ says that out of 297 epidemics in America, 159 were in coast towns, 133 in towns on navigable rivers, and only 5 in places from nine to twelve miles inland. It may be transported by ships, which, when they have been infected, become moving local centres of disease, and by them implanted in ports where they arrive. When yellow fever spreads it does so by the successive formation of fresh local foci, these being produced by persons (whether in good health or suffering from the disease), or through coming from the original or a previously existing focus. The disease is, in the vast majority of cases, contracted by persons living in or going into a place where it is prevalent, or, in a much smaller number, by those who have been in contact with persons or things coming from such a place.

³ Practice of Medicine, vol. i., p. 327.

⁴ Mélier, Relation de la Fièvre jaune survenue à Saint Nazaire en 1861.

⁵ Mélier, op. cit.

⁶ Ueber die Verbreitungsart von Gelbfieber. Vierteljahr. für öffentl. Gesundheitspflege. B. iv., Heft 3.

The local character of yellow fever is strongly shown by the numerous instances in which numbers of persons, many of them suffering from the disease, have left an infected locality and settled down at a short distance from it, with the result of very speedily checking the progress of the epidemic amongst them. During the Gibraltar epidemic of 1828 four thousand persons left the town, and were encamped in the neutral ground, taking with them their furniture, &c. The disease was checked at once.⁷ At Leghorn, in 1804, six thousand persons left that city for Pisa; at the same time the French army removed to the same place, taking with them 180 men suffering from the disease; yet it was not propagated in Pisa. A good instance of the same kind also occurred at Bermuda in 1864.⁸ The 2nd Batt. 2nd Regt. arrived at St. George on July 15th, five days after the occurrence of the first case of yellow fever in the town, and remained there until August 1st. During this time seventeen cases had occurred, with two deaths. The battalion was then sent to the camp at Ferry-point on the same island. Up to September 3rd men were being constantly sent into St. George on duty, and cases occurred amongst them. After this date, however, coloured men were employed in the work, and the disease soon disappeared. At the conclusion of the epidemic the convalescents were paraded and questioned, when it appeared that out of sixty-one cases only seven had, in all probability, originated at Ferry-point. It is interesting to note that at Ferry-point there were thirty-four orderlies attending on the sick, of whom four were attacked; while at the hospital at St. George, out of the thirty-four so employed, thirty were attacked and eighteen died.

It is needless to point out that the characteristics of yellow fever are not those of an ordinary infectious disease. Several theories have been brought forward to account for the contradictory facts observed. I myself am disposed to hold provisionally the view that yellow fever is a miasmatico-contagious disease (to borrow a German expression) analogous to cholera and to enteric fever. It would seem—to speak in terms of the germ theory—that the germs in the blood of a patient cannot reproduce themselves in the blood of another person, but that they require to undergo further development, or to produce a second generation, in a suitable nidus, before they, or the new generation, can multiply in the human body and produce yellow fever. This nidus is chiefly found on board ship, or in seaport towns; and yellow fever may be propagated by its being conveyed, with the disease-germs in it, by ships, persons, or articles. The germs in this nidus at the time of its conveyance may be in an advanced state of development, and ready at once to seize upon the human frame. Under favourable circumstances fresh foci of disease may thus be formed, or persons may be directly infected. The disease seems at times to spread with greater facility than at other times, especially when the temperature is unusually high. This may be due to the rapid development and great activity of the germs.

CASE OF CONSECUTIVE EXCISION OF BOTH KNEE-JOINTS FOR DISEASE,

TERMINATING IN RECOVERY.

BY JAMES BARRON, M.R.C.S. ENG.,
HON. SURGEON TO THE SUNDERLAND INFIRMARY.

STEPHEN H—, aged forty-five years, married, was admitted into the Sunderland Infirmary on May 29th, 1877. Eight years ago, whilst at sea as a ship-carpenter, and sleeping frequently in wet clothes, his knees began to be painful and swollen, with nocturnal exacerbations of pain. Returning from this voyage, he did not feel able again to go to sea, but commenced work as a shipwright on shore, and so continued until the summer of 1875. In this interval he seems to have been regularly at work, but the knees were gradually becoming more swollen and the pain more severe. About this time he was recommended to go to Croft Spa, and was there some weeks taking the sulphur baths; but not experiencing the benefit anticipated, he was removed

thence, and admitted into the Durham County Hospital, where he remained seven weeks. He came home, however, unrelieved, and resumed work, though the swelling was still increasing and the knees were now becoming somewhat flexed. On the 12th October, 1876, he was admitted into the Edinburgh Infirmary, where he was first, for three weeks, placed in the surgical wards. After that time he was transferred to the medical wards, and the knees were painted and blistered. After eleven weeks, receiving no benefit, he was discharged, and he returned home in the last week of December, 1876. When he left the Edinburgh Infirmary the contraction of the knees had so much increased that he was unable to walk about.

Five months after leaving Edinburgh he was admitted into the Sunderland Infirmary under my care. On admission he was in a somewhat debilitated condition, but, except in the knees, there were no physical signs of disease. Examining his knees I found them fixed at a right angle, the thighs flexed and abducted, and the feet approximated, so that his position in bed was much like that of a tailor at his board. The right knee-joint was much swollen and hot; surface glossy, pink, and marked with a network of distended veins. The left knee was less swollen; skin nearly natural in appearance and temperature. The muscles of both legs were much wasted. His previous health had always been good; had never had any illness; had been a steady man, but seven years ago had a slight attack of gonorrhœa, which lasted only a few days, and was not succeeded by any secondary symptoms. Married fifteen years; no family living, but his wife had borne him four children, the first of whom died a few hours after birth, and the others at six, eight, and twelve months respectively, as he said, of inflammation of the lungs. His wife had had no miscarriages. His family history was entirely negative: no rheumatism; no joint affection; no chorea; and no consumption.

For about three weeks after admission iodine was painted on both knees, and a blister was applied, but without any apparent advantage. After much consideration, and consultation with my colleagues, I decided to perform excision of the joints, as the most likely means of relieving my patient from his unfortunate position, and procuring for him, as I hoped, good and useful limbs. He and his family were duly informed of all the possible evil consequences that might result from a failure in the operation, but he quickly decided in favour of my recommendation, desiring me, if possible, to save his leg.

First operation.—On June 20th, 1877, with the kind help of my colleagues, the patient being placed under the influence of chloroform, I proceeded to excise the right knee-joint. The operation was performed in the usual way, by a curved incision, and under complete antiseptic precautions. On opening the joint, the synovial membrane was found in an advanced stage of gelatinous degeneration; the cartilage on the inner condyle of the femur was ulcerated, as was that on the corresponding surface of the tibia; and there was partial dislocation of the tibia backwards. Before closing the wound, as much as possible of the diseased synovial membrane was removed. A drainage-tube was inserted along the floor of the wound from side to side, and the limb put up straight on an ordinary Macintyre splint.

There was nothing particular to record in the progress of the case. The drainage-tube was removed on the 29th of August (the 70th day of treatment); and on September 15th (the 87th day) bony union was complete, and the splint was discarded. During this time, the swelling and heat of the left knee had subsided, and there was partial bony ankylosis.

Second operation.—The first operation having been so successful, I was encouraged to proceed with the excision of the left knee, and accordingly on October 10th, I performed the second operation. This was done, as before, under complete antiseptic precautions. On opening the joint, there was found bony ankylosis between the tibia and posterior portion of the articular surface of the femur, the tibia being dislocated backwards. In the first operation, adaptation of the bones was easily accomplished; but in the second perfect adjustment was not effected until a second piece was removed from the tibia, and two or three of the flexor tendons divided. The patella was removed in both operations. The case progressed even more favourably than on the first occasion. The drainage-tube was removed on Dec. 10th (the 61st day of the treatment), and bony union being complete on Jan. 18th, 1878 (the 100th day), he was allowed to be up for the first time, the two operations having been brought to a successful termination within a

⁷ Board of Health's Second Report on Quarantine.

⁸ Army Medical Report for 1863, p. 307.