iodoform gauze plug placed in the abdomen during a gall-stone operation ulcerated its way into the bowel and caused symptoms of obstruction. This was an operation performed in the presence of infection and the passage of the plug into the lumen of the gut was clearly due to infective ulceration from its peritoneal aspect, the site of entry being healed over and protected by adhesions. It was successfully removed some 14 days after the gall-stone operation.

The consideration of the secondary infections of drained wounds would be incomplete without reference to aerial contamination, and whereas such infection may be held to be very rare during the course of an operation it is well known that any wound, abdominal or otherwise, treated by tube drainage for any length of time is apt to yield cultures of such organisms as the bacillus proteus in addition to the original causative bacterium, or this latter may be entirely outgrown by such a contaminating bacillus. It is clear, therefore, that in the case of persistently drained abdominal wounds the primary infection may have added to it organisms from without or within; the former may be derived from the external air or implanted during the dressing of the open wound, the latter may come from the lumen of the bowel as the result of pressure on the bowel wall.

From these experimental and clinical observations the following conclusions appear to me to be justified. 1. That owing to the adhesions which rapidly form around drainage-tubes their value is distinctly limited in the treatment of peritoneal infections. 2. That since the tissues of the abdominal wall possess a much lower degree of resistance than the peritoneum, drainage of the former may frequently be called for where drainage of the peritoneum is not necessary. 3. That the presence and pressure of a drainage-tube in peritonitis may determine the transudation of organisms from the lumen of the gut to the peritoneal cavity.

With regard to the last observation this secondary infection is of less serious importance than might be supposed owing to the fact that the wound area has usually been shut off by surrounding adhesions before this occurs. To prevent adhesions between drainage-tubes and adjacent structures the tube should always be rotated whenever the dressing is done, and its gradual withdrawal from the wound should be begun from the third day after operation. I have said nothing about so-called gauze drains because I believe they are more properly termed gauze dams, except when used in the form of a small wick down the lumen of a tube as a "cigarette drain," and there the capillary action of the gauze may be of value in securing the escape of thin inflammatory fluid. In the case of thick pus they more often act as a cork at the orifice of the wound than as an external drain.

As to the value of the Fowler position and the pelvic drainage-tube there is still some difference of opinion. I think there can be no doubt that the former represents the ideal attitude of the patient both for the surgeon and the nurse during the early days of abdominal convalescence, and it seems clear that the complications of subphrenic, perisplenic, and other residual abscesses have been greatly reduced in frequency by its adoption. Pelvic drainage-tubes do not appear to form adhesions quite as readily as those in other situations, and when the tube is correctly placed so that its deep end lies about half an inch from the bottom of the pelvic basin drainage even in this uphill fashion can be secured for the first few days of convalescence.

At the present time it seems totally irrational to be a slavish adherent to the policy of either always draining or never draining, but it appears to me that much of the drainage of the peritoneal cavity now practised would be better limited in scope to the abdominal wall alone.

Devonshire-place, W.

THE WILD MONKEY AS A RESERVOIR FOR THE VIRUS OF YELLOW FEVER.

BY ANDREW BALFOUR, C.M.G., M.D. EDIN., F.R.C.P. EDIN.,
DIRECTOR-IN-CHIEF, WELLCOME BUREAU OF SCIENTIFIC RESEARCH.

THE possibility of the wild monkey acting in certain places as a reservoir for the virus of yellow fever is a very interesting one. On my way to the West Indies I was told by a lady, Mrs. Randolph Rust, who has been long resident in Trinidad, that the old negroes there say they can always tell when there is going to be an epidemic of yellow fever in the island owing to the fact that prior to its appearance the red howler monkeys are found dying and dead in the High Woods. In view of Manson's injunction to search for a reservoir of the virus amongst some of the lower animals this negro belief struck me as curious and interesting, though naturally one did not place overmuch credence in it. Still it seems worthy of investigation, and Mr. Randolph Rust, the chief pioneer of the petroleum oil industry in Trinidad, whose land is in the south of the island at a place where monkeys abound, kindly promised to make further inquiries.

When I finally reached Port-of-Spain this question of the monkey assumed fresh interest and importance. Dr. H. L. Clare, the Surgeon-General, gave me particulars regarding the recent small outbreak of yellow fever at Brighton near the pitch lake in the south-west of Trinidad. He told me that the origin of the infection was shrouded in mystery, for there was no question of importation from the mainland or elsewhere, and, so far as could be told, there had been no mild cases of fever or illness amongst the coloured employees of the American company which is boring for oil in this vicinity. Further, the two men first attacked were employed during the day on an oil bore situated at the end of a road which had recently been cut for eight miles into the dense virgin forest. In this locality the red howlers occur, though they are only seen as a rule in the early morning. I told Dr. Clare the story about the negro belief. He had not heard of the latter, but was much interested, and agreed that it merited careful inquiry and investigation. At a later period I found several people who knew all about the negro view, but it had not struck them as deserving special attention. Mr. Rust got hold of his chief hunter, who confirmed the negro statement in every particular, and said that before the last large epidemic of yellow fever in Trinidad the red howlers were found dying and dead in large numbers. Further confirmation was forthcoming from Mr. F. W. Urich, entomologist to the Board of Agriculture, an acute and trained observer, who told me that he himself had seen the monkeys lying dead, and at the time thought they had probably perished from the effects of a protozoal

blood parasite. He had not, however, specially associated this mortality with the subsequent outbreak of yellow fever.

Dr. Clare very kindly let me see the report written by Mr. A. L. George, the colonial medical officer who was sent to make inquiries as regards the Brighton outbreak. Briefly, the chief points are as follows. There were 10 cases of undoubted yellow fever, 9 amongst Americans, 1 in a Trinidadian. Three of the cases, including the first, proved fatal, and the diagnosis made by Dr. Campbell, medical officer to the Petroleum Company, was confirmed by post-mortem examination made by Dr. J. R. Dickson, who has charge of the public health laboratory at Port-of-Spain. It is only necessary to consider here such facts as may have a bearing on the monkey question.

Case 1, an American, had been five days in Trinidad. He had been in the West Indies before for one and a half years. He took ill on Nov. 21st, 1913, and the illness, as stated, terminated fatally. Case 2, an American, had been one year and seven months in Trinidad. He had never been in Trinidad prior to this period of residence. *He was also attacked on Nov. 21st, 1913*, and though he had the black vomit recovered. Cases 3, 4, and 5 were all attacked on Dec. 5th, 1913. Cases 3 and 4 were living at a bungalow in the forest, and Case 5 worked in the forest during the day, returning to Brighton at night. These three cases and those following them can all be explained by the ordinary man-mosquito infection, but the first two cannot be so easily accounted for and require some consideration. Case 1 had arrived in Trinidad from the Venezuelan mainland five days before being struck down, but Case 2, *who was taken ill almost at the same time as Case 1*, had not been out of Trinidad for one year and seven months.

Mr. George in his report expressly states: "There is not the slightest evidence that the disease was conveyed here from any outside source." His own view was that in all probability there must have been a mild unrecognised case amongst the coloured population, but he did not think this very probable, and it was admitted that the source of the outbreak was very puzzling. At my request Dr. Clare most kindly accompanied me to the oil-fields on Feb. 14th. The last case of yellow fever, and one which had terminated fatally, had occurred on Dec. 30th, 1913.

I had the pleasure of meeting Mr. George and Dr. Campbell and discussing the matter with them. It was ascertained that the two men first and well-nigh simultaneously attacked were, as already stated, working on an oil well at the end of the new asphalted motor road which had been cut into the heart of the High Woods, and which is a remarkable, indeed wonderful, engineering exploit. Further, it was found that these men spent ten hours a day on the oil-fields, going there every morning from Brighton and returning every evening. A bungalow stood in the clearing at the end of the road, and at the time of his inspection Mr. George found stegomyia fasciata vel calopus breeding there. I was informed that monkeys occurred in the woods, but there was nothing to show that they had been sick or dying. Attention had not been directed to the question. A point of interest is that there exists in these High Woods a forest species of stegomyia—namely, S. sexlineata—which closely resembles the known yellow fever carrier.

I had hoped to secure some specimens of blood from monkeys, but though I penetrated a short way into the forest, an education in itself, I saw nothing of the animals, which can only be approached when they are feeding, and then only by hunters familiar with their habits. It would, however, be interesting to examine the blood of these red howlers, both in the case of healthy and sick individuals, if the latter can be obtained. If Seidelin's paraplasma flavigenum was found to occur in their red cells it would certainly be a remarkable coincidence and perhaps go some way towards establishing the importance of the negro belief. At the same time there may be nothing in the latter.

Thanks to the kindness of Dr. Clare, to whom I am greatly indebted for much assistance when in Trinidad, of Mr. George, Dr. Campbell, and Mr. Urich, who also helped me greatly, I hope eventually to procure specimens of monkey's blood from Trinidad. In any case Dr. Clare intends to make further inquiries, so that possibly fresh light may be thrown on the question. My own time was, unfortunately, very limited, as I had a long journey before me. An attempt to get at monkeys in another part of the island ended in failure.

From Trinidad I proceeded to Ciudad Bolivar on the Orinoco, but yellow fever is rare there and, according to Dr. T. W. Parry, only imported cases occur. I was, however, told by a Canadian mining engineer that in the vicinity of Maracaibo in north-western Venezuela it had been noted that the men of the Caribbean Petroleum Company who caught yellow fever were those employed out in the bush. Accordingly, I decided to go to Maracaibo and prosecute inquiries there. On my way I visited Caracas and met Mr. Proctor, the manager of the company in Venezuela. He told me that my information was erroneous, and that the men attacked had all been resident in the town of Maracaibo. He had been much interested in the Trinidad outbreak, and informed me that, being greatly puzzled as to its origin, he had written to New York asking if monkeys were known to suffer from yellow fever as they were almost the only wild animals found near the oilfields. This is interesting as showing the view of a layman conversant with the local conditions.

On reaching Maracaibo I had the pleasure of meeting Dr. J. M. Coates Cole, who is ably upholding the prestige of British medicine in this out-of-the-way part of the world. He confirmed Mr. Proctor's statement. It seems that monkeys are not found at Maracaibo itself, but they occur along the banks of a river some 50 miles distant, and are also found abundantly in the bush on the other side of the lake, which, opposite the town, is some eight miles across. I could learn nothing about epidemics amongst monkeys there, but have set inquiries afoot and hope to obtain information through Dr. Cole, who knows the district well. Here I had an opportunity of reading the interesting paper by Dr. J. W. Scott Macfie and Dr. J. E. L. Johnston in the Proceedings of the Royal Society of Medicine, Vol. VII., No. 3, and I trust these observers will direct their attention to the monkeys of the West Coast of Africa.

The conditions in Trinidad were so interesting that, with Dr. Clare's kind permission, I wrote to Mr. H. J. Read, C.M.G., at the Colonial Office, suggesting that workers on the West Coast and elsewhere should be asked to make inquiries as regards monkey mortality, and to secure the blood of monkeys for examination.

There must, of course, be many places where

monkeys can play no possible part in the etiology of yellow fever. Such a place is Curaçao, where there are no monkeys, but where yellow fever is said to be endemic and where certainly from time to time epidemics occurred, notably when warships from Holland containing non-immunes visited the island. At the same time, wild monkeys may conceivably act as a reservoir of the virus, be this Seidelin's bodies or some unknown parasite, in certain localities; and if such proved to be the case some puzzling outbreaks, like the recent one in Trinidad, would be explained. In any case it certainly seems advisable to direct attention to the possibility, and in this connexion I would cite a passage in a paper by Dr. J. Martinez Santamaria on the Tropical Diseases of Colombia which appeared in the *Journal of Tropical Medicine and Hygiene* of April 1st, 1913. Speaking of yellow fever he says:—

> This occurs endemically and at long intervals along the Atlantic coast, Cucuta, all along the shores of the Magdalena river, and at the Muyo emerald mines. The last focus is important, as it is situated right in the middle of the country and in an uncultivated and isolated zone, with no possible communication with the Magdalena River, from which it is separated by thick and immense forests. The epidemic there is quite independent of the other focus. For the last ten years we have not had a single case in the Magdalena zone, while in Muyo three epidemics have occurred.

I am now on my way to Colombia, where I hope to obtain some more information as regards the possible association of the wild monkey, and more especially the red howler, with the occurrence of yellow fever.

Curaçao.

VACCINATION AGAINST HAY FEVER:

REPORT OF RESULTS DURING THE LAST THREE YEARS.

BY JOHN FREEMAN, M.D. OXON.,

ASSISTANT IN THE INOCULATION DEPARTMENT AND ASSISTANT BACTERIOLOGIST, ST. MARY'S HOSPITAL, LONDON.

IN 1910 and the early part of 1911 the late Mr. L. Noon worked at active immunisation against hay fever by inoculations with a pollen vaccine. He was compelled by an illness which proved fatal to stop this work in the spring of 1911 before it had been actually put to the test in a hay fever season. He published his methods of work up to date in THE LANCET of June 10th, 1911. I was able to give the first results of my colleague's method of treatment as proved by the summer of 1911 in a paper published in THE LANCET of Sept. 16th, 1911, and I now propose to give further results.

Table I., How Compiled.

In the table given below the results are shown in the case of 84 patients with English hay fever who have been inoculated with phleum pratense pollen. The first 18 in the list were inoculated before the summer of 1911; the figures in brackets correspond to the serial numbers in the report of these cases published in THE LANCET of Sept. 16th, 1911. Nos. 19 to 43 were inoculated before the summer of 1912, and Nos. 44 to 84 were inoculated before the summer of 1913. This list of 84 cases is a complete one, and therefore includes cases which received inadequate treatment.

In the case of many patients it was found convenient, after they had undergone the preliminary eye-testing and had received from me the initial inoculations, to have the treatment continued by the local practitioner. In nearly all these cases the treatment was carried out as well as possible, but there were exceptions, which are dealt with under heading No. 2 when dealing with the discrepancy in result.

The value of the symbols by which the various results have been indicated is shown underneath the table, and some further indication of the standard of marking may be gained by comparing the marks here awarded to the first 18 cases inoculated in the year 1911 with the more detailed report of these same results published in the earlier paper.

The results here recorded have in nearly all cases been derived from the patient's own report. For instance, whenever the patient has reported after the hay fever season that the doses "did not seem to make much difference," the result has been recorded by the symbol "0." In the two cases where the "—" sign has been employed there was no suggestion that the treatment had been injurious, and the "0" would, in my opinion, have been more in accordance with fact. Nevertheless, since the reports of the patients suggested that the benefits received from treatment had been a minus quantity, that result has been ruthlessly recorded.

TABLE I.

Cases.	1911.	1912.	1913.	Cases	1912.	1913.	Cases.	1913.
1 (1)	+++	.....	.....	29	0	.....	57	++
2 (3)	0	.....	.....	30	++	.....	58	+
3 (4)	++	.....	.....	31	++	.....	59	+
4 (5)	+++	+++	.....	32	+++	+++	60	++
5 (6)	+++	+++	+++	33	++	.....	61	+
6 (7)	+	.....	.....	34	+	.....	62	+
7 (8)	++	.....	.....	35	++	+++	63	+
8 (9)	++	++	+	36	.....	++	64	++
9 (10)	0	+	+	37	+	.....	65	0
10 (11)	++	+	.....	38	++	+++	66	++
11 (12)	+++	+++	+++	39	++	+++	67	+++
12 (13)	++	.....	.....	40	+	++	68	++
13 (14)	++	++	.....	41	+	++	69	+++
14 (16)	++	+++	.....	42	+	.....	70	++
15 (17)	+++	.....	.....	43	0	++	71	+++
16 (18)	+++	.....	.....	44	.....	+++	72	+
17 (19)	+	++	.....	45	.....	+	73	++
18 (20)	+	++	+	46	.....	+++	74	0
19	.....	+	++	47	.....	0	75	0
20	.....	++	++	48	.....	+++	76	++
21	.....	++	++	49	.....	+++	77	+
22	.....	+	+	50	.....	-	78	++
23	.....	+++	.....	51	.....	0	79	0
24	.....	++	.....	52	.....	+++	80	++
25	.....	+++	+++	53	.....	+++	81	+
26	.....	++	+++	54	.....	+++	82	+++
27	.....	—	.....	55	.....	0	83	++
28	.....	++	+	56	.....	+++	84	+++

The symbols by which the results in each year are indicated have the following values +++ = complete freedom from hay fever, or only the most insignificant symptoms. ++ = hay fever symptoms greatly diminished. + = hay fever symptoms diminished, but only to a slight degree. 0 = symptoms unchanged. — = symptoms worse. ..... = patient not inoculated in that year.

Results.

It will be seen from the table that the average of success shown in the earlier paper has been fairly well maintained in the succeeding years, both with the old cases and with the cases commenced since then. For the convenience of the reader they are here abstracted. The 84 cases have between them experienced 113 hay fever seasons