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LITHOLAPAXY

(MODERN LITHOTRITY):

One Hundred and Eleven Cases.

BY

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PREFACE.

THE following paper, which recently appeared in *The Lancet*, is now published, in pamphlet form, at the suggestion of several of my fellow-labourers in this country.

The favorable manner in which my work in Litholapaxy has, from time to time, been reviewed by the Medical Journals at Home, in America and in India, encourages me in the hope that this monograph may contain some original observations of practical interest to the litholapaxist.

P. J. F.

Mussoorie, 1885.

One Hundred and Eleven Cases

OF

LITHOLAPAXY.

BY

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In the December number of the *Indian Medical Gazette* of 1882, the February number of 1883, and the March number of 1884, three papers of mine appeared, in which full details of my first seventy-six cases of litholapaxy were given. Having now completed 111 cases of the operation, I propose dealing with them comprehensively in the present article, repeating some remarks recorded in the papers above alluded to, and adding some further observations of a practical nature which I have learnt from an increased experience of the operation. It is hoped that the record of a large number of cases of this operation from the practice of a single operator may prove interesting to the profession at large, and that the results obtained may have some effect in bringing into more general practice amongst my professional brethren in India an operation which, though still in its infancy, has, undoubtedly, a brilliant future in store for it.

The extreme aversion with which the natives of India

regard any mode of treatment that involves several distinct surgical proceedings, extended over an unlimited period, is well known to every surgeon who has practised amongst the people of this country. The knowledge of this fact alone, putting aside altogether the comparative merits of lithotrity and lithotomy, was sufficient to deter surgeons from practising the old operation of lithotrity. When, however, some five years ago, Professor Bigelow, of Harvard, U.S., introduced his new operation of "litholapaxy" to the notice of the profession, and, scattering to the winds all previously held theories as to the deleterious effects of prolonged instrumentation in the bladder, demonstrated, by a series of successful cases, that, instead of subjecting the patient suffering from stone to a number of short sittings extended over an indefinite period, the calculus might be crushed and evacuated at one sitting, the chief objection to the crushing operation, so far as this country was concerned, seemed to have been removed. As with many of my professional brethren in India, lithotomy in my hands had proved a fairly successful operation. I did not therefore at first abandon the cutting for the crushing operation. I must confess, however, that the main cause of my hesitation in adopting the new operation was the depreciative manner in which it was criticised by Sir Henry Thompson on its first introduction. I need scarcely say that English surgeons have been in the habit of receiving as almost equivalent to divine law the utterances of that distinguished surgeon on any point connected with the surgery of the urinary organs. When, therefore, I read of the "disastrous" results that he anticipated from Bigelow's operation, I naturally hesitated in adopting it. And it was not till I

had subsequently read of the brilliant results Sir Henry himself had obtained from that operation, with reference to which he had uttered such gloomy forebodings, that I finally decided on giving the operation a trial.

During the period that has elapsed since I adopted the operation of litholapaxy in my practice 203 cases of stone in the urinary passages have come under my immediate treatment. Amongst these there were four cases of impacted urethral calculus, in all of which external urethrotomy was successfully performed. There were seventy-five cases of vesical calculus in male children or lads under sixteen years of age. Lateral lithotomy was the operation performed by me in all these cases, and amongst them there was no death, all having made excellent recoveries. In the remaining 124 cases, all of which occurred in adult males, with the exception of three female children, the operation of litholapaxy was entertained, but for various reasons there were thirteen instances in which the operation could not be performed. Five of these latter were the subjects of severe stricture of the urethra, so that the instruments would not pass into the bladder; in one case there was a greatly enlarged prostate with tortuous urethral canal; and in seven cases the stone was so large that it could not be grasped by the lithotrite, or so hard that after being grasped the lithotrite could make no impression on it. In twelve of these cases I performed lateral lithotomy, with two deaths. In the remaining case the calculus was extremely large and hard, weighing over twelve ounces. In this case the suprapublic operation was performed, but the patient died six hours afterwards.

I have considered it advisable to mention the above

facts so that it may be clearly understood that the operation was given a fair trial, having been performed in every possible instance in the adult without reference to age, state of health, size of calculus, &c. It is in this way only that a true estimate of the comparative value of the operation can be obtained, and not by performing it in selected cases only.

A detailed description of the operation of litholapaxy would be foreign to the aim and scope of this paper. A word, however, about the instruments employed. The lithotrites used by me are: a large fenestrated instrument for crushing large and hard calculi; a smaller one of the same description for medium-sized and small stones; and a flat-bladed lithotrite for crushing débris. All these lithotrites are constructed on the well-known model of Thompson and Weiss, and admirable instruments they are. Recently I have had a very large and powerful fenestrated lithotrite constructed for me by Weiss on the same model. Its crushing power is excellent; but in dealing with very large and hard calculi, it might be rendered more efficient by some slight alterations that I shall have to suggest further on. The aspirator used by me is that modification of Bigelow's instrument known as Thompson's, and subsequently improved by Weiss. The evacuating canulæ vary in size from No. 12 to No. 18. Larger than the latter I have not used, that being the largest size I possess. I have, however, met with a few cases in which a larger size might have been passed with ease, and I have, consequently, ordered a No. 20 canula. The larger the evacuating canula, the less necessity there will be for crushing the calculus into fine powder, and, consequently, the less time will the operation require for its performance—a matter

of no small importance when we have to deal with a large stone in a patient whose constitution has been very much worn by the disease. It is, therefore, advisable to employ the largest canula that will pass with ease into the bladder. I cannot too strongly deprecate the use of any force in passing a catheter, or, indeed, any instrument, into the bladder; but the deleterious effects which Sir Henry Thompson anticipated from the use of large instruments experience has shown to be mythical. Sir Henry says that the instruments should be proportionate to the size of the stone; but experience has taught me that the capacity of the urethral canal is of much more importance in determining the size of the instruments, and that the largest lithotrite and canula that can be passed without the use of any force should be employed. A large lithotrite is much handier in the bladder, less liable to get clogged by débris, much more efficient not only for crushing large calculi, but for disposing of fragments of débris, than a small one, and I fully agree with Bigelow that when one gets accustomed to the use of a large lithotrite he does not willingly abandon it for a smaller instrument.

The accompanying table will show at a glance some of the most important points connected with the 111 cases of litholapaxy which are the subject of this paper. The subjects of these operations were all natives of India. All, with the exception of a few cases in private practice, were in-patients of the Civil Hospitals at Moradabad and Bareilly, throughout the whole course of their treatment. Detailed notes in every case have been kept by my assistant-surgeons and myself, and no case has been permitted to leave the hospital till he had quite recovered.

Table showing Particulars of 111 Litholapaxy Operations.

Serial No.	Date of operation.	Age.	Sex.	Caste.	No. of days in hospl.	No. of sittings.	Size of canula.	Weight of calculus.			Variety.	Time occupied by operation.	Duration of disease.	Result.
		Years.					No.	Oz.	Drs.	Grs.		Minutes.		
1	3rd July, 1882	50	M.	H.*	10	1	18	—	4	—	Phosph.	30	9 months	Successful
2	25th „ „	85	M.	M.†	10	1	16	—	6	—	Uric	25	3 years	„
3	4th Aug. „	55	M.	M.	13	1	16	—	1	20	„	10	9 months	„
4	6th Sept. „	65	M.	M.	10	1	18	—	3	55	Phosph.	25	5 years	„
5	14th „ „	65	M.	M.	6	1	16	—	6	40	„	35	2 „	„
6	15th „ „	50	M.	H.	5	1	18	—	4	—	Oxalate	25	4 „	„
7	22nd „ „	65	M.	H.	7	1	18	1	3	—	Phosph.	18	½ „	„
8	23rd „ „	40	M.	H.	7	1	16	—	1	—	Uric	33	2 „	Died
9	24th „ „	40	M.	H.	6	1	18	—	7	30	„	5	1 „	Successful
10	26th „ „	60	M.	H.	7	1	16	—	2	—	Phosph.	25	3 months	„
11	17th Oct. „	45	M.	H.	9	1	18	—	7	5	Oxalate	12	2 years	„
12	23rd „ „	20	M.	H.	5	1	16	—	2	50	„	25	4 „	„
13	2nd Nov. „	50	M.	H.	5	1	18	—	—	15	Uric	11	4 „	„
14	17th „ „	35	M.	M.	8	1	16	—	5	25	Oxalate	6	20 days	„
15	28th „ „	40	M.	M.	10	1	16	—	5	35	„	20	12 years	„
16	6th Dec. „	60	M.	M.	11	1	18	3	2	—	Phosph.	23	2 „	„
17	6th „ „	36	M.	M.	7	1	16	—	2	5	Uric	66	3 „	„
18	19th „ „	35	M.	H.	7	1	14	—	—	45	Phosph.	8	1 „	„
19	27th „ „	40	M.	M.	12	1	16	1	2	—	Uric	5	4 „	„
20	1st Jan., 1883	40	M.	H.	12	1	16	—	—	10	Phosph.	25	12 „	„
21	1st „ „	18	M.	H.	12	1	16	—	—	50	Oxalate	2	1 month	„
22	3rd „ „	55	M.	M.	13	1	16	—	5	—	Uric	10	6 „	„
23	6th „ „	41	M.	H.	5	1	14	—	—	15	„	25	3 years	„
24	9th „ „	48	M.	M.	5	1	18	—	—	20	Oxalate	5	2 days	„
25	17th „ „	32	M.	M.	11	1	18	1	1	—	Uric	8	9 months	Died
26	17th Feb. „	17	M.	H.	9	1	16	—	5	40	Oxalate	35	2 years	Successful
27	19th „ „	40	M.	H.	11	1	16	—	2	55	Uric	25	2 „	„
28	22nd „ „	40	M.	M.	7	1	18	—	2	—	Phosph.	25	5 „	„
29	5th March „	46	M.	H.	22	1	16	—	2	10	Uric	10	3 „	„
30	19th „ „	35	M.	M.	21	1	16	—	3	5	„	25	2 „	„
31	4th April „	32	M.	H.	8	1	16	—	6	30	Phosph.	20	1½ „	„
32	12th „ „	35	M.	H.	8	1	16	—	—	35	„	35	2 „	„
33	12th „ „	50	M.	H.	8	1	18	1	—	—	Uric	6	1 „	„
34	15th „ „	85	M.	M.	11	2	18	—	6	40	Oxalate	45	2 „	„
								—	—	40	Uric	20	2 „	„
35	16th „ „	55	M.	H.	4	1	16	—	1	20	Phosph.	3	1 „	„
36	20th „ „	65	M.	H.	12	1	16	—	6	—	„	5	½ „	„
37	21st „ „	40	M.	M.	13	1	16	—	2	20	„	35	3 months	„
38	24th „ „	36	M.	M.	11	1	16	—	—	10	Uric	29	3 „	„
39	28th „ „	52	M.	H.	6	1	16	—	2	40	„	5	1 year	„
40	27th „ „	50	M.	M.	8	1	18	—	5	30	„	12	1 „	„
41	8th May „	16	M.	M.	7	1	16	—	3	15	Oxalate	20	1 „	„
42	18th „ „	45	M.	M.	11	1	16	2	2	—	Carb. lime	12	1 „	„
43	19th „ „	17	M.	H.	6	1	14	—	—	40	Uric	70	10 „	„
44	25th „ „	45	M.	H.	—	—	18	2	6	—	„	7	2 „	„
45	28th „ „	19	M.	H.	8	1	16	1	—	40	Phosph.	75	8 „	Died
46	3rd June „	35	M.	M.	12	1	18	—	—	12	Uric	14	½ „	Successful
47	8th „ „	65	M.	H.	9	1	18	1	6	30	Oxalate	6	4 days	„
48	21st „ „	85	M.	H.	9	1	16	1	1	—	Uric	35	4 years	„
49	24th „ „	36	M.	H.	8	1	18	1	4	30	„	29	3 „	„
50	3rd July „	55	M.	M.	6	1	16	—	1	40	„	57	3 „	„
51	8th „ „	65	M.	H.	7	1	18	—	1	30	„	12	1 „	„
52	10th „ „	65	M.	H.	10	1	16	—	1	6	Oxalate	10	3 „	„
53	27th „ „	18	M.	H.	8	1	14	—	5	10	Uric	6	3 „	„
54	1st Aug. „	50	M.	H.	3	1	16	—	—	10	„	25	4 „	„
55	10th „ „	4½	F.	H.	3	1	16	—	1	26	„	2	6 weeks	„
56	12th „ „	85	M.	M.	12	1	18	1	5	44	„	8	1 year	„
57	14th „ „	65	M.	M.	10	1	18	1	5	24	„	55	5 „	„
58	22nd „ „	30	M.	M.	19	1	16	—	5	40	Phosph.	56	3 „	„
59	23rd „ „	32	M.	M.	5	1	16	—	—	19	Uric	35	2 „	„
												10	½ „	„

* Hindoo. † Mahomedan.

Table showing Particulars of 111 Litholapaxy Operations.

Serial No.	Date of operation.	Age.	Sex.	Caste.	No. of days in hospl.	No. of sittings.	Size of canula.	Weight of calculus.			Variety.	Time occupied by operation.	Duration of disease.	Result.
		Years.					No.	Oz.	Drs.	Grs.		Minutes.		
60	24th Aug., 1883	60	M.	H.	7	1	18	—	4	30	Uric	30	2½ years	Successful.
61	25th " "	45	M.	H.	12	1	16	—	2	5	"	10	1½ "	"
62	3rd Sept. "	50	M.	H.	6	1	18	—	—	32	"	5	1 "	"
63	16th " "	36	M.	M.	6	1	16	—	2	26	Phosph.	10	½ "	"
64	16th " "	42	M.	H.	6	1	18	—	3	25	Uric	20	15 "	"
65	20th " "	8½	F.	H.	3	1	14	—	1	30	Phosph.	15	1 "	"
66	2nd Oct. "	4	F.	M.	5	1	14	—	—	57	"	10	3 "	"
67	15th " "	65	M.	H.	15	1	16	—	1	15	"	5	½ "	"
68	26th " "	55	M.	H.	5	1	18	1	—	35	"	34	8 months	"
69	28th " "	26	M.	M.	15	1	18	1	1	35	Uric	60	4 years	"
70	21st Nov. "	80	M.	M.	17	1	16	2	—	55	"	30	4 "	"
71	4th Dec. "	50	M.	M.	8	1	16	—	1	25	Phosph.	6	½ "	"
72	5th " "	35	M.	H.	7	1	16	—	—	11	Uric	3	½ "	"
73	11th " "	50	M.	M.	6	1	18	—	2	45	Phosph.	8	½ "	"
74	13th " "	60	M.	H.	5	1	18	—	1	46	"	10	1 "	"
75	14th " "	26	M.	H.	12	1	16	—	2	35	"	20	3 "	"
76	27th " "	45	M.	M.	1	1	18	—	—	15	Uric	1	2 months	"
77	5th Jan., 1884	49	M.	M.	2	1	16	—	—	13	"	1	2 "	"
78	24th " "	18	M.	H.	6	1	16	—	3	10	"	17	9 years	"
79	2nd March "	75	M.	M.	14	1	16	1	—	—	"	35	3 "	"
80	11th " "	45	M.	M.	6	1	16	1	2	30	"	29	3 "	"
81	25rd " "	70	M.	M.	14	1	16	—	—	30	"	5	3 months	"
82	15th " "	60	M.	H.	5	1	16	—	6	30	"	20	1 year	"
83	16th " "	58	M.	H.	8	1	18	—	5	48	Phosph.	25	2½ "	"
84	29th " "	60	M.	H.	9	1	16	—	2	—	"	10	2 "	"
85	15th April "	75	M.	H.	10	1	16	1	1	40	Uric	54	3 "	"
86	16th " "	60	M.	H.	9	1	14	—	1	54	Phosph.	15	1 "	"
87	25th " "	69	M.	M.	6	1	18	—	1	3	"	5	3 months	"
88	23rd May "	45	M.	H.	11	1	16	1	4	—	{Uric and Phosph.}	11	2 years	"
89	23rd " "	35	M.	H.	8	1	16	—	1	10	Oxalate	7	1 "	"
90	7th June "	16	M.	H.	6	1	16	—	3	15	Uric	10	2 "	"
91	31st " "	35	M.	H.	9	1	18	2	—	—	Phosph.	17	2½ "	"
92	27th " "	55	M.	M.	19	1	16	1	4	—	{Uric and Oxalate}	28	3 "	"
93	29th " "	55	M.	H.	12	1	16	—	5	10	Uric	17	2 "	"
94	11th July "	45	M.	H.	7	1	18	—	3	15	"	11	3 "	"
95	16th " "	55	M.	H.	9	1	14	1	1	20	"	27	1½ "	"
96	1st Aug. "	40	M.	M.	4	1	16	—	—	20	"	4	½ "	"
97	13th " "	26	M.	M.	3	1	18	3	1	30	Oxalate	52	6 "	Died
98	16th " "	75	M.	H.	9	1	16	—	1	25	{Uric and Oxalate}	6	½ "	Successful
99	18th " "	34	M.	M.	2	1	14	—	—	5	Uric	2	1 month	"
100	24th " "	20	M.	H.	5	1	18	—	7	—	Phosph.	18	3 years	"
101	25th " "	56	M.	H.	11	1	16	—	—	15	"	5	15 days	"
102	26th " "	35	M.	M.	7	1	16	—	5	30	Uric	36	3 years	"
103	6th Sept. "	80	M.	H.	9	1	16	—	1	20	Phosph.	15	9 months	"
104	8th " "	56	M.	H.	5	1	16	—	—	12	Uric	5	2 "	"
105	10th " "	50	M.	H.	4	1	14	—	—	15	"	5	4 "	"
106	19th " "	36	M.	M.	14	1	16	—	4	—	"	10	1 year	"
107	24th " "	45	M.	M.	10	1	18	1	1	11	"	35	4 "	"
108	29th " "	25	M.	H.	14	1	16	1	2	5	"	20	2 "	"
109	6th Oct. "	35	M.	M.	7	1	14	—	—	8	"	2	10 days	"
110	11th " "	25	M.	H.	5	1	16	—	2	5	Oxalate	11	3 months	"
111	26th " "	55	M.	H.	7	1	18	2	—	45	{Uric and Oxalate}	26	2½ years	"

From this table it will be observed that amongst the 111 cases in which litholapaxy was performed there were four deaths. The details of these cases are as follows:—

Case 7.—A Hindoo male, aged sixty-five, was admitted into the Moradabad Civil Hospital on September 21st, 1882, with symptoms of stone in the bladder, which had existed three years. There was painful and difficult micturition, with frequent desire to make water; passing of blood occasionally; and the urine for some months had been mixed with pus, giving a very offensive smell on standing. There was great enlargement of the prostate. A catheter was passed and about ten ounces of fetid urine drawn off. On passing a sound several small calculi were detected. The man's health was very bad. On September 2nd I performed litholapaxy. The operation lasted thirty-five minutes, the lithotrite being introduced four times. The débris weighed eleven drachms. No. 18 canula passed easily—after previously slitting the floor of the meatus slightly—as far as the prostatic portion of the urethra, where some manipulation was necessary to pass it into the bladder. It was evident from the appearance of the débris that there were several calculi, varying in size from that of a pea upwards. Before withdrawing the canula finally the bladder was washed out with a weak carbolic solution. In addition to the usual after-treatment, a catheter was ordered to be passed morning and evening, owing to the atony of the bladder and enlargement of the prostate that existed. During the first two days little urine passed except through the catheter. The urine continued fetid and sanious: the bladder was washed out daily, a faint trace of carbolic acid being added to the water. There was no pain, but the patient continued very anxious and depressed, and died on September 27th from exhaustion. No post-mortem examination was permitted.

Case 24.—A Mahomedan male, aged forty-eight, was admitted on January 9th, 1883, with retention of urine which had existed thirty-six hours. He had suffered from symptoms of stone for nine months, being much exhausted, with pinched, anxious expression. On passing a full-sized catheter a small calculus was detected at the neck of the bladder, blocking up the urethral passage. This was pushed back with some force and the retention of urine relieved. The patient was at once anæsthetised and litholapaxy performed. The operation lasted only eight minutes; the débris weighed twenty grains.—10th: Passed very little urine since yesterday. Pain in region of the bladder. Catheter passed and a small quantity of urine drawn off. Hot fomentations and hot poultices to the hypogastrium were ordered; also one grain of opium internally every three hours.—11th: Well-marked peritonitis present. Temperature 102°F.; respiration 44 per minute; urine scanty. The catheter was used twice daily.—13th: No pain. Great distension of abdomen; patient very weak; passing a little urine. He died quietly in the evening.

Post-mortem examination.—On opening the abdominal cavity it was found distended with clear serous fluid. The bladder was embedded in a mass of amber-coloured gelatinous lymph, which broke like cold jelly on application of the fingers; congestion of the cellular tissue at the base and neck of the bladder; mucous membrane of the

urethra near the neck of the bladder highly inflamed; no congestion of the bladder or kidneys.

Case 44.—A Hindoo, aged forty-five, was admitted on May 23, 1883, with symptoms of stone, the most prominent of which were agonising pain in the region of the bladder and passing of blood and pus in the urine. The symptoms had existed eight years, and the patient was in a very weak state. A large calculus was detected by the sound. On the 25th, I performed litholapaxy. Considerable difficulty was experienced owing to the large size and hardness of the calculus, and the operation progressed very slowly, fourteen drachms being removed in seventy-five minutes. No more fragments could be felt by the lithotrite, but on passing the sound a large fragment was detected high up at the fundus of the bladder. As the patient was very much exhausted, I was afraid to keep him longer under chloroform, so I had, unwillingly, to postpone the completion of the operation to another day.—24th: He had great pain in the region of the bladder the whole of yesterday, which was controlled by one grain of opium administered every three hours. Urine scanty and blood-stained. Temperature 104° F.; great thirst. Fever mixture ordered and opium continued.—27th: Had a severe rigor last night; fever continued all night; patient anxious and depressed. Ten grains of quinine were ordered, and to be repeated in the evening. No pain in the abdomen, but great pain in the left hip.—28th: Abscess forming in the left hip. All the symptoms of blood-poisoning present. Carbonate of ammonia and bark mixture ordered. The patient died at 10 P.M.

Post-mortem examination.—The walls of the bladder

were greatly thickened, sacculated, and contracted; in the bladder were found a large fragment of calculus (about one ounce in weight) in a sacculum at the fundus, several small patches of inflammation of the mucous membrane in the vicinity of the fragment of stone, and some milky urine. The tissues of the left gluteal region and round the left hip-joint were swollen and infiltrated with dark-red fluid. Cause of death—pyæmia.

Case 97.—A Mahomedan male, aged twenty-six, was admitted into the Bareilly Civil Hospital on August 12th, 1884, suffering from the usual symptoms of stone, which had existed six years. During the last three years the symptoms became much aggravated. On admission micturition was extremely painful and difficult, and the urine was mixed with pus and blood. The patient was in extremely bad health; very thin and anæmic; scarcely able to stand. On August 14th I performed litholapaxy. Chloroform was given by Dr. Corbett, Bengal Medical Service, by means of Junker's inhaler. The calculus was a large hard one, weighing three ounces and a drachm and a half, and consisted of oxalate of lime. The operation lasted fifty-two minutes. After slitting the floor of the meatus slightly a large lithotrite was passed, but, owing to the contraction of the walls of the bladder on the stone, it could not be grasped. The lithotrite was withdrawn and four or five ounces of water injected by the aspirator, and then the calculus was easily grasped after a little manipulation. I had to use all the force I was capable of before the stone gave way. The fragments were then disposed of one by one, great force being necessary in dealing with even the smallest fragment. The

lithotrite had to be introduced six or seven times, and the evacuator as often, before the whole of the calculus was disposed of. My new large lithotrite did great execution amongst the fragments, the glass receiver of the aspirator being nearly filled each time after withdrawal of the lithotrite. Two or three times during the operation the patient was very faint and almost ceased breathing, and after the operation the pulse was extremely weak. Artificial breathing had to be maintained, occasionally, for an hour after the operation. Evening: Passed urine freely during the day, the earlier portions were blood-stained, the later clear; no sand or fragments passed; no pain in the bladder, but tenderness along the course of the urethra. Pulse 90 (very weak).—15th: Patient very weak; passed three loose stools during the night; urine passing freely, and quite clear: vomited twice this morning; pulse almost imperceptible; hands and feet cold; drowsy and restless; wanders in his conversation at times. During the course of the day the patient revived a little, but he grew weaker towards evening and died at 9 P.M.

Post-mortem examination.—Bladder empty (not a trace of sand was present), contracted, and its walls much thickened: mucous coat was corrugated and slightly sacculated; urethra congested along its whole length; ureters dilated along their whole length, so that the tip of the little finger could be passed into them from the bladder; kidneys extensively diseased; the calyces were much dilated, and the medullary portions had almost disappeared; the cortical portions were hard and pale; pericardium distended with clear straw-coloured fluid.

The patient in Case 7 was almost moribund on admission to hospital. There was complete atony of the bladder, and the condition of the mucous membrane of the bladder may be imagined from the fetid state of the urine. The man was so weak and exhausted that I could not entertain the idea of performing lithotomy, so I gave him the only prospect of recovery in performing litholapaxy. In Case 24 death resulted from peritonitis, evidently produced by extension of inflammation from the neck of the bladder It is difficult to say if this was due to the operation or to impaction of the calculus at the neck of the bladder, and the force necessary to displace it. I am inclined to think the latter was the cause. There was no difficulty whatever in performing the operation; the stone was very small, and the operation lasted only eight minutes. The bladder was healthy except at the neck, where the calculus had lain impacted for two days. The kidneys also were healthy. It is probable that the calculus, forced on by the accumulation of urine and efforts at micturition, acted as an irritant to the mucous membrane, and that the force necessary to push back the stone into the bladder increased the irritation, thereby producing inflammation. In Case 44 the cause of death was pyæmia, the result of cystitis supervening on the operation, and caused by the irritation of a large fragment, which it was found impossible to dispose of at the first sitting, owing to the exhausted state of the patient. Death in Case 97 resulted apparently from the shock of the operation and exhaustion produced by the chloroform, acting on a constitution worn out by the painful nature of the malady and extensive disease of the bladder, ureters and kidneys. Emboldened by having previously removed a larger calculus, successfully, in

Case 16—the details of which will subsequently appear—and by a long-continued series of successful cases, from Case 45 to Case 96, inclusive, in many of them the calculi being very large, I had no hesitation in attacking the stone in this case, and I feel confident the patient would have recovered but for the extensive disease of the kidneys which existed, and from which the patient must have succumbed to any operation. The large and powerful lithotrite sent me lately by Weiss worked admirably in this case. The manner in which a large oxalate of lime calculus, weighing over three ounces, and as hard as flint, was crushed to atoms and completely removed by the aspirator in fifty-two minutes, without the necessity of any dilatation of the urethra or injury to the urinary passage, shows what can be done by the new operation. Though the crushing power of this instrument is very great and the strength of the steel enormous, it was noticed both by the surgeons who honoured me with their presence during the operation and by myself that when dealing with a large and hard stone of this kind the leverage in the instrument is deficient. This might be easily remedied by having long knobs to the wheel-shaped handle of the male blade, and by the addition of a light crossbar to the cylindrical handle of the female blade, both the knobs and crossbar being removable, so that they might be applied only when large calculi are dealt with.

With the exception of the four deaths above referred to, all the cases made excellent recoveries. Cystitis, which so frequently followed on the old operation of lithotrity, and which often left the patients in almost as bad a condition as they were in before the operation, seldom occurs

after litholapaxy; and when it does occur, or had existed previous to the operation, it is very amenable to treatment.

The 111 operations were performed on 109 different individuals, the disease having only twice recurred. In one of these cases the patient, aged sixty-five, was suffering from enlargement of the prostate. Six months after the first operation, in which four drachms of a soft phosphatic calculus were removed, he returned and had a similar calculus weighing eighty-five grains removed. In old patients of this kind, with enlarged prostate, the disease frequently recurs, owing to the fact that the bladder is never completely emptied of urine during micturition. In the other case several small calculi, varying in size from that of a pea to that of an almond, and weighing in all one ounce, were removed, and the patient left the hospital quite well. A month afterwards he returned with symptoms of stone. At first I imagined that a fragment had been left behind at the first operation, but on applying the canula and aspirator I removed twenty-seven minute calculi, weighing half a drachm in all. I then found that in the interval the patient had suffered from severe kindney colic, and that the minute calculi, each of which consisted of a uric-acid nucleus with phosphatic deposit, were the result of a shower of uric-acid particles from the kidney.

The calculi removed varied in weight from five grains to three and a quarter ounces. There were fifty weighing half an ounce and upwards, twenty-seven one ounce and more, seven two ounces and upwards, and two over three ounces. The following are the details of the case in

which the largest stone was removed, to which I wish to call especial attention :—

Case 16.—Illahi Buksh, a Mahomedan aged sixty, was admitted into the Moradabad Civil Hospital on Dec. 3rd, 1882, with all the symptoms of stone in the bladder, which had existed eleven years. On admission the patient could only pass urine in drops continuously throughout the day and night, and the passage of urine was attended with great pain. His penis and foreskin were hypertrophied from the patient's constantly rubbing the organ to relieve the pain and irritation. A urethral calculus was felt in the fossa navicularis. When the patient tried to pass urine he had to rub and pull the penis, and in this way push the urine past the calculus in the urethra. The urine was mixed with pus and blood. The fæces passed were ribbon-shaped, due to pressure of the stone in the rectum. On passing the finger into the rectum a large stone could be felt in the bladder. The patient's health was very bad. He was pale, thin, weak and anæmic, and he had a pinched, anxious expression, the result of long suffering. On Dec. 5th I performed litholapaxy, the urethral calculus having first been removed after slitting the floor of the urethra slightly. The operation lasted sixty-six minutes, and the débris weighed three ounces and a quarter, the calculus being a hard uric-acid one. Considerable trouble was at first experienced in grasping the stone, owing to the contraction of the walls of the bladder on it. This was obviated by injecting water into the bladder. The lithotrite was introduced at least a dozen times, and after each crushing a large quantity of débris was washed out through a No. 18 canula. With

the exception of slight pain in micturition during the first day or two, the patient had no after-trouble. He made a rapid recovery, and on Dec. 15th, when discharged from the hospital, the following entry in my note-book describes his condition :—" Patient now rid of all bladder symptoms. Urine quite clear; bladder retains a large quantity at a time. Has grown fat and strong. Says he has not been so well for several years. This man was a miserable creature on admission to hospital ten days ago, and now leaves it in excellent health."

This is, as far as I am aware, the largest calculus that has ever been removed from the bladder by the crushing operation at one sitting. The amount of manual labour required in crushing a large and hard stone of this kind is something excessive. I felt completely exhausted after the operation. My hands were blistered from the lithotrite, and the muscles of my arms ached for two or three days subsequently.

I cannot too strongly advocate the desirability of removing the whole of the calculus at one sitting. This is the essential principle of the operation. This fact seems to be lost sight of by some surgeons, for I have noticed that in some of their recorded cases a second, and even third operation was necessary. An operation prolonged over several sittings in this way involves all the dangers of the old operation of lithotrity and ceases to be litholapaxy in my acceptation of the term.

Male patients of all ages, from sixteen to ninety-six years, were operated on. The average age was forty-seven years and a half. Several patients of eighty years and over had large calculi removed successfully; and in one

case I removed a hard uric-acid stone weighing nine drachms and a half from a patient aged ninety-six, the operation lasting one hour. The details of the case are interesting, so I give them.

Case 69.—This was a case in private practice. The patient, a Mahomedan of Moradabad, stated that he was close on 100 years of age, and by calculation he appeared to be ninety-six. He was a dried-up, withered creature, without a tooth in his head, and consisting almost of skin and bone only. Several of his sons were living, and one of them looked seventy years of age. Till about a month before coming under my treatment he had enjoyed good health, and used to walk about the bazaar daily. He was suffering from well-marked symptoms of stone, especially great pain in passing urine. He was very weak and unable to leave his bed. On October 28th, 1883, I performed litholapaxy, the débris weighing nine drachms and a half, and the operation lasting one hour. The patient made a rapid recovery and was able to walk about on November 13th. Five months afterwards I had the pleasure of showing this old gentleman to Surgeon-General W. Walker. He was then in excellent health, so much so that Dr. Walker writing of him at the time amusingly says; "He must certainly be ninety, and looks as if he might live for thirty years more and then do service as an old rail."

My experience is that old patients bear the operation much better than young men, and that in them it is less likely to be attended by urethral fever. The explanation of this is to be found, I have no doubt, in the fact that the mucous membrane of the bladder and urethra gets less

sensitive as life advances, and also that the urethra is more capacious in old than in young men.

In several instances very small calculi were removed. The diagnosis of such calculi by the sound is often a matter of extreme difficulty; and experience teaches us that a patient is often sent away from hospital with a stone in his bladder after an opinion to the contrary has been expressed. I cannot speak in terms of too high praise of Sir Henry Thompson's sound. Any sound will detect a large or moderate-sized stone; but when I have failed with all other sounds in detecting a small calculus, Sir Henry Thompson's sound has brought it to light. I find from experience, however, that, even with Sir Henry Thompson's sound, a small calculus lying in some peculiar position in the bladder may evade detection. In the *Indian Medical Gazette* of March last I called attention to a new method of diagnosis for small calculi by means of the aspirator. The method of employing it is indicated in Case 54.

Case 54.—On Aug. 1st, 1883, a Hindoo male, aged fifty, came to hospital with symptoms of stone, the most marked of which were sudden stoppage of the flow of urine and increased frequency of micturition. After a most careful exploration of the bladder by sounds of various kinds, including Sir Henry Thompson's, no calculus could be detected. I felt certain, however, from the symptoms that there was a small stone present, and determined to employ the aspirator for the purpose of diagnosis. I introduced a No. 14 catheter, and applied the aspirator. After going through the performance of pumping water into the bladder and exhausting it, once or twice, a distinct

click was heard. The canula was withdrawn, the lithotrite introduced, and the stone crushed. The fragments weighed eleven grains only. Next day the man was walking about quite well.

In the above case a most careful search was made by sounds of various kinds, but no calculus could be detected till the aspirator was employed, when a distinct click was heard during the exhaustion of the water, due to the calculus being carried with force against the eye of the canula by the outward stream. The sound of the fragments clicking against the canula during aspiration in the operation of litholapaxy first suggested to me this mode of diagnosis, and I now always employ it when the symptoms of stone are present, and the sound fails to detect one. In this way I have detected several small calculi.

Not alone may the aspirator be usefully employed for diagnostic purposes, but by means of it a small calculus or number of small calculi may be removed entire without the necessity of having recourse to the lithotrite at all. Case 76 is a practical illustration of this. The patient, a warder in the Moradabad gaol, aged fifty, had been passing gravel for two or three years, when suddenly one day his urine ceased to flow. He went to the Hospital Assistant, who passed a catheter and relieved the retention. Next day he had retention again, when he consulted me at my morning visit to the gaol. I sent him to the civil hospital, placed him under the influence of chloroform, and passed a No. 18 catheter at once with the greatest ease. The aspirator was then applied and the click of a stone heard. The calculus, which weighed only fifteen grains,

passed into the apparatus, and was removed in this way without the use of the lithotrite. Next day the warder returned to his work quite well, having suffered no unpleasant symptoms. I had for some time previously contemplated the removal of a small calculus in this way by the aspirator alone; but this was the first opportunity I had of putting my idea to a practical test. Since then I have removed calculi in four or five instances in this manner without the aid of the lithotrite. For instance, in Case 81 I removed twenty-seven small hard uric-acid calculi, the whole weighing half a drachm, all passing through a No. 16 canula into the receiver; and in Cases 98 and 109 small calculi were both diagnosed and removed by the aspirator alone.

During the process of aspiration, with each expansion of the Indiarubber bulb, the fragments of calculi are carried against the eye of the canula by the outward rush of water, and a clicking sound is thus produced, which, whilst it continues, indicates that some fragments remain in the bladder. There is, however, a peculiar sound sometimes produced, which I have not seen mentioned in any text-book or journal, and the occurrence of which the young litholapaxist should be acquainted with, as it is very likely to be confounded with the sound produced by a fragment. This "false sound," as it may be called, is produced by the mucous membrane of the bladder being sucked into the eye of the canula during exhaustion of the water. It is most likely to occur towards the end of the operation, when all, or nearly all, the fragments have been exhausted, and especially when the bladder contains no surplus water, only that quantity which is pumped in

and withdrawn during compression and expansion, respectively, of the bulb. It may, however, be produced at any time if, after compressing the bulb, the eye of the canula be turned towards the sides or directed up against the fundus of the bladder, and then the bulb of the aspirator be allowed to expand. The sound itself, though difficult to describe, can never be mistaken when once recognised. The sensation communicated to the hand is of a fluttering, jerky character, accompanied by a dull, muffled sound as contrasted with the clear, ringing click which the impact of fragments imparts to the instrument. On its occurrence the outward stream receives a sudden and complete check; whereas when a fragment obstructs the stream a portion of the water continues to flow. The sound does not recur if the canula be partially withdrawn and raised towards the perpendicular position, so as to bring the eye close to the neck of the bladder, with the end of the canula resting on the trigone; whereas a fragment will produce obstruction there as well as in any other position. On first practising litholapaxy I was deceived by this sound, and quite recently I have seen a brother officer of the Indian Medical Service, who performed his first litholapaxy operation in my presence, similarly deceived, I having forgotten to mention to him the possibility of its occurrence.

Towards the completion of the operation it will be found, as a rule, that the last particles of débris lie close to the neck of the bladder, just behind the prostate. This is due to the fact that the eye of the canula being turned towards the fundus and sides of the bladder, the water is less disturbed by currents in the position referred to than in any

other. Consequently, the last particles of débris gravitate towards this spot. Towards the end of the operation, therefore, the eye of the canula should always be turned right round towards the prostate and water forcibly injected, so as to dislodge the débris from this position. This manœuvre is especially necessary where enlargement of the prostate coexists, otherwise a fragment might be left behind.

Litholapaxy was performed in three instances on female children, with the happiest results. The patient in each instance was running about the day after the operation, and suffering no inconvenience whatever. The operation is comparatively easy of performance in females, owing to the urethra being so short and wide. No forcible dilatation or disturbance of the parts was necessary, and, consequently, no incontinence of urine ensued,—that troublesome sequel which so often results from the operation by dilatation.

Excluding the three cases in which the operation was performed in female children, there were 108 cases of litholapaxy in adult males performed by me, with four deaths, or a mortality of 3·7 per cent. Compare these results with the recognised mortality of one in four or five, or from 20 to 25 per cent., resulting from lithotomy in the adult! The accompanying table shows the results of 2,592 lithotomy operations performed in the Indian hospitals during the year 1882. The figures are taken from the Medical Administration Reports of the various Provinces for that year, which, with the exception of that for the North-West Provinces and Oudh, are the latest available.

Table showing the results of Lithotomy Operations performed in Indian Hospitals in 1882.

Presidency or Province.	Remaining on December 31st, 1881.	Performed during 1882.	Total.	Result.				Remaining.	Percentage fatal.	Proportion fatal.
				Cured.	Relieved.	Otherwise.	Died.			
Bengal ..	6	181	187	148	5	5	19	10	10·5	1 in 9·5
N.-W. Provinces and Oudh..	49	933	982	824	11	11	85	51	9·1	1 in 11
Punjab ..	50	949	999	768	25	28	127	51	13·4	1 in 7·4
Central Provinces ..	5	73	78	68	..	1	9	..	12·3	1 in 8·0
Bombay and Sindh ..	27	450	477	377	6	6	45	43	10·0	1 in 10
Madras ..	1	6	7	5	1	..	1	..	14·4	1 in 7·0
Total ..	138	2,592	2,730	2,190	48	51	286	155	11·9	1 in 9·0

I regret that I am unable to give the statistics of mortality according to age. The only administration report which is complete in this respect is that for the North-West Provinces and Oudh. Taking the latest statistics available for these provinces, those for 1883, I find that amongst 987 cases of lithotomy performed in 1883, the mortality up to the age of twenty years was 5·1 per cent., or nearly 1 in 20; between the ages of twenty and forty, 10·7 per cent., or about 1 in 9·5; and above forty, 31·9 per cent., or nearly 1 in 3. The mortality here recorded for the North-West Provinces is higher than that recorded for former years; but I have taken the latest statistics as they are likely to be the most correct.

An impression seems to prevail in England as well as in India that lithotomy is a much more successful operation amongst natives of India than amongst Europeans. This impression is erroneous. According to the above table the mortality that occurred actually in hospital was 1 in 9,

or 11 per cent. There were, however, 51 cases discharged "otherwise" than "cured," "relieved," or "died;" and it may be presumed that these, or the great majority of them, were taken away from hospital by their friends in a moribund condition to die at home. If these cases be added to those dying actually in hospital, the total mortality will be about 13 per cent., or nearly 1 in 8, which is practically the same as that recorded in Erichsen's Surgery as occurring amongst Europeans. In my own practice I have performed lithotomy in 182 cases; of these 50 were adult males, and amongst them there were 9 deaths, giving a mortality of 18 per cent., or about 1 in 5½. In the remaining 132 cases the patients were male children or boys under the age of puberty, and *amongst them there was no death.*

Lithotomy in the male child, when skilfully performed, has always been recognised as a successful operation. It appears to me that litholapaxy is quite unsuited to such cases owing to the undeveloped condition of the genito-urinary organs, the bladder being small, the urethra narrow, and the mucous membrane of both extremely sensitive and liable to laceration. It must be remembered that, in order that the aspirator may be of any practical utility for removing fragments, a much larger canula must be used than could with safety be introduced through the urethra of a male child. I am, however, unable to speak from practical experience in the matter, never having attempted litholapaxy in a male below the age of puberty; nor have I any inclination to do so whilst my success from lithotomy in such cases continues to be what it has hitherto been.

With litholapaxy in the adult male and females of all ages, and lithotomy in males below the age of puberty, the mortality from operations for stone should be extremely small. Second only to the reduction in mortality that litholapaxy can claim is the comparatively short period in hospital which the operation involves. It is no exaggeration to state that a disease which formerly involved weeks of confinement and suffering for its cure can now be disposed of in as many days.

I have referred to the impression which prevails that the mortality from lithotomy in natives is less than in the case of Europeans. There is also an impression prevalent to the effect that natives of India have no fear of the surgical knife. In fact, from the way some people talk and write it might be almost inferred that a native submits to a surgical operation as a kind of harmless diversion. This impression is altogether erroneous. A native of India will not, as a rule, submit to a surgical operation till all other modes of treatment fail, and he is driven to it through extreme pain, inconvenience, or danger to life. And it is for this reason that such large calculi are met with in this country, and that patients suffering from cancer and other diseases present themselves in hospital at a stage when surgical interference is useless. I can testify to the immense popularity of litholapaxy amongst the natives of India; and it may be reasonably hoped that when it becomes generally known that a small calculus may be removed from the bladder by an operation which involves no cutting, little or no pain, and confinement to hospital for a few days only, patients will present themselves at an early stage of the disease, when it is most amenable to

treatment, and when the operation is almost unattended with danger.

In concluding this paper, I would venture to suggest that the time has now come when Bigelow's operation should be universally recognised to be, what it undoubtedly is, a distinctly new operation. Shortly after the introduction of litholapaxy, Sir Henry Thompson assumed a position of opposition to this view. In a lecture delivered at University College Hospital in December 1878,[1] an attempt was made by him to show that the new operation had been gradually developed out of the old operation of lithotrity, the previous existence of Clover's syringe and the assertion that Sir Henry himself had, during the previous two years, been in the habit of doing more at each sitting, both in the way of crushing and removal of fragments than formerly, being mainly relied on as the connecting links in establishing their identity. Bigelow's share in the construction of the operation was minimised; his instruments were denounced as incapable of performing the work assigned to them, and held up to ridicule as "enormous and unwieldy,"—suggesting to Sir Henry's mind "some resuscitated relics of the early history of lithotrity,"—reminding him of "the terrible engines used by Heurtaloup,"—and disastrous results were anticipated from the alleged proposal of Bigelow "to make the rule absolute to remove at one sitting an entire stone no matter how large it may be or what the condition of the patient," a proposal which would seem to have had its origin in the imagination of the lecturer, for Bigelow asserts[2] that no

[1] The Lancet, vol. i., p. 145, 1879.
[2] The Lancet, vol. i., p. 693, 1879.

such proposal had ever been made by him. Read by the light of five years' practical experience of the operation all over the world, the gloomy anticipations then expressed do not seem to have been realised. Sir Henry seems to have altered his opinions very materially since that time, for we find from his most recent writings that lithotrites and evacuating catheters which were then pronounced dangerous and unnecessary are now held to be admissible, and even necessary, when dealing with large calculi.

Now, everyone who knows anything about the history of lithotrity must be aware that previous to the appearance of Bigelow on the scene, in 1878, the tendency of all lithotritists was (1) to restrict to the lowest possible limit the time occupied at each sitting, four or five minutes being the utmost time allowed as safe; (2) to employ instruments of the smallest size possible; and (3) to leave the evacuation of the fragments as much as possible to natural efforts. The principles on which this practice was founded were: (1) That the bladder was extremely intolerant of the presence of instruments; and (2) that in direct proportion with the length of time instruments were manipulated there was the prospect of evil consequences resulting. There can be no doubt as to the teaching that prevailed on the subject. A reference to the latest editions of all the ordinary text-books published prior to 1879 will show that the authors were unanimous on these points; and there was no one who inculcated the principles and practice referred to more strongly than Sir Henry Thompson himself, as might be illustrated by numerous extracts from his writings. It was reserved for Professor Bigelow to show that these principles were

altogether wrong, and to introduce a practice entirely at variance with the old proceeding. The hypotheses on which the new operation was based were: (1) That the bladder was much more tolerant of prolonged manipulation than was previously supposed, and (2) that the temporary manipulation of blunt and polished instruments in the bladder was less irritating than the continued presence in the organ of sharp fragments of calculus. For the purpose of working out his idea Bigelow introduced larger lithotrites than had previously been used, and invented an entirely new evacuating apparatus by which débris might be rapidly extracted from the bladder. In the introduction of the large evacuating canulæ, Bigelow availed himself of the discovery of Otis that the urethra is much more capacious than was previously recognised.

It is rather strange that Sir Henry Thompson should claim for Clover's syringe an efficiency as an aspirator which in its original and unmodified form it never possessed. The apparatus is referred to by most authors as a pretty and ingenious one for washing out the bladder. Its use in lithotrity is, however, deprecated, save in exceptional cases, such as where enlargement of the prostate or atony of the bladder coexists; and then the only efficiency claimed for it is that of washing out sand. Thus Mr. Cadge of Norwich writes:[1] "In doing this [removing calculi under the circumstances above referred to] I have sometimes used Clover's syringe, but more frequently have trusted to the quicker and less disturbing action of the scoop lithotrite." In 1869 Sir Henry Thompson, writing of the removal of fragments by Clover's

[1] The Lancet, vol. i., p. 471, 1879.

syringe,[1] says: "The process is rather trying, however, for the bladder; and it costs rather more pain and time than an ordinary sitting for lithotrity." Again, in 1871, he writes:[2] "Having used it [Clover's apparatus] very frequently, I would add that it is necessary to use all such apparatus with extreme gentleness, and I prefer to do without it, if possible." And that, even so late as 1878, Sir Henry relied much more on the flat-bladed lithotrite for the evacuation of the débris (Fergusson's method) than on Clover's syringe is apparent from a passage in the lecture delivered by him in December, 1878, already referred to.

Bigelow applied the name "litholapaxy" to his operation; but to this Sir Henry Thompson objects, suggesting "lithotrity at one sitting" as more appropriate. Now, I think there are many advantages in having a distinctly new name for a distinctly new departure in surgery. The word litholapaxy (λιθος, a stone, and λαπαξις, evacuation) seems to me the one most expressive of the procedure involved in the new operation. Bigelow's operation involves much more than the crushing of the stone, the essential feature being its complete and rapid evacuation. Besides, as I have already pointed out, there are many cases in which a small calculus can be removed by the aspirator alone, in which no crushing is required, and to which, consequently, the name lithotrity at one sitting cannot be applied, whereas the word litholapaxy will also embrace these. There can be no doubt whatever that Bigelow's operation was a distinct innovation both as regards the principles involved and the means by which it

[1] Diseases of the Urinary Organs, p. 125.
[2] Practical Lithotrity and Lithotomy, p. 215.

was accomplished. The operation struck at the root of all previously held tenets regarding lithotrity; and its introduction caused at the time considerable astonishment to the profession all over the world. I must confess my surprise that Sir Henry Thompson, after employing Bigelow's operation in all its essential details during the past five years, and obtaining from it such brilliant results as those recorded by him, should still persist in saying[1] that "no new form of instrument is required by this operation," and that he should refrain from according to Bigelow that credit to which he is justly entitled for his originality.

Received at first with caution, the operation is steadily growing in favour with the profession in America, England, and Europe generally. In India it has as yet been adopted by a few surgeons only; but so thoroughly satisfied with its results in their hands is the Surgeon-General of the North-West Provinces and Oudh, Dr. W. Walker (always alive to the interests of surgery), that he has ordered several cases of the instruments required, for employment in the most important hospitals under his administration. I believe I have pushed the operation, as regards the size and hardness of the calculi attacked, the ages and debilitated conditions of the patients operated on, as far as any other surgeon, and I cannot speak too highly of it. By it the surgery of the bladder has been truly revolutionised, and I confidently believe that, with increased perfection in the instruments and apparatus employed, larger calculi than any hitherto attacked will successfully yield to the operation.

Bareilly, India.

[1] *Vide* Diseases of the Urinary Organs, seventh edition, p. 98, 1883.

Chirurgische Beiträge.

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Dr. Theodor Billroth,

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ehrerbietigst zugeeignet.

An

Herrn Professor Billroth!

Das Unternehmen meiner Widmung soll nur der Ausdruck sein meines dankbarsten Gedenkens Ihrer bei diesen Ausarbeitungen. Was Ihnen Schüler verdanken, wissen nur diese und können nur sie sagen; meine gewagte Widmung hat diesen einzigen Zweck nur. Möchten nicht anders Sie, hochverehrtester Lehrer, das Wagniss deuten, so Geringfügiges Ihnen öffentlich zu bieten.

Berlin, im October 1884.

Dr. Hashimoto,
Kais. jap. Generalarzt in Tokio.

Vorbemerkung.

Während meines Aufenthaltes von Mitte Juli bis Mitte October 1884 in Wien und Berlin in amtlicher Mission, habe ich in den wenigen mir vergönnt gewesenen, jedoch durch Störungen aller Art, welche ein Reiseaufenthalt mit sich bringt, sehr verkürzten Mussestunden die nachfolgenden kleinen Aufsätze niedergeschrieben. Mein Zweck dabei war ein doppelter: einmal ein sachlicher, der Mittheilung mir interessant und wichtig erscheinender Operationen aus meiner Hospitalpraxis in Tokio, alsdann aber, Gelegenheit zu nehmen, im Interesse meines Vaterlandes Japan einen wissenschaftlichen Verkehr mit der deutschen Chirurgie einzuleiten; dies ist mein regstes Bestreben.

Berlin, im October 1884.

Der Verfasser.

1. Ueber Exostosen,

beobachtet auf der Klinik der med. Fakultät in Tokio, zweites Hospital, Shitaya.

(Hierzu Fig. 1—5.)

I. Am 23. December 1880 kam in das oben genannte Hospital ein 15jähr. Mädchen, das seit seinem 7. Lebensjahre auf der rechten Seite des Nackens eine kleine Verhärtung von zweitheiliger, mit einander zusammenhängender Gestalt hatte, die mit den Jahren immer grösser geworden war.

Status praesens: Körperliche Entwickelung normal. Die Geschwulst hatte etwa die Form einer Feldflasche, mit einer Verengerung in der Mitte. Sie lag zwischen der Nacken- und Rückenlinie, jedoch mehr nach rechts verschoben, begann in der Gegend der Protuberantia occipitalis und reichte bis zur unteren Ecke des Schulterblattes. Ihre Oberfläche war glatt, ohne irgend eine rauhe Erhebung, die Haut unverändert, die Abgrenzung der Geschwulst ganz deutlich erkennbar, Beweglichkeit leicht, doch der untere Theil beweglicher. Der obere Theil der Geschwulst hatte einen Längendurchmesser von 14 Ctm., der untere von 14,5 Ctm., der Querschnitt des oberen betrug 13 Ctm., der des unteren 12,5 Ctm., der ganze Umfang des Halses mit der Geschwulst zusammen war 45 Ctm.

Die Diagnose wurde auf ein Osteom gestellt, das sich von einem Wirbel aus entwickelt hatte, obwohl Dies bis jetzt selten beobachtet worden zu sein scheint. Wie weiter zu ersehen, hat sich diese Annahme vollkommen bestätigt. — Es waren nun folgende 3 Hauptmomente zu erwägen: a) ist die Geschwulst überhaupt exstirpirbar? b) bietet die Operation irgend welchen Vortheil? c) musste die Möglichkeit eines Recidivs erwogen werden, für den Fall, dass es kein Osteom war.

ad a) wurde die Beweglichkeit der Geschwulst in der Narkose untersucht, wobei sich fand, dass der untere Theil nun beweglicher war als zuvor, während die Beweglichkeit des oberen der Erwartung nicht entsprach. Nichtsdestoweniger war ich der Ansicht, dass ein Stiel vorhanden sei. — ad b) war der Vortheil der Operation klar, einmal zur Beseitigung der Verunstaltung

des Halses, das andere Mal zur Beseitigung des Druckes auf die grossen Halsgefässe, der sich durch Kopfschmerzen kundgab. — ad c) Beim Osteom treten zwar Recidive nicht ein, wie bei den bösartigen Geschwülsten, aber es ist nach der Operation desselben die Osteomyelitis häufig beobachtet worden. — Diese und andere ähnliche Gründe bestimmten mich endlich, an die Operation wirklich zu gehen. Ja, ich glaube hier noch erwähnen zu können, dass ich die Operation für unumgänglich nothwendig hielt, da die Geschwulst noch immer im Wachsthum begriffen war, was während des Monate langen Aufenthaltes im Hospitale beobachtet worden war.

Operation am 28. April 1881. Als Assistenten fungirten die Herren H. Namba, I. Assistent, und T. Tavara, welcher Letztere diesen Fall in japanischer Sprache beschrieben hat. Die Pat., in halb liegender Stellung, wurde tiefer als gewöhnlich narkotisirt, um eine vollständige Entspannung der Muskeln herbeizuführen. Nachdem diese eingetreten war, machte ich über den längeren Durchmesser des unteren Theiles der Geschwulst, der Länge derselben genau entsprechend, eine Incision, schichtenweise, bis ich, nach Durchschneidung der tiefen Rückenmuskelfascien, zu der Geschwulst selbst gelangte, die sich als ein wirkliches Osteom erwies. Darauf machte ich eine zweite Incision, als Verlängerung der ersteren, nach der oberen Partie der Geschwulst, und versuchte die Weichtheile von der Geschwulst möglichst stumpf abzulösen, was nicht gelingen wollte, so dass ich in dem Niveau des oberen Theiles der oberen Geschwulst quer eine dritte Incision von ungefähr 7 Ctm. Länge machen musste. Dann begann ich die Weichtheile von der unteren Geschwulst abzulösen, weil diese sehr leicht beweglich war. Hierauf wurde die Ablösung von unten nach oben und von einer Seite zur anderen vollendet, wonach das Ganze wider alles Erwarten von selbst abfiel. Der Stiel, in der Dicke eines kleinen Fingers, blieb an dem Proc. transversus und Proc. spinosus des 7. Halswirbels zurück. Unmittelbar nach der Lostrennung der Geschwulst traten bei der Pat. die Erscheinungen des Shock ein und machte ich nun in der Herzgegend subcutane Injectionen mit Campher, 3 Spritzen voll während einer Viertelstunde, und nebenbei Faradisation des Nervus phrenicus. Nach 20 Minuten fing Pat. wieder an sich zu erholen, so dass sie in einer Stunde ihr volles Bewusstsein wieder hatte. Die Blutung war während der eine Stunde dauernden Operation nicht sehr gross; Unterbindungen waren 10 erforderlich; an Chloroform wurden 10 Grm. verwendet. Viele Nähte der Wunde in der Länge und Quere; Nachbehandlung nach Lister's Methode.

Am 1. Tage Pat. unruhig, 7maliges Erbrechen, Schlaf erst nach Mitternacht. Puls besser, Respiration nach und nach leichter. — 2. Tag: Temperatur bis auf 39,3 gestiegen, Puls frequenter, Brechneigung noch immer anhaltend, doch die ganze Nacht Schlaf. — 3. Tag: Erbrechen ganz aufgehört, Pat. konnte zum ersten Male eine sehr leichte Reissuppe zu sich nehmen. Erneuerung des Verbandes. — 4. Tag: Erstmaliges Eintreten der Menstruation, die bisher nicht stattgefungen hatte. Die Wundsecretion vermehrt, der Verband abermals erneuert und, da die Wundränder nicht per primam intentionem

verheilt waren, die Nähte entfernt, um die Entleerung der Wundsecrete zu erleichtern. — 5., 6., 7. Tag ganz normal. — Am 8. Tage begann die Wundfläche zu granuliren. — Am 14. Tage wurde die Temperatur wieder normal, die Wunde fing an zu vernarben. Ein nekrotisirtes Knochenstück des Stieles fiel von selbst ab. — 12 Wochen nach der Operation waren die Wundflächen ganz vernarbt.

Anatomischer Befund: Das Knochengewebe in der Geschwulst war von dem gewöhnlichen Knochengewebe in der Art verschieden, dass es an manchen Stellen sehr hart war und viel kohlensaueren Kalk enthielt; andere Stellen zeigten nur Bindegewebe mit wenigen Kalkbestandtheilen; in Folge dessen waren sie weich, an einigen anderen Stellen schwammig. Knochenkörperchen waren sehr unregelmässig, bald gross, bald klein, ihre Fortsätze zwar vorhanden, aber unregelmässig geformt; die Knochencanälchen ebenfalls von unregelmässiger Gestalt, dagegen konnte man an manchen Stellen regelmässig geformte Knochenkörperchen und Knochencanälchen finden.

Zum Schlusse möchte ich mir noch die Bemerkungen erlauben, dass a) das Osteom bekanntermaassen an der Wirbelsäule nicht oft vorkommt, b) freute ich mich, bei der Operation keine Verletzung der Rückenmarkshäute gemacht zu haben, weil die Geschwulst wirklich eine Exostose war.

2. Eine 30jährige Bauersfrau lebte bis zu ihrem 20. Lebensjahre ohne alle Beschwerden; jetzt erst fühlte sie am linken Os parietale Schmerzen, die 2 Monate lang andauerten, worauf sich an derselben Stelle eine Erhebung bildete. Von dieser Zeit an litt sie an einem dumpfen Kopfschmerze und Neigung zu Ohnmachten.

Bei der Aufnahme im Hospital am 25. December 1881 zeigte sich folgender Status praesens: Constitution ziemlich gut, am Kopfe eine Geschwulst in der Gestalt einer Doppelkugel, neben einander liegend, und zwar von vorne nach hinten. Vorne reichte dieselbe bis zum Stirn-, hinten bis zum Hinterhauptsbeine, rechts und links bis zum oberen Rande der Schläfenbeine. Die Abgrenzungen waren nur stellenweise deutlich, so dass man die Geschwulst von den Knochen nicht unterscheiden konnte. Ihre Consistenz war knochenhart. An ihrer Wurzel hatte sie einen Umfang von 43 Ctm., der Längendurchmesser von vorne nach hinten war 21 Ctm., der Querdurchmesser 24 Ctm. Beweglichkeit der Geschwulst war nicht vorhanden.

Diagnose: Zweifellos Osteom der Schädelknochen. Da die Geschwulst keine Beweglichkeit besass, ihre Begrenzung nicht sichtbar war, folglich eine etwaige Operation schwerlich einen Erfolg gehabt hätte, und die Pat. dabei wohl unterlegen wäre, fühlte ich mich nicht veranlasst, eine solche vorzunehmen. Pat. verblieb im Hospital im Ganzen einen Monat und litt während dieser Zeit an Kopfschmerzen, so dass sie gar nicht ausgehen konnte; Erbrechen kam häufig vor; die Pupillen waren stets erweitert, ein Umstand, über den ich nicht klar werden konnte, ob er mit der Geschwulst im Zusammenhange stand, oder eine hysterische Erscheinung war.

3. Im April 1883 trat in das Hospital ein 15jähr. Knabe, der schon in seinem 9. Lebensjahre am inneren Knöchel des rechten Fusses eine kleine

Knochenauftreibung bekommen hatte. Dergleichen Exostosen fanden sich fast an allen Röhrenknochenenden in folgender Weise: Am äusseren Ende des linken Schlüsselbeines waren vorne und hinten je 2; am inneren Rande des rechten Schulterblattes 1; das obere Ende des Oberarmes hatte 1, das untere Ende in- und auswendig je 1; oberes Ende des linken Radius 1, unteres Ende 2; erste Phalanx des linken Ringfingers 1; oberes Ende des rechten Oberarmes 1, unteres Ende desselben 1; unteres Ende des Radius vorne und hinten 2; unteres Ende der rechten Ulna 1; rechter Daumen, Zeige-, Mittel-, Ring- und kleiner Finger je 1. Untere Extremitäten: Das untere Ende des linken Oberschenkels hatte in- und auswendig je 2; das obere Ende der linken Tibia in- und auswendig je 2, dessen unteres Ende nur 1; das untere Ende der Fibula 2; das untere Ende des rechten Oberschenkels 3, und zwar innen, aussen und hinten: das obere Ende der rechten Tibia nach hinten 2; deren unteres Ende innen und vorne 2; beide Enden der rechten Fibula je 2. Die rechte 5.. 4., 2. Zehe an der Basis je 1. Dies macht im Ganzen 52 Exostosen. — Behandlung selbstverständlich — keine. Die meisten dieser Exostosen hatten die Grösse eines Hühnereies, kleinere fanden sich aber ebenfalls vor; die Gestalt dieser letzteren war fast kugelförmig. Die Geschwulst steif und unbeweglich. Das Osteom, möchte ich sagen, entwickelte sich hier augenscheinlich am ganzen Körper und daher würde ich diesen Fall mit dem Namen Osteom universale bezeichnen. — Interessant war es für mich, dass die Schwester des Pat. ganz ähnliche Auftreibungen an beiden Handgelenken hatte; es konnte also die Krankheit als eine ererbte bezeichnet werden.

Aus diesem Grunde möchte ich die Ursache des Osteom nicht local suchen, sondern annehmen, dass die Störungen des Knochenwachsthums, gerade wie bei der Rhachitis, irgend anderswo liegen müssen.

Es ist mir nicht möglich, daraus schon zu urtheilen, ob die Cohnheim'sche Theorie ihre Bestätigung hier findet, nämlich dass alle Geschwulstkeime fötalen Ursprunges seien.

Ausser den drei hier aufgezählten Fällen beobachtete ich im Garnison-Hospitale zu Tokio noch zwei andere. Der eine Fall betraf den Oberkiefer; dabei war es mir sehr schwierig, die Grenzen zwischen den neugebildeten und den Kieferknochen selbst zu finden. — Der andere Fall betraf die innere Seite des rechten Oberschenkels, gerade wie Prof. Bardeleben in Abbildungen gezeigt hat.

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2. Ein Fall von Elephantiasis scroti.

(Hierzu Fig. 6 und 1 Holzschnitt.)

Ein 30jähr. Bauer, am 20. Mai 1879 wegen Elephantiasis scroti in das Hospital aufgenommen, gab an, dass weder diese noch irgend eine andere Krankheit in seiner Familie erblich vorkomme, dass er von Kindheit an kränklich sei, ferner, dass er in seinem 3. Lebensjahre von einem Augenleiden befallen worden sei, in Folge dessen er auf beiden Augen erblindete. Da ihn dieses Gebrechen an der Wahl eines anderen Lebensberufes hinderte, widmete er sich der Musik. Die sitzende Lebensweise machte ihn zum Hypochonder. Im Jahre 1869 stellte sich bei ihm plötzlich eine Anschwellung des Hodensackes ein, welche zwar von selbst verschwand, jedoch im Jahre 1878 von Neuem auftrat. Das Wiederauftreten der Anschwellung war von Fiebererscheinungen und wiederholtem Frösteln begleitet*). Ausserdem fühlte der Kranke heftige, krampfartige Schmerzen, die sich vom Unterleibe längs des linken Hodens hinzogen. Diesmal nahm Patient ärztliche Hülfe in Anspruch, und in Folge derselben verschwand die Anschwellung nach einer Woche. Im nächsten Jahre aber stellte sich dieselbe von Neuem ein. Der Kranke verspürte ausserdem heftiges Jucken am Hodensack. Eine leichte Besserung, die noch einmal in dem Zustande des Kranken eintrat, war nicht von langer Dauer. Nach und nach verdickte sich die Haut des Hodensackes, die Anschwellung desselben nahm nicht mehr ab und erreichte allmälig einen solchen Umfang, dass Patient ohne fremde Hülfe weder stehen noch gehen, ja nicht einmal sitzen konnte.

Der gegenwärtige allgemeine Ernährungszustand war mittelmässig, die Gesichtsfarbe blass, beide Augen erblindet, Respirations- und Circulationsorgane normal. Wenn der Kranke stand, reichte der Hodensack, der ungefähr die Form einer umgekehrten Flasche hatte, beinahe bis zum Malleolus internus. Die rothbräunliche Haut des Scrotums glänzte ein wenig; sie war uneben und zeigte auf der Oberfläche hier und da Erhebungen und Vertiefungen; auch fanden sich an derselben da und dort zusammengeschrumpfte Härchen. Beim Betasten finden sich hier und da harte und weiche, fluctuirende Stellen. In der Mittellinie des Hodensackes befand sich eine 8 Ctm. lange Spalte, aus der der Urin sich nicht in einem Strahle, sondern tropfenweise entleerte. Beim Einführen des Katheters in diese Spalte stiess man auf einen Widerstand leistenden Körper, in welchem ich den Penis vermuthete. Von aussen war von dem Penis und den Hoden Nichts wahrzunehmen. Bei der Percussion überall Dämpfung. Die Messung des Hodensackes ergab folgendes Resultat: Länge vom Schamberg an ungefähr 76 Ctm., grösster Umfang 134 Ctm., kleinster Umfang am Ursprungsorte 48 Ctm. Das Gewicht betrug 94 Kgrm.

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*) Dieselben krankhaften Erscheinungen bei Beginn der Anschwellung führt auch schon im Jahre 1845 Curling in seinem noch immer höchst wichtigen Werke an. (Deutsche Ausgabe. S. 378.)

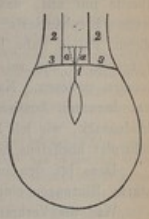
Die Diagnose war unzweifelhaft Elephantiasis scroti. Es konnte darüber um so weniger ein Zweifel obwalten, als der Kranke aus der westlichen Provinz Nagasaki-Ken stammte, in welcher die Elephantiasis des Scrotum und der unteren Extremitäten sehr häufig vorkommt. Was die Behandlung anbetrifft, so sind in Japan einige Fälle bekannt, in denen die Elephantiasis scroti durch die Operation geheilt worden ist. Dieselben sind theils von den in Nagasaki ansässigen holländischen Aerzten, theils von japanischen Aerzten ausgeführt worden. Wie Prof. Kocher mittheilt, sterben nach der Operation viele Kranke in Folge von Hämorrhagie, Embolie und Pyämie. Trotz dieser allgemein ungünstigen Prognose hat Clot-Bey einen Fall operirt, wo die Geschwulst 110 Pfund wog; ferner ward Charles Delacroix, ehemaliger Minister des Auswärtigen, von einer Geschwulst von 32 Pfund glücklich operirt. Liston operirte eine Geschwulst von mehr als 40 Pfund an einem 22jährigen Manne, und Delpech eine Geschwulst von 60 Pfund; eben so Titley. Curling erzählt von einem Falle, in welchem der Hodensack bis 200 Pfund (also 100 Kgrm.), Larrey einem solchen von 50 Kgrm., und Key behandelte einen Chinesen mit einer Geschwulst von über 56 Pfund Schwere.

Da die Geschwulst allmälig eine solche Grösse erreichte, dass der Kranke selbst die Entfernung derselben auf operativem Wege verlangte, und mir ein anderweitiges Mittel zu deren Beseitigung nicht zu Gebote stand, so entschloss ich mich zur Vornahme der Amputatio scroti. Am Tage vor der Operation brachte ich den Hodensack in eine erhöhte Lage, um das Blut und die Flüssigkeit zum Zurückströmen zu bringen. Um die Verkleinerung der Geschwulst herbeizuführen, legte ich einige Minuten vor der Operation eine Esmarch'sche Binde von unten bis zum Schambeine um dieselbe; oben band ich sie mit einer elastischen Binde fest.

Die Operation führte ich am 10. Juli in folgender Weise aus: Zuerst führte ich den Katheter in die oben erwähnte Längenspalte ein bis zu dem Widerstand bietenden Körper; dann führte ich einen Längsschnitt von 12 Ctm. (in dem nachstehenden Holzschnitt mit 1 markirt), worauf ich die beträchtlich verkleinerte Corona glandis fand*). Die zweite Incision bestand aus zwei Parallelschnitten (mit 2 bezeichnet) an der Stelle, wo sich die Samenstränge befinden. Ich suchte nach den Hoden und Samensträngen; den linken Samenstrang fand ich in der Tiefe, von dem rechten war keine Spur vorhanden; wahrscheinlich war derselbe atrophirt. Hierauf führte ich quer über die erste

*) Mit den punctirten Linien ist hier die künstliche Anlegung der Vorhaut angedeutet worden, welche nachher ausgeführt wurde (s. w. u.).

Incision die dritte Incision (mit 3 angedeutet), durch welche ich einen Hautlappen bildete. Ich musste ungefähr 15 Arterien und grössere Venen unterbinden. Ich führte die Querincision bis zur hinteren Seite des Hodensackes und amputirte denselben circulär. An der Schnittfläche fanden sich mit Flüssigkeit angefüllte Hohlräume von Tauben- bis Hühnereigrösse. Die dazwischen liegende Substanz war fibröses Bindegewebe, aus welchem man eine grosse Quantität seröse Flüssigkeit herauspressen konnte. Mein College Miake, Director der medicinischen Facultät in Tokio, untersuchte sofort diese Flüssigkeit; er fand in derselben nur Lymphkörperchen; Spaltpilze, welche nach dem gegenwärtigen Stande der Theorie als die eigentlichen Krankheitsträger angesehen werden, waren in der Flüssigkeit nicht nachweisbar. Der abgetragene Theil des Scrotums wog 43 Kgrm.



Nachdem ich die Nähte angelegt hatte, bildete ich aus dem durch den Querschnitt entstandenen Hautlappen eine Vorhaut für den Penis, welche ich unten durch Nähte vereinigte. Aus dem unteren Rande des Hautlappens bildete ich die vordere Fläche das Hodensackes, dessen hintere Fläche offen blieb, weil die zur Bildung der hinteren Fläche verwendete Haut so dünn war, dass die Anlegung von Nähten unterlassen werden musste. Die Blutung war während der Operation nicht so stark, wie ich befürchtet hatte. Es waren im Ganzen 20 Ligaturen nöthig. Varicöse Erweiterungen der Venen waren ebenfalls nicht in grosser Anzahl vorhanden. Dagegen enthielt, wie schon bemerkt, das Bindegewebe in den in demselben befindlichen Hohlräumen eine grosse Quantität Flüssigkeit, deren Sammlung und genaue Messung trotz der hierzu getroffenen Vorkehrungen nicht möglich war. — Das Befinden des Kranken nach der Operation war durchaus nicht befriedigend. Er erbrach sich in der Nacht öfters, sah sehr blass aus, sein Gesicht war von klebrigem Schweisse bedeckt, die Respiration war schwer, der Puls sehr frequent. Ich machte Campherinjectionen, durch welche indessen diese Erscheinungen nicht gehoben wurden. Am 11. Juli, im Laufe des Vormittags, starb der Kranke an Collaps.

An dem Misserfolge der Operation hatte wahrscheinlich die übermässige Ansammlung von Flüssigkeit in den Hohlräumen des Bindegewebes des Hodensackes einen grossen Antheil. Man kann sagen, dass nicht die Geschwulst von dem Körper, sondern dass der Körper von der Geschwulst abgetrennt wurde. Wahrscheinlich war in Japan in allen Fällen, in denen die Operation mit günstigem Erfolge vorgenommen wurde, die Geschwulst nicht so gross, sondern sie ging höchstens bis an das Knie und das Bindegewebe wog in ihr vor; in diesem Falle ist die Geschwulst dichter und fester und nicht so weich und fluctuirend.

Da die Elephantiasis scroti in Tokio selten vorkommt, ist kein weiterer Fall dieser Krankheit zu meiner Beobachtung gelangt. Aber mein Freund Yoshida, Chef des Hospitales in Nagasaki, theilte mir mit, dass er schon mehrmals die Amputatio scroti wegen Elephantiasis mit günstigem Erfolge ausgeführt habe. In diesen Fällen habe jedoch hypertrophirte Haut und Bindegewebe vorgewogen und sei nur eine geringe Quantität Flüssigkeit vorhanden gewesen. Nach meiner Ueberzeugung kann die Operation nur dann mit Aussicht auf Erfolg vorgenommen werden, wenn der Hodensack, wie ich bereits gesagt, sich fest anfühlt und die Geschwulst höchstens bis an die Kniee reicht.

Dass bei den in Japan ausgeführten Operationen dieser Art starke Blutungen eingetreten wären, habe ich nie gehört.

Was die Verbreitung und Aetiologie der Elephantiasis scroti betrifft, so kommt nach Kocher diese Krankheit besonders in wärmeren Ländern (Aegypten, Indien, Brasilien, Mexiko, Türkei) vor; in Europa, insbesondere in Deutschland und Frankreich, ist sie selten. In Japan kommt sie im südwestlichen Theile des Landes, am westlichen Rande der Kiushiu-Insel, vor; auf der Goto-Insel ist sie, wie man behaupten darf, endemisch. Die Angaben der Bewohner der Goto-Insel scheinen die Ansicht Virchow's über die Natur und Aetiologie dieser Krankheit zu bestätigen. Auch der von mir beobachtete Fall bestätigt hinsichtlich des anatomischen Befundes die Ansicht Virchow's. Auf der Insel Goto behauptet man, dass die Krankheit erblich sei, doch liegen keine Beweise für die Richtigkeit dieser Behauptung vor.

Für das häufigere Vorkommen der Krankheit in den wärmeren Ländern weiss ich keinen Erklärungsgrund zu geben; man nimmt an, dass die reichlichere Schweissabsonderung eine Reizung der Haut bewirkt, welche das Entstehen erysipelatöser Hautentzündungen begünstigt, in deren Gefolge die Elephantiasis auftritt.

Da ich bis jetzt bloss ein einziges Mal Gelegenheit hatte, eine Elephantiasis scroti zu behandeln, so kann ich das behauptete Vorkommen von Spaltpilzen in den erkrankten Geweben weder bestätigen, noch in Abrede stellen.

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3. Ein Fall von Galaktocele (nach Vidal) und ein Fall von Hämatocele in Folge einer Dermoidcyste.

A. Ein 28jähriger Bauer wurde am 22. November 1880 mit beträchtlicher Vergrösserung des Hodensackes in das Hospital Shitaya aufgenommen. Nach seiner Angabe hatte er 4 Jahre vorher eine Contusion des Hodensackes erlitten. Unmittelbar nach diesem Unfalle trat weder Schmerz noch Hodensackanschwellung ein. Erst 5 Tage nachher fühlte er einigen Schmerz beim Gehen. Gleichzeitig trat eine Anschwellung des Hodensackes ein, die nach und nach eine beträchtliche Grösse erreichte.

Status praesens: Die Länge des Hodensackes beträgt 32½ Ctm., der Umfang desselben 22 Ctm. Die Geschwulst hat das Aussehen einer grossen Hydrocele. Bei der Betastung fühlte man in der Geschwulst Fluctuation, aber die Flüssigkeit war nicht durchscheinend. Ich nahm eine Probepunction vor. Aus dem Einstiche entleerte sich eine weissgelbliche, milchartige, getrübte, undurchscheinende Flüssigkeit, in welcher ich bei der sofort vorgenommenen mikroskopischen Untersuchung Fettkügelchen verschiedener Grösse fand. Dagegen entdeckte ich in derselben nichts dem Filariawurm Aehnliches. (Ein mir persönlich bekannter portugisischer Arzt, Dr. P. de Magalhaes aus Rio de Janeiro, theilte mir mit, in drei Fällen in der geronnenen Flüssigkeit Filaria gefunden zu haben.)

Diagnose: Nach dem Ergebnisse der mikroskopischen Untersuchung der durch die Probepunction gewonnenen Flüssigkeit leidet der Kranke nicht an Hydrocele, sondern an Galaktocele.

Behandlung: Ich machte mit dem grossen Trocart eine Punction. Es entleerten sich aus der Geschwulst 350 Grm. Flüssigkeit, worauf der Hodensack seine normale Grösse wieder erlangte. Ich machte hierauf eine Jodinjection, um zu sehen, ob sich in dem Hodensack wieder eine ähnliche Flüssigkeit ansammeln würde. Ich forderte den Kranken auf, er solle sich wieder im Hospitale einfinden, sobald er die Bildung einer neuen Ansammlung wahrnehmen würde. Ich habe aber seitdem nichts mehr von ihm gehört.

B. Am 20. September 1881 wurde ein 51jähriger Kaufmann mit einer Hodensackgeschwulst in das Hospital aufgenommen. Der Kranke gab an, die Hodensackgeschwulst bestehe schon seit seiner Kindheit, in seinem 25. Lebensjahre habe sie an Umfang zugenommen, worauf sie bis zum Jahre 1881 stationär geblieben sei, von da an sei von Neuem eine Vergrösserung derselben eingetreten; im März 1881 habe ein Arzt die Punction der Geschwulst ausgeführt, wobei sich nur Blut aus derselben entleert habe.

Status praesens: Umfang der Geschwulst 68 Ctm., Länge derselben 74 Ctm. Der Penis ist nicht vollständig in der Geschwulst verschwunden.

Diagnose: Ich machte eine Probepunction, wobei sich aus der Geschwulst eine blutige Flüssigkeit entleerte, in welcher ich bei der mikroskopischen Untersuchung gesternte, alte, rothe Blutkörperchen fand. Ich war

deshalb der Ansicht, dass ich es mit einer Haematocele zu thun hätte, welche aus einer alten Hydrocele mit verdickten Scheidewänden entstanden wäre.

Behandlung: Ich dachte, dass bei einer Hämatocele mit verdickten Scheidewänden die Vornahme der Incision angezeigt wäre. Die Operation nahm folgenden Verlauf: Ich machte einen Längsschnitt in die Geschwulst, worauf sich aus derselben 1400 Grm. entfärbtes halbgeronnenes Fibrin entleerte, wie wenn dieselbe ein Aneurysma gewesen wäre. Als ich den Grund der Wunde untersuchte, fand ich in demselben eine Cystenähnliche Geschwulst, in welche ich die Probepunction machte. In der entleerten Flüssigkeit fand ich bei der mikroskopischen Untersuchung wieder gesternte, alte, rothe Blutkörperchen. Ich führte hierauf die Incision der Geschwulst aus, wobei sich aus derselben 720 Grm. blutige Flüssigkeit entleerte. Nachdem ich die Wundfläche mit einprocentiger Carbolsäure gereinigt hatte, fand ich auf dem Boden der Geschwulst einen weiteren Tumor, welchen ich gleichfalls öffnete. Es befand sich in demselben aber keine blutige Flüssigkeit, sondern eine weisse klebrige Substanz. Da, wie ich aus diesem Befunde schloss, die Hoden gleichfalls entartet waren, führte ich die Castration aus. Ich entfernte auch einen Theil der verdickten Scheidewand, weil dieselbe mit kalkigen Concrementen besäet war. Ich behandelte die Wunde mit Jodoformgaze und legte eine Drainage an. In den Hoden, welche ich der Länge nach auseinanderschnitt, fand ich vollständig entwickelte Haare und Gallenfett. Die oben erwähnte weisse, klebrige Substanz war also wahrscheinlich ebenfalls Cholestearin. Aus dem Befunde ging hervor, dass es sich in diesem Falle um eine in Folge einer Dermoidcyste eingetretene Hämatocele handelte. Die Geschwulst sah nach der Incision gerade wie ein gespaltener Aneurysmasack aus. Auf diese Aehnlichkeit haben auch schon Andere hingewiesen, z. B. Erichsen.

C. Die Hydrocele ist in Japan eben so häufig, wie in Europa, und ich habe vielfach Gelegenheit gehabt, sie zu behandeln. Das Wichtigste bei der Diagnose derselben ist vor Allem, mit Sicherheit zu erfahren, was für eine Flüssigkeit sich gebildet habe. Das bisher anempfohlene Kriterium des Durchscheinens kann ich, wie Manche wollen, als durchaus zutreffend nicht erachten; ich möchte dabei auf eine hier nothwendige Distinction aufmerksam machen. Wenn nämlich ein Durchscheinen nicht Statt hat, so ist keineswegs schon der Schluss zu machen, dass Wasseransammlung nicht vorhanden, denn es kann die Scheidenhaut verdickt, sogar mit kalkigem Concrement übersäet sein. In diesem Falle lässt sich nur durch Punction die Natur der Flüssigkeit erkennen. Findet sich die Scheidenhaut verdickt, so führe ich nur Incision aus, jedoch unter strenger antiseptischer Behandlung. Die sich durch Durchscheinen offenbarende Hydrocele behandele ich natürlicher Weise durch Punction mit oder ohne Jodinjection; bei Knaben aber habe ich die Injection nicht angewendet. In meiner militärischen Praxis sind mir 3 Fälle von Recidiv nach der Incision vorgekommen.

4. Beitrag zur Behandlung der Sarkome und Carcinome.

Wenn man durch den besonderen Vorzug einer reichen Praxis in der Behandlung der Sarkome und Carcinome vielfach Gelegenheit gehabt hat, mannichfaltigste Erfahrungen zu sammeln, so wird man leicht dahin geführt, auf Grund dieser Erfahrungen sich das Urtheil über Sarkome und Carcinome zu verallgemeinern, um ihrer räthselhaften Natur näher zu treten. In dieser besonderen Lage bin ich; allein ich sage mir durchaus, dass, ohne umfassende Erfahrungen über alle Arten von Geschwulstbildungen, ein verallgemeinerndes Urtheil doch sehr fehlgehen müsse, und ich mag darum nicht über das Gebiet der Beobachtungen mich hinaus wagen, welche freilich mich überzeugt haben, dass zu einer rationellen Behandlung es noch an jeder sicheren Grundlage uns gebricht. Keineswegs verkenne ich die wichtige, die Forschung leitende und fördernde Bedeutung der ätiologischen Theorieen von Virchow, Cohnheim, Billroth, Lücke u. A., aber mir hat die Praxis in der Behandlung der Sarkome und Carcinome Fragen aufgeworfen, die jeder befriedigenden Beantwortung sich noch entziehen und uns daher auf die Beobachtung lediglich noch immer verweisen, so dass Mittheilungen aus der Praxis lange noch von Wichtigkeit bleiben werden für kritische Beurtheilung, oder für Bestätigung und für Modificirung aufgestellter Behauptungen. Mittheilungen aus meinen Erfahrungen in Japan dürften daher mich immerhin von einiger allgemeinen Wichtigkeit bedünken, und so lasse ich hier folgen, was lediglich Erfahrung mir gewiesen hat.

Dass Sarkome und Carcinome zu den sogenannten bösartigen Geschwülsten gehören, darüber kann kein Zweifel bestehen, wenn auch sonst diese Begriffe des Gutartigen und Bösartigen streng genommen relativ sind, als z. B. das sonst „gutartig“ zu nennende Lipom in der Halsgegend wohl als eine bösartige Geschwulst genommen werden muss. Als Kriterium der Bösartigkeit muss un-

streitig vor Allem das Recidiviren gelten; Sarkome und Carcinome haben stets grosse Neigung zu Recidiven und sind zugleich mehr oder weniger von allgemeiner Dyskrasie begleitet. Die Sarkome sind daher bösartiger zu nennen, als die Carcinome, da bei ihnen bei Weitem die Recidive schneller sich entwickeln. In Japan treten Sarkome der verschiedenen Körpertheile leider sehr häufig auf — häufiger, als in Europa —, sowohl bei jungen, als bei älteren und alten Leuten, am Häufigsten in den mittleren Lebensjahren (zwischen 30. und 50. Jahre). In Betreff der Multiplicität, Diagnose, Recidive, Aetiologie, Therapie, speciell der operativen Seite derselben, möge aus meinen Erfahrungen in Japan hier Allgemeines zunächst folgen, dem einige praktische Fälle sich dann anschliessen sollen.

Zur Multiplicität hätte ich zu sagen, dass vielfache Beobachtung mich die Ueberzeugung hat gewinnen lassen, dass die Art der Verbreitung der Carcinome über den Körper auf dem Wege der Saftcanäle und der feineren Lymphgefässe Statt hat, und dass hier nur einige Lymphgefässe als die Träger der Infection erscheinen, welche alsdann den zunächst gelegenen Lymphdrüsen mitgetheilt wird. In Betreff der Sarkome aber habe ich Virchow's Ansicht vollkommen in meiner Praxis bestätigt gefunden; die Verbreitung geschieht hier wirklich ohne Betheiligung der zunächst liegenden Lymphdrüsen, und auch ich muss meinen, dass dies eine nicht mehr zu erschütternde Thatsache genannt werden darf.

Was die Diagnose anbetrifft, so kann ich von gar manchen Schwierigkeiten berichten. Zunächst unterstützt hierbei einigermaassen die Localkenntniss des Vorkommens; zu bemerken wäre hier, dass Sarkome gerne am unteren Ende des Oberschenkels und am oberen Ende der Tibia entstehen. Was die sonstigen Mittel der Geschwulstdiagnose überhaupt, namentlich Palpation und Inspection gewähren, ist von weniger sicherem Anhalt für die Entscheidung; bei der Palpation ist besonders auf genaues Erkennen der Abgrenzung der Schwellung die Untersuchung zu richten. Im Uebrigen ist die Anamnese weniger werthvoll, namentlich wenn man sie nicht — was freilich nur eine grosse Praxis an die Hand giebt — durch Vergleichung in Benutzung zu ziehen weiss; das Alter der Krankheit ist jedoch wichtig zu wissen. Allein trotz alle dem sind

mir Verwechselungen passirt, z. B. des Sarkoms mit nicht mehr pulsirendem Aneurysma in der Kniekehle und am Halse; erst wiederholte Punction verschaffte mir sichere Diagnose*). Ferner gelangte ich bei dem seltener vorkommenden primären Carcinom der Inframaxillardrüse, das ich Anfangs, namentlich wegen der besonders starken Schmerzen, als eine entzündete Drüsenschwellung nahm, erst durch Vornahme einer Probeincision zu der richtigen Diagnose. Auch giebt es Fälle, wo selbst die mikroskopische Untersuchung ein sicheres Resultat nicht ergiebt, und uns Nichts übrig bleibt, um sicher zu gehen, als den Verlauf der Krankheit beobachtend abzuwarten; so beim Anfangsstadium des Uteruscarcinoms mit Ulceration in Folge von Entzündung bei noch jüngeren Frauen in den Dreissigern, und solcher Art verdächtige Geschwüre kommen in Japan bei Frauen oft vor. Ferner konnte ich ein Netzcarcinom auch erst durch Punction mit Sicherheit diagnosticiren. Ich mache kein Hehl aus meinen Irrthümern in der Diagnose; ich berichte diese Fälle, um zu erweisen, dass man es mit der Diagnose nicht sorgsam genug nehmen könne. Mein Resultat der Erfahrung ist, dass das einzig sichere Mittel zur Feststellung der Diagnose der Geschwulst oftmals nur die Probeincision und -Punction ist; für die letztere ist das Middeldorpf'sche Harpunenartige Stilet zu empfehlen, und was die Incision anbetrifft, so ist ihre Gefahr ja durch strenge antiseptische Behandlung äusserst gemindert.

Wenn ich nun zu den Recidiven übergehe, so ist hier nur im Allgemeinen zu bemerken, dass die Recidive bei Sarkomen bei Weitem schneller Statt haben, als bei Carcinomen; auch scheinen bei jüngeren Personen die Recidive sich bei Carcinom schneller zu entwickeln, als bei älteren Personen**). Von besonders langsamer Entwickelung scheinen die Recidive der Scirrhus zu sein, also bei

*) Vergl. Aneurysmen.

**) Mir hat sich in meiner Praxis auch bestätigt, dass das Sarkom mehr bei jüngeren, das Carcinom dagegen mehr bei älteren Personen vorkommt. Was nun die Recidive des Carcinoms betrifft, so nehmen auch sie eine langsamere Entwickelung bei älteren Personen, so dass die Vermuthung wohl nahe liegen dürfte, dass, abgesehen von der speciellen Beschaffenheit gefässreicher Carcinome, hier die allgemeine senile Involution der Zellen des gesammten Organismus eine grosse Rolle spielt. Ich mache diese Anmerkung für die Therapie, um die Operation indicirt zu erkennen.

denjenigen Carcinomen, bei welchen das Bindegewebe vorwiegend von den Geschwülsten ergriffen ist.

In Betreff der Aetiologie der Sarkome und Carcinome kann ich nur sagen, dass die verschiedentlich aufgestellten Theorieen für die Therapie noch gänzlich ohne Bedeutung sind, und dass alle bisher angewendeten Medicamente nur palliativen Werth haben, weil sie die Ursache nicht treffen. Selbst das Allgemeinste, ob die Ursache eine locale, oder ob eine weitere, ist bis jetzt nicht zu entscheiden gewesen.

Von dem Wichtigsten, der Therapie, und insbesondere von der operativen Seite*) derselben, möchte ich nun, unter Verweisung auf die nachfolgend dargestellten Krankheitsfälle, einiges Wenige andeutungsweise hier folgen lassen. — Im Allgemeinen ist von der operativen Behandlung der Sarkome und Carcinome zu sagen, dass sie wenigstens eine zeitweise Heilung zur Folge hat. Indessen muss doch in jedem einzelnen Falle auf das Sorgsamste erwogen werden, ob eine Operation unternommen werden dürfe. Denn manchmal, und zwar besonders bei Sarkomen, tritt nach der Operation anstatt der Besserung eine Verschlimmerung ein; diese Fälle sind zumal diejenigen, wo entweder nicht alle erkrankten Partieen exstirpirt werden konnten, oder wo die Operation erst in bereits weit vorgeschrittenen Stadien der Krankheit vorgenommen wurde. In diesen Fällen traten nach wenigen Wochen erschrecklich grosse Recidive ein. Sonst aber kann doch durch das operative Verfahren das Leben 2—3 Jahre erhalten werden, trotz der gefahrvollsten Operationen, wie denn von Billroth in Wien Darmresection**)

*) Unter den Medicamenten ist Arsenik das beliebteste, und auch ich habe schon bei Sarkomen der Lymphdrüsen (nach Billroth) durch Injection von Sol. ars. Fowl. 0,3 Grm. und Aq. dest. 0,6 Grm. wohl überraschende, jedoch nur palliative Wirkung gesehen. Ein englischer Arzt will die Carcinome mit Terpenthin curiren.

**) Eine Darmresection wegen Carcinom habe ich noch nicht vorgenommen; aber von einer Gastrotomie, die ich ausführte, möchte ich hier Gelegenheit nehmen zu berichten. Ich führte diese Operation bei einem jungen Manne (einem Barbier) aus, der eine Zahnbürste durch unvorsichtige Führung beim Reinigen des Halses in die Speiseröhre hatte hineingleiten lassen. (Die im Volke gebräuchlichen Zahnbürsten sind ca. 30 Ctm. lang, bestehen ganz aus Holz mit einem besenartigen Büschel am unteren Ende, der jedoch nicht angesetzt ist, sondern aus den feinen Zerspaltungen des Holzes besteht.) Am Tage des Verschluckens verspürte der Mann noch kein Schmerzgefühl, erst am 3. Tage stellten sich Schmerzen ein, und der Mann kam zu mir. Der Pat. zeigte erhöhte Temperatur, hatte Neigung zum Erbrechen, der Unterleib erschien angeschwollen, der Schall am Bauche tympanitisch. Zwischen dem Processus xiphoideus und dem Nabel war eine bei der Betastung sehr empfindliche Stelle und hier der Schall etwas gedämpft. Unter Narkose nahm ich sofort die Operation vor. Ich legte durch Incision längs der Linea alba den Magen bloss; in der Mitte der vorderen Wand des Magens fühlte ich die Spitze der Zahnbürste. Ich hob nun die Magenwand und zog durch eine bereits durch die Bürstenspitze verursachte Perforation die Bürste heraus. An dem Orte der Perforation war bereits Entzündung und Ulceration entstanden; ich incidirte nun, unter strenger antiseptischer Behandlung, rund herum um die Perforationsöffnung und schaffte dadurch Anfrischung der Wunde, alsdann führte ich die Nähte nach Emmert's Methode aus. Der Pat. befand sich nach der Operation wohl; ich liess ihn vom 3. Tage an Milch geniessen. Am 4. Tage entstand Erbrechen, die Temperatur, die anfänglich 38,5, stieg auf 39,5, der Puls von 100 auf 120. Die Schwellung des Bauches ward stärker. Am 7. Tage nach der Operation trat durch Collaps der Tod ein. Wahrscheinlich war durch die Perforation der Mageninhalt bereits in die Bauchhöhle getreten.

wegen Carcinom vorgenommen worden ist; auch Uterusexstirpation von der Vagina aus. — Bei Sarkomen der Extremitäten entscheide ich mich in denjenigen Fällen, wo nach der Exstirpation doch nur Recidiv eintreten würde, durchaus für die Amputation, um dadurch doch noch längere Zeit das Leben erhalten zu haben.

Als Mittel der operativen Wegschaffung dürften als die sichersten nur Exstirpation und Aetzung gelten; untergeordnete Mittel von ganz fraglichem Erfolge sind Ligatur, Ecrasement, Galvanokaustik, Elektrolyse, obwohl dann und wann noch verwendet.

Ich will nun hier aus meiner reichen Hospitalpraxis in Japan einige mir besonders lehrreich gewesene Fälle von Sarkom und Carcinom folgen lassen, welche das oben im Allgemeinen Gesagte näher begründen werden. Zunächst Fälle von Sarkomen.

I. Ein 27jähriger Zimmermann wurde am 15. Mai 1879 wegen einer Kindkopfgrossen Geschwulst an der vorderen äusseren Seite des oberen Drittels des rechten Oberschenkels in das Hospital Shitaya aufgenommen. Der Kranke gab an, dass er vor 5 Jahren am Oberschenkel eine Quetschung erlitten, welche die Bildung einer Hühnereigrossen Verhärtung zur Folge gehabt. Diese Verhärtung verursachte Anfangs keine besonderen Beschwerden; aber seit Februar 1878 nahm dieselbe nach und nach einen grösseren Umfang an. (Die Angabe des Kranken über die Entstehungsursache der Anschwellung wird jedoch mit Vorsicht aufzunehmen sein.)

Die allgemeine Ernährung des Patienten erschien mittelmässig, Respirations- und Circulationsorgane aber normal; die Hautdecke der Geschwulst war nicht geröthet, die Hautvenen waren erweitert; die Geschwulst elastisch, gegen Druck aber unempfindlich; sie war nicht deutlich abgegrenzt, vielmehr continuirlich in die gesunden Theile übergehend, nicht hin und her beweglich. Der Umfang des kranken Beines 48 Ctm., Umfang des gesunden 32 Ctm.

Die Diagnose ergab mir, dass nach dem Entwicklungsprocesse und der äusseren Erscheinung der Geschwulst mit grosser Wahrscheinlichkeit anzunehmen, dass dieselbe sarkomatöser Natur sei. Wegen der überhaupt stets ungünstigen Prognose für die Exarticulation des Hüftgelenkes beschloss ich, die Geschwulst zu exstirpiren. Der Kranke willigte in die Vornahme der Operation ein. Am 16. Mai führte ich die Exstirpation in folgender Weise aus: Ich führte zuerst einen Längsschnitt von 20 Ctm. Nach der Durchschneidung der Haut und der Fascien versuchte ich die Geschwulst abzulösen. Dies gelang mir jedoch nicht. Ich sah mich genöthigt, den Tumor mit den angrenzenden gesunden Partieen des Musculus quadriceps herauszuschneiden. Das Befinden des Kranken nach der Operation war befriedigend. Die Wunde, welche nach der Lister'schen Methode behandelt wurde, war nach 3 Wochen beinahe zugeheilt. Die Wundränder vereinigten sich zum Theil per primam intentionem, zum Theil per granulationem. Wegen der grossen Ausdehnung der Wundfläche habe ich mit einigen Hautpartieen die Reverdin'sche Transplantation vorgenommen. Fünf Wochen nach der Operation war die Wunde vollständig zugeheilt. Der Kranke wurde aus dem Hospitale entlassen und ging wieder seinen Arbeiten nach. Die mikroskopische Untersuchung der Substanz der Geschwulst wies viele Spindelzellen und vereinzelte Rundzellen nach.

Im 6. Monate nach der Entlassung aus dem Hospitale wurde der Mann von einem heftigen Husten befallen, der mit reichlichem Schleimauswurfe verbunden war. Der Husten liess nach einiger Zeit ein wenig nach, stellte sich aber bald wieder mit erneuter Heftigkeit ein. In dem ausgeworfenen Schleime waren zuweilen Blutstreifen bemerkbar. Von Zeit zu Zeit fühlte der Kranke auch Brustschmerzen. Nachdem derselbe einige Zeit in ärztlicher Privatbehandlung gestanden, liess er sich wieder in's Hospital aufnehmen. — Jetzt war die Abmagerung nicht auffallend; Gesichtsfarbe auch nicht anämisch. (Bei den an Sarkom Erkrankten beobachtete ich gerade, wie dies bei den an Carcinom leidenden Personen der Fall ist, ein eigenthümliches kachektisches Aussehen.) Die auffallendste Erscheinung war eine Dyspnoe, die so heftig war, dass der Kranke kaum sprechen konnte. Bei der Percussion constatirte ich auf der linken Brust eine ausgedehnte Dämpfung, die sich von der Lungenspitze bis zum sechsten Rippenraume erstreckte. Die Lage des Herzens hatte eine rechtsseitige Verschiebung erlitten; die Herzspitze hörte ich in der rechten Linea mammalis. Bei der Auscultation der linken Lunge hörte ich deutlich Bronchialathmen. Der Pectoralfremitus war auf der linken Seite schwächer, als auf der rechten. Bei der Untersuchung des Sputums wurden Schleim und hie und da Blutkörperchen, aber keine elastischen Fasern nachgewiesen. Puls 102, Temp. 37. — Seit der Aufnahme des Patienten in's Hospital wurde eine aufsteigende Abendtemperatur nicht beobachtet; Pulsfrequenz über 100 und Dyspnoe stets vorhanden. Alle Symptome wiesen auf das Bestehen einer ausgedehnten Infiltration des Lungengewebes und eines Exsudates in der Pleurahöhle hin. Die Infiltration war aller Wahrscheinlichkeit nach sarcomatöser Natur, weil früher Geschwulst da gewesen und die Abmagerung des Körpers nicht so wie bei anderen wirklichen Lungenkranken der Fall ist. Der Kranke starb nach ungefähr einem Monate an Lungenoedem. Er war, selbst zur Zeit seines Todes, nicht auffallend abgemagert.

Die Section ergab Oedem der unteren Extremitäten. Eine Hautnarbe an der vorderen Seite des rechten Oberschenkels; Verschiebung des Herzens von der linken nach der rechten Seite, dem Percussionsbefunde entsprechend. Eine geringe Quantität Blutcoagula in dem Herzbeutel. Sehr starke Verwachsung der linken Pleura mit der Lunge; in der linken Pleurahöhle Blutcoagula in grosser Menge, halb geronnenes Blut und eine breiartige Flüssigkeit, welche sich unter dem Mikroskope als Spindel- und Rundzellensarkom erwies. Auf der Oberfläche der linken Lunge, welche ich herausnahm, fanden sich vereinzelte Knoten, welche bei der Untersuchung mit blossem Auge eine grosse Aehnlichkeit mit den Sarkomknoten zeigten. Nur in dem unteren Lungenlappen ist ein wenig Luft enthalten. Die linke Lunge, welche ich ebenfalls herausnahm, war hyperämisch, aber zum grössten Theile mit Luft gefüllt. Die Herzklappen waren normal.

Dieser Fall ist deshalb interessant, weil die Recidive in einem von dem ursprünglichen Krankheitssitze so entfernten Organ auftraten, und daher findet man gerade bei Sarkomen viel häufiger als bei Carcinomen, dass die nächstgelegenen Lymphdrüsen übersprungen und sofort Metastasen in der Lunge neu verursacht werden, was mit der vorwiegenden Neigung der Sarkome, in die benachbarten Blutgefässe einzuwuchern, zusammenhängen dürfte.

2. Ein 31jähriger Mann wurde am 11. Januar 1879 mit Tumor an dem rechten Unterschenkel in das Hospital Shitaya aufgenommen. Derselbe gab an, dass er vor 3 Jahren zum ersten Male Schmerzen am rechten Unterschenkel gefühlt, die durch eine an demselben entstandene Hühnereigrosse Geschwulst hervorgerufen wurden. Er habe später einen in Japan ansässigen englischen Arzt zu Rathe gezogen; dieser habe die Punction des Tumors ausgeführt, die aus demselben entleerte Flüssigkeit mikroskopisch untersucht und an das kranke Bein einen Compressivverband angelegt. Die Behandlung mit dem Compressivverbande sei wirkungslos gewesen, vielmehr habe die Geschwulst während seines einmonatlichen Aufenthaltes in dem von dem englischen Arzte geleiteten Hospitale an Grösse zugenommen. — Gegenwärtig erschien der allgemeine Ernährungszustand gut; das Aussehen nicht anämisch, nicht kachektisch; Respirations- und Circulations-Apparat normal. An der vorderen äusseren Seite des linken Unterschenkels befindet sich eine bedeutende Anschwellung, die Hautdecke der Geschwulst ist nicht geröthet; die Hautvenen sind sehr erweitert. Die Geschwulst ist nicht deutlich abgegrenzt; sie fühlt sich elastisch an und ist nicht verschiebbar.

Die Diagnose ergab mir unzweifelhaft Sarkom. Ich schlug nun dem Kranken vor, die Amputation des unteren Drittels des Oberschenkels vorzunehmen. Die Gründe, welche mich bestimmten, der Amputation am unteren Drittel des Oberschenkels den Vorzug zu geben, waren folgende: 1) ich vermuthete, dass die Tibia durch den Krankheitsprocess in Mitleidenschaft gezogen sei, indem die Geschwulst selbst entweder vielleicht vom Knochenmark

oder von dem Periost ausgegangen sei; 2) fürchtete ich, dass, wenn ich die Exstirpation vornähme, Recidive doch wieder schnell eintreten würden; 3) ist die Prognose für die Amputation an diesem Orte verhältnissmässig günstiger als am oberen Theile. Der Kranke willigte indessen in die Vornahme der Operation nicht ein. Ich machte ihm hierauf den Vorschlag, er möchte gestatten, dass ich die Exstirpation der Geschwulst versuchte und die Amputation nur dann vornähme, wenn sich die Nothwendigkeit derselben im Verlaufe der Operation herausstellen würde. Diesem Vorschlage gab der Kranke seine Zustimmung. Ich führte die Operation in folgender Weise aus:

Ich machte eine grosse Incision in den Tumor, durch welche ich die Tibia mit dem Finger untersuchte. Da ich dieselbe brüchig fand, so nahm ich die Amputation des Beines im unteren Drittel des Oberschenkels vor. Die anatomische Untersuchung der Geschwulst ergab, dass sie gleich unmittelbar unter dem Kniegelenk begann und circa zwei Drittel des Unterschenkels erfasst hatte. Die gesammte Musculatur der Wade war sarkomatös entartet; die Vorderseite des Unterschenkels war hier und dort mit sarkomatösen Knoten besäet. Tibia und Fibula hatten ihre Gestalt verloren; die zurückgebliebenen Knochenpartieen davon waren bröckelig. Zu sagen freilich ist hiernach nicht, ob die Sarkombildung von dem Knochenmark, der Beinhaut oder von den Fascien ausgegangen. Die Wunde wurde nach der Lister'schen Methode behandelt. Die Wundränder vereinigten sich grösstentheils per primam intentionem. Nach 4 Wochen wurde der Kranke aus dem Hospitale entlassen, nach 2 Monaten konnte er mit einem künstlichen Beine gehen. Es war sogar in der Entwickelung der Musculatur kein Unterschied zwischen der gesunden und der kranken Seite bemerkbar. Der Mann befand sich während 2 Jahren den Umständen entsprechend wohl. Ein Kind, welches ihm zu dieser Zeit geboren wurde, starb im Alter von 6 Monaten an Meningitis tuberculosa. Zwei Jahre nach seiner Entlassung aus dem Hospitale bekam er eine kleine Anschwellung an der linken Schläfe, derentwegen er wieder in's Hospital Shitaya aufgenommen wurde. Ich musste damals gerade eine Militär-Inspectionsreise antreten. Der Kranke liess sich in ein anderes Hospital aufnehmen, in welchem man die Exstirpation der Geschwulst vornahm. Noch während der Wundheilung traten Recidive ein und machten so rasche Fortschritte, dass jeder neue operative Eingriff aussichtslos erschien. Der Kranke starb an Druck auf das Gehirn.

Wenn man die beiden vorstehenden Fälle vergleicht, findet man, dass das Recidiv sowohl nach der Exstirpation, als auch nach der Amputation eintrat; nur stellte es sich nach der ersteren früher ein, als nach der letzteren. In beiden Fällen war die Natur der Geschwulst die nämliche. Ich bitte, das oben im Allgemeinen Angedeutete hier zu vergleichen.

3. Ein 22jähriger Bauer wurde am 3. Mai 1879 mit Tumor am Oberschenkel in das Hospital Shitaya aufgenommen. Ein Jahr vorher hatte er beim Arbeiten plötzlich einen heftigen Schmerz an der inneren Seite des

rechten Oberschenkels verspürt. Vierzehn Tage nachher bemerkte er an derselben Stelle eine kleine Anschwellung, zu deren Heilung er sich einer Massage-Cur unterzog. (Seit den ältesten Zeiten, seit ungefähr 1000 Jahren, ist die Massage ein in Japan sehr beliebtes Volksheilmittel gegen die verschiedenartigsten Krankheiten.) Der Tumor verschwand jedoch nicht; er nahm vielmehr in dem Maasse zu, dass der Kranke im October 1878 nur noch mit Mühe gehen konnte. Seit Januar 1879 machte die Krankheit so rasche Fortschritte, dass der Kranke gar nicht mehr gehen konnte. — Gegenwärtig war die Gesichtsfarbe anämisch, das Gesicht auffallend abgemagert. Die Geschwulst beginnt unmittelbar unter dem Ligamentum Poupartii und reicht bis unmittelbar unter das Knie; dieselbe ist eiförmig. Der grösste Umfang des angeschwollenen Oberschenkels beträgt 104 Ctm. Der ganze Oberschenkel bildete eine enorme Geschwulst. Die Hautvenen sind erweitert, geschlängelt; an dem Tumor sind hie und da harte und weiche Stellen zu fühlen.

Die Diagnose musste unzweifelhaft auf ein Sarkom hindeuten. Da der Tumor so überaus gross war, so hätte allerdings die Exarticulatio femoris vorgenommen werden müssen; allein der Kranke befand sich in einem so herabgekommenen Zustande, dass vorauszusehen war, er würde die Operation nicht mehr überstehen können.

Dieser Fall ist der exorbitanten Geschwulst wegen interessant, und ich glaube, dass ein Sarkom von solcher Grösse kaum jemals von Anderen in Japan beobachtet worden ist.

Ich hatte ausserdem Gelegenheit, Sarkome des Oberkiefers mit Nasenpolypen und solche des Unterkiefers zu beobachten; die Krankheit war aber in allen Fällen schon so weit vorgeschritten, dass die Resection des Kiefers unausführbar erschien. In anderen Spitälern wurde in Fällen, in denen der Krankheitsprocess schon sehr grosse Fortschritte gemacht hatte, die Resection ausgeführt; aber die Recidive traten sehr bald ein und die neue Geschwulst erreichte einen grösseren Umfang, als früher.

Wegen Epulis resecirte ich in 3 Fällen den Unterkiefer; in keinem der 3 Fälle trat Recidiv ein — eine Thatsache, welche Virchow's Ansicht bestätigt. Freilich wollen Andere dennoch auch hier über Recidive Erfahrungen besitzen. Der ganze Verlauf der Epulis ist langsamer, als der anderer Sarkome; hier ist auch kein kachektischer Zustand selbst in den vorgeschrittenen Stadien.

Wegen Hodensarkoms führte ich einige Male die Castration aus. Die Recidive traten indessen schon nach einigen Monaten ein, obgleich ich die gesunden Theile mit fortnahm.

Lympho-Sarkome des Halses kommen in Japan nicht selten

vor. Ich behandelte dieselben nach der Vorschrift Billroth's mit Tinctura arsenicalis Fowleri. In einigen Fällen hatte diese Behandlung einen sehr guten Erfolg; in anderen dagegen erwies sie sich minder wirkungsvoll.

Einmal führte ich bei einer älteren Frau wegen Sarkoms die Exarticulatio humeri aus; das Recidiv trat indessen schon nach 3 Monaten ein.

Was die Indication der Exstirpation betrifft, so sollte, wie bereits oben bemerkt, nach meiner Ansicht diese Operation nur dann vorgenommen werden, wenn es möglich ist, durch dieselbe alles Krankhafte in der Umgebung des Tumors vollständig zu entfernen.

Wenden wir uns nun zu einigen Fällen von Carcinom.

1) Bei einem 40jähr. Manne, den ich wegen eines harten Brustcarcinoms (das bei Männern sehr selten auftritt) operirte, trat das Recidiv nach 3 Jahren ein. Da die Achsel- und Halsdrüsen bereits infiltrirt waren, so unterliess ich es, eine neue Operation vorzunehmen.

Brustcarcinom der Frauen kommt in Japan sehr häufig vor und schon seit den ältesten Zeiten wird diese Krankheit operativ behandelt. Bei allen von mir operirten Frauenspersonen (ungefähr 40 in 7 Jahren) traten Recidive ein; aber bei jüngeren Individuen stellte sich, wenn der Tumor ein hartes Carcinom war, das Recidiv trotz gründlicher Ausräumung der Achseldrüsen, viel früher ein als bei älteren Personen. Bei den über 50 Jahre alten Frauen, bei welchen die Menstruation schon aufgehört hatte, trat das Recidiv zwar auch ein, aber das Wachsthum des Tumors ging sehr langsam vor sich, wahrscheinlich, weil bei diesen Individuen der Rückbildungsprocess schon begonnen hatte. Uebrigens wäre bei einer Statistik der Recidivfälle bei den Frauenspersonen in Bezug auf das Aufhören der Menstruation eine Sonderung einzuhalten nöthig.

2) Uterus-Carcinome sind in Japan sehr häufig; dieselben gelangen indessen gewöhnlich so spät zur ärztlichen Behandlung, dass sich Nichts mehr dagegen thun lässt. Mir sind mehrere Fälle vorgekommen, in welchen beim Beginne der Erkrankung die Diagnose sehr schwierig war. Besonders bei jüngeren Individuen war es oft kaum möglich, sich mit Bestimmtheit darüber auszusprechen, ob

man es mit einer bösartigen Geschwulst, oder mit einem ulcerativen Process zu thun habe; ich erinnere hier an das bereits oben Bemerkte. Wegen der Blutung habe ich bis jetzt nur Auskratzung mit nachfolgender Aetzung angewendet; das Resultat, wenn auch nur ein palliatives, war doch wegen der längeren Erhaltung des Lebens meistens glücklich.

3) Der Zungenkrebs kann in Japan sehr häufig beobachtet werden. In 7 Jahren sind mir 6 Fälle vorgekommen. Ich nahm nach der Vorschrift Billroth's vorher immer die Ligatur der Lingualis am Halse vor. Von den 6 Operirten lebt nur noch ein 65jähr. Mann, bei welchem ich die Operation vor 3 Jahren ausgeführt habe. Es besteht bei demselben indessen schon seit einiger Zeit eine leichte Verhärtung der Halsdrüsen.

4) Lippencarcinome sind in Japan sehr selten. Mir ist noch kein Fall vorgekommen. Ich weiss, dass für die Erklärung des häufigen Erscheinens in Europa die „Pfeifentheorie" aufgestellt wird; allerdings ist man in Japan im Volke gewohnt, die Pfeife anders, als in Europa, zu halten.

5) Ich hatte auch Gelegenheit, eine Operation wegen eines primären submaxillaren Drüsencarcinoms auszuführen, wie bereits oben bemerkt worden.

Da die Infiltration schon ziemlich ausgedehnt war, so habe ich wegen der zu befürchtenden starken Blutung die Ligatur der Carotis externa ausgeführt, und zwar unterhalb der Abzweigung der Art. thyreoidea superior, obgleich viele Chirurgen hiergegen sehr eingenommen sind; jedoch habe ich die Nachtheile wohl erwogen und einzig darnach meine Wahl getroffen. Ich wollte die Ligatur der Carotis communis nicht vornehmen, weil der Kranke eine leichte halbseitige Lähmung hatte. Die Operation verlief ohne Zwischenfälle. Die Wundheilung nahm einen günstigen Verlauf. Am 7. Tage nach der Operation wurde der Kranke, gerade als er aus dem Bette steigen wollte, von dem Tode überrascht. Da ich die Section nicht vornehmen konnte, bin ich bezüglich der Todesursache auf Vermuthungen angewiesen. Der Tod dürfte entweder durch einen erneuten Schlaganfall, oder durch Embolie herbeigeführt worden sein, wie andere Chirurgen auch nach der Unterbindung der Carotis externa unterhalb der Art. thyreoidea superior beobachtet haben.

6) Das Oesophagus-Carcinom kommt in Japan sehr häufig vor, besonders bei Sake-Trinkern (Sake ist ein aus Reis bereitetes Weinartiges, berauschendes Getränk). Es scheint dieses Carcinom erblich zu sein. In Japan ist der Volksausdruck für diese seit Urzeiten bekannte Krankheit „Kak", d. h. nicht mehr fähig, Reis

zu essen. — Für die Behandlung ist vor Allem zunächst sorgfältigste Untersuchung nothwendig, ob auch wirklich Carcinom vorhanden ist, damit namentlich nicht ein Aneurysma der Aorta täusche, in welchem Falle bei Sondirung sofort der Tod eintreten könnte; also eine Untersuchung des Circulationsapparates anzustellen nothwendig. Die Behandlung ist nun entweder die der Sonden-Einlegung, oder Nicht-Einlegung einer Sonde. Wenn bereits Dyskrasie eingetreten, ist die Einlegung der Sonde nicht mehr anzurathen, weil alsdann die Geschwulst sicherlich doch schon etwas entzündet und auch bereits ziemlich erweicht ist, indem durch Festsetzung von Speisen leicht Spaltpilze sich erzeugen, welche alsdann in das Gewebe des Carcinoms gelangen; denn in solchem Falle wird das fortwährende Sondiren den Tod nur beschleunigen. Der Verlauf der Krankheit, von dem ersten Bemerken derselben an gerechnet, ist in Japan ca. 8—10 Monate.

7) Erkrankungen an Rectum-Carcinom sind auch nicht selten. In allen Fällen, die mir vorgekommen sind, hatte die Infiltration schon eine solche Ausdehnung erreicht, dass ich die Exstirpation nicht mehr vornehmen konnte.

8) Pylorus- und Coecum-Carcinome kommen gleichfalls dem Arzte nicht selten vor.

9) Dagegen hatte ich nur einmal Gelegenheit, ein Netz-Carcinom zu beobachten. Dass es sich wirklich um Netz-Carcinom handelte, hat die mikroskopische Untersuchung unzweifelhaft dargethan.

10) Cancroide am Handrücken sind mir nur einmal vorgekommen. Ich amputirte den Vorderarm. Man hat früher bereits erkannt, dass diese Geschwulst nicht bösartig sei, aber der Verlauf sich lange hinzöge. Für meinen Fall habe ich von dem etwaigen Recidiv noch keine Nachricht.

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5. Ueber Aneurysmen.

Pulsation und Geräusch werden allgemein als sehr wichtige Anhaltspunkte für die Diagnose der Aneurysmen betrachtet. In manchen Fällen ist indessen, wie auch von Anderen beobachtet worden ist, das Geräusch nicht wahrnehmbar. Erichsen bemerkt, dass dasselbe bei sackförmigen Aneurysmen am deutlichsten zu

vernehmen sei, wenn der Sack nicht gar zu sehr vom Blute ausgedehnt oder theilweise entleert ist. Diese Bemerkung scheint mir vollkommen richtig zu sein. Ich habe 2 Fälle von Aneurysma an der Aorta beobachtet, bei welchen das Geräusch nicht wahrzunehmen, obgleich Dämpfung und Pulsation an der rechten Seite des Brustbeines ganz deutlich war.

Einer der Kranken war ein 35jähriger Kaufmann, ein Trinker, welcher angab, dass er in seinem 20. Lebensjahre an einem Schanker gelitten, 3 Monate später am Hals und Rücken Roseola bemerkt und einige Zeit nachher Gelenkschmerzen verspürt habe. Der Kranke schien also an Syphilis gelitten zu haben. Im August 1883 trat Lähmung seiner unteren Extremitäten ein, welche so rapide Fortschritte machte, besonders auf der rechten Seite, dass er im September nicht mehr gehen konnte. Am 30. September 1883 wurde der Kranke in das Hospital Shitaya aufgenommen.

Status praesens: Der Kranke ist so schwach, dass er im Bette nicht sitzen kann. Mittelst des Katheters wurde eine grosse Quantität Urin abgelassen. Abmagerung der unteren Extremitäten, bedeutende Motilitätsstörungen. Am Perineum befindet sich eine eiternde Fistel und rechts vom Brustbein eine Anschwellung mit deutlicher Pulsation. Bei der Percussion wird an dieser Stelle Dämpfung gefunden. Der Kranke kann die Entstehungszeit der Geschwulst nicht angeben. Die Herzdämpfung ist normal. Bei der Auscultation werden die Herztöne normal gefunden, an der angeschwollenen Stelle ist kein Geräusch vernehmbar. Ich vermuthete eine Rückenmarkskrankheit mit Erweiterung der Aorta, obgleich keine Druckerscheinungen der Aorta, als Heiserkeit der Stimme, Schlingbeschwerden, Dyspnoe vorhanden waren. Nach vierzehn Tagen starb der Kranke an Entkräftung.

Die Section ergab Hyperämie des Rückenmarkes in der Lumbar-Gegend, Caries der Lendenwirbel und des Os sacrum, welche letztere wahrscheinlich syphilitischer Natur war. An den cariösen Stellen befanden sich Eitergänge. Nach der Eröffnung der Brusthöhle fand ich eine Gänseeigrosse Erweiterung des aufsteigenden Astes der Aorta. Ich spaltete die erweiterte Stelle und fand die drei Aortahäute verdickt und in der Mitte erweitert. Auf der Innenfläche der Intima waren durch den atheromatösen Process Unebenheiten entstanden. An manchen Stellen fand sich ein kalkiger Belag. Es waren keine Coagula und kein Fibrin vorhanden.

Wie oben schon erwähnt, habe ich kein Geräusch gehört, obwohl die Section das Vorhandensein von Unebenheiten nachgewiesen hat. Ob die Form des Aneurysma einen Einfluss auf die Entstehung des Geräusches hat, weiss ich nicht. In dem vorliegenden Falle war das Aneurysma spindelförmig.

Ein 39jähriger Kaufmann hatte nach seiner Angabe früher an Syphilis gelitten. Im März 1880 fühlte er in der rechten Brust Schmerzen, was ihn veranlasste, ärztliche Hülfe in Anspruch zu nehmen. Im Jahre 1881 fühlte

er an der rechten unteren Extremität Schmerzen, die sich in Folge ärztlicher Behandlung linderten. Im Juli 1882 bemerkte er, dass sein Gesicht anschwoll; er hatte von Zeit zu Zeit Husten, Schling- und Athembeschwerden, welche letzteren anfallsweise, wie beim Asthma, auftraten.

Status praesens: Anschwellung des Gesichtes und der oberen Extremitäten. Pulsschlag der beiden Radial-Arterien ungleichzeitig. Heiserkeit, schwere Respiration, an der rechten Seite des Brustbeines eine geringe Anschwellung mit nicht deutlich wahrnehmbarer Pulsation. An der angeschwollenen Stelle constatirte ich bei der Percussion Dämpfung. Bei der Auscultation vernahm ich kein Geräusch. Der Kranke starb nach 8 Tagen an Lungenoedem.

Ich durfte die Section nicht vornehmen. Es ist indessen sehr wahrscheinlich, dass der Kranke an einer Erweiterung der Aorta litt. Die Druckerscheinungen der Aorta waren sehr auffallend. Ueber die Form der Erweiterung kann ich nur Vermuthungen aussprechen; es ist wahrscheinlich, dass es sich in diesem Falle um ein spindelförmiges Aneurysma handelte, welches schon lange bestand, bis endlich unter Druckerscheinungen auf den Nervus vagus der Tod eintrat.

Erichsen hat die Beobachtung gemacht, dass, wenn ein umschriebenes sackförmiges Aneurysma durch Zerreissung diffus wird, der Kranke plötzlich einen heftigen Schmerz fühlt und man bei der Untersuchung die Geschwulst bedeutend vergrössert und ohne die früher begrenzte Form findet, auch Pulsation und Geräusch nicht mehr so deutlich wahrnehmbar seien. Ich hatte Gelegenheit, die nämlichen Erscheinungen zu beobachten. In einem Falle von Aneurysma der Arteria iliaca externa war Anfangs Pulsation und Geräusch deutlich wahrnehmbar. Eines Tages fühlte der Kranke einen heftigen Schmerz. Tages darauf war die Pulsation nicht mehr wahrzunehmen. Ich machte den Versuch, die Iliaca communis zu unterbinden; ich musste aber davon abstehen, weil ich nach der Incision fand, dass sie von dem Aneurysmasack bedeckt war. Es sind mir mehrere Fälle vorgekommen, in denen die Anfangs deutlich wahrnehmbare Pulsation später verschwand.

Die nicht pulsirenden Aneurysmen können sehr leicht mit anderen Geschwülsten, als Sarkomen, Abscessen, verwechselt werden. Namentlich Aneurysmen der Arteria poplitea und der Schenkelarterie zeigen, wenn Pulsation und Geräusch verschwunden ist, eine so grosse Aehnlichkeit mit Sarkomen, dass es mir in manchen Fällen Anfangs unmöglich war, eine bestimmte Diagnose zu stellen. Das nicht pulsirende Aneurysma bietet ganz die nämlichen Erscheinungen, wie das Sarkom, z. B. Oedem des unter dem Krank-

heitssitze befindlichen Theiles der Extremität, Erweiterung der Hautvenen, und als charakteristisches Symptom erschwerte Streckung der in halber Beugung gehaltenen unteren Extremität. In zweifelhaften Fällen musste ich sehr oft die Probepunction machen, welche indessen nicht immer sichere Aufschlüsse über die Natur der Krankheit giebt, weil zuweilen auch in den Sarkomen sternförmige, alte, rothe Blutkörperchen vorkommen. Auch der Verlauf der Krankheit liefert nicht immer einen sicheren Anhaltspunkt für die Diagnose, weil auch die Aneurysmen sich rasch entwickeln können. Die Angaben der Kranken über die Entstehungsursache der Erkrankung müssen erfahrungsgemäss mit grosser Vorsicht aufgenommen werden. Es blieb mir nichts übrig, als den Kranken längere Zeit hindurch zu beobachten und die Probepunction zu wiederholen. In vielen Fällen musste ich die Probepunction mehrmals wiederholen, ehe ich mich mit Bestimmtheit über die Natur der Krankheit aussprechen konnte. Ja, ich musste in einzelnen Fällen sogar die Probeincision ausführen, nach welcher ich sogleich, wenn sie zu einem bestimmten Resultate führte, zu der durch die Natur der Krankheit angezeigten Operation schritt.

In einem Falle, in welchem das Aneurysma seinen Sitz an der Theilungsstelle der rechten Carotis communis und der Arteria subclavia hatte, war gleichfalls nur eine Wahrscheinlichkeitsdiagnose möglich, die übrigens durch den Sectionsbefund bestätigt wurde.

Ausser mit Sarkomen können die Aneurysmen sehr leicht mit Abscessen verwechselt werden.

Ich hielt einen Abscess an der inneren Seite des Oberschenkels für ein Aneurysma, weil bei der wiederholt vorgenommenen Punction kein Eiter erschienen war und die Symptome die nämlichen waren wie diejenigen des nicht pulsirenden Aneurysma, nämlich Oedem des ganzen Beines, Venenerweiterung, gebeugter Zustand des Kniegelenkes. Ueberdies war nach der Angabe des Kranken die Anschwellung am Oberschenkel plötzlich unter heftigem Schmerz eingetreten, wie bei einem umschriebenen sackförmigen Aneurysma, das durch Berstung diffus wird. Ich beobachtete die Krankheit einige Zeit hindurch und nahm mehrmals die Punction vor, bis endlich das Erscheinen von Eiter meine Zweifel löste. Ich machte eine Incision und fand an einer Stelle des M. quadriceps ein Loch, in welches man mit Leichtigkeit den kleinen Finger einführen konnte. Zwischen den Muskelschichten lagerte Eiter. Jetzt konnte ich mir die oben geschilderten Erscheinungen erklären. Der Durchbruch des Eiters durch den Muskel und die Ausbreitung desselben in den Muskelschichten hatten, wie man wohl annehmen darf, den plötzlichen

heftigen Schmerz und die Anschwellung hervorgerufen. Bei der mehrmals wiederholten Punction war ich nie auf den Eiterherd gestossen.

Es ist mir umgekehrt auch begegnet, dass ich Anfangs ein Aneurysma für einen Abscess hielt. Die Symptome waren die nämlichen, wie im vorhergehenden Falle. Durch längere Beobachtung und wiederholte Vornahme der Punction verschaffte ich mir Gewissheit über die Natur der Krankheit.

Die Ansichten über die Rolle, welche die Syphilis in der Aetiologie des Aneurysma spielt, stehen einander noch schroff gegenüber. Für Diejenigen, welche die Syphilis als eine der Entstehungsursachen des Aneurysma ansehen, dürfte die Thatsache von Interesse sein, dass bei mehr als der Hälfte der 40 Personen, welche ich während meiner 7jährigen Privat- und Hospital-Praxis wegen Aneurysma behandelt habe, Syphilis nachweisbar war. Bardeleben behauptet, dass die Alkoholdyskrasie das Entstehen von Aneurysmen begünstige. Diese Ansicht ist nach meiner Erfahrung vollkommen richtig. Fast sämmtliche Individuen, welche ich wegen Aneurysma behandelt habe, besonders die jüngeren, waren Trinker.

In 10 Fällen von Aneurysma habe ich die Central-Ligatur ausgeführt. In allen diesen Fällen war das Aneurysma noch nicht sehr gross und die Möglichkeit der Resorption noch vorhanden. Es dauerte 2—3 Monate, bis die vollständige Resorption eintrat. Es schien mir, als ob man unmittelbar nach der Vornahme der Ligatur eine kleine Abnahme der Geschwulst und der Schmerzen bemerken konnte. Dieser Zustand hielt in den meisten Fällen ungefähr bis zum Ende des 2. Monates an. Von dieser Zeit an nahmen alle Krankheitserscheinungen sehr rasch ab. In keinem der 10 Fälle ist Gangrän eingetreten, noch hat sich die Pulsation wieder eingestellt. In einem dieser 10 Fälle nahm ich die Central-Ligatur der Arteria iliaca vor, in den übrigen die der Schenkelarterie. Sämmtliche 10 Fälle verliefen sehr günstig.

Einmal versuchte ich wegen eines Aneurysma an dem unteren Theile der rechten Carotis communis die peripherische Ligatur der Arteria communis vorzunehmen. Dieser Versuch misslang.

In einem Falle habe ich bei Aneurysma der Arteria poplitea die Compression mit Esmarch'schen Binden nach Billroth vorgenommen, und zwar mit gutem Erfolge.

Ueber Aneurysmen hat mein Assistent Konisi eine sehr sorgfältig gearbeitete Abhandlung in Japan erscheinen lassen.

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6. Ueber Resectionen.

Nebst einem Anhange über accidentelle Wundkrankheiten und über Luxationen des Hüftgelenkes.

Resection des Oberarmkopfes und des Schulterblattes wegen Caries und Nekrose.

(Hierzu Fig. 7a, b.)

Ein 34jähriger Mann wurde am 29. April 1879 in des Hospital aufgenommen. Der Kranke gab an, dass vor 3 Jahren, unter Röthung der Haut, eine Anfangs nicht bedeutende, aber schmerzhafte Anschwellung seines Schultergelenkes entstanden wäre. Die Geschwulst sei von selbst aufgebrochen und, nachdem eine grosse Menge Eiter sich aus derselben entleert, hätten sich an der kranken Schulter Fistelgänge gebildet. — Der Kranke sah anämisch aus, war auffallend abgemagert; an der kranken Schulter befanden sich mehrere Fistelgänge, aus welchen eine sehr übelriechende Flüssigkeit hervordrang. Die Beweglichkeit des Schultergelenkes war sehr vermindert. Der Kranke konnte keine activen Bewegungen ausführen, passive Bewegungen verursachten ihm grosse Schmerzen. Er litt an einem chronischen Bronchialkatarrh; eine Infiltration des Lungengewebes war aber nicht nachweisbar. Ich fand nach Einführung der Sonde durch die Fistelgänge die Knochenoberfläche rauh, und stellte deshalb die Diagnose auf Caries.

Ich ging nun sogleich an die Behandlung. Da der herabgekommene Zustand des Kranken und der bestehende Bronchialkatarrh bei längerer Fortdauer der Eiterung den Ausbruch der Phthisis befürchten liess, erschien mir die conservative Behandlung contraindicirt. Ich entschloss mich deshalb zur Resection des kranken Theiles des Oberarmknochens und nahm am 11. Mai diese Operation nach der Methode Robert vor. Der Oberarmknochen wurde in einer Länge von 9 Ctm. resecirt, aber wegen der Erkrankung der Beinhaut nicht nach der subperiostealen Methode. Nach der Operation befand sich der Kranke relativ wohl, hatte aber Neigung zum Erbrechen; die Wundheilung verlief normal; die Empfindlichkeit des kranken Theiles verschwand, das Allgemeinbefinden war befriedigend. Indessen nach kurzer Zeit bildeten sich von Neuem Fistelgänge. Ich nahm eine abermalige Untersuchung des Kranken vor und fand bei der Sondirung die Oberfläche der Scapula rauh. Ich entschloss mich nun, die kranke Knochenpartie auf operativem Wege zu entfernen. Die Operation, welche ich am 3. October vornahm, geschah in folgender Weise: Ich machte einen Schnitt längs des inneren Randes der Scapula und einen kleineren Querschnitt vom oberen Ende des Längsschnittes aus (nach dem Vorgange Linhart's). Ich fand jetzt, dass die

Oberfläche der Scapula zum grossen Theile nekrotisch war. Nachdem ich mit der Kettensäge den Processus coracoideus und die Spina durchsägt hatte, löste ich die Beinhaut von der Scapula ab. Ein grosser Theil der Beinhaut war von dem Krankheitsprocesse ergriffen und musste deshalb entfernt werden. Der nekrotische Theil des Schulterblattes liess sich nach der Beseitigung der kranken Beinhaut mit Leichtigkeit wegnehmen. Die Blutung während der Operation war unbedeutend, weil in Folge des entzündlichen Processes die Obliteration der Gefässe eingetreten war. Ich legte, ausser mehreren kleineren Vereinigungsnähten, 4 Hauptnähte und eine Drainage an. Die Wunde wurde nach der Lister'schen Methode behandelt. Am 8. Tage nach der Operation entfernte ich alle Nähte. Die Wundränder vereinigten sich zum Theil per primam intentionem. Am 7. November trat allgemeines Oedem auf. Im Urin war eine geringe Quantität Eiweiss nachweisbar. Specifisches Gewicht des Urins 1020. Reaction sauer. Das Oedem hielt bis zum 19. November an. Nach dem Verschwinden desselben trat, obgleich die Fistelgänge noch offen waren, in dem Allgemeinbefinden des Kranken eine bedeutende Besserung ein. Ich hatte schon früher angefangen, mit dem Arme passive Bewegungen vorzunehmen und active ausführen zu lassen. Der Kranke wurde Ende December aus dem Hospitale entlassen, obschon die Fistelgänge noch nicht vollständig geschlossen waren. Auf meinen Rath begab er sich in einen Curort. Ein Jahr war seit der Operation verflossen, als er sich mir wieder vorstellte. Die Fisteln waren jetzt vollständig geschlossen. Er hatte bedeutend an Gewicht zugenommen. Trotz der Wegnahme von 9 Ctm. Oberarmknochen und der Beseitigung des Schulterblattes konnte er seinen Arm ziemlich gut gebrauchen.

Die nachfolgenden allgemeinen Bemerkungen über Resectionen überhaupt gründen sich auf meine vielfachen, in Japan gemachten Erfahrungen, und glaube ich daher, dieselben wohl der Prüfung vorlegen zu dürfen.

Nach meinen Erfahrungen kann die subperiostale Resection keinesweges immer mit Erfolg angewendet werden, obwohl manche Chirurgen grosses Gewicht auf diese Methode gelegt haben; in manchen Fällen sogar, z. B. bei nicht traumatischer Caries, kann sie schwere Nachtheile für den Kranken mit sich bringen; denn wenn man die erkrankten Partieen zurücklässt, werden sie bald wieder krank, also muss man durchaus alle verdächtig scheinenden Partieen sorgfältig wegnehmen, sonst würde man immer wieder genöthigt sein, die Resection zu wiederholen; wirklich sind solche Fälle mir recht oft vorgekommen. Bei frischen Schusswunden dagegen ist, wie ich sehr oft zu beobachten Gelegenheit gehabt habe, die Anwendung der besagten Methode in den meisten Fällen von sehr günstigem Erfolge begleitet gewesen, weil die zurückbleibenden,

d. h. nicht erkrankt gewesenen Partieen noch in ganz gesundem Zustande sind, und die gesunde Beinhaut vollkommen fähig ist, neuen Knochen zu produciren. Für die subperiostalen, bereits cariös gewordenen Schusswunden gilt das Nämliche, was ich über die Anwendung dieser Methode im Allgemeinen bemerkt habe. — Die Erfahrungen, welche ich bei der Anwendung der subperiostalen Resection machte, bestätigen übrigens in allen Punkten Das, was die Proff. Hueter und Linhart in ihren Lehrbüchern über diese Methode gesagt haben.

1) Das Evidement des os ist nach meiner Erfahrung bei Caries kleiner Knochen, z. B. der Fusswurzelknochen, von ausgezeichnetem Erfolge begleitet; besonders wenn nach der Auskratzung mittelst des Paquelin'schen Apparates kauterisirt und die kauterisirte Fläche nach Mosetig mit Jodoform bestreut wird. Dagegen bei Röhrenknochen, z. B. Oberschenkelknochen, habe ich mit diesem Verfahren keine so günstigen Resultate erzielt. In einigen durch chronische Osteomyelitis des Oberschenkels verursachten Erkrankungen an Caries mit Fistelbildung habe ich diese Methode ohne besonderen Erfolg angewendet. Bei mehreren Kranken trat der Tod in Folge acuter Nephritis ein. Ich vermuthe, dass durch den operativen Eingriff die chronische Osteomyelitis wieder acut wurde und, in Folge der mangelhaften Entleerung der Entzündungsproducte, Nephritis eintrat. Die Drainage der Knochenhöhle war mir damals noch nicht bekannt; seit einiger Zeit wende ich dieselbe beim Evidement von Röhrenknochen mit dem besten Erfolge an.

2) Die Sequestrotomie ergab immer sehr günstige Resultate, besonders wenn die Kauterisation angewendet und die Wundfläche mit Jodoform bestreut wurde.

Zu einigen Bemerkungen über die Resectionen der grossen Gelenkenden drängen mich hier meine besonderen Erfahrungen, und zwar zunächst in Betreff der Hand- und Fussgelenke.

Wegen nicht traumatischer Caries habe ich hier die Resection ausgeführt; jedoch der Erfolg entsprach selbst bei totalen Resectionen nicht meinen Erwartungen. Ich war oft genöthigt, später wegen des Umsichgreifens des krankhaften Processes die Amputation dennoch vorzunehmen; aber trotzdem möchte ich empfehlen, bei Caries dieser Gelenke dennoch, und zwar möglichst bald, die Resection zu machen.

Kniegelenks-Resectionen habe ich nur Gelegenheit gehabt, bei frischen Schusswunden vorzunehmen; sie hatten meistens, trotz ziemlich strenger antiseptischer Behandlung, einen ungünstigen Verlauf, woran jedoch wahrscheinlich dem Transporte, der nicht immer für solche Kranke unter den günstigsten Umständen geschehen kann, die Schuld beizumessen ist. Bei nicht traumatischer Caries dagegen verliefen die Resectionen günstig.

Anders war es mit Hüftgelenks-Resectionen, die ich zum ersten Male in Japan wegen bereits cariös gewordener Schusswunden ausführte; sie hatten einen ziemlich guten Erfolg; wegen frischer Schusswunden derartige Operationen vorzunehmen, hatte ich keine Gelegenheit.

Wegen Coxitis bei Kindern hatte ich öfter Gelegenheit, die Resection auszuführen; sämmtliche Patienten befanden sich bereits in einem vorgerückteren Stadium der Krankheit. Die Operation hatte in den meisten Fällen einen günstigen Erfolg. Es trat keine Tuberkulose ein. Dass die Ansicht C. Hueter's, bei Coxitis die Operation möglichst frühzeitig vornehmen zu müssen, richtig sei, wage ich bis jetzt nach meinen Erfahrungen nicht zu bestätigen. Ich habe bei vielen Kindern durch die conservative Behandlung (Beförderung des Eiterabflusses, Extension, Hebung des Ernährungszustandes, Landaufenthalt u. s. w.) ziemlich günstige Resultate erzielt.

Was Ellenbogen-Resectionen anbetrifft, so darf ich hierbei, auf Grund meiner vielfachen Erfahrungen, bestätigen, dass bei traumatischer Caries die Wunden leicht und gut heilen; ebenso haben die Resectionen des Oberarmkopfes sowohl bei frischen, als bei alten Wunden, bei traumatischer und nicht traumatischer Caries stets glücklichen Erfolg.

3) In Betreff der frischen Schusswunden will mir, nach meinen Erfahrungen, durchaus als das Wichtigste erscheinen, stets die Wunde gehörig zu erweitern, um alle fremden Körper sorgfältig herausschaffen zu können, wobei ich jedoch bemerke, dass alle noch mit der Beinhaut zusammenhängenden Knochensplitter mit dieser in ihrer organischen Verbindung gelassen werden müssen, und also nicht erst gewaltsam davon getrennt werden dürfen. Bei der Wunderweiterung muss natürlich für Blutstillung stets Sorge getragen werden.

Was endlich den functionellen Process nach der Resection anlangt, so möchte ich bemerken, dass selbst bei der Resection von Knochenstücken, deren Länge 7 Ctm. überschreitet, und zwar auch bei Nichtanwendung der subperiostalen Methode, Falls die Knochenhaut noch irgend gesund geblieben ist, günstige Resultate erzielt werden können, wenn man der Ernährung des Patienten die grösste Aufmerksamkeit zuwendet und zur Stärkung der Muskeln frühzeitig mit der Vornahme activer und passiver Bewegungen beginnt.

Noch zwei wichtige Fragen möchte ich hier berühren, über welche die Meinungen sehr controvers auseinander gehen, nämlich die Frage wegen des Schnittes der Weichtheile bei Resectionen, und die Frage wegen der Indication der Resection überhaupt.

Was die erstere Frage anlangt, so glaube ich hier einen Unterschied des Schnittes bei frischen Wunden und bei nicht traumatischer Caries hervorheben zu müssen. Bei frischen Wunden kann ich stets nur den Längsschnitt anempfehlen, und zwar ist auch möglichst nur subperiostal zu operiren; bei nicht traumatischer Caries dagegen ist, je nach den speciellen Umständen, in Bezug auf die völlige Wegschaffung aller erkrankten Partieen entweder der Längsschnitt, oder der Querschnitt anzuwenden; in diesem Punkte muss ich überhaupt Hueter völlig beistimmen.

Was die Indicationsfrage anbetrifft, so ist freilich die Resection alle Mal wünschenswerth, jedoch spricht bei dieser Frage leider zu sehr der Transport als ein wichtiger und entscheidender Factor mit, indem Störungen durch ihn nach Resection überaus gefahrvoll werden, und zuweilen der Kranke stirbt; in diesem Falle ist Amputation leider geradezu geboten. Ich muss bei dieser Gelegenheit entschieden betonen, dass die Anlegung künstlicher Glieder nicht bloss als Ersatz der verlorenen natürlichen Gliedmassen nöthig, sondern nach meiner Erfahrung für die Ernährung der amputirten Extremität geradezu unentbehrlich ist. Wenn nach Amputationen keine künstlichen Glieder getragen werden, kann sehr oft ein Rückgang in der Ernährung der entsprechenden Körperhälfte beobachtet werden. Besonders bei jungen Leuten sollte man die Anlegung künstlicher Extremitäten niemals unterlassen.

Zum Schlusse meiner Mittheilungen sei mir noch eine historische Notiz in Betreff der Vornahme der Resection in Japan gestattet.

Bis vor circa 10 Jahren haben nur englische Aerzte in Japan amputirt. In der Zeit von 1874—75 nahm Dr. Elmenenz eine Oberkiefer-Resection vor, und alsdann haben im Jahre 1876, während des Aufstandes, Dr. Sato und Ssiguro Resectionen ausgeführt*). In militärischer Praxis habe ich in Japan zuerst, und zwar nach der älteren Lister'schen Methode, Hüftgelenks-Resectionen vorgenommen.

Anhang.

I. Von den accidentellen Wundkrankheiten.

A. Traumatischer Tetanus.

Bekanntlich will Billroth ihn als Infectionskrankheit erkennen. Ich habe Erfahrungen gemacht, welche mir diese Hypothese für die Aetiologie überaus wichtig erscheinen lassen; darum möchte ich mit meinen in Japan gemachten Beobachtungen nicht zurückhalten.

Was zunächst das Vorkommen anbetrifft, so kann dasselbe durchaus nicht schlechtweg „endemisch" genannt werden; denn es ist das Vorkommen zwar wohl local beschränkt, aber keineswegs vereinzelt, sondern erscheint doch immer zugleich auch mehr oder weniger verbreitet. Und dieser epidemische Charakterzug des Auftretens dürfte nun doch wohl durchaus auf die Infectionsnatur der Ursache hinweisen können. Ferner ist mir in Japan aufgefallen, dass zur Zeit des Auftretens des Tetanus in einer Gegend hier zugleich stets auch Eclampsie in der Schwangerschaft vielfach sich zeigt. Ich möchte diese Erscheinung nur angeführt haben. Aus den von mir behandelten Tetanusfällen möchte ich einen Fall besonders hervorheben, welcher bei Erfrierung der Haut an den Zehen des rechten Fusses auftrat. Zunächst kann ich constatiren, dass wieder zu gleicher Zeit in Japan mit Erfrierungen auch Tetanus mehrfach beobachtet wurde.

Im Jahre 1883, am 4. März, wurde ein an Erfrieren der Zehen erkrankter Arbeitsmann vom Tetanus befallen; ich behandelte ihn mittelst

*) Die Generalärzte Dr. Sato und Dr. Ssiguro haben mir mitgetheilt, dass sie während des Aufstandes im Jahre 1876 im Hospitale zu Osaka wegen frischer und cariöser Schusswunden jeder Art Resectionen vorgenommen, und zwar in den meisten Fällen mit günstigem Erfolge. Dr. Ssiguro stellte im Jahre 1876 die Fälle der Operationen, die zu jener Zeit im Hospitale zu Osaka gemacht worden, statistisch zusammen.

Nervendehnung des rechten N. ischiadicus gemäss der bekannten Vorschrift des Herrn Langenbuch; am 9. d. M. trat der Tod ein. Während des Krampfzustandes war die Temperatur zwischen 36.4 bis 38.5, also überhaupt nicht eine hohe. Die Todtenstarre war 24 Stunden nach Eintritt des Todes noch sehr bedeutend. Kaumuskeln und Extremitäten waren sehr steif. — Die Section wies geschlängelte Dehnung und hyperämischen Zustand der Venen der Pia mater im Seitengehirn der Ventrikel auf; ebenso waren am Rückenmark in der Kreuzgegend die Venen der Dura mater geschlängelt, gedehnt und hyperämisch. — Das Gewicht des Gehirnes betrug 1400 Grm. (Das japanische Gehirn wiegt im Mittel 1300 Grm.) — Durch die vorgenommene Nervendehnung war Blutextravasat nach Oben bis zur Wurzel des Nervus ischiadicus und nach Unten bis zur Kniekehle hin verursacht worden. — Bacterien habe ich nirgends, weder im Blut noch in den Frostellen gefunden.

B. Wundrose.

Trotz strenger antiseptischer Behandlung der Wunden trat die Wundrose in Japan sehr häufig auf. Ich möchte hier davon nur so viel anmerken, dass sie durch extensive Ausdehnung über den Körper niemals gefährlich war; wohl aber durch die local beschränkte Intensität der Infiltration der Haut, namentlich am Halse und am Kopfe, gefährlich wurde.

Ferner trat die Wundrose sehr häufig bei Carbunkel auf, wobei eigenthümlich war, dass nach Verschwinden der Rose die Granulation auffällig gut von Statten ging. In Betreff der subcutanen Injection bei der Wundrose mit Carbol (nach Hueter) muss ich bemerken, dass diese stets ohne Erfolg gewesen.

C. Wunddiphtheritis und Croup der Granulationen.

In Bezug auf diese möchte ich nur die von mir gemachte Beobachtung mittheilen, dass beide stets zu gleicher Zeit erscheinen.

II. Bemerkungen über Luxationen des Hüftgelenkes.

Zunächst dürfte die Bemerkung nicht uninteressant sein, dass in Japan Luxationen und auch Fracturen nur sehr wenig, und die meisten Fälle nur unter dem Militär vorkommen — eine Erscheinung, die sich sehr einfach daraus erklärt, dass unsere Arbeiter nur erst noch zum allergeringsten Theile in Fabriken in schwerer Arbeit thätig sind; ein anderer Grund lässt sich füglich für diese Thatsache nicht aufstellen.

Speciell von Luxationen des Hüftgelenkes habe ich nun in meiner sonst ja der Natur der Verhältnisse nach reichen Hospital-Praxis im Ganzen erst 6 Fälle behandelt, und zwar waren sie sämmtlich Fälle von Luxatio ischiadica und iliaca, und darunter sowohl frische, als alte, und zwar wohl bis 5 Monate alte Luxationen. — Für die Reposition benutzte ich niemals Maschinen, theils weil sie gefährlich, theils weil man mit diesen niemals die für den individuellen Fall gerade angemessenste Behandlung gewinnen kann, sondern ich wende, auch bei alten Luxationen, stets nur das allein accommodationsfähige manuelle Verfahren an. Ich lasse den Patienten entkleidet und chloroformirt auf ein fest ruhendes Brett legen, bringe alsdann das Becken durch Binden in eine feste Lage und lasse ausserdem, weil auf eine feste Lage bei dieser Manipulation Alles ankommt, einen Assistenten das Becken gegen die Platte noch andrücken, so dass eine vollkommen feste Lage bewirkt wird. Nun hebe ich das Bein, im Kniegelenke es fassend, auf und beuge es gegen den Leib, wodurch ich den luxirten Kopf in Bewegung bringe, mache nun eine Seitenbewegung und lasse durch Streckung des Beines auf diese Weise den luxirten Kopf durch den Kapselriss hindurch in das Hüftgelenk reponiren. Dieses Verfahren hat immer sicheren Erfolg gehabt; natürlich muss es unter Umständen wiederholt werden.

Jedoch giebt es auch unreponirbare Luxationen des Hüftgelenkes und diese sind diejenigen selteneren, bei welchen der Kapselriss ein Querriss in der Nähe der Kapselenden, und nicht ein Längsriss ist. Ich habe einen Fall gehabt, in welchem diese Möglichkeit vielleicht Statt gehabt hat; es war eine 3 Monate alte Luxation bei einem jungen Manne. Ich hatte durch das oben angegebene Verfahren den luxirten Kopf in Bewegung gesetzt, so dass er deutlich erkennbar war und ich die Ueberzeugung hatte, dass keine Verwachsung mit dem Becken eingetreten war. Alle Repositionsversuche in zwei Sitzungen waren aus dem angegebenen Grunde unmöglich. Es wirft sich hier nun die Frage auf, was in solchem Falle zu thun sei? etwa Incision der Weichtheile und alsdann unter Anwendung strenger antiseptischer Behandlung das Reponiren?

Noch eine Notiz ist hier vielleicht von Interesse. — Bei einer mehr als 1 Jahr alten Luxation des Hüftgelenkes hatte ein japanischer Arzt die manuelle Reposition unternommen und nun auch

gemeint, dass sie vollkommen gelungen sei; allein nach meiner genauen Untersuchung schien mir, dass hier nicht Reposition geschehen, sondern dass durch die Behandlung ein künstlicher Bruch gemacht worden; was jedoch für den Patienten wirklich ganz vortheilhaft sich gestaltet hatte.

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7. Beitrag zur antiseptischen Wundbehandlung.

Wenn ich im Nachstehenden über die, nach meinen in Japan gemachten Erfahrungen, in der Militärpraxis bis jetzt allein als mit wirklichem Vortheile anwendbare antiseptische Wundbehandlung ein Wort sage, so will ich keinesweges damit in Betreff anderer antiseptischer Behandlungsarten schon irgendwie ein Urtheil auch nur indirect gefällt haben; zu einer solchen umfassenden und eingehenderen Kritik fehlen der Wissenschaft überhaupt noch die Erfahrungen.

Seit der wissenschaftlichen Begründung der Theorie der Fäulniss durch Pasteur und Schwann, sowie seit der ersten glücklichen praktischen Verwerthung durch Lister ist die Richtigkeit des Principes der antiseptischen Wundbehandlung durch rationelle Praxis, vorzüglich aber durch Billroth in Wien, Volkmann in Halle, Bardeleben in Berlin immer mehr bestätigt und unerschütterlich gemacht worden, obwohl die antiseptische Behandlung von einigen, sogar berühmten Chirurgen und Klinikern in Frankreich noch immer nicht vollkommen gewürdigt und daher auch mehr oder weniger vernachlässigt wird. Jedoch besteht noch grosse Verschiedenheit, sowohl in Bezug auf die Wahl des antiseptischen Stoffes, als auch in Bezug auf die Behandlungsart, und ich habe auf meinen jüngst unternommenen Reisen durch Italien, Frankreich, England, Deutschland in dieser Beziehung das Verschiedenartigste kennen zu lernen Gelegenheit gehabt; zwischen Carbolsäure, Sublimat, Jodoform, Chlorzink, essigsaurer Thonerde schwankt die Wahl und auf mein Befragen nach den Gründen für eine bestimmte Bevorzugung eines dieser antiseptischen Stoffe und dieser oder jener Behandlungsweise habe ich stets nur ein Gutdünken angetroffen. Es ist also hier erst noch viele Erfahrung abzuwarten, und einen Beitrag dazu möchte ich aus meiner in Japan gemachten Militärpraxis liefern.

Für die Militärpraxis, zumal für die eigentliche Kriegspraxis, ist das antiseptische Verfahren von der allergrössten Wichtigkeit, weil man es hier fast ausschliesslich nur mit frischen Wunden zu thun hat, also hier die Lymphgefässe noch offen sind, welche aseptisch zu erhalten das nächste Ziel sein muss. Indessen die allgemeine Ausführung stösst gerade hier auf Hindernisse, welche in der Praxis sehr mitsprechen und welche vor Allem in dem Kostenpunkte und in der gewöhnlichen Ungeeignetheit des Nothverbandplatzes hervortreten. Aber dennoch muss die erste Wundbehandlung durchaus antiseptisch sein und muss vorzüglich sorgfältig ausgeübt werden, um Operationen (Amputationen, Resectionen etc.), so viel als möglich conservativ bleibend, zu verhüten.

Wenn man nun die der antiseptischen Behandlung sich entgegenstellenden Bedenken in Betracht zieht, so ergiebt sich für die militärische Praxis die Forderung, dass der anzuwendende antiseptische Stoff billig, bequem transportabel und von sicherer, dauernder Wirkung sein müsse (namentlich wegen des Transportes), und zugleich die Behandlungsweise die einfachste. Nach meinen Erfahrungen würde nun Jodoform den Anforderungen der militärischen Praxis zwar nicht absolut, aber relativ am meisten entsprechen, und zwar nach der neusten Behandlungsmethode Billroth's mit Jodoformgaze.

Das Jodoform wendete ich in Japan seit 1881 zuerst an und zwar bei Angina Ludowigi aus Anlass eines Artikels in der Berliner klinischen Wochenschrift über Fälle von Jodoformanwendung in Billroth's Klinik. Ich erzielte einen überraschend guten Erfolg damit; das Jodoform wurde damals nur in Pulverform durch Bestreuen angewendet, auf die Bestreuung wurde Gaze und darauf wasserdichtes Papier gelegt, alsdann kam die Anlegung des gewöhnlichen Verbandes.

Zum Unterbinden und Nähen verwende ich Seidenfäden, welche mit verdünnter und kochender Sublimatlösung (1:1000 nach Czerny) durchtränkt sind. Die Seidenfäden*) ziehe ich durchaus den Darmsaiten vor, weil diese letzteren leicht brechen. Desinficirte Charpie will mir immer noch verdächtig erscheinen, und wende ich sie nicht an, sondern statt derselben stets Jodoformgaze.

*) Generalarzt Dr. Issiguro hat im Jahre 1876 statt Darmsaiten Sehnenfäden angewendet, die weniger leicht brechen, als Darmsaiten.

Was die Sicherheit und Dauer der Wirkung des Jodoforms anbelangt, so übertrifft in dieser Beziehung das Jodoform durchaus die Carbolsäure, und der Erfolg ist nach meinen Erfahrungen bei frischen Wunden, sowohl bei einfachen, als bei complicirten Fracturen ein gleich ausgezeichneter, bei alten Wunden weniger günstig. — Hier möchte ich Gelegenheit nehmen zu bemerken, dass bei bereits in Fäulniss übergegangenen Wunden oftmals Chlorzinklösung überraschende Wirkung hat.

In Betreff des Spray muss ich bemerken, dass ich von dem gewöhnlich gepflegten Verfahren abweiche. Während der Operation selbst mache ich vom Spray keine Anwendung; nur vorher lasse ich Spray 1—2 Stunden wirken, um das Operationszimmer zu desinficiren, namentlich bei grossen Operationen, besonders bei Eingeweide-Operationen. Ferner benutze ich die Carbolsäure für Waschung der Hände und der Instrumente, sowie der Schwämme. Zum Spray sowohl als zur Waschung nehme ich 2proc. Carbolsäure, ebenso zum Auswaschen der Wunde. Uebrigens stehe ich nicht an, zu behaupten, dass, trotz des antiseptischen Verfahrens, doch sorgfältigst die völlige Reinigung der Wunde geschehen muss, ja ich meine, dass die völlige Reinigung sogar als die Bedingung sine qua non eines guten Erfolges antiseptischer Behandlung angesehen werden müsse.

In Hinsicht auf die antiseptische Wirkung des Jodoforms kann ich Mosetig, welcher diese Wirkung sehr lobt, nur beipflichten. Von Intoxication ist mir bis jetzt noch kein Fall vorgekommen, obgleich im Urin der Behandelten stets Jod nachweisbar war. Mosetig verwendet als Maximaldosis 20 Grm., ich bis höchstens 10 Grm.

Zur Geschichte der antiseptischen Wundbehandlung in Japan hätte ich zu bemerken, dass ich diese Wundbehandlung im Jahre 1877 mit aus Deutschland in mein Vaterland gebracht habe, und zwar die damals in Deutschland allein angewendete Lister'sche Methode. Ich schrieb in Japan darüber eine Abhandlung in japanischer Sprache. — Noch ist vielleicht die Notiz interessant, dass in der älteren und alten chinesisch-japanischen medicinischen Literatur gelehrt wird, alle Wunden mit Alkohol auszuwaschen, und dass bei der Mundfäule die Aerzte Borax reichten. Dieses uralte Verfahren war bis vor circa 30 Jahren in Japan stets gebräuchlich.

8. Zwei Fälle von Leberabscess.

I. Ein 24jähriger Soldat wurde am 27. Januar 1878 in das Militärhospital zu Tokio aufgenommen. Derselbe hatte 2 Jahre vorher eine Expedition zur Unterdrückung eines Aufstandes mitgemacht. Während er im Felde stand, war er zweimal vom Febris intermittens befallen worden. Darnach fühlte er zuweilen eine grosse Trägheit; von Zeit zu Zeit traten dumpfe Schmerzen in der Lebergegend auf; indessen bemerkten die ihn damals behandelnden Militärärzte noch keine Lebervergrösserung. Er konnte bis zum Eintritt in's Hospital seinen Dienst verrichten, obwohl er zuweilen bettlägerig war.

Status praesens: Schwere Respiration, belegte Zunge, Appetitlosigkeit, abwechselnd Gefühl von Hitze und Kälte, Verstopfung, Puls 110, Temp. 39,3. Anschwellung der Lebergegend, Lebergegend auf Druck schmerzhaft.

Bis zum 3. Februar bleibt das Krankheitsbild das nämliche; nur die Temperatur bewegt sich zwischen 38—40 auf und ab. Vom 3. Februar an nimmt die Anschwellung und die Schmerzhaftigkeit der Lebergegend zu. Die Lebervergrösserung hat folgende Dimensionen: Der obere Rand der Leber reicht bis zur dritten Rippe, auf der linken Seite reicht die Dämpfung über den linken Rand des Brustbeines hinüber, nach unten geht sie bis zur Spina anterior superior des Darmbeines, auf der hinteren Seite erstreckt sie sich über die Lendenmuskeln. Die ganze linke Hälfte der Brust und des Bauches ist aufgetrieben. Die Farbe des Urins ist bräunlich, Gallenfarbstoffe sind in demselben nicht nachweisbar, wohl aber eine kleine Quantität Eiweiss. Specifisches Gewicht des Urins 1003. Bis dahin in Folge der Anwendung von Klystieren und Laxantien regelmässiger Stuhlgang. Die Stuhlentleerungen sind gelblich gefärbt; sonst normal.

Am 12. Februar Nachmittags hatte der Kranke einen Schüttelfrostanfall, nach welchem die Temperatur auf 39,7 stieg. Diese Erscheinung und die bei der Betastung unter dem Rippenbogen fast fluctuirende Prominenz liess mich vermuthen, dass bereits Eiterung eingetreten sei. Ich machte deshalb am nächsten Tage eine Probepunction, die aber kein Resultat ergab. Trotzdem machte ich an der Stelle, wo die Fluctuation wahrnehmbar war, eine vom Rippenbogen ausgehende 9 Ctm. lange Incision, natürlich schichtweise (Haut, Unterhautzellgewebe, Fascie, Musculus rectus). Nach dem Auseinanderschieben des Musculus rectus gelangte ich zur hinteren Wand der Rectusscheide. Ich fühlte nun in der Leber keine Fluctuation mehr. Die vor der Incision deutlich wahrnehmbare Pseudo-Fluctuation war verschwunden; ich führte daher die Incision nicht tiefer. Bei der Operation assistirte mir der Oberstabsarzt Dr. Jisasaka. Die vor der Incision wahrnehmbar gewesene Pseudo-Fluctuation ist möglicherweise bloss eine Folge des durch die vergrösserte Leber bewirkten partiellen Empordrängens des Musculus rectus gewesen. Nach der Incision fühlte sich der Kranke ziemlich wohl; der dumpfe Schmerz in der Lebergegend hatte nachgelassen, wahrscheinlich weil in Folge der Incision eine Entspannung der durch die Geschwulst gespannten

Bauchmuskeln eingetreten war. Ich erwartete, dass, wenn der Eiter an die Oberfläche käme, die Fluctuationen fühlbar würden. — Vom 16. bis 19. Februar hatte der Kranke einmal des Tages Frostgefühl, darnach steigende Temperatur (39,3). Vom 20.— 27. immer einmal des Tages Frostgefühl mit nachfolgender Temperaturzunahme. Am 27. Februar Vormittags fanden sich weisse milchartige Niederschläge im Urin. Reaction des Urins alkalisch; die Niederschläge im Urin erwiesen sich unter dem Mikroskope als Eiterkörperchen. An diesem Tage wieder Frostgefühl, darnach Steigen der Temperatur auf 39,5. Die im Urin nachgewiesenen Eiterkörperchen konnten nur dadurch in denselben gelangt sein, dass sich der Eiter aus der Leber in das Nierenbecken oder in den Harnleiter oder in die Blase entleert hatte. Um zu ermitteln, ob die Eiter-Entleerung vielleicht in die Blase stattgefunden habe, führte ich nach der Methode von Simon mit dem Katheter 20 Unzen Milch in die Blase ein. Das Gewicht der aus der Blase wieder abgelassenen Milch betrug 17 Unzen. Durch diesen Versuch war erwiesen, dass kein Loch in der Blase vorhanden war. Es blieb also nur noch die Möglichkeit des Eiter-Austrittes in das Nierenbecken oder den Harnleiter. Am 2. März fühlte sich der Kranke sehr wohl; die Schmerzen hatten bedeutend nachgelassen, selbst tiefe Inspirationen verursachten keine grossen Schmerzen mehr. Mit der Eiterentleerung hielt die Abnahme der Anschwellung gleichen Schritt; indessen war noch immer eine Vergrösserung der Leberdämpfung nachweisbar. Bis zum 19. März fand sich Eiter im Urin vor. Um diese Zeit war der Kranke soweit hergestellt, dass er ein wenig im Zimmer umhergehen konnte. Am 16. April war eine Zunahme der Eiterkörperchen im Urin nachweisbar und der Kranke hatte Schmerzen in der rechten Nierengegend. Am nächsten Morgen waren die Niederschläge im Urin geringer. Am 5. Mai war der Kranke soweit hergestellt, dass er seinen Dienst wieder versehen konnte; indessen war noch immer vergrösserte Leberdämpfung vorhanden.

Im Semptember des nämlichen Jahres fühlte derselbe wieder Schmerz in der Lebergegend. Er wurde auf's Neue in's Hospital aufgenommen. Diesmal war die Leberanschwellung nicht so beträchtlich. Nach einigen Tagen trat wieder Eiter im Urin auf. Nach der Eiter-Entleerung war eine beträchtliche Abnahme der Leberdämpfung nachweisbar und es trat im Befinden des Kranken eine solche Besserung ein, dass er am 25. September aus dem Hospital entlassen werden konnte. Wie ich einige Zeit darauf vernahm, ist er bald vollständig genesen.

Es ist nicht unwahrscheinlich, dass in dem beschriebenen Falle die Ursache des Leberleidens in den vorausgegangenen Erkrankungen an Febris intermittens zu suchen ist. Der Kranke hatte früher weder an Trauma, noch an Magenkrankheiten oder Hämorrhoiden gelitten. Sicher ist, dass das Leiden schon lange vor der Aufnahme des Patienten in das Hospital bestanden hatte. Offenbar trat dasselbe Anfangs schleichend auf, so dass die den Kranken damals behandelnden Aerzte über die Natur der Krankheit im Unklaren blieben.

Dass die Diagnose beginnender Leberabscesse grosse Schwierigkeiten bietet, wird auch von den holländischen Aerzten in Indien, Java u. s. w. anerkannt. Der Icterus ist, wie gleichfalls von den holländischen Aerzten bestätigt wird, keine constante Erscheinung bei Leberabscessen. In 2 Fällen, die ich zu beobachten Gelegenheit gehabt, fehlte Icterus vollständig. Schulterschmerz (welcher nach Sachs dadurch entsteht, dass durch das Empordrängen des Zwerchfelles der Nervus phrenicus in Mitleidenschaft gezogen wird) ist ebenfalls keine constante Erscheinung. Auch in diesem Punkte stimmen meine Beobachtungen mit denen der holländischen Aerzte vollkommen überein. Ausser den localen Erscheinungen (Anschwellung und Schmerzhaftigkeit der Lebergegend) sind also bei Beginn der Krankheit keine constanten Erscheinungen vorhanden. Das, worauf einige Autoritäten in Betreff der differentialen Diagnose der rechtsseitigen Pleuritis und der Leberanschwellung bei dem Leberabscesse aufmerksam gemacht haben, halte ich für vollständig zutreffend; nach meinen Wahrnehmungen scheint die Geschwulst in der That von oben nach unten fortzuschreiten. Bis jetzt ist die Perforation des Abscesses in die Bronchien, in die Brusthöhle und in den Darmcanal (der günstigste Fall) am häufigsten beobachtet worden, während die Perforation in die Harnwege sehr selten zur Beobachtung gelangt ist.

Ich wollte, wie schon erwähnt, mittelst der Incision dem Eiter einen Ausweg schaffen, stand aber aus den früher angegebenen Gründen von diesem Vorhaben ab. Der Abscess dürfte sich an der hinteren Seite der Leber befunden haben.

2. Ein 30jähriger Militärbeamter wurde am 19. Juli 1879 von heftigen Schmerzen in der Magengegend befallen; gleichzeitig stellte sich Erbrechen bei ihm ein. Ein Arzt, dessen Hilfe er in Anspruch nahm, machte subcutane Morphium-Injectionen, nach welchen der Schmerz ein wenig nachliess, aber ein Gefühl der Spannung in der Magengegend und die Neigung zum Erbrechen zurückblieb. Der Kranke wurde am 21. Juli in das Militärhospital aufgenommen.

Status praesens: Körper nur mässig entwickelt. Pulsschläge 130, Temperatur 38,2, Respiration unruhig, heftiger Durst. Verstopfung seit 3 Tagen, Reaction des Urins normal, Farbe desselben bräunlich, Lebergegend mässig angeschwollen, auf Druck schmerzhaft. Leberdämpfung reicht 8 Ctm. unter den Rippenbogen.

Am 22. Morgentemperatur 38,3, Pulsfrequenz 120, Abendtemperatur 38. Fortdauer der Schmerzen in der Lebergegend. Am 23. nach 2maligem

Klystieren und Reichung eines Laxans reichliche Stuhlentleerung, wonach sich der Kranke sehr erleichtert fühlt. Zunahme der Schmerzen in der Lebergegend, namentlich bei Druck. Am 24. 2malige Stuhlentleerung ohne Klystier. Die Anschwellung in der Lebergegend tritt schärfer hervor, die Schmerzen nehmen zu, Temperatur 38—38,2. Am 25. keine Veränderung im Zustande des Kranken. Am 26. Temperatur 38,2, Puls 96, Respiration 42, alle übrigen Erscheinungen in der Zunahme begriffen. Vom 27.—29. keine Veränderungen im Zustande des Kranken. Am 30. Nachmittags hatte der Kranke plötzlich einen 30 Minuten währenden Schüttelfrostanfall, nach welchem die Temperatur plötzlich auf 42,5 stieg, gerade wie bei Febris intermittens. Sehr unruhige Nacht, viel Durst, dumpfe Schmerzen in der Lebergegend, Schlaflosigkeit. Am 31. trat zum ersten Male Icterus am ganzen Körper auf; Bindehaut ebenfalls icterisch gefärbt — Nachweisung von Gallenfarbestoff im Urin. Reaction des letzteren sauer. Morgentemperatur 38,2, Puls 91, Respiration 36. Am 1. Aug. seit 12 Uhr Nachts sehr beschleunigte Respiration; Brustbeklemmungen, kalter Schweiss auf der Stirn, Schlaflosigkeit. In der Frühe hustete der Kranke eine grosse Quantität Eiter aus. Morgentemperatur 39,3, Puls 118, Respiration 50. Abendtemperatur 39,8, Pus 120, Respiration 50. 2. August. Der Kranke hustet immer noch Eiter aus. Er stirbt unter den Symptomen des Lungenoedems.

Der Verstorbene war nach der Angabe seiner Verwandten früher nie krank; er hatte weder an Trauma, noch an Magenkrankheiten oder an Hämorrhoiden oder an Febris intermittens gelitten. Die Krankheit trat ohne eine bekannte Ursache auf. Leider wurde die Vornahme der Section von den Angehörigen des Verstorbenen nicht gestattet; aber die Anschwellung und Schmerzhaftigkeit der Lebergegend, die ausgedehnte Leberdämpfung, der Schüttelfrostanfall mit der nachfolgenden sehr beträchtlichen Temperaturzunahme, der Eiterauswurf weisen mit grosser Wahrscheinlichkeit auf einen acuten Leberabscess mit Perforation der grossen Bronchien hin. (Wie ich in holländischen Werken gelesen habe, tritt zuweilen bei Leberabscessen mit Perforation der grossen Bronchien Heilung ein.) Ich hatte leider noch keine Gelegenheit, einen Leberabscess nach Dysenterie zu beobachten. Die ziemlich selten vorkommende Complication eines Leberabscesses mit Typhus habe ich nur ein einziges Mal angetroffen, der Fall nahm einen tödtlichen Ausgang.

Ausserdem hatte ich im Jahre 1876 während des Aufstandes in Japan oft Gelegenheit, metastatische Leberabscesse nach Verletzungen durch Schusswunden zu beobachten. Ueberhaupt in Betreff des Vorkommens von Leberkrankheiten in Japan will ich hier noch bemerken, dass Lebersyphilis sehr häufig auftritt und dass

die in Europa so häufige Lebercirrhose ich während 7 Jahren in Japan noch nicht zu beobachten Gelegenheit gehabt habe.

Die amyloide Degeneration der Leber habe ich bis jetzt nicht beobachtet. Leber-Echinococcen habe ich nur einmal beobachtet (durch Punction habe ich die sogenannten Echinococcenhaken erkannt); ebenso verhält es sich mit den amyloiden Degenerationen anderer Theile.

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9. Ein Fall von Angina Ludovici.

Am 26. Februar 1881 wurde ein 2jähriges Kind von einem leichten Husten befallen. Gleichzeitig bildete sich an der Oberlippe eine Pustel. Am folgenden Tage stieg die Temperatur auf 39 und der Husten nahm zu. Mein Privatassistent Tanno untersuchte das Kind; er hörte in der rechten unteren Lunge Crepitation und fand ausserdem am ganzen Körper in der Abheilung begriffene Roseola. Nach 3 Tagen verschwand Roseola und Husten und von der Pustel war nur noch eine kleine Spur vorhanden. In dem Zustande des Kindes schien damals — wenigstens nach der Angabe der Eltern — eine Besserung eingetreten zu sein. Ich sah das Kind zum ersten Male am 5. Tage nach dem Ausbruche der Krankheit. Damalige Temperatur 40,3. Puls 120. Das Schlucken schien dem Kinde sehr schwer zu fallen. Die Eltern gaben an, dass der Husten seit dem vorhergehenden Abend ein wenig zugenommen habe; sie glaubten, dass das Kind sich in der Nacht vorher erkältet habe. Bei der Percussion constatirte ich eine Dämpfung des unteren Lappens der rechten Lunge; bei der Auscultation hörte ich kleinblasiges klingendes Rasseln. Bei der Besichtigung der Mundhöhle entdeckte ich an der Zungenspitze und der Innenfläche der Unterlippe weisse Flecken, wie sie bei den Aphthen vorkommen. Ich ätzte dieselben sofort mit Höllenstein. Ohne Zweifel litt das Kind an einer Pneumonie, complicirt mit aphthenähnlichem Belage in der Mundhöhle. Am 6. Krankheitstage war Schwellung der Mandeln von aussen wahrnehmbar; ich fand aber keine weissen Flecken an den Tonsillen. Die Schlingbeschwerden hatten so zugenommen, dass das Kind seit der vorigen Nacht keine Nahrung hatte zu sich nehmen können. Das Kind hatte die Stimme verloren. Es bildeten sich einige Pusteln auf der Haut des linken Unterkiefers, und ich bemerkte auch einige auf der Haut der falschen Rippen, während diejenige an der Lippe fast vollständig geheilt war. Ich ätzte die neu entstandenen Pusteln sofort. Den Inhalt der Pusteln untersuchte ich mikroskopisch, weil ich glaubte, dieselben könnten von einer Infectionskrankheit herrühren; es waren aber keine Bacterien darin nachweisbar. Bis zum 8. Krankheitstage hatten alle Erscheinungen noch etwas zugenommen. Ein Theil der Pusteln begann nekrotisch zu werden. Die Anschwellung der Mandeln war nicht viel grösser geworden; die äussere Anschwellung dagegen hatte bedeutend zugenommen; sie ging bis zum oberen Theile der Brust und war hart wie ein Brett. Das Kind musste wegen der Grösse der

Anschwellung den Kopf zurückbiegen, um athmen zu können. Diese Erscheinungen brachten mich auf die Vermuthung, dass das Kind an Angina Ludovici leide, obgleich diese Krankheit bis jetzt in Japan noch nicht beobachtet worden war. Herr Dr. Bältz, den ich zur Consultation mit zuzog, theilte meine Ansicht über die Natur der Erkrankung und rieth mir Jodoform anzuwenden, während ich bisher die Pusteln mit Carbolsäure behandelt hatte. Bis dahin war mir nämlich die Wundbehandlung mit Jodoform nicht bekannt; erst um diese Zeit las ich in der Berliner medicinischen Wochenschrift einen Artikel über die Anwendung desselben in der Billroth'schen Klinik in Wien. Am 10. Krankheitstage war eine Erweichung der Halsgeschwulst bemerkbar. Das Kind athmete so mühsam, dass ich fürchtete, es würde Kehlkopfoedem eintreten. Ich machte daher am Halse eine tiefe, vom unteren Rande des Kinnes bis zum Sternum reichende Incision. Aus der Wunde entleerte sich eine übelriechende ichoröse Flüssigkeit. An dem nämlichen Tage trat eine Anschwellung der rechten Achseldrüsen mit Röthung der Haut und eine Anschwellung der Inguinaldrüsen, ebenfalls mit Röthung der Haut auf. Auch im Kreuze bildeten sich zwei Anschwellungen mit Röthung der Haut. Ich machte an all' diesen Stellen die Incision, wobei sich eine übelriechende ichoröse Flüssigkeit entleerte. Nach der Operation befand sich das Kind viel besser, es athmete ruhig und konnte ein wenig Nahrung zu sich nehmen; die Wundsecretionen hatten ihren üblen Geruch verloren, die Erscheinungen der Pneumonie traten zurück. Ich touchirte die weissen Flecken im Munde jeden Tag einmal, um das Hinabdringen derselben in die Mundhöhle zu hindern. Die Mundhöhle reinigte ich sorgfältig mit chlorsaurer Kalilösung. Durch diese Behandlung wurde glücklicherweise die Weiterverbreitung der Flecken hintangehalten. Nach weiteren 14 Tagen war das Kind wieder hergestellt. Nur die Incisionswunde am Halse war noch nicht vollständig geheilt.

Der vorstehende Fall ist dadurch interessant, dass die Erkrankung mit Pustelbildung und Pneumonie begann. Ob die Pusteln infectiöser Natur waren, weiss ich nicht. Ebenso wenig ist mir bekannt, welcher Natur die zu Beginn der Krankheit wahrgenommene Roseola war und ob dieselbe mit Angina Ludovici in irgend einem Zusammenhange stand.

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10. Beitrag zur Behandlung der Harnröhrenstrictur.

Wegen der Wichtigkeit, welche dieser Krankheit beigelegt werden muss, da sie in ihrem Verlaufe sehr leicht gefährlich werden kann, möchte ich nicht unterlassen mitzutheilen, was ich in Japan durch viele und mannichfaltige Beobachtungen an Erfahrung besonders für die Behandlung gewonnen habe. Es werden meine Mittheilungen hier zwar nicht gerade völlig Neues bringen,

indessen sollen sie nur enthalten, was ich mit vollster Bestimmtheit als wirkliche Thatsache hinstellen kann, und dadurch werden sie nicht ohne Werth für die Praxis sein, welche die gewonnenen Erfahrungen Anderer, wenn sie treu übermittelt werden, stets als Zuwachs der eigenen Erfahrung verwerthen kann; denn wir müssen für einander arbeiten und Jeder von dem Anderen annehmen und lernen, was jedoch erspriesslich nur geschehen kann, wenn bei den Mittheilungen stets als oberstes Princip das Bestreben leitet, für jedes Wort verantwortlich zu bleiben.

Cystitis und Pyelonephritis sind bekanntermassen oftmals die Folgen von Strictur. Ueber beide möchte ich zuvor einiges Weniges mittheilen.

In Japan kommt Cystitis sowohl in Folge von Strictur als auch häufig ohne Harnröhrenstrictur in Folge von Gonorrhoe vor. Ich habe denn nun vielfach die lokale Behandlung praktisch ausgeübt — Ausspülung mit salicylsaurem Natron, mit Carbolsäure, mit benzoësaurem Natron; ferner Aetzung der Blasenschleimhaut mit Lösung von Argentum nitricum — und sonst alle bekannten Verfahrungsweisen der lokalen Behandlung habe ich versucht, machmal gute Resultate, manchmal aber auch nur vorübergehende gewonnen. Ich bin nun zu der wichtigen Ueberzeugung dabei gekommen, dass in Betreff der Lokalbehandlung die grösseste Vorsicht geboten erscheint, und dass vor Allem nöthig ist, sie nach und nach und zunächst nur vorzunehmen, um so allmälig den Patienten daran zu gewöhnen, da manche Personen selbst Ausspülungen mit blossem warmen Wasser nicht sogleich vertragen, ja oftmals so empfindlich sind, dass davon Fieber entsteht. Es giebt keine andere als die Lokalbehandlung, und gerade darum müssen wir mit dieser einzigen Behandlungsweise recht vorsichtig zu Werke gehen.

Ich will nun aber auch noch von einem Falle berichten und zwar von einem Falle von chronischer, eitriger Cystitis ohne Strictur.

Der Kranke hatte mehrmals Gonorrhoe gehabt, endlich war Cystitis entstanden, die Jahre lang, zeitweise besser oder schlimmer auftretend, andauerte; erst nach dem dritten Jahre der Entstehung kam der Patient in meine Behandlung. Die Untersuchung ergab also keine Strictur, unter dem Mikro-

skope im Urin Eiterkörperchen, Krystalle (Tripelphosphate) und viele Bacterien; die Reaction des Urins alkalisch. Der Unterleib war in der Gegend der Blase bei der Betastung sehr empfindlich; die Untersuchung des Rectums mit dem Finger liess mich den Eindruck empfangen, als ob die Blasenwand sehr verdickt wäre. Fieber war nicht vorhanden.

Ich begann nun mit der örtlichen Behandlung anzufangen und führte sie in der gewöhnlichen Weise nach und nach durch; also Ausspülung zuerst mit Carbolsäure, dann mit Chlorzinklösung (von höchstens 3 pCt.) u. s. w. In Folge dieser Ausspülungen wurde der Urin besser, und es verging einige Zeit in dieser Besserung. Plötzlich eines Tages empfand der Patient ein theilweises todtes Gefühl an den unteren Extremitäten, das von Tag zu Tag sich weiter verbreitete und bis zur Lähmung besonders der Bewegungsfunctionen sich steigerte, so dass nach einer Woche das Gehen unmöglich wurde; dabei war jedoch die Sensibilität verhältnissmässig nur wenig geschwächt. In der zweiten Woche ging dieser Zustand auf die oberen Extremitäten über. Die Kreuzgegend zeigte sich bei dem Klopfen höchst empfindlich, ebenso die Wadenmuskulatur beim Umfassen. Inzwischen war auch der Urin wieder eitrig geworden und reagirte wie Anfangs alkalisch. Es begann Erbrechen einzutreten, die Temperatur stieg und hielt sich zwischen 38—40°; das Herzklopfen fing an sehr heftig zu werden und steigerte sich immer mehr, so dass endlich der Tod durch Herzlähmung eintrat. Bis zum Tode blieb das Sensorium von Störung fast völlig frei.

An diesem Ausgange der Cystitis hätte ich glauben mögen, ganz ein Bild der Beriberi (Ka-ke) vor mir zu haben; da jedoch eine Section nicht unternommen werden konnte, so bin ich ohne jeden sicheren Anhalt, um zu entscheiden, ob die Blasenentzündung wirklich die letzte Ursache der Lähmungserscheinungen gewesen sei oder nicht, ob vielleicht eine anderweitige Ursache dafür obgewaltet habe. Jedoch in Betreff der Ansicht deutscher Autoritäten, dass bei Cystitis Lähmungen der unteren Extremitäten eintreten können, habe ich in Japan bei zwei Fällen von acuter Blasenentzündung vorübergehende Lähmungserscheinungen mit Zuverlässigkeit beobachtet.

Was die Pyelonephritis anbelangt, so ist zu bemerken, dass sie bei uns in Japan sehr häufig und oftmals auch mit tödtlichem Ausgange vorkommt. — Dies wollte ich in Betreff der oft auftretenden Folgekrankheiten, der Harnröhrenstrictur nur mitgetheilt haben.

Was nun die Behandlung der Strictur anlangt, so muss hierbei immer die permeable und die impermeable Strictur zunächst unterschieden werden. Zuvörderst mögen hier einige Bemerkungen über die Behandlung der permeablen Strictur folgen.

Ehe man rationell mit der Therapie vorgehen kann, muss man sich für eine bestimmte Verfahrungsart entscheiden. Zu diesem Ende ist es wichtig und nöthig zu ermitteln: einmal das Alter der Krankheit; alsdann die etwaigen Complicationen derselben, namentlich ob mit Cystitis oder mit Nephritis; alsdann ferner ist Ort, Form und Beschaffenheit der Strictur festzustellen; endlich auch die individuelle Empfindlichkeit der Schleimhaut. Erst nach Klarlegung dieser Momente kann man sich rationell für eine bestimmte Verfahrungsart entscheiden, was nicht genug zu betonen sein dürfte.

Dass die Dilatation entweder die allmälige oder die beschleunigte oder die gewaltsame ist (Cathétérisme forcé), ist bekannt; indessen ist zugleich auch die Ansicht sehr verbreitet, dass die Dilatation völlig gefahrlos sei und eine leicht zu erlernende Technik, die man darum verabsäume zu üben, auch deshalb, weil durch die Narkose*) bereits die durch die Empfindlichkeit des Patienten entstehenden Hindernisse weggeräumt würden. Allein gegen diese Ansicht muss ich, auf Grund meiner Erfahrungen, Einspruch erheben und kann nicht genug anrathen, bei der Führung des Katheters überaus vorsichtig zu Werke zu gehen; denn es kann durch nicht sorgfältig genug ausgeführte Katheterisirung leicht Muskelkrampf des Compressor urethrae entstehen, wodurch der Arzt getäuscht wird und eine Strictur diagnosticiren zu müssen glaubt, wo eine solche gar nicht vorhanden ist.**) Mancherlei Vorkommnisse in Japan haben mich veranlasst, auf die Uebung des Katheterisirens den allergrössten Fleiss verwenden zu lassen, und ich möchte bei dieser Gelegenheit insbesondere meinem Assistenten Namba Lob ertheilen wegen seiner bewunderungswürdigen Geschicklichkeit, die er sich im Gebrauche des Katheters erworben hat. — Uebrigens hat auch schon Professor Dittel sich zu Ermahnungen zur Vorsicht veranlasst gesehen, um nicht bei der Diagnose auf Strictur irregeleitet zu werden.

In Betreff der Instrumente habe ich die Ueberzeugung gewonnen, dass die Metallkatheter oder Metallbougies denen aus Gummi

*) Die Narkose muss doch immer nur als Nothbehelf und darum nur in den dringend nöthigen Fällen angewendet werden.

**) Bei dieser Gelegenheit will ich bemerken, dass auch eine Anschwellung der Prostata den Arzt bei der Diagnose täuschen kann.

doch vorzuziehen sind, weil die letzteren sehr leicht Falten in der Schleimhaut der Harnröhre bilden; auch Gummikatheter mit Mandrin sind nicht so zuverlässig und so sicher wie Metallkatheter, weil die letzteren von Aussen besser fühlbar sind; auch kommt es vor, dass manche Patienten den Gummikatheter gar nicht vertragen mögen. Selbstverständlich sind im Uebrigen Katheter von verschiedener Nummer, Krümmung und von verschiedener Form der Spitze bei dem Dilatationsverfahren erforderlich. Bei schwerer Strictur sind stets auch verschiedene Nummern der Fischbeinsonde unerlässlich; jedoch ist nicht genug zu warnen, bei Einführung feiner Sonden die höchste Vorsicht anzuwenden, weil sonst die Sonde einmal sehr leicht durch Faltenbildung auf einen falschen Weg oder auch in die Follikel gerathen kann, deren Mündungen mit der Mündung der Blase einerlei Richtung haben, was bereits auch Dittel angemerkt hat. Ferner möchte ich noch in Betreff der Ausführung der Sondirung mit der feinen Fischbeinsonde und namentlich, um sich zu vergewissern, dass die Sonde den richtigen Weg nimmt und wirklich in die Blase hineingelangt, anrathen, behutsam die Sonde öfters hin und her zu ziehen, wodurch man bald das sichere Gefühl für die richtige Führung gewinnt. Sobald als es nun der Zustand des Patienten zulässt, wird die Gouley'sche Sonde längs neben der Fischbeinsonde eingeführt; alsdann vom nächsten Tage an nehme ich bereits einen dicken Katheter und gehe so durch verschiedene Nummern immer weiter, wodurch ich auf Grundlage der allmäligen Dilatation eine Art beschleunigter Dilatation — selbstverständlich wenn keine Contraindication vorhanden ist — vornehme.

Katheterfieber (Urethralfieber) ist in meiner Praxis mir oft vorgekommen, jedoch gewöhnten die Patienten sich meistens nach einiger Zeit der Behandlung an das Katheterisiren; einige freilich niemals. Auch habe ich einen Fall gehabt, wo nach der vierten Sitzung wirkliche Nephritiserscheinungen und endlich der Tod eintrat. Wahrscheinlich war hier Nephritis bereits vorhanden; jedoch ich kann nicht Zweifel darüber hegen, dass sie durch das Katheterisiren beschleunigt worden war.

Was die beschleunigte Dilatation anlangt, so meine ich, dass dieses Verfahren mit dem Dilatator Stearns, wenn es unter Berücksichtigung der oben bezeichneten vier Momente ange-

wendet wird, durchaus günstige Resultate zu Wege bringen muss. Dagegen muss ich die gewaltsame Dilatation als völlig irrationell verwerfen.

Wenn allmälige oder beschleunigte Dilatation nicht zum Ziele führt, namentlich wegen breiter Narbenstrictur, oder wenn wegen der Complicationen Gefahr im Verzuge ist, alsdann ist nun die Urethrotomie indicirt. Man hat das Gebiet der Urethrotomie sehr einschränken wollen, aber viele Autoritäten in neuester Zeit haben es wieder erweitert, und ich habe in Japan in meiner Praxis mich stets an die oben ausgesprochenen Bedingungen, die eine weitere Ausdehnung der Urethrotomie zulassen, unbeirrt gehalten.

Was die innere Urethrotomie betrifft, so mag ich mich nicht für ihre Ausführung entscheiden: denn bei schweren Stricturen ist sie nicht anwendbar und bei leichteren Fällen kann man die Heilung durch die immerhin gefahrloseren Mittel der Dilatation erzielen. Nach Japan hat vor ca. 14 Jahren der holländische Arzt Dr. Elmenenz die Instrumente von Maisonneuve gebracht und in Osaka mehr als hundert Fälle von Strictur durch Urethrotomia interna operirt; jedoch weiss ich nicht, welcher Art die Fälle waren, die er behandelte. Einige Patienten, die er ehemals operirt, und die im Laufe der Zeit Recidive erhalten, kamen später in meine Behandlung. Auch ist mir von einem Falle, den Dr. Elmenenz behandelt hatte und der unglücklich verlief, Kenntniss geworden.

Der Mann, ein in Japan bekannter und mir befreundeter Arzt, war aus der Provinz Etisen nach Osaka gekommen zum Zwecke seiner Operation. Seit dreissig Jahren litt er an der Strictur. Die Voruntersuchung mit der feinen Sonde liess diese durchgehen. Gleich am nächsten Tage darauf ward die Operation angestellt; sie soll anderthalb Stunde gedauert haben. Das Resultat schien zunächst ganz glücklich; der Urin ging mit aller Leichtigkeit ab und der Patient befand sich den Tag über wohl. Am Abend jedoch wurde die Temperatur eine sehr erhöhte; Patient klagte über Kopfschmerz, der Urin zeigte sich blutig; von Zeit zu Zeit stellte sich Erbrechen ein. Am andern Morgen dauert das Fieber fort; der Kranke war den Tag über schläfrig; am Abend bewusstlos; am folgenden Morgen trat der Tod ein unter apoplectischen Erscheinungen. Eine Section ist nicht vorgenommen worden, daher allerdings nicht zu sagen ist, ob die Todesart mit der Operation im Zusammenhange gestanden, oder ob wirkliche Apoplexie eingetreten. Der Mann war 55 Jahre alt.

Wenn ich nun zur äusseren Urethrotomie übergehe, so glaube

ich zunächst gegen ein Vorurtheil, das dieses operative Verfahren für sehr gefahrvoll ausgiebt, doch Einiges auf Grund meiner Erfahrungen vorbringen zu dürfen. Freilich mag ich nicht sagen, dass die äussere Urethrotomie absolut gefahrlos wäre, obwohl bei meinen vielen in Japan vorgenommenen Operationen ich einen unglücklichen Ausgang nicht zu verzeichnen gehabt habe; aber dieses Operationsverfahren ist nach meiner Ueberzeugung doch nur ein bedingt gefahrvolles; denn es kommt einzig hier Alles auf das methodische Verfahren des Operateurs an; darum ist das Wichtigste hier in der That die technische Seite. Alsdann darf behauptet werden, dass die Gefahr eine überaus verminderte, ja, die Operation eine gefahrlose zu nennen ist.

Für das Verfahren der Operation selbst fordere ich drei Acte: Blosslegung der Harnröhre in dem gesunden Theile, mit selbstverständlich beliebiger Verlängerung der Incision in den callösen Theil derselben; Sondirung nach der noch im callösen Gewebe vorhandenen Harnröhre; Erweiterung der noch vorhandenen Wege der Harnröhre in dem callösen Gewebe der Strictur und Einführung des dicken Katheters in die Blase, sowie Ausspülung der Blase mit antiseptischen Mitteln. Selbstverständlich geht der Vornahme der Operation eine Vorbereitung vorher: Sorgfältigste antiseptische Vorbereitung des Zimmers und des Patienten, Zurhandlegung der Instrumente (verschiedene Katheter, Bougies, Sonden verschiedener Art, zumal Fischbeinsonden in verschiedenen Nummern, Messer, der Dittel'sche Katheterhalter, die Gouley'sche Sonde, stumpfe Haken u. s. w.) Für die Lage des Patienten ist die Steinschnittlage die angemessene. Narkose wende ich nur an, wenn der Patient es durchaus will oder höchst empfindlich ist; denn die Narkose macht einen Vortheil für die Operation unmöglich, der von grosser Wichtigkeit ist, wie sich weiter unten ergeben wird. — Die oben angedeuteten Acte der Operation führe ich nun in folgender Weise durch:

Erster Act: Ich führe den grossen dicken Katheter — der desinficirt ist — in die Harnröhre ein bis zum callösen Theile der Strictur; alsdann lasse ich vom Assistenten den Katheter vorsichtig ruhig halten — selbstverständlich muss, wenn die Strictur in der Pars membranacea befindlich ist, der Assistent mit der anderen

Hand den Hodensack halten, gerade wie beim Steinschnitt. Jetzt mache ich eine Incision auf der Mittellinie in der Raphe, bis ich die Spitze des Katheters sehen kann, und lasse alsdann durch stumpfe Haken beide Ränder nach allen Richtungen, namentlich nach den oberen und unteren Winkeln zu, auseinanderhalten; sodann gehe ich zur Stillung der Blutung durch Tamponnirung mit Jodoformgaze über und unterbinde nur die grösseren Gefässe.

Zweiter Act: Ich lasse nun, nach vollständig gestillter Blutung, den Patienten uriniren — und hierin liegt der oben angedeutete Grund, warum ich die Narkose nicht anwende; alsdann gehe ich von dem Orte aus, wo der Harntropfen erscheint, zur weiteren Aufsuchung der Harnröhre über, indem ich die feine Fischbeinsonde bis zur Blase durchzuführen und die Harnröhre zu erweitern suche, wobei ich jedoch aufmerksam mache, dass die Sonde vertical geführt werden muss, wodurch das Treffen der Mündung der Harnröhre erleichtert wird. Kann nun dies Alles mit gutem Erfolge geschehen, so erfolgt der Uebergang zum dritten Acte des Operationsverfahrens sofort, und die Urethrotomia externa ist eine einfache und ebenso gefahrlose Operation zu nennen. Indessen trotz dieses bewährten Verfahrens kann es doch geschehen, dass die Harnröhre nicht gefunden wird. In diesem Falle nun verschiebe ich die weitere Operation bis zum nächsten Tage und mache nicht die immerhin gefährlichen und doch vergeblichen Versuche der Auffindung der Harnröhre, um die Operation absolut in einem Tage zu Ende zu bringen; denn durch das Aufsuchen nach allen Richtungen hin entstehen gar zu viele Verletzungen*); ich tamponnire natürlich (mit Jodoformgaze). Am folgenden Tage oder auch erst am zweiten, ja am dritten Tage lasse ich neue Harnversuche machen, denn der zweite, wichtigste Act darf durchaus nicht übereilt werden.

Dritter Act: Einführung der Gouley'schen Sonde in die Blase, indem die bereits eingelegte Fischbeinsonde die Leitung und zwar dadurch gewährt, dass neben ihr hin die Gouley-

*) Die durch das viele und doch vergebliche Herumsuchen nach dem Eingange zur Harnröhre unvermeidlich verursachten Verletzungen in dem sehr spongiösen Theile, wodurch Venenentzündung, Embolie u. a. m. leicht entstehen kann, machen freilich ganz unstreitig die Urethrotomia externa zu einer höchst gefahrvollen Operation, und es ist mir auf diese Weise erklärlich, dass Manche, wie oben bereits erwähnt, die Urethrotomie an und für sich für gefährlich halten.

Sonde geführt wird; alsdann Spaltung der noch vorhandenen Harnröhre bis zur Blase hin, worauf ich sofort den dicken Katheter einführe und Ausspülung der Blase mit antiseptischen Mitteln vornehme.

Für die Nachbehandlung bleibt der dicke Katheter, durch den Dittel'schen Halter befestigt, in der Blase liegen*), wobei jedoch — abgesehen von etwa schon vorhandener Cystitis — sorgfältig auf die Reaction des Urins Acht gegeben werden muss, indem der Katheter, sobald der Urin anfängt alkalisch zu reagiren, sogleich entfernt werden muss. Zur Verhütung des Eindringens von Bacterien befindet sich an dem Katheter ein Schlauch, welcher in ein Gefäss mit Carbolsäure mündet. Den Katheter**) lasse ich die ersten drei bis vier Tage liegen, obwohl in dieser Zeit der Urin fast ausschliesslich doch durch die Wunde seinen Ausgang nimmt; denn es ist doch danach zu trachten, den Urin seinen Weg durch den Katheter nehmen zu lassen; jedoch komme ich hier der Empfindlichkeit des Patienten durchaus nach, indem ich den Katheter während der ganzen Nachbehandlung andauernd liegen zu lassen durchaus nicht für nöthig halte. Das Herausnehmen des Katheters aber möglichst jeden Morgen zur Reinigung und Desinficirung darf natürlich dabei nicht unterlassen werden. Indessen muss das Uriniren allezeit durch den Katheter geschehen; jedoch ist dem Patienten zu rathen, den Urin stets so lange als möglich anzuhalten. Tägliche Ausspülungen der Blase mit salicylsaurem Natron oder mit Carbolsäure dürfen nicht unterlassen werden. Die Wundfläche heilt durch Granulation in 2—3 Wochen.

Zum Schlusse seien mir noch einige Bemerkungen aus meiner Praxis erlaubt.

*) Man könnte auch, da in den ersten Tagen der Nachbehandlung der Urin doch nur stets seinen Ausweg durch die Wunde nimmt, den Katheter aus der Harnröhre ganz entfernen und eine dicke, mit Jodoformgaze umwundene Gummiröhre mit einem im Carbolgefässe endenden Schlauche durch die Wunde legen, und durch diese den Urin, so lange bis Granulation eintritt, entweichen lassen, alsdann aber die Röhre nach und nach wieder wegnehmen und das Uriniren nun erst durch einen Katheter in der Harnröhre beginnen zu lassen.

**) Obwohl ich weiss, dass man den Nélaton'schen Katheter neuester Construction gerne anwendet, so ziehe ich doch den Metallkatheter mit dem Dittel'schen Halter vor, weil dieser die Blase nur an einem Punkte berührt, während jener eine Berührung der Schleimhaut an vielen Stellen mit sich bringt.

Ich habe im Ganzen bis jetzt während 7 Jahren 10 Operationen schwerer narbiger Harnröhrenstrictur mit und ohne Fistelbildung durch Urethrotomia externa in Japan ausgeführt, davon jedoch die ersten drei in anderer Weise, als in der von mir geschilderten. Die Operationen wurden alle glücklich zu Ende gebracht. Bei einem der letzten Fälle trat unmittelbar nach der Operation ein eigenartiger Zustand ein, weshalb ich von diesem Falle hier doch noch Mittheilung machen möchte. Die Operation war sehr leicht vor sich gegangen, sogar das Auffinden der Harnröhre war so günstig, dass dabei Weichtheile gar nicht weiter unnütz verletzt wurden. Der Patient aber zeigte unmittelbar nach der Operation schwachen Puls, die Extremitäten waren kalt, so dass also Shock-Erscheinungen vorlagen, vielleicht nur als Reflexvorgänge. Am zweiten Tage nach der Operation urinirte Patient, der Urin war nur in geringer Quantität, voller Eiweiss, Blutkörperchen und Cylinder. Am dritten Tage stieg die Temperatur hoch, am vierten Tage zeigte die Haut des Hodensackes eine Veränderung in der Farbe; am fünften wurde die Haut entschieden phlegmonös, so dass ich sofort und wiederholt Incisionen machte, um Brand zu verhüten. Unter sorgfältigster antiseptischer Behandlung ist Patient glücklich durchgekommen und war nach 4—6 Wochen völlig wiederhergestellt. Was die hier eingetretene Phlegmone anbetrifft, so ist nicht zu sagen, ob vielleicht während der Operation eine Risswunde irgendwo entstanden, oder ob sie ihre Ursache in der früher von einem Privatarzte ausgeführten Behandlung mit der Fischbeinsonde, durch welche bei nicht vorsichtiger Manipulation Verletzungen hervorgebracht und alsdann Harninfiltration auf diese Weise oder auch auf andere Weise möglich geworden war.

Mittheilen möchte ich auch, dass ich auf die Methode der Benutzung des vorquellenden Urintropfens zur Aufsuchung der Harnröhre ganz zufällig gebracht war, indem ein während der Operation aus der Narkose erwachender Patient dabei urinirte und so selbst den Weg zur Harnröhre mir wies. Dass Heineke und auch Hueter bereits auf dieses zuverlässige Hülfsmittel, den Patienten uriniren zu lassen, aufmerksam gemacht, war mir in Japan damals noch unbekannt geblieben. Mit dieser Methode ist entschieden ein grosser und nicht hoch genug anzuschlagender Vortheil gewonnen worden; denn durch das noch so sorgfältig ausgeführte Auseinanderhalten der Wundränder nach allen Richtungen mit den stumpfen Haken kann es dennoch nicht anders geschehen und kann nicht verhütet werden, dass nicht kleine Faltenhöhlen und Faltengänge entstehen, welche den Operateur verführen, sie für die Eingänge zur Harnröhre zu halten.

Wenn ich durchaus das allergrösste Gewicht darauf lege, die bei der permeablen Strictur nothwendigerweise noch und wenn auch in kleinsten Ueberresten vorhandene Harnröhre aufzusuchen, um sie zur Bildung der neuen möglichst zu verwerthen, — wobei ich natürlich keineswegs der Meinung sein kann, mit der Incision genau dem Gange der noch vorhandenen Harnröhre zu folgen — so ist mein Grund dafür einzig der, dass die Epithelien der Schleimhaut, welche hier allemal noch da sein müssen, das Verwachsen der neuen Harnröhre verhüten und folglich das Wichtigste für das zu erstrebende Ergebniss der Operation beitragen müssen. Diese Epithelien ignoriren, heisst die natürliche Unterstützung verschmähen.

Von den so sehr lästigen Fistelgängen, die bei den schwereren Fällen der Harnröhrenstrictur gar nicht selten vorkommen, weil vor und hinter der Strictur eine Erweiterung der Harnröhre entsteht, kann ich aus meiner Praxis nur sagen, dass sie nach richtiger Behandlung der Strictur, sei es durch Urethrotomie, oder durch die allmälige Dilatation, sehr bald und befriedigend von selbst heilen.

Ein Wort noch von der sogen. impermeabelen Strictur. Wir müssen hier eine zwiefache Impermeabilität unterscheiden: ob nämlich die Strictur nur impermeabel für die Instrumente, oder auch impermeabel für den Urin; ist dies letztere der Fall, so haben wir aber einfach Retentio urinae vor uns, und bleibt nur die Wahl zwischen Urethrotomie und Blasenstich. Ich ziehe, Falls es die Retentio urinae nur irgend erlaubt, also durchaus keine Gefahr im Verzuge ist, Urethrotomie vor; sonst führe ich den Blasenstich aus, und zwar nach dem auch in meiner Praxis vorzüglich bewährten Dittel'schen Verfahren. Nach dem Blasenstiche, den ich wegen seiner leichten und schnell zu bewerkstelligenden Ausführung immerhin vorziehe und, da die Urethrotomie Zeitaufwand erfordert, auch natürlich unter Umständen vorziehen muss, unternehme ich dann Urethrotomie.

Anhang.

Bemerkung zum Blasenschnitt.

Während 7 Jahren habe ich bis jetzt nur 2 Mal Gelegenheit gehabt, den Blasenschnitt auszuführen; einmal bei einem 6jährigen Kinde mit ziemlich grossem Steine, und das andere Mal bei einem Manne zur Herausschaffung der durch ungeschickten eigenen Gebrauch abgebrochenen Stücke eines Gummikatheters. Ich habe bei dem Kinde, weil der Stein ziemlich gross war, die Ausführung des oberen Blasenschnittes (unter Anwendung des Hueter'schen Metalldrain in der Nachbehandlung), bei dem Manne den Medianschnitt unternommen gehabt. Den Medianschnitt, der jetzt wieder sehr anempfohlen wird, möchte ich jedoch bei Kindern nicht für geeignet halten wegen der noch hohen Lage der Blase, obwohl ich weiss, dass Scriba (ein deutscher Arzt und Professor der Universität in Tokio) den Medianschnitt bei Kindern sehr glücklich ausgeführt hat, und dass Generalarzt Dr. Sato und sonst auch unsere Militärärzte, besonders die Oberstabsärzte Jokoi und Isisaka, einige Male den Lateralschnitt gemacht haben. Bei Erwachsenen dürfte indessen dem Medianschnitte der Vorzug gebühren, weil durch ihn selbst grössere Stücke des Steines aus der Blase mit Leichtigkeit sich entfernen lassen, und die Operation und die Heilung leicht von Statten geht. — Zur Ausführung der Methode der Zertrümmerung, die in unseren Tagen wieder sehr empfohlen wird, gehört unstreitig eine ganz besondere Gewandtheit, die nur sehr lange Uebung und specielle praktische Bethätigung gewinnen lassen kann. Es ist in dieser Hinsicht in der That überaus interessant, den berühmten Thompson in London in seinen erstaunlichen Operationen zu sehen; die Instrumente, und namentlich der von ihm verbesserte Bigelow'sche Ausspülungsapparat, leisten unter seiner Verwendung, was nur gewünscht werden kann.

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11. Einige Fälle von Atresia ani congenita und über einen Fall von Atresia duodeni.

Ich habe bis jetzt 9 Fälle von Atresia ani congenita beobachtet. In 3 Fällen war gar kein Anus vorhanden, in den übrigen Fällen war der Anus zwar vorhanden, aber das Rectum endigte

als blinder Sack. In sämmtlichen 9 Fällen kamen die Kinder zwischen dem 3. und 8. Tage nach der Geburt zur ärztlichen Behandlung. Zur Zeit ihrer Aufnahme in das Hospital waren die hauptsächlichsten Erscheinungen: Dunkelroth gefärbter, aufgetriebener Bauch, Ausdehnung der Hautvenen, Erbrechen. (Wenn Erbrechen vorhanden, ist die Spannung des Bauches nicht stark. Selbst wenn Erbrechen vorhanden ist, sterben nach meiner Erfahrung die meisten Kinder mit Atresia ani zwischen dem 8. und 10. Tage nach der Geburt.) Ich habe die Beobachtung gemacht, dass der Anus und der Enddarm sich durchaus nicht parallel entwickeln; es kommt vor, dass die Analöffnung vollkommen entwickelt ist, während das untere Stück des Enddarmes in der Entwickelung ganz zurückgeblieben ist. In anderen Fällen dagegen ist das untere Stück des Enddarmes zur vollständigen Entwickelung gelangt, während der Anus ganz fehlt. Im letzteren Falle bietet die Eröffnung des Rectums keine Schwierigkeit und die Prognose derartiger Operationen kann als sehr günstig bezeichnet werden. Nach der Incision der Haut kann das mit Meconium gefüllte Rectum als eine fluctuirende Anschwellung durch die Haut durchgefühlt werden, besonders wenn das Kind schreit. Im ersteren Falle hängt das Gelingen der Operation von der Lage des unteren Stückes des Enddarmes ab; da dieses Stück bald nach vorne, bald nach hinten, bald rechts oder links seitwärts liegt, so verursacht die Auffindung desselben oft viele Mühe. Wenn dieses Darmstück hoch liegt, so ist die Nähteanlegung selbst bei gelungener Operation sehr schwierig.

In 4 von den 9 zu meiner Beobachtung gelangten Fällen der Atresia ani war ich nach der vergeblichen Vornahme einer bis zu 3 Ctm. langen Incision genöthigt, zur Colotomie zu schreiten. Ich führte die Colotomie nach der Methode von Littré aus, weil man auf diese Weise am leichtesten in das S Romanum gelangt. Bei einem der 4 Kinder war die Analöffnung vorhanden, bei den anderen nicht. Eines der Kinder, an welchem ich die Operation am 8. Tage nach der Geburt vornahm, starb. Es bestand bei demselben schon vor der Geburt in Folge der Zersetzung des Meconiums eine leichte Peritonitis. In keinem der 3 übrigen Fälle trat nach der Operation Peritonitis auf. Die Unterscheidung des Dick- und Dünndarmes ist zuweilen sehr schwierig. Das Meconium schimmert

nicht immer durch die Darmwände hindurch. Wenn bereits Zersetzung der Meconiummasse eingetreten ist, erweitert sich in Folge der Gasbildung der Darm und das Meconium schimmert nicht durch die Darmwand. Zudem tritt in solchen Fällen Röthung des Darmes ein und die Dickdarmstructur wird dadurch schwer erkennbar. Der einzige sichere Anhaltspunkt für die Unterscheidung ist und bleibt die anatomische Lage des Darmes.

In den oben erwähnten günstig verlaufenen Fällen führte ich 6—12 Monate nach der Operation den Metallkatheter in den Anus artificialis ein, um das untere Stück des Enddarmes zu suchen. Da ich die Spitze des Katheters nicht durch die Weichtheile hindurch fühlte, machte ich eine neue Incision, aber es gelang mir in keinem der 3 Fälle, den natürlichen Anus zu bilden. Die drei Kinder blieben in ihrer Entwickelung zurück und starben nach einigen Jahren; zwei an Meningitis, das dritte in seinem 5. Lebensjahre an Peritonitis.

Bei 4 anderen mit Atresia ani behafteten Kindern, worunter zwei ohne Anus, machte ich bloss eine Incision, und zwar mit bestem Erfolge. In einem dieser Fälle, in welchem die Analöffnung gänzlich fehlte, bot die Operation wegen der hohen Lage des Enddarmes grosse Schwierigkeiten.

Der letzte der von mir beobachteten 9 Fälle betraf ein mit Atresia duodeni behaftetes Mädchen, das Kind eines Arztes. Das Mädchen erbrach sich nach der Geburt mehrere Male und aus dem Anus entleerten sich ungefähr 6 Grm. käseartiger Substanz. Da das Kind keinen Stuhlgang hatte, gab ihm der Vater am 3. Tage nach der Geburt ein Klystier, das aber keine Wirkung hatte. Er nahm nun eine Untersuchung mit dem Bougie vor. Das Bougie drang ungefähr 4 Ctm. tief ein und stiess dann auf Widerstand. Der Vater vermuthete deshalb, dass das Rectum keine Oeffnung habe. Das Kind wurde am 4. Tage nach der Geburt in das Hospital aufgenommen.

Status praesens: Neigung zum Erbrechen. Der Bauch ist nicht sehr gespannt, trotzdem ist die Bauchdecke dunkelroth gefärbt. Die Hautvenen sind ziemlich ausgedehnt.

Ich nahm zuerst mit dem Finger, dann mit dem Bougie eine Untersuchung vor, welche die Vermuthung des Vaters vollkommen bestätigte. Ich machte mich nun daran, das Rectum aufzusuchen. Ich machte zu diesem Zwecke einen Kreuzschnitt an der Analöffnung, worauf sich aus derselben wieder eine käseartige Flüssigkeit im Gewichte von ungefähr 6 Grm. entleerte. Obgleich ich die Incision ungefähr 3 Ctm. tief führte, fand ich das Rectum nicht. Ich fragte mich nun, woher die aus der Analöffnung entleerte käseartige, dem Meconium ähnliche Substanz gekommen war.

Da die Incision vergeblich war, wollte ich die Colotomie am S Romanum vornehmen; aber der Vater des Kindes gestattete dies nicht. Das Kind starb am 7. Tage nach der Geburt unter den Erscheinungen des Collapsus. Der Vater erlaubte mir die Section vorzunehmen. Ich machte zuerst die Colotomie am S Romanum, dann führte ich einen Katheter vom S Romanum nach unten an die Analöffnung. Ich fand einen Blindsack an der linken Seite des Steissbeines. In dem Blindsacke und im S Romanum befand sich kein Meconium, sondern eine geringe Quantität Schleim. Ich machte hierauf einen Kreuzschnitt am Bauche, fing vom S Romanum aus zu untersuchen an, drang nach und nach längs des S Romanum, des Colon descendens und Quercolons bis an das Ende des Colon ascendens vor, wo sich eine Erweiterung befand. Aber nirgends fand ich Meconium; das Colon enthielt nichts als Schleim. Das Colon war so dünn, dass man es kaum vom Dünndarm unterscheiden konnte. Vom Colon ascendens ging eine Art Strang bis zum Duodenum. Das Duodenum war bedeutend erweitert, es befand sich eine grosse Quantität grüngefärbte, käseartige, dem Meconium ähnliche Substanz darin. Der Magen war mit der nämlichen Substanz angefüllt. Abgesehen von den oben erwähnten Abnormitäten, waren die Eingeweide normal. Der Fall zeigte die grösste Aehnlichkeit mit einer von Förster beschriebenen Atresia duodeni.

Woher kam die aus dem Anus entleerte Substanz? Professor Förster behauptet, das Meconium sei nicht ein Secretionsproduct der Darmschleimhaut, sondern ein Hautschmer, weil sich in demselben viel Vernix caseosa und Haare vorfinden. Dieses Hautschmer löse sich im Fruchtwasser und gelange durch den Mund des Fötus in den Darm. Wenn diese Theorie richtig wäre, so könnte man annehmen, dass die aus dem Anus entleerte käseartige, dem Meconium ähnliche Substanz von aussen in den Körper des Kindes gelangt sei.

Gedruckt bei L. Schumacher in Berlin.

Erklärung der Abbildungen.

Fig. 1—5 zur Exostose.
Fig. 1. Seiten-.
Fig. 2. Hinter-Ansicht der Patientin.
Fig. 3. Die Exostose in natürlicher Grösse.
a. Stiel derselben.
Fig. 4. Querdurchschnitt durch dieselbe.
Fig. 5. Mikroskopische Structur derselben.
Fig. 6. Ansicht des Patienten mit Elephantiasis scroti.
Fig. 7. a. Nekrotische Scapula.
b. Resecirtes Knochenstück des Oberarmes.

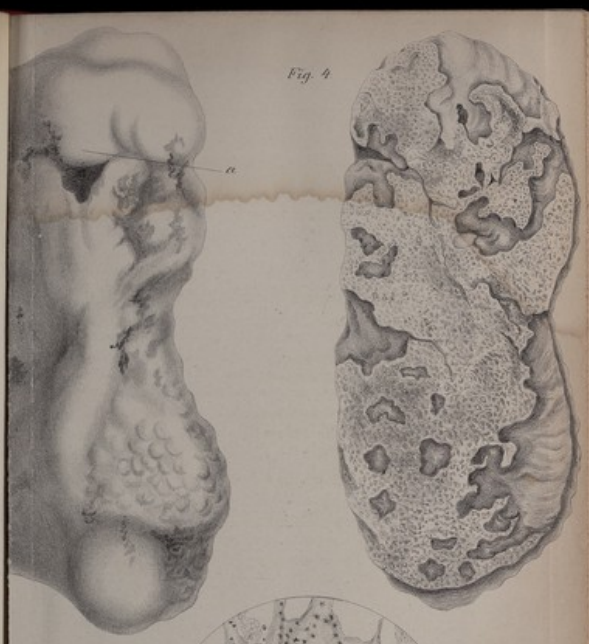

Fig. 1.

Fig. 2.

Fig. 7a.

Fig. 3.

Fig. 4

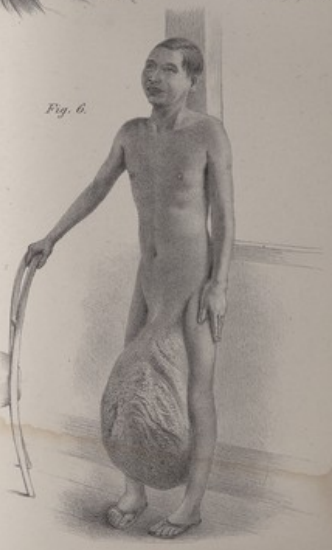

Fig. 6.

Fig. 7b.

Fig. 5.

Dr. H. Sakaki del.

Alb. Schütze Lith. Inst. Berlin.

ON A

NEW METHOD OF TREATING WOUNDS,

(GRUBY'S SYSTEM)

AND THE

MEDICAL AND SURGICAL ASPECTS

OF THE

SIEGE OF PARIS;

OUTLINES FOR A NON-OFFICIAL REPORT

(MÉMOIRES POUR SERVIR)

TO THE

Physician to the Rt. Hon. the Minister of State for India.

(INCLUDING INVESTIGATIONS CONCERNING PYÆMIA, THE DANGER OF THE INTRODUCTION OF CALF VACCINATION INTO OUR INDIAN POSSESSIONS, THE TENT-HOSPITAL SYSTEM, &c.)

BY

CAMERON J. F. STUART MACDOWALL,

SURGEON INDIAN ARMY, 3RD BOMBAY LIGHT CAVALRY,

(PRESENT DURING THE WHOLE OF THE SIEGE.)

LONDON:
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A NON-OFFICIAL REPORT.

THE importance, in a sanitary point of view, to the vast populations of India (and to our admirably-organised *Indian Vaccination* Department) of knowing the truth with regard to calf-vaccination, so much practised in France,

ERRATA TO

"On a New Method of Treating Wounds, &c."

Page 8, line 4 from the bottom, for "*of*" read "*after.*"

" 11, to make the sense clearer insert :—*"And human virus is difficult to implant on the calf," before the note * at the bottom of the page.

" 13, line 11, for "*sixteen*" read "*fifty.*"

" 20, " 12, for "Cubic *feet*" read "Cubic *mètres.*"

meantime, [illegible]
especially the true ventilation of the question of animal vaccination, which these gentlemen may not have studied, as being less interesting to the army (where vaccination is carefully carried out) than it must be to our Indian, civil, and military populations, where it is so difficult to render

* Drs. Gordon and Wyat (since created Chevaliers of the Legion of Honour), two distinguished medical officers, were sent to Paris by the War Office. It was therefore, I presume, not deemed necessary to send medical commissioners from the Indian army. I had no *official* facilities for obtaining information in consequence.

A NON-OFFICIAL REPORT.

THE importance, in a sanitary point of view, to the vast populations of India (and to our admirably-organised *Indian Vaccination* Department) of knowing the truth with regard to calf-vaccination, so much practised in France, and which — having been introduced into England — threatens to be imported into our Indian possessions, where the native mind would too easily lean towards its adoption; the connection (Ricord's theory) between the spread of *smallpox* (inseparable from anything which affects vaccination) and *pyæmia*, which has carried off in round numbers nine-tenths of *all amputations* performed in Paris; the study of the theories as to this truly fearful complication of all military surgery; the investigation as to whether mere cubic space of air, and change thereof, is sufficient to prevent it; these three points alone seem to make it peculiarly desirable for the Indian Government to know all that possibly can be learnt on the subjects of this report. I have no doubt that the reports of Drs. Gordon and Wyat* will be supplied to the Indian authorities; but, in the meantime, I think it my duty to assist, as much as I can, *especially the true ventilation of the question of animal vaccination*, which these gentlemen may not have studied, as being less interesting to the army (where vaccination is carefully carried out) than it must be to our Indian, civil, and military populations, where it is so difficult to render

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human vaccination effective, owing to the vast numbers of individuals which it has to embrace, and as to which our European army is but as a drop in an ocean. If animal, instead of human, vaccination be largely practised in India, I have no hesitation in saying that *all* trustworthy written evidence (both academic and official) tends distinctly to show that it will lead to an inevitable and prodigious increase in the number and violence of epidemics of small-pox, to the exhaustion of the human vaccine, and to the weakening of the effects of even good vaccine.

It is evident, therefore, that no other excuse is required for writing this crude paper on sanitary matters. Indeed, it would appear that even *spontaneous* "vaccinia" in a calf, so rare now-a-days, so easily found in Jenner's time, is difficult to communicate to the human subject, and only acquires its strength when it has become naturalised, as it were, in the human subject; the already weak vaccine from a child's arm, given to a calf loses all its virtues; in fact, the calf is proof against it in nine times out of ten—it only causes a *common sore*. This is propagated, and the terrible deception begins, the ultimate effects of which no one can calculate.

I now proceed to the arrangement of the report.

Having been confined to bed with dysentery* at the time of the investment of Paris, it was impossible for me to gather, during *that* period of illness, much information concerning the existence of medical or surgical epidemics. At a later period, when I found that it was impossible for

* I was attacked with dysentery only on my arrival in Europe—slight *diarrhœa* at Alexandria. I came home for an affection of the sight. Strange to say, soon after the cold weather began, and when I was feeding exclusively on horse flesh, I *steadily improved* in health until the ration was reduced to 300 grammes a day. Strange to say, also, my first good dinner after the armistice made me quite sick, and caused me to vomit, although eating slowly and with excellent appetite. The only medicines that *did me good were 2 drachm* doses (4 to 6 grammes) of bismuth with quince syrup, *ter. die.*

me to leave the place, I gave my humble services for the cause of suffering humanity, as an unpaid medical volunteer with the war-companies of the National Guard. Active medical occupations and frequent absences in the trenches, the forts, outposts, and redoubts of the besieged city, rendered it almost impossible for me, amid the turmoil of the bivouac, to follow very accurately and continuously the practice of the regimental and civil hospitals and the ambulances of Paris. Nevertheless, when occasion offered, and on my periodical returns to the town, I made it my duty to visit these, to inquire into the practice pursued, the march of mortality, and the organisation of the army and hospitals. In the battalion to which I attached myself I became acquainted with the organisation of the regimental medical system—a system which, with the exception of some excellent points, I must mention with unqualified disapproval. This in no way applies to the medical staff themselves—a class of highly-educated and zealous men; but to the limitation of the functions which they are allowed to perform—a limitation which crushes their utility, and almost puts a stop to their progress in clinical knowledge; which renders them the subordinates, not of their immediate medical superiors, or of their regimental commanding officers, but of what is called the "Intendance Militaire," or Commissariat, a very worthy body in itself, but as incapable of guiding the medical department of an army as the surgeons would be of organising the Commissariat.*

The following remarks will therefore naturally class themselves under two distinct heads: First, sickness and mortality, both in the civil and military population,

* It will suffice here to mention an occurrence which took place at the sortie in November, across the Marne. As many as forty staff surgeons, with ambulances, carts, stretchers, &c. &c. were kept all day long, by order of the Intendance, on a plateau, whilst wounded and dying literally *died outright* for want of their services. On *subsequent* occasions many surgeons moved rapidly to the front *without orders*, and were of course welcomed and useful.

including treatment. Secondly, organisation, both in the army medical department and the military and civil hospitals.

It must be premised that, as far as the statistics of the army go, it will be only after a lapse of a year and a half, or two years, that the statistics of the war will be made public. What little information I have been able to acquire has been (as before stated) from the efforts of personal friends, viz. surgeons, who have given me the results of their own hospital practice, and which do not show, of course, the ratio to the strength of the corps to which their sick belonged. The registrar's bulletin "hebdomadaire" shows the mortality in the civil and, roughly, in the military population; it will be found further on in this report.*

It is certain that the number of deaths in 1870 were more numerous than in 1869. In ordinary times the deaths per week were about 800. This gradually increased since October to the enormous cypher of 4,671 per *week*, and in January and February, 1871, the number again increased. As the mortality amongst the soldiers is certainly always much greater than amongst the civil population, including in winter numerous deaths from frost-bite as well as in combat, there is little doubt that the army lost in proportion a good third more than this, for the registrar assured me that their returns were very provisional and approximative as yet.†

Two very salient facts arrest the attention in looking at the mortality of the civil and military populations of Paris and the immediate environs. First, the mortality from *small-pox* (average—latterly—246 per week) amongst the former;

* I have condensed all the weekly returns I could get at the Hôtel de Ville into two, and given the percentages worked out from them.

† A really fine statistical document is published *monthly*, but it is in arrear always of six months, as most statistical documents generally are.

secondly, the fearful losses from what the French call "infection purulente," and which we call, almost as unsatisfactorily, "pyæmia."

With regard to smallpox, the weekly mortality is seen in the annexed condensation of the bulletins hebdomadaires, being an average of—latterly—246 fatal cases per week. The practice of *vaccinating* a *calf*, and afterwards vaccinating *children from* the animal, seems to have had a good deal to do with the permanent weakening of the genuine "Jennerian" vaccine matter, as the French very complimentarily call it in honour of the discoverer. As very erroneous opinions seem to exist on this subject even in England, where of late years calf-vaccination seems to have been to some degree practised, and as the subject is one of great interest in our *Indian Empire*, I have made it my duty to endeavour to arrive at the truth of the whole matter, to examine patiently the results of experiments, and to chronicle the opinions of those who have given the system a fair trial. I have registered their deliberate and unbiassed opinion, and the unimpeachably exact results of their practice. It will be seen that there would be no slight danger of doing irreparable mischief in India (where the native mind would be sure to lean towards calf-vaccination) if this system were ever introduced, instead of the arm-to-arm vaccination, which alone can safely guarantee from smallpox (if properly performed and with *good* matter).

It had at last become a fixed belief in France that the vaccine matter had become "*weakened*"; that the frequent cases of smallpox were consequent on the degeneration of the true virus of vaccine, and not because the operation was badly performed, &c. In short, vaccination began to fall into *disrepute** for many reasons. *First*, because possibly in many cases the virus *was* weak, that is, was

* My friend Dr. Piojey, Chevalier of the Legion of Honour, told me of fifty cases of smallpox in his ambulance, *none* of whom had ever been vaccinated.

(and always has been in *some* cases) a spurious virus. *Secondly*, when the virus was good, the operation was badly performed by midwives, &c., who never saw their patients more than *once*, and consequently many mothers thought their children vaccinated who were really not so. *Thirdly*, because *diseases* were said to be transmitted with the vaccine matter. And, indeed, certain ugly sores had appeared, instead of healthy pustules, on the arms of many men of regiments in the south of France.

Now, it has been proved before the Academy that when the lancet does *not* draw *blood* from the *pustule* which furnishes the vaccine matter, no other disease is ever transmitted to the healthy arm but the vaccine disease itself. If *blood* be drawn *with* the vaccine matter, and inserted into the arm of a healthy person, both a bad vaccination and the inoculation of contagious disorders *may* be the result. This is very rare, and ordinary precautions are sufficient. Vaccine pustules must not be squeezed in taking out the matter, or simple serosity and blood are also expressed on the surface, and much epithelium is found. As in everything else, the *juste milieu* is best. The child must not be too young or too old.

To obviate all these difficulties and dangers, it was proposed to vaccinate a cow-calf with good healthy matter, or to find a cow-calf with the original vaccine disease *spontaneously* developed on its teats, and to vaccinate children therefrom. (I wonder that inoculating *smallpox* on the calf was not tried; would not this produce in the calf a disease identical with Vaccinia?)

Messrs. De Paul and Lanois vaccinated immense numbers of people from the cow, and made large sums of money thereby.

Let it first be premised that no case of revaccination with human virus ever occurred after a lapse of twelve days out of 3,000 cases. Three cases occurred, however, within a few days of the operation, and were fatal. They

were cases where the diseases—smallpox and vaccinia—were simultaneous; the former preceding the latter probably, and being in a state of incubation. This period is much longer than we would suppose; a child born in a house where its sister was ill with smallpox—the mother being already pitted of old with the disease—was immediately (Dr. Fillette, Medical Conference, Paris, 15th June, 1870), even before the cord was cut, vaccinated (from *glasses*, however). Nevertheless, although the pustules seemed to progress pretty well, on the twelfth day smallpox appeared, confluent and fatal. It is generally about the *twelfth* day or the *fourteenth* after being exposed to the contagion that the disease begins. Vaccination sometimes seems to modify the disease and render it less fatal even after the contagion has been certainly contracted. (Dr. Dagand, ditto.)

Dr. Quinquand had one-third successful cases with the virus from the calf, but ALL his cases were successful with virus from a child's arm (Jennerian vaccine, as the French complimentarily call it).

Dr. Thevenot with calf-vaccine had only *two* successes in twenty-one cases!

On the Orleans Railway, all the surgeons sent reports to their chief, Dr. Gallard. *All* of them found that human arm-to-arm vaccination stopped the ravages of the disease, and that of the protected very few had anything but what is called varioloid diseases. One case only thus protected died. Thirty-two surgeons thus sent in reports on the vaccination from the calf. *One* says that vaccine from the calf became much better after transmission through the arms of three or four children, though bad and difficult to introduce the first time. The rest (thirty-one) found vaccination from the calf most provokingly unsuccessful, succeeding at the very utmost only in a fourth of the children vaccinated directly, and *much* less from calf-virus-tubes, or glasses. The vaccine pustules were always small, and contained little real

matter and much epithelium. The fifth day is recommended for collecting the calf-virus. Calf-virus which has *spontaneously* appeared on the animal has been found to be better than that which is inoculated—given to the calf from a child's arm. It is a rare disease, however, amongst the herds, and it is difficult to find a single case. I ask, again, is not the vaccine disease a *smallpox, modified by its passage through the cow?* If so, would it not be well to inoculate perseveringly the teats of the cow with real *smallpox* until the disease called vaccine appear? Has this been perseveringly tried?

To return to calf-virus, Dr. Vallot (*Gazette des Hopitaux*) says: "The matter of the calf sent me by the Academy entirely failed."

Dr. Lalagade: "I never succeeded once with the calf-virus sent me by the Academy." He did afterwards with matter from a calf with the spontaneous disease.

Dr. Pomarel directly from the calf succeeded in children; failed with adults.

Dr. Miguel: "The calf-virus which you sent me is useless."

Dr. Caril: "Not having succeeded with calf-virus."

Dr. Maury, with calf-virus transmitted to a *child* vaccinated from this child, and *succeeded* twenty-four times out of forty-three.

Dr. Faton: "The calf-virus sent from Paris by Dr. Lanois, and only four days old, failed *completely* (*echoua complètement*)."

Dr. Jollet: "The calf-virus, which I have carefully followed in its operations, gave always most ludicrous results (*dérisoires*)."

Dr. Aussat: With calf-virus, results nil.

Dr. Picard: Ditto.

Dr. Diard: Ditto.

Dr. Guerinean: Ditto.

Dr. Huguel: Ditto.

Dr. Lacombe: Ditto.

Dr. Besnard: Complete failure with the calf-virus, &c. &c.

Dr. Gaillard: With Jennerian vaccine 2,740 successful out of 2,856. But with calf-vaccine 170 successful out of 283 children. Revaccinations, 126 successes out of 522.

It is asserted that calf-virus gets stronger (?) the oftener it is passed through children; that is, that it is difficult* to implant, and is probably quite inefficacious also at first. "It is this calf-vaccine so weak in itself *which has caused the degeneration of the real Jennerian virus.* We must return to it. Vaccine from a child of six weeks is bad vaccine; wait till four or five months, and no purer vaccine can be had, for *no* child, who has not shown contagious (syphilitic, &c. &c.) maladies before this, is afflicted with them."—(Dr. Bouchut.)

Messrs. Garecki and Ruaux, house-surgeons of La Charité, described after the hæmorrhagic primary, hæmorrhagic secondary, and the variolous rash, a fourth variety, which, from their description, I shall call the Bullœferous variety; it occurs at the period of maturation; the patient is hideous with "sero-sanguinolent phlyctenœ, as large as blisters; one expects gangrene, but the fever abates, the appetite returns, and the cure is rapid."

The reasons for the failure of vaccination from calf-virus are obvious. The spontaneous disease is rare, and is not easily communicable to other calves or to children, and spurious pustules are formed not very easily distinguishable from real ones. Besides, the passage through the calf (which *takes* the disease with so much *difficulty*) probably weakens the virus (already often weak) which is implanted

* Even with vaccine which is successful on children. The calf's teat must be clean shaved at the base, near the inguinal fold. An induration forms, not quite a pustule. "From the fourth to the eighth day this is scratched with the lancet to extract it with pressure." (?)

into its teats from a child's arm. The inexorable logic of facts have hitherto proved the above statement.

Again, if vaccinia in the calf be only a modified smallpox (Jenner, Bennet, &c. &c.), if the calf can modify smallpox, how much more easily will it not modify a modified smallpox, either from an affected calf or child?

I now proceed to the subject of Pyæmia.

PYÆMIA AND HOSPITAL GANGRENE.

The mortality from pyæmia and gangrene amongst the wounded and amputated during the siege of Paris is known to have been greater than has ever occurred before in the annals of military surgery. Nearly two years will probably elapse before official returns and the expected great work of Dr. Chenu, C.B., &c., shall enlighten us as to the exact statistics of these maladies. But some approximation to a knowledge of their ravages can be obtained from other sources; and the remarkable results obtained by particular modes of treatment give us perhaps some clue to their etiology. I hope at some future time to be able to furnish fuller information on the subject, as I am still expecting promised documents from Paris.

Dr. J. Worms, the "Inspecteur of the Service Medicale" (Sanitary Commissioner), told me *viva voce* that: "in round numbers *all* the cases of amputation in the last few weeks of the siege *died;* that the case was the same with nearly *all* the wounded who were not operated upon, and that the deaths were almost all from infection purulente." He added that "a distinguished friend of his had utterly renounced operating, as he invariably lost even the apparently most favourable cases calling for such interference."

Dr. Demarquay, one of the best operators I have ever seen, and a most distinguished man of science, told me that "he had not succeeded in saving a *single case* of amputation at the newly planned and constructed wooden hut-hospital at Passy."

Even at the American tent-ambulance, as will be shown further on, and which will be seen to be almost the open-air treatment, Dr. Swinburne told me (complainingly!) in anticipation of exact statistics, that he had only saved three amputations of the thigh out of seven cases; three cases of resection of the elbow out of five, and about the same proportion of amputations of the leg. But three out of seven cases of compound fracture of the thigh recovered also by conservative surgery, one being of the neck of the femur.

In the Hôtel Dieu all amputations of the thigh died, but Dr. Maisonneuve by conservative surgery saved sixteen cases of severe wounds out of sixty-six. At the Jesuits' Hospital at Vaugirard he was even more successful.

But I subjoin my notes taken on the spot, generally from the surgeons' own mouths.

AMERICAN AMBULANCE.—TENTS.

The tents are heated by a pipe of common sheet-iron which runs under the boarded floor. The boards are laid side by side on traverses, but not quite touching each other. The stove is a simple brick and mud furnace, sunk into the ground outside the tent at one end. The tube passes under the whole length of the floor, and ends in a common sheet-iron chimney at the other extremity of the tent.

The tents are about 30 feet long and 20 broad, exactly the shape of a cottage, and are laced to each other, gable-end on, so as to form blocks of four or six tents. The end walls of each intervening tent form a screen to separate the sick during operations, death, &c. These end walls open up in the middle like a curtain. The height of the ridge poles (singularly light) is about 15 feet, and of the side walls about 6 feet. There are double flies. It has rained incessantly these days past; not a drop has penetrated. These tents resemble large and rather flat-roofed Indian "rowties" or hill tents. Duck is good for Europe or the States, but is, as we know, quite insufficient

under an Indian sun, and our thick four-fold cotton ones alone can prevent sunstroke; but the shape is excellent. The walls are supported, like "bichauba tents," by poles.

150 beds; 260 admissions. No pyæmia (?). Only one case of erysipelas. No gangrene. No scurvy (?). 13 deaths in all up to *now* (February). (Up to November, only 2 deaths out of 62 admissions, one of them from tetanus.) Dressed with finely-picked and carded oakum and nitric acid lotion. The deaths were, 4 amputations of thigh (out of 7 operated on), 1 with symptoms of tetanus, 2 of exhaustion (?), 1 gastritis (?). No purulent deposits in viscera (?); (post-mortems were *not* made). 2 cases of resection of elbow died (*out* of 5 operated), both secondary operations. 2 amputations of leg from exhaustion (?), one with *rigors*. 1 compound fracture of ankle-joint died; he had lately had smallpox. 1 of bullet through the liver died; 1 ditto through abdomen. 1 case of ball through lungs, grazing liver, died; and 1 ditto through lung; all from exhaustion (?). Of the recoveries, there were 3 out of 7 amputations of thigh, 3 resections of elbow out of 5,* and *several* amputations of the leg. 3 out of 7 compound fractures of thigh recovered, 1 through neck of femur opening hip-joint. 3 of compound fracture of ankle recovered out of 4. One of the recoveries with visceral wounds had two ribs broken and chest opened.

HOTEL DIEU (THREE WARDS ONLY).

All amputations of thigh died. In Maisonneuve's ward, 66 cases (conservative surgery). No amputations. 4 deaths from pyæmia, with visceral abscesses. 12 from their wounds alone (5 of these thoracic).

* 6 out of 12 recorded operations = 50 per cent. I could not get the exact returns of all the cases. The success in the thigh amputations was remarkable.

CORPS LEGISLATIF.

105 beds; 200 cases admitted; 9 amputations; 5 died; 1 *thigh*, 1 leg, and 2 arms recovered; 10 cases of wounded died: Total 20. Carbolic acid lotion treatment, and disinfection.

ITALIAN AMBULANCE.

Forty beds; *two deaths in 200 treated* (*not* good accommodation as to site, &c.). Among cured I *saw* one case of ball through ankle-joint, one through shoulder ditto. 16 waggons; 160 men. This ambulance corps conveyed immense numbers of sick and wounded to *other* hospitals and ambulances. OIL AND COTTON-WOOL TREATMENT OF GRUBY.* Conservative surgery exclusively. (I am promised full details.)

SOCIÉTÉ INTERNATIONALE OF GENEVA (DR. CHENU, ETC.).

At first, in Exhibition Building; *326 mètres cubes* to *each* bed; much pyæmia. In Grand Hotel *only 35 cubic mètres* (1 mètre 50 centimes between each bed); LESS PYÆMIA, about 30 getting well. Dr. Bidard has a case of amputation of the thigh getting well, Dr. Vidal one, and another surgeon saved one: all other thigh amputations whatever *died*. Two cases of penetration of the knee recovered (Messrs. Vidal and Bidard one each). Dr. Guillon lost almost all amputations also.

WOODEN HUT-AMBULANCES DE LA PRESSE AT PASSY (RICORD, DEMARQUAY, ETC.).

Assisted in all about 20,000; *lodged* 2,000 wounded. At Passy 1,400 square feet to each bed. M. Demarquay,

* For details of this treatment, see "Remarks" further on.

with great frankness, told me that both at the Tuileries and here he had lost *all* his cases of amputation through pyæmia, &c. I saw a case of his of resection of knee-joint getting well and two of elbow-joint ditto. About 400 cases now in the barracks.

The huts were built on the most approved new plan, 21 in number (20 beds in each). The height was 8 mètres; length, 30 mètres; width, 10 mètres. There was a lantern roof running the whole length of the building for ventilation and light, with practicable windows. The urinals, &c., constantly disinfected, were of slate and porcelain, and the ordure was carried away in closed vessels. All seemed perfectly sweet, except the room for soiled sheets, pillow-cases, &c. (temporarily used before transporting the foul linen to laundry). The barracks are too close to each other, however. The distance is not twice the height, only about *once*. Papering is on the inner walls to half the height, which seemed to me to interfere with the infiltration of air (through fissures, joints, and cracks in the planking) so usefully supplied by the interstices of the cloth in tents.

PALACE OF THE LUXEMBOURG.

400 beds were at first in barracks (21 beds in each) in the park; 8 mètres high and 30 mètres long, 10 wide, with a lantern roof running the whole length for ventilation, &c. Barracks much too close. Shells drove the patients into the palace. Only 93 cases of *wounds* (gunshot); 3 deaths (out of these 93) occurred, 2 of them were amputations. All died of pyæmia. Other statistics not yet obtainable. These are probably only approximative.

VAL DE GRACE.

The largest and finest military intramural hospital in Paris, perhaps in Europe. Splendid grounds on the culminating point of town; buildings well detached; 50 to

60 cubic mètres to each bed; *can* accommodate 2,000 sick; 800 are usually in the house. About 1,800 sick and wounded have been treated since the siege. Conservative surgery has generally proved a *failure;* even the civil surgeons who were employed as "*extraordinary* officers" came to this conclusion. Dr., or rather Assistant-Surgeon Mounier, son of the distinguished surgeon-in-chief, amputated fifteen times: (of these amputations two were disarticulations of the shoulder-joint,) nine recovered (this is *authentic*), being about 60 per cent.! The other surgeons of the establishment were less successful, but an average of 50 per cent. of amputations *recovered*, which is better, probably, than any civil hospital establishment can boast.* There were ten cases of pyæmia with metastatic abscesses, all fatal. There was *no hospital gangrene* and no erysipelas; indeed, the hospital is admirably managed both by the religious sisters and by the medical and military authorities. Official statistics will appear soon.

"*Ambulance of Clichy and its annexes, from 23rd Sept., 1870, to 7th Feb.*, 1871.

"Admissions, sick and slightly wounded ... 4,721

"Deaths 382

"Of these 1,156 were cases of smallpox, and 117 of them died.

"No amputations, no pyæmia. Many frost-bites. Local cases of congelation got well. General, diffuse congelation, three cases; all died with symptoms of torpor and asphyxia.

"Erysipelas did not prevent many recoveries from smallpox, nor did large burrowing abscesses. The majority of the deaths were from the hæmorrhagic form of the disease."
—*Dr. Piojey.*

* Not even excepting perhaps the American Ambulance, if we take Assistant-Surgeon Mounier's 60 per cent. of successful amputations. But it is fair to say that the American average, 50 per cent., is that of seven amputations of the *thigh* and five *resections* of elbow —all very serious cases.

REMARKS.

It will be seen from these notes that pyæmia was the almost universal cause of death in all the wounded, and was so without exception in amputations even at the American tent-ambulance. For where a *post-mortem* examination is not made it is almost impossible to deny the presence of metastatic abscesses in the lungs, liver, &c. I hope to be able to give correct statistics at some future date. We shall examine further on the probable causes of the greater success in treatment at some establishments than at others.

First, we must carefully distinguish "infection purulente" —pyæmia—from hospital gangrene. Secondly, we must distinguish one form of pyæmia (*septicæmia*) from another form of the malady accompanied by metastatic abscesses. The one is a rapidly increasing typhoid state from a general *poisoning* of the blood, ending in a few days in death; the other a succession of rigors and hectic fever, followed by suppuration in the viscera or limbs. Both are accompanied by a grey ashy appearance of the wound, and by an unmistakeable and peculiar smell. So also is hospital ulceration or gangrene proper. It has been proved by many experimentalists (see Bennett's "Clinical Lectures") that the injection of laudable pus into the veins of animals (large quantities, half a pint and more) such as horses, asses, &c., does not necessarily cause either gangrene or metastatic abscesses. Notwithstanding that experiments on animals have not the same weight as those performed on man, this fact should not be lost sight of. It would indeed appear that the mere presence of *pus* in the blood is not capable of producing typhoid symptoms and metastatic abscesses, or the daily cases of reabsorption of purulent matter in ordinary civil practice would *always*

be followed by such. Is not the exudation in pneumonia often reabsorbed and eliminated in this way? The idea that abscess in the liver in India is always preceded by pus-producing ulceration in the intestines, is quite exploded and disproved. Besides, in metastatic abscesses pus is not very easily discovered in the veins (although probably there), unless phlebitis be present; and as we cannot suppose that the absorbents and veins do not continue to act in cases that get *well*, in the same way that they do in cases that *die* of pyæmia, mere absorption of *pus* will *not* account for the disease.

Dr. Ricord, in a most interesting conversation which I held with the distinguished Professor, told me that in the new large, well-ventilated, wooden-hut hospital erected on one of the most healthy spots in Passy, pyæmia caused fearful ravages; that one or two patients in a room, large enough for *twenty beds*, by no means escaped the disease, although certainly less liable to it (as in all contagious maladies) than when crowded.

The great syphilographer described to me, in excellent English, his belief that one powerful—the most powerful —cause of the disease is the prevailing *variolous or small-pox-poisoning* of the atmosphere, &c.; a *pyogenic* influence at *all times*, and producing a pyogenic *diathesis* (if not small-pox) in those who are protected by vaccination. He by no means denies the influence of other causes of the disease, such as crowding, bad hygienic conditions of all sorts, and (with Nelaton) osteo-myelitis, but he believes firmly in the variolic influence, as having a great deal to do with a large proportion of the cases.

The great surgeon Nelaton affirms that osteo-myelitis (a pathological alteration in bone, in its membranes, and chiefly in the marrow) has much to do with the disease. But as successful cases of fractures and amputations at all times are accompanied by division of bone, its membrane and contents, it must only be in cases where jolting in

ambulance-waggons and delay in operating obtain, that this pathological condition probably arises. I therefore maintain that amputation should, if possible, always take place near the field of battle, that *one* operating tent, &c. could serve for large bodies of men, and could almost always be put up in a safe place. *Well-bandaged* amputation cases are easily transportable.

Has not scurvy something to do with pyæmia?

That crowding alone will not account for it seems to be demonstrated at the Palais de l'Industrie, where Dr. Chenu, C.B., President of the Medical Committee of the International Society, told me that, with 326 cubic feet of air per bed, pyæmia was very much more frequent than at the Grand Hotel to which the patients were moved afterwards, and in which each had had only thirty-five cubic mètres of air. In the Palais de l'Industrie the rooms were enormous and lofty, about the height of an ordinary second storey (in London) from the pavement, but I think the air was rather stagnant.

The wards were on the first floor—not on the ground. Nevertheless the ground floor is used, at the period of Exhibitions, for storing cheeses, and provisions of all kinds, animal and vegetable, stalling prize cattle, &c. &c. Besides, although the cubic space of air was quite in excess of that ever seen in hospitals or barracks, its very extent prevented its easy *renewal*. The means for effecting this were only such temporary measures as could be adopted in a building never intended for a hospital. Nevertheless the astounding fact remains that pyæmia and hospital gangrene were *exceptionally and unprecedentedly fatal* in a building where there were 326 *cubic mètres of air to each bed*, and this not at the *end* of the siege.

Scurvy is appearing *now* in Paris. The French surgeons certainly did not find many typical cases at the time of the great mortality from pyæmia, &c.; but we know that the scorbutic diathesis exists long before the gums are affected.

Slight ulceration and an aphthous state of the mouth I saw frequently in January, 1871.*

Dr. Gruby, the eminent histologist and practitioner, has long been of opinion that charpie—*old linen*† fretted out by *hand*—is a real nest for animal and vegetable germs. He has long taught that the exclusion of the atmosphere‡ is an important element in the prevention of hospital gangrene, &c. &c., and he has sought to get rid of microscopic or even invisible germs by the use of oil and cotton-wool dressing. He has practised this for many years, and his certainly remarkable success in conservative surgery at the Ambulance Italienne seems to confirm some of his views. Oil is known to be destructive of germs, cotton acts as a filter even of the atmosphere, and the union of both forms not simply an imbibition, but a real *mixture*. If a piece of cotton be dipped into oil and held up against the light, not only a drop will be seen to depend from it, but a transparent *cylinder* or *column* will gradually form and elongate itself, in which (on forcible disrupture) the exquisitely fine fibres of cotton will be found intermixed and carried down. This intimate admixture of the oil and cotton fibre (a perfectly clean non-fermented and non-*manipulated* vegetable fibre) forms a packing, as it were, to all the interstices of a wound; it insinuates itself gradually into all the corners,

* The French soldier is underfed, and I believe that soup may almost be said to be the ruin of the French army. It takes at least four hours to make and much albumen is lost in skimming the pot. Hot coffee or tea, with meat, cold or roasted, broiled or "braisé," should alone be tolerated on marching-days.

† The flax from which linen (often foul) is made is, as he points out, already subjected to *fermentation*, and in *water*. *Fermentation* is, probably enough, a germination of a low type. Dr. Gruby objects to water (unless boiled previously) even in the washing of wounds. He as much as possible removes dirt, coagula, &c., with dry or slightly oil-moistened cotton wool.

‡ Dr. Guerin has invented an admirable apparatus for *this* purpose.

and nooks, and crannies; the oil carrying with it perhaps only one fibre, or even none at all (where its passage is impossible). Here another principle comes into play. When oil comes into contact with albumen, it is well known that they, especially if motion be present, form an emulsion, that the oil is separated into globules, each covered with a fine pelicle of albumen. Dr. Gruby maintains that in a wound there is always sufficient motion for this. First, there is the constant beat of neighbouring arteries; there is the motion through the capillaries, which must cause some commotion, however imperceptible; and there is the vermicular and involuntary motion in muscle almost constantly going on. Indeed, in all the wounds I have seen dressed by this method, I have distinctly seen an emulsion formed; the secretion of pus has been very moderate in quantity, and there has been (even on holding a twenty-four hours' dressing close to the nostrils) no offensive smell whatever. A pad of cotton is placed over the pellets imbibed with oil, and, being bandaged, it is to be remarked that motion on the outer surface of the pad does not necessarily affect those portions of oiled cotton sticking in the wound.*

In India, where lint is so great a source of expenditure to the State, and where cotton is abundant and cheap, where grain-oils (such as sesame-seed oil, &c. &c.) are not expensive, this mode of dressing wounds, from which I have witnessed some wonderful cures, seems to be peculiarly applicable. Not a single case of pyæmia or gangrene occurred amongst Dr. Gruby's patients.

* This probably accounts for the fact that all the patients I saw told me that they had never suffered any *pain*, to speak of, from first to last.

A GLANCE AT THE MEDICAL ORGANISATION IN THE ARMY AND AMBULANCES.

In a regiment, the surgeon visits the barrack-rooms before morning report (to commandant at orderly-room). He receives at the office, or orderly-room, tickets, or small company-books—left there for him by each sergeant—on which the men who have reported sick are inscribed (by the sergeant). He then proceeds to the respective dormitories in turn, and with the sergeant's help examines the sick. He has thus an opportunity of ordering the ventilation of the salle, opening windows, seeing as to its cleanliness, &c. &c. It is impossible for him, however, to diagnose a case *properly*, either by auscultation or percussion, under these circumstances, in an immense room where work of all sorts—washing, cleaning of accoutrements, brushing of clothes and boots, making the beds, sweeping the floor, &c. &c.—is going on, even if silence at the word "Attention!" were possible—which it is not. Besides, the *daily* inspection of barracks is a duty which should not fall to a medical officer, whose orders, there, can only be carried out through the captains in a roundabout way,—and seldom are. A soldier will open a window for the surgeon out of politeness; but he will shut it again as soon as he is gone, if he choose. Again, the quartermasters, captains, sergeants, &c., are almost daily in the salle, for the very same purpose. However, there is much to be said in favour of the morning visit of the surgeon to each barrack-room. It would be impossible in India, where the large splendid barracks occupy a large area of ground. The visit could not be finished before evening. The diseases which the surgeon is allowed to treat in the dormitories or regimental infirmaries are chiefly scabies (if not inveterate) and gonorrhœa; visceral affections only when unaccompanied by fever, or when *not* of a contagious nature, and

slight wounds or trifling skin diseases; in fact, every serious case—any real case of disease—must be packed off at once to the large military hospitals. In this way the surgeon who remains with a regiment, although he must have studied for years at the military school of Strasbourg, and pass competitive examinations of great difficulty, becomes a mere registering clerk—not even a clinical clerk, be it observed—for he can never follow the march of a single serious case to the end. He has no opportunities of acquiring clinical knowledge, whether surgical or medical, and very soon forgets what he knew, and the officers and men never think of consulting him or coming for his medical opinion. The morning, weekly, and monthly returns are as simple as possible in the National Guard, but in the Line they are vexatiously and, I think, needlessly numerous. In consequence, all returns and statistical documents are much in arrears.

AMBULANCE HAVERSACK.

Medicines, and a box of instruments (amputation), with means for dressing twenty-five wounds, splints, &c. are thus carried on the back of the hospital orderlies (two men per company). There is one sack of this description per battalion, independently of two mule-panniers, and the last model is perfect. Unfortunately old pattern ones are still much in use. It is an admirable thing if well-made and judiciously furnished, or fitted with useful medicines, such as pills and powders, instead of *fluids*, &c. It is astonishing what a comfort it is to have a man *beside* one on the field, with almost everything required for an emergency, in a haversack. There is a difficulty, however, which constantly arises, viz. the providing for the man's own private kit-haversack. This, of course, he could not carry as well, and we had to put in some of the carts the best way we could. The regimental hospital haversack is, however,

one of the most useful and best arrangements of the French army. There is no running about looking for the mules and mule-panniers, or waiting for their arrival. A hospital orderly (each in his turn; there are two per company) carrying it, accompanies and stays by the surgeon during the whole action. In the Cavalry this sack is replaced by a valise. But as colonels think it hurts a horse's back it is necessarily changed from one cavalier to another, and is thus often carried by men who are not hospital orderlies.

MULE-PANNIERS.

The mule-panniers, or rather chests, for they are not the elegant wicker things known to us as such, are allowed in proportion of one pair per battalion. They also contain another amputating case* and means of dressing 200 wounds. (They are often carried in carriages, waggons.)

One single pattern of waggon is now ordered for the transport of hospital, commissariat, or other stores. They weigh 2,000 pounds, and at least 3,600 pounds when loaded. Mules, &c., are used for the transport of the contents in Algiers, where waggons are not so useful. All this constitutes what is called a light or flying hospital. As many as 364 mules are required for the ambulance of a corps of 10,000 men. Cavalry are, however, better followed by light carts than by mules, who, if they trot, smash everything to pieces. The proportion allowed is as follows:

* Therefore that in the ambulance haversack, which is complained of as heavy, might be replaced by a single knife and light saw, with forceps, thread, &c. Every orderly should carry two tourniquets, I think, *handy*, and not *packed away*.

Designation of the Means of Transport, and of the Load.	Proportion allowed.			
	For Head-quarters.	For a Division of Infantry.	For a Division of Cavalry.	For a Park of Artillery.
Waggons with load complete	5	4	2	2
Ditto, to follow empty for emergency	1	1	1	1
Litter mules	15	10	5	2
Cacolet mules	30	20	10	5
Stretchers	50	40	20	6
Barrels of 50 litres	8	6	4	4
Blankets	25	20	10	10
Cotton sheets	16	10	6	6
Mattresses	8	5	3	3
Bags for straw	8	5	3	3
Cotton shirts	30	20	10	5

PERSONNEL.

The *personnel* is not nearly sufficient in the infantry regiments, thus:—

Designation of the Personnel.	Head-quarters Staff Hospital.	Staff of Division of Infantry Hospital.	Staff of Division of Cavalry Hospital.	Staff of Park of Reserve Artillery Hospital.	Regiment of 3 Battalions Infantry.	Regiment of Cavalry.	Battalion (large) of Chasseurs (rifles).
Principal Surgeon	1	—	—	—	—	—	—
Surgeons Major	2	1	1	1	1*	—	—
Asst.-Surgeons Major	4	3	3	—	2	2	2
Apothecaries Major	1	—	—	—	—	—	—
Asst. Do. Do.	2	1	1	1	1	1	1
Responsible Commissariat Officers	1	1	1	1	1	1	1
Adjutants of Commissariat	4	3	2	2	1	1	1
Infirmary Men	5	3	2	2	—	—	—
Infirmary Soldiers (Hospital Orderlies)	20	17	8	8	8†	8	8

* Indifferently a Surgeon or an Asst.-Surgeon Major.
† This depends much on commanding officer.

But, indeed, it will be seen that the regimental surgeons, one to about 1,000 men, are expected to send their sick and wounded to the large divisional and headquarter hospitals. I suppose I am too old to learn, but I cannot see the great advantage of this. It requires constant transport. It leads to secondary amputation; it takes the soldier out of his regiment, and on a long, sometimes jolting trip—wounded and sick though he may be—amongst perfect strangers. The separate hospitals of regiments under a particular medical officer's charge should form in themselves the divisional and other hospitals, by *merely being assembled* together in one locality, and should only leave their wounded and sick in depôt hospitals, when the march *requires* their being left behind.

At the Grand Hotel, Baron Mundy held two conferences on the modes of transporting sick and wounded. He condemned all side-saddle chairs carried on horse or mule back as thus causing unavoidable jolting.* He had models of all sorts of hand-stretchers, wheel-stretchers, and wheeled carriages. The carriages of the American Ambulance were much praised, the German ones also; the New Zealand and Punjab hammock-stretchers were much liked, but he gave the preference to *wheeled carriages* with good springs and indiarubber stays, &c. They should open freely at the sides for depositing the wounded, and have *a step* all round to facilitate dressing the patient.

Baron Mundy's own model seemed to be the best. As for stretchers, surely (it appears to me) the simplest and lightest of common cheap stretchers, carried by two men, are all that are necessary and are the best for conveying the wounded to the wheeled carriage. A *number* of these can be rolled up and carried in the ambulance waggon or on the roof. This cannot be done with wheelbarrow stretchers, which are scarcely required thus at all, except

* In mountain paths, however, they are necessary.

where there are no wheeled carriages. It requires at least two men to load a wheelbarrow-stretcher. It *jolts* over the uneven ground of battlefields, until it can reach the wheeled carriage, and when once there the wheeled spring vehicle renders it unnecessary. Hospitals and ambulances are treated of in the previous division of the subject—Pyæmia. But we may add, that besides the hospitals of the town (civil and military), there were numerous subscription ambulances, such as the Great International of Geneva, the immense Ambulances de la Presse (subscriptions collected by the Press), of the theatres, the American Ambulance, the English one, founded by Mr. R. Wallace, the Italian, the Belgian, private ones in convents, monasteries, families, &c. &c.

The Mayors of Paris also established numerous ambulances in their different districts (arrondissements), and also Ambulances de Rampart for the immediate relief of the wounded, sent from the ramparts by the surgeons of the National Guard. These last ambulances are all well paid by the respective municipalities or mayoralties. They are supplied with beds, stretchers, splints (chiefly of straw), bandages, medicine, &c. A first dressing is applied to the wound, and the patient is carried to the nearest *fixed* ambulance to be operated upon or permanently treated. The regiments of the line, &c. have also large fixed and moveable ambulances in the pay of the Military "Administration." The surgeons of the Regiments and National Guard accompany their battalions into action, and only stop hæmorrhage, apply splints, a bandage, &c., and superintend the despatch of the sick to the ambulances. The most eminent surgeons have given their services gratis to the ambulances.

The word or name "ambulance" is singularly misapplied, and moveable or flying ambulance is simply to be considered as tautological. Infirmary or hospital is the proper designation for all "fixed ambulances," and "moveable hospitals" or simply "ambulances," for "Ambulances Volantes."

A very simple stretcher is in use, a straw mat exactly like an Indian "tattie," made of whisps of straw (instead of *fragrant roots*), the thickness of two fingers, which are tied, side by side, by three twines knotted between each whisp, and these twines, thus knotted, run the whole length of the mat, which is 7 feet long by 2½ broad. It can be rolled up like a roll of music, and is strengthened at every foot or two by sticks instead of whisps. Two light poles run under it into four loose strong twine loops, and the whole is lifted, with or without brace-straps, by two men. It is very light and cheap.

Tents, such as those at the American Ambulance, and light wooden barracks, are excellent for treating the wounded in; so also are open sheds as long as the weather will permit.

No ornamentation or outward luxury or expensive material ought to exist in temporary camp or field hospitals, for all experience tends to show that they *ought to be burnt down without remorse and at once, the moment that pyæmia or gangrene*, &c. may appear.

STATISTICAL NOTES

ON THE

MORTALITY FROM SICKNESS, COMBAT, BOMBARDMENT, &c.

I here give 2 tables compiled from the Registrar's weekly returns. The first (B) is the most important, as being constructed from the bulletins made during the bombardment by the new Registrar, Dr. Worms, on a new plan. Unfortunately he only once (one week) could get returns from the *army*, &c., or from all the *mobilised* National Guard. These, like the Army Returns, were sent, of course, to the War Office, and will appear in about two years. The accidents from bombardment are only those caused directly by shells or splinters, not by falling in of walls, &c., which were, of course, more numerous.

Drs. Gordon and Wyat will, I have no doubt, be able to furnish fuller details. These gentlemen were accredited to the French Government by our War Office. They had better official opportunities for obtaining information; they have both been made Chevaliers of the Legion of Honour.

(A.)

Specimen of Weekly Return, from which is compiled the annexed Table.

MAIRIE DE PARIS.

DIRECTION DES AFFAIRES MUNICIPALES.

2e Division.—2e Bureau.

Bulletin Hebdomadaire des décès déclarés à l'état civil du 28 janvier au 3 février, 1871.

CAUSES DE DECES.	Population civile d'après le recensement arrêté le 7 janvier 1871: 2,019,877 habitants — AGES				ARMEE.	TOTAUX.
	au-dessous de 1 an.	de 1 an à 15 ans.	de 15 ans à 50 ans.	de 15 ans et au-dessus.	Troupe de ligne et garde mobile.	
Variole	52	54	109	22	21	258
Scarlatine	...	7	6	...	4	17
Rougeole	1	19	1	...	8	29
Fièvre typhoïde	1	58	57	15	193	324
Erysipèle	6	1	2	3	...	12
Bronchite	112	139	96	166	114	627
Pneumonie	31	52	88	138	156	465
Diarrhée	57	38	11	43	1	150
Dysentérie	4	14	10	26	9	63
Cholérine	...	...	...	...	...	...
Angine couenneuse	3	9	3	1	...	16
Croup	2	6	...	...	...	8
Affections puerpérales	...	...	14	...	...	14
Affections chroniques et accidents divers.	447	301	594	916	134	2,392
Accidents de guerre: Combat	...	...	273*	14	...†	287
Bombardement	...	...	9	9	...	18
Totaux	716	695	1,269	1,353	638	4,671

Vu: *l'Inspecteur du Service médical,*
Dr. JULES WORMS.

* Does not include all the mobilised National Guards *outside* of the Paris ramparts.

† Not received.

Observations.—Le détail de ces causes de mort est indiqué dans le *Bulletin de Statistique municipale* publié mensuellement.

(B.)

Tabular Statement,

Compiled from the Weekly Returns given in at the "Mairie de Paris," from the 21st January to 17th February, 1871, inclusive.

(Four Weeks.)

Causes of Death.	Civil Population, according to Census taken 7th Jan. 1871, 2,019,877, including 300,000 National Guards. — Under 1 year.	From 1 year to 15 years.	From 15 years to 60 years.	Above 60 years.	Total.	Percentage of Deaths on Civil Population.	Army. — Line and Mobiles (say) 55,000.	Percent. of Deaths.	Grand Totals, Civil & Army. — Deaths.	Percent. Population & Army.
Smallpox	165	159	503	70	897	·044	87	·158	984	·049
Scarlatina	1	20	10	1	32	·002	5	·009	37	·002
Measles	23	86	2	...	111	·005	22	·040	133	·006
Typhus	2	196	270	47	515	·025	680	1·236	1,195	·059
Erysipelas	11	8	9	9	32	·002	1	·002	33	·002
Bronchitis	394	527	336	646	1,093	·094	404	·735	2,307	·114
Lung Diseases	115	204	361	572	1,252	·062	630	1·141	1,882	·093
Diarrhœa	191	182	46	158	577	·028	9	·016	586	·029
Dysentery	11	38	50	96	195	·009	32	·058	227	·011
Cholera	...	...	3	...	3	...	2	·003	5	...
Diphtheria	5	28	9	7	49	·002	1	·002	50	·002
Croup	21	45	2	...	68	·003	1	·002	69	·003
Puerperal cases	...	...	60	1	61	·003	...	...	61	·003
Chronic affections and Accidents*	1,858	1,304	2,292	3,281	8,735	·434	433	·788	9,168	·454
War cases— Combat	...	2	579	23	†604	·030	171	·311	775	·038
Bombardment	...	12	33	27	72	·004	17	·031	89	·004
Totals	2,797	2,806	4,565	4,938	15,106	·747	2,495	4·532	17,601	·871

* Are detailed in a Monthly Return, which is just now six months *in arrear*. Many were accidents indirectly caused by the bombardment.

† This does not include all the Mobilised National Guard *outside* of the Paris ramparts at sorties, forts, trenches, &c. &c. Like the Army Returns, some were only received during *one* week this month, being sent with them to the War Office, not to the Municipality.

SPECIMEN OF WEEKLY RETURN.

(C.)

MAIRIE DE PARIS.

BULLETIN HEBDOMADAIRE DES DÉCÈS CAUSÉS PAR LES PRINCIPALES MALADIES REGNANTES D'APRÈS LES DÉCLARATIONS A L'ÉTAT CIVIL.

CAUSES DE DECES.	PARIS. POPULATION (1866) 1,820,276h. Du 18 au 24 septemb. 1870.	LONDRES. POPULATION : (1870) 3,214,707h. Du au 187	BRUXELLES. POPULATION : 1er jan. 1870. 176,709h. Du au 187	NEW-YORK. POPULATION : (1869) 1,000,000h. Du au 187	FLORENCE. POPULATION : (31 déc. 1869) 904,001h. Du 28 août au 3 sept. 1870.	Du 4 au 10 sept. 1870.
Variole	158	Les renseignements ne sont pas parvenus.	...	Les renseignements ne sont pas parvenus.	...	...
Scarlatine ...	15		...		...	...
Rougeole ...	6		...		...	...
Fièvre typhoïde	45		...		3	7
Typhus	...		...		...	...
Scorbut	1		...		...	...
Erysipèle ..	3		...		...	...
Bronchite ...	61		...		} 9	2
Pneumonia ...	62		...			
Diarrhée	43		...		...	...
Dysentérie ...	9		...		...	...
Choléra	...		...		...	...
Angine couenneuse* ...	6		...		...	...
Croup	5		...		...	...
Affections puerpérales...	6		...		...	...
Autres causes ...	852		...		76	78
Total ...	1,272		...		88	87

* Diphtheria.

(D.)

TABULAR STATEMENT.—Compiled from the Weekly Returns given in at the Mairie de Paris from 18th September, 1870, to 30th January, 1871, inclusive (18 weeks). A specimen of this first form of Weekly Return is attached. It does not contain the distinction of age, or that of army from civil population, as was afterwards the case.

Weekly Returns.	Small Pox.	Scarlatina.	Measles.	Typhoid Fever.	Typhus.	Scurvy.	Erysipelas.	Bronchitis.	Pneumonia.	Diarrhœa.	Dysentery.	Cholera.	Diphtheria.	Croup.	Puerperal Cases.	Other Causes.	Total Number of Deaths.
1870.																	
Sept. 18 to Sept. 24	158	15	6	45	...	1	3	61	62	43	9	...	6	5	6	852	1272
,, 25 Oct. 1	210	4	5	56	...	...	8	56	46	46	33	1	5	8	10	896	1344
Oct. 1 ,, 8	212	13	16	54	...	...	56	56	60	69	18	2	2	8	5	972	1453
,, 9 ,, 15	311	15	12	51	...	...	11	55	64	72	26	2	2	9	10	964	1610
,, 16 ,, 22	360	7	7	55	...	...	10	70	66	76	33	3	5	5	4	1056	1746
,, 23 ,, 29	378	9	5	62	...	...	6	77	71	89	49	1	9	7	5	1004	1878
,, 30 Nov. 5	380	6	12	61	...	...	11	72	69	87	32	1	9	6	12	1004	1762
Nov. 6 ,, 12	419	7	9	62	...	...	7	92	79	91	39	1	14	5	6	1064	1885
,, 13 ,, 19	431	14	9	94	...	...	12	89	73	93	35	2	5	10	8	1198	2064
,, 20 ,, 26	386	17	11	103	...	...	17	89	81	76	25	1	9	11	11	1074	1907
,, 27 Dec. 3	412	9	21	140	...	...	9	99	92	76	25	1	6	10	8	1115	2023
Dec. 4 ,, 10	311	10	22	173	...	...	7	107	108	83	33	1	8	6	9	1326	2455
,, 11 ,, 17	391	11	23	173	...	...	16	100	131	103	38	2	9	12	15	1615	2728
,, 18 ,, 24	368	11	14	227	...	...	14	172	147	73	30	3	6	11	6	1627	2728
,, 25 ,, 31	454	19	19	250	...	...	10	988	201	98	51	...	13	16	8	1897	3350
1871.																	
Jan. 1 to Jan. 6	339	13	31	251	...	...	9	343	262	151	52	3	19	20	11	2186	3680
,, 7 ,, 13	339	11	40	301	...	...	10	457	390	143	46	3	22	20	11	2189	3862
,, 14 ,, 20	380	8	44	375	...	...	18	598	436	137	43	...	13	27	15	2382	4465
Totals	6236	185	310	2494	..	1	186	3914	2418	1690	586	27	167	189	163	24706	42312
Percentage* ...	317	009	015	125	...	...	009	146	121	815	029	001	008	009	008	1235	2115

* On Estimated Population of 2,000,000.

THE

SURGICAL FUNCTION

OF

THE OMENTUM.

BY

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THE

SURGICAL FUNCTION OF THE OMENTUM.

During my service in India, my attention has been drawn to a series of cases, in which penetrating wounds of the abdominal wall have been followed by protrusion of the omentum. These cases appear to me to suggest important surgical principles connected with the omentum. I have, accordingly, sought the privilege of briefly laying them before this Society, and, in order to make the matter more complete and better fitted to elicit the experience and observations of its members, I have been at some pains to ascertain, by reference to books and periodicals, what has been recorded in surgical literature on the subject, and have, in addition, made a few observations on the dead body with a view to render our conceptions regarding the anatomy, or rather the topographical anatomy of the organ in question, more precise. I shall, without further prelude, proceed to give a short narrative of my cases, and shall then, in the light of the references and dissections which I have made, discuss the general principles to which they seem to point.

The first case of the series came under my own observation while I was civil surgeon of Jessore. On the 21st of May 1866, a lad, seven years of age, was admitted into the charitable dispensary of that station, with a penetrating wound of the abdomen caused by a spiked bamboo. The wound was situated in the left hypochondrium, just below the cartilages of the ribs. A fleshy gangrenous looking mass, 3 inches long and 1 inch broad, protruded from the wound, which constricted the base of it. The injury had been sustained some days before. The protrusion was solid throughout, painless and irreducible, and there was no disturbance of general health nor any symptom of abdominal distress. Part of the mass was removed by incision, but, hæmorrhage occurring from a vessel of some size, a ligature was placed around the base of the protrusion. The remaining portion of it sloughed off, the wound healed by granulation, no abnormal symptoms of any sort supervened, and the patient absconded on the 7th of June, seventeen days after admission. On dissecting the part of the protrusion which had been removed, it was found to consist of

omentum containing small lobulated masses of fat, and matted together by inflammatory adhesion. The membrane could be unravelled and spread out. The case was published by my assistant, Baboo Gopal Chunder Deb, in the August number of the *Indian Medical Gazette*.

In the July number of the same periodical, Surgeon F. Odevaine had published a case of alleged protrusion of the tail of the pancreas through an abdominal wound. The patient, a young man aged eighteen years, of slender make, had received, on the 8th of May, a spear wound in the epigastrium. He came under Surgeon Odevaine's observation on the 24th of May, sixteen days after the injury. A red and white fleshy substance had protruded from the wound. On the patient's admission, it was found to consist of a long pendent fleshy mass emerging through the wound about 2 inches below the ensiform cartilage, measuring 5 inches by 1½, nearly round, but lobulated. No abnormal symptoms, abdominal or otherwise, existed. Surgeon Odevaine concluded that the protrusion was pancreas. It was tied at its base; no bad symptoms followed. The mass separated on the 3d of June, and the patient absconded on the 7th of the same month, thirteen days after the ligature had been placed on the protrusion.

In the September number of the *Gazette*, Surgeon Adam Taylor published a parallel case. A coolie, aged twenty, had stabbed himself midway between the ensiform cartilage and umbilicus, with a *cookree*. A protrusion had taken place, which was found on patient's admission to consist of a thick, firm, solid, dark-red mass, measuring 3 inches by 1½, irregular in shape, surface granular and lacerated in places. The mass was constricted by the wound; a ligature was placed on it on the 29th of July; no bad symptoms existed or arose. The man recovered and was discharged well on the 13th of August, fifteen days after the operation. Surgeon Taylor found the mass to be "solid and glandular, with a fibrous covering dipping into its lobules," and concluded that it was not omentum, and was pancreas.

These three cases, occurring about the same time, and reported in the same journal, raised the questions whether the protrusion observed in them was of the same character, and whether it was possible that the pancreas could issue to the extent described, or indeed could issue at all, through an abdominal wound of the size and situation noted. On these questions I published a short paper in the September number of the *Gazette*, in which I endeavoured to demonstrate, as the result of special dissections and manipulations on the dead body, that the anatomical position and attachments of the pancreas were such as to render protrusion of the organ through a wound in the anterior wall of the abdomen a physical impossibility. There was no difficulty in obtaining a protrusion of the omentum through wounds made at the sites of those described, and to the extent observed in these three cases;

but, even by traction, the tail of the pancreas could not be dislodged and pulled out so as to imitate the appearances described by Surgeons Odevaine and Taylor. I observed, moreover, that in putrid bodies, where the gases contained in the abdominal cavity supplied a strong *vis-a-tergo*, a wound placed above the umbilicus emitted, in the first place, a jet of gas; secondly, omentum; and thirdly, if large enough, intestine. From these considerations a strong opinion was formed that the protrusion in all the cases was omentum, so altered by inflammatory changes as to simulate pancreas, an appearance which Mr Syme states, in his *Principles of Surgery*, the membrane assumes in cases of incarcerated epiplocele.

Subsequent numbers of the *Indian Medical Gazette* contain three additional cases of the same kind. One is by Surgeon-Major W. B. Beatson, civil surgeon of Dacca. The patient, a man aged thirty-six, had sustained, ten days previous to his admission into the Mitford Hospital, a bullet-wound on the left side of the thorax. The exact site is not described, but it is stated that it was plugged by omentum—"a fleshy-looking pale pink mass, about the size of the forefinger, strongly resembling pancreas." It was ligatured and cut off. The man suffered from pleurisy, pericarditis, and pneumonia, and made a protracted recovery, but the wound quickly healed without any untoward symptom. Dr Beatson, in commenting on the case, strongly held that the pancreas, from its relations and connexions, could not be protruded through such a wound, and that the omentum, when congested, inflamed and matted together, may simulate pancreas.

Another good case in point is communicated by Dr Greene, civil medical officer of Serampore, in July 1871. A woman, aged sixty, was gored by a cow five days before admission to hospital. The wound measured 5 inches by 2, and extended from the crest of the left ilium downwards. Omentum protruded, measuring 5 inches long, 3 inches broad, and 1½ inches thick. It was flattened, had the shape of the tongue, looked exactly like a bit of pancreas, was of a brown colour, fœtid, and adhered to the lips of the wound. There was no pain or fever, and the bowels acted normally. A ligature was applied. The mass separated, the wound healed kindly, and the old woman made a good recovery. Dr Greene remarks that, had the wound been in the epigastrium, he would have believed that the protruded mass was pancreas.

The third case is from the station of Oraie, in the North-Western Provinces, and occurred in September 1870. It is reported in the *Indian Medical Gazette* for September 1871. A girl, aged twelve, was gored by a cow four hours before admission into hospital. The wound was in the region of the liver. Two inches of omentum protruded. Emphysema of the right side of the chest existed, and air was observed to escape from the wound. Respiration was reported to be difficult, and the pulse excited. The omental protrusion could not be reduced. It was dressed with carbolic acid. In fourteen days all

tenderness and emphysema had disappeared, the mass was ligatured and removed, and the patient made a good recovery, being discharged well in a month and four days from the date of admission.

Two other cases of a similar kind are recorded in a small work entitled *Medico-legal Experience in Bengal*, which I prepared in 1870, founded upon a collection of returns submitted to the office of the Inspector-General of Hospitals. One of these occurred in the station of Goruckpore, in the North-Western Provinces, in 1869. A suicidal abdominal wound, with protrusion of the omentum, resulted in recovery. The other was reported in October 1868 by Surgeon O'Connell Raye, from Nursingpore, in the Central Provinces. A male, aged fifty, stabbed himself with a sword in the belly. There was a penetrating wound of the abdomen to the right of the median line, and 2½ to 3 inches long, above the umbilicus; a piece of semi-gangrenous omentum, 5 inches long or thereabout, about 3 inches wide and 1½ to 2 inches thick, protruded through the wound. The man stated he had stabbed himself three days previously; the piece of omentum was ligatured and removed by a knife, and the wound in the abdomen closed. The patient ran away from hospital a few days afterwards, and was not subsequently heard of.

These eight cases illustrate well the main position which I desire to establish, namely, that, in penetrating wounds of the abdomen of small size, the omentum is prone to protrude, and, protruding, acts as a plug which stops up the wound and prevents the protrusion of the other viscera contained in the cavity. In so doing, the organ subserves a most important surgical purpose. The cases further indicate what all the evidence which I have collected fully supports, that lesions of this description do not seriously imperil life. All the cases, it will be observed, did well. Among fifty similar cases which I have collected from various sources, and tabulated in the sheets before me, there was only one fatal case, and the fatal issue in that one would appear to have arisen mainly from other causes. The list of cases, though a pretty long one, and sufficiently so to show that the event is not an uncommon one, is by no means presented as an exhaustive list, and is subject to the very necessary reservation that it probably represents a higher rate of recoveries than if it contained every case of the sort which has ever occurred; for men are prone, and particularly so in the instance of abdominal injuries, to record cases whose issue has been favourable, leaving others which have ended unfavourably unrecorded. I have taken the cases as I found them, and as many of them as I have been able to collect,[1] and by way of comment upon them I shall offer a few remarks on the following points:—1st, The circumstances, as regards the site, size, and cause of the

[1] For the majority of these cases I am indebted to the industry of Assistant-Surgeon G. A. Otis, of the U. S. A., who has recorded abstracts of a large number of them in the *Medical and Surgical History of the War of the Rebellion*, a magnificent compilation, full of most valuable material.

wounds, under which the protrusion occurs. 2d, The topographical anatomy of the omentum, its size and position in the abdomen, and the extent to which it covers the abdominal viscera—conditions upon which the event in question must necessarily depend. 3d, The natural history of the occurrence, or what happens when no surgical interference is adopted. 4th, The treatment of the lesion with special reference to the principles of antiseptic surgery, and whether the considerations adduced warrant any modification of existing practice.

I. The situation of the wounds through which omental protrusions took place, is more or less precisely specified in 39 out of the 57 cases. In 17 of the 39, the wound was situated near the umbilicus. In seven cases it was above the level of the umbilicus. Of these, in four the left hypochondrium was the part pierced, in one the left side of the thorax, in one the space between the ninth and tenth ribs, and in one "the region of the liver." In six cases the sides of the abdomen were wounded. In one the side is not specified, one was on the right side, one on the left, one just above the crest of the right ilium, and two above that of the left. Nine wounds were situated in the hypogastric and inguinal regions—three on the right side and six on the left. It thus appears that, while omental protrusions are liable to take place from any point in the anterior and lateral walls of the abdomen, and even through wounds of the lower chest-wall, they are more liable to occur in the umbilical region, more common below than above the umbilicus, and more frequent on the left than on the right side.

These conclusions are generally confirmed by recorded experience in the case of hernial protrusions; umbilical herniæ most frequently containing omentum, inguinal next, and crural herniæ least. The relation of these facts to the anatomy of the organ will be presently alluded to. I am unable to indicate in what proportion of wounds in the several regions of the abdomen omental protrusions occur. An inquiry of this kind would involve a study of a large number of abdominal wounds of every kind indiscriminately, an induction for which I have neither had the opportunity nor time, but it would obviously be of considerable interest and of some practical importance.

The information regarding the manner in which the wounds were sustained is pretty complete. In 5 cases the point has not been recorded. Of the remaining 52, 24 were wounds of various sorts, 16 stabs, 7 gunshot wounds, and 5 gores by horned animals. Observers are agreed that protrusions of the abdominal viscera are more apt to take place in wounds and stabs than in gunshot injuries, and the statement just made accords with this view. Of the 24 wounds, 6 are described as incised, penetrating, or punctured, 5 were caused by knife, 4 by lance or sword, 3 by sabre or spear, 2 by pieces of glass or crockery, and 1 by the blade of a scissors, a pointed bamboo, a *cookree*, and a spike of iron-railing severally. The size of the wound is rarely described, but probably

an inch, or an inch and a half, would be about the average. The extent of protrusion is given in 22 cases,[1] but the data do not lead to any useful conclusion. The length of the mass varied from 1 to 6 inches. Some of the longer protrusions came through the lower abdominal wall, and some of the shorter through wounds of the umbilical region. The amount of the membrane escaping most probably depends upon other circumstances than the mere situation of the wound—on the anatomical condition of the organ, on the size of the wound, and perhaps on the amount of exertion taken after its infliction.

These data refer to wounds of the abdominal parietes; but the conservative property of the omentum, which I am endeavouring to illustrate, is not confined to these. In three of the cases, the diaphragm was probably wounded and plugged, and I can conceive it possible that a punctured wound of the left half of the diaphragm might be stopped up by omentum, though I have not met with a case in which this was demonstrated to have taken place. In cases of phrenic hernia, in which the viscera have found their way into the cavity of the chest through wounds and dilated natural orifices, the omentum has been found along with other organs, and has contracted adhesions to the orifice of the aperture.[2] Wounds of the intestines are sometimes plugged by omentum. Dr Gross, in his *System of Surgery*, 5th ed., p. 664, states that, "in some in-

[1] Size of Omental Protrusions :—

Description of Protrusion.	Site of Wound.
Size of an orange,	Side of abdomen.
4 inches long,	Above crest of left ilium.
Size of a small orange,	Near and below umbilicus.
Size of a walnut,	Not stated.
3 inches long,	Left side of abdomen.
6 " "	Below umbilicus.
4 " "	Left iliac region.
3 " "	Between ninth and tenth ribs.
3 " "	Left of umbilicus.
3 " "	Right of umbilicus.
1 " "	Left of umbilicus.
3 " "	Left of hypochondrium.
5 " "	Epigastrium.
3 " "	Above umbilicus.
Size of the forefinger,	Left side of thorax.
5 inches long,	Left iliac region.
2 " "	Region of liver.
5 " "	Right of umbilicus.
2 " "	Below navel.
Size of palm of hand,	Left groin.
6 inches long,	Left iliac region.
6 " "	Above crest of left ilium.

[2] A case of this kind is related by Dr A. Taylor in Guy's Hospital Reports, vol. iii., for 1838. A case of strangulated hernia through the œsophageal opening of the diaphragm is given in part ii. of the *Medical and Surgical History of the War of the Rebellion*, p. 25, in which part of the great omentum was found in the thorax. See also several cases in Sir Astley Cooper's standard work on *The Anatomy and Surgical Treatment of Abdominal Hernia*.

stances of wounds of the intestines, the breach is closed by a piece of omentum, which, projecting into it, fills it up like a tampon. When this occurs, the contiguous serous surfaces become firmly adherent to each other, and that portion of the plug which lies within the bowel, and assists in maintaining its continuity, is eventually absorbed,—a circumstance which leads to the gradual approximation of the lips of the wound, and their ultimate reunion." Poland, in an article "on Contusions of the Abdomen" in vol. iv. of the third series of Guy's Hospital Reports (p. 152) quotes a case by Jobert. A man, aged 22, was injured by the wheel of a carriage passing over his abdomen. This was followed by no uneasiness nor tympanitis. He progressed favourably, and was almost convalescent, when, after being two months in hospital, sudden hæmoptysis took place and death. On post-mortem examination, a rupture of intestine was found, which was filled up by a plug of omentum projecting half an inch into the bowel.

In more extensive solutions of the continuity of the intestinal tube, the omentum has been found, by adhering to the extremities of the injured gut, to prevent extravasation of fæces into the cavity of the peritoneum, and assist materially in restoring the continuity of the tube. Travers, in his classical work on the *Process of Nature in repairing Injuries of the Intestines*,[1] gives several illustrations of this event. He found that intestinal wounds in dogs were frequently closed up by omentum,—an event which Dr Gross remarks (*op. cit.*, p. 665) occurs more frequently in these animals than in man.[2] Finally, there is in the *Medical and Surgical History of the War of the Rebellion* (page 171), a most remarkable case in which a bullet made its way into the abdominal cavity, and was held by a pouch of omentum as if by a purse. It thus appears that the omentum fulfils beneficial conservative or curative purposes in other circumstances besides those with which I am more particularly concerned at present.

II. Without entering at any great length on the anatomy of the omentum, there are some points connected with its structure and position which bear so directly upon the subject in hand that they merit prominent attention. The omentum is usually represented in anatomical plates, such as those of Quain and Sibson, as an apron hanging down from the greater curvature of the stomach, thinly veiling the transverse colon, and below it concealing the small intestines from view, loaded with fat, traversed by large vessels, and ending inferiorly in a crenated, scolloped or fimbriated margin. The sketch which I exhibit, and which was given me by Mr Chiene as a true picture of a normal omentum, bears out this description exactly. But, in reality, the organ varies immensely in its

[1] Pages 58, 95, 99, 114, 118, 343, 346, 347.

[2] The omentum similarly seals up perforated intestinal ulcers; see a very remarkable case related by Travers at page 43 of the work quoted. In No. 8 of the tabulated post-mortem examinations, omental adhesions to the peritoneal aspect of perforating tubercular ulcers were observed.

size and character, and in its position in the abdominal cavity. This I have had amply illustrated by the observation of 20 post-mortem examinations in which I was kindly allowed by Dr Wyllie to take notice of the state of the omentum, the appearances in which I have roughly tabulated. Before alluding to this table, I would direct the attention of the Society to the tinted tracings which I exhibit of Braune's sections of frozen bodies.[1] These display very accurately and instructively the topographical anatomy of the normal omentum—its size and position. The sketches are six in number (Nos. I., II., XVI., XVII., XVIII., and XIX. of Braune's plates). Two are vertical sections in the mesial plane, one of a healthy man of 21, and the other of a healthy woman of 25, both of whom had committed suicide by hanging. The remaining four are transverse sections of the abdomen at different levels, the subject being a healthy young man of 22.

It will be observed from these sections[2] that the omentum forms a complete and substantial layer interposed between the anterior wall of the abdomen and the small intestine below the level of the umbilicus, and, above that point, more thinly covers the transverse colon and its left flexure. It is further apparent that the extent of the organ corresponds generally with the comparatively unprotected part of the abdominal parietes. It is wanting where the cavity is protected by thick walls of bone and muscle. It is also evident that, while the anterior surface is flatly applied to the inside of the anterior wall of the abdomen, the posterior surface applies itself to and fits into the inequalities of the intestinal surface. This circumstance becomes very apparent from an inspection of the front and back of a thick omentum, the former being smooth, flat, and glazed, and the latter uneven and lobulated. The intestines, in their varying states and motions, must present very considerable variations of their general surface, and it would seem as if one use of the omentum were to adapt itself as a yielding surface to these variations; for the cavity of the abdomen is normally fully occupied with its contents, and there is really no such thing as a cavity of the peritoneum. The sections finally show that the omentum extends rather farther to the left than to the right. Now, reverting to the particulars already given regarding the situation where omental protrusions are most liable to take place, it is so clear that these tally with the normal anatomy of the organ, that it is unnecessary to dwell on the subject in detail. The protrusions which take place in the left hypochondriac region are dependent on the fact that the omentum is more free and voluminous on the left side, and often hangs down from the fundus of the stomach, having little or no connexion with the colon. Indeed, there is often a supplementary fold, or several of them, in this situation.

[1] *An Atlas of Topographical Anatomy*, by Wilhelm Braune.
[2] It has been considered unnecessary to reproduce these sketches.

The observations detailed in the table which I have constructed, show that the descriptions and representations of the normal omentum, just alluded to, are subject to great modification. The subjects were mostly bed-ridden persons who had died of lingering and often wasting diseases, and it is possible that a prolonged recumbent position may account for the crumpling or tucking up of the omentum observed in so many of them. Still, it is right to indicate briefly what the observed departures from the normal actually were. The organ itself varies greatly in its length and breadth. Measured from the lower border of the transverse colon, its depth may vary in adults, from 2 or 3 inches to a foot, or a foot and a quarter, and, while some omenta cannot by pulling be brought do town within 2 or 3 inches of the symphysis pubis, others can be drawn considerably beyond it. This circumstance would satisfactorily account for the varying length of protrusions, even if other causes of difference were absent. Then the breadth differs, though not so widely, and the points at which the organ commences and ends on the colon. Most frequently the omentum is not symmetrical. It is usually deeper and more voluminous on the left than on the right side, and extends further along the left than the right flexure of the colon. The amount of fat contained in it differs much in different bodies. As age advances the tendency to deposit is greater. A really thick omentum would be a better protection to the hollow organs than a membranous one, and would be less likely to protrude through a small wound. Omenta also vary in the place and degree of adhesion to the transverse colon, and of the adhesion of its component layers to each other, and in the capacity of the omental sac. The distance between the stomach and colon varies, and the mobility of the colon and the thickness of membrane interposed between it and the parietal peritoneum. The colon is often more free on the left side than on the right. Again, the position of the omentum in the abdominal cavity depends on a number of circumstances independently of its anatomy—on age (the omenta of children being relatively scanty and thin); on the size of the liver (enlargement favouring its lower level in the abdomen); on the state of distension of the stomach and colon (dilatation of either shortening it); on the condition of the small intestines (distension tending to push it up); on the existence of fluid in the peritoneal cavity (ascites causing floating of the intestine and pushing up the omentum); on peritonitis and its consequences; on abdominal tumours and pregnancy.

The lower border of the omentum is most frequently scolloped or fimbriated, but the degree of this condition varies considerably. It almost looks as if the organ were intended, by means of these fimbriæ, to present a series of perpetual feelers in the abdomen in constant watch for mischief. This idea is countenanced by the fact, that it is these fimbriæ which most frequently contract adhesion to ulcers and tumours, and form by their elongation the omental bands which are so well known and in some cases so in-

convenient. I have endeavoured in the table to indicate, by measurements from fixed points, the position of the stomach and transverse colon, and the level to which the omentum hangs in the abdomen. I have also briefly described what viscera were seen in each case on opening the abdomen. Without entering on a detailed analysis, suffice it to state that, in only one case did the lower border of the omentum reach the symphysis pubis. In the rest, the distance of this margin above the symphysis pubis was considerable, as much as 9 inches in two cases and 12 inches in another. The small intestine was, accordingly, in very few cases entirely covered by omentum. It was partially covered in most cases, but entirely uncovered in a considerable minority. The omentum was, in extreme cases, crumpled or tucked up above the level of the umbilicus; but it could, in almost every case, be pulled down to the symphysis pubis, near it or over it.

The practical upshot of all this anatomical detail is, that the condition of the omentum and its position in the abdominal cavity are extremely variable, and that it is impossible to predict what either or both may be in any given case. This fact of variability and its practical consequences are, however, things to be held in memory.

III. With respect to the natural history of omental protrusions, the first point worthy of note is, that they appear always to take place immediately after the injury has occurred. Travers, in the work which I have quoted (note, page 97), alludes to the frequency of omental protrusions being a serious impediment in making experimental observations on wounds of the intestines. The degree of efficacy with which the protrusion acts as a plug may be inferred from a case recorded by Baron Larrey (No. 6 in the table), in which bleeding from the epigastric artery was completely controlled by the omental tumour, on whose reduction the artery bled and had to be tied. An interesting case, illustrating the same point, has been communicated to me by Dr R. Black, of Greenock. A sailor had been stabbed in the epigastrium, and was brought to hospital soon after with an omental protrusion, which was apparently reduced, and the wound stitched. He died next day. Bleeding from the wound had been slight, and none took place after his admission. On dissection, the omentum was found still plugging up the deep part of the wound. The stomach, duodenum, and mesentery, had been perforated, and a large branch of the mesenteric artery divided. Forty ounces of blood were found in the cavity of the abdomen. The lesson taught by this case is, that, if the omentum can prevent the issue of blood from the abdominal cavity, how much more efficiently can it prevent the exit of the intestine. Though the cases in which the prevention of intestinal hernia by the omentum has been observed have been almost all cases of stab or small wound, it would appear that, even in large wounds, under favourable circumstances, the organ is capable of exercising this function. In Dr Greene's case of gore related

above (No. 43 in the table), the wound was a very large one. A still more remarkable case is related by Hennen in his work on military surgery (page 453). It is that of an officer "almost the whole of the anterior part of whose abdominal parietes had been blown off by a shell, with the exception of the peritoneum, though sorely lacerated and deprived of the muscles. Where the umbilicus had been there was a large rent through which the omentum protruded, though not to a great extent, and scarcely above the surface. Spots of the stomach and of the arch of the colon were visible through smaller rents, and, what was remarkable, no part of the intestines protruded through these openings," probably because the omentum, as it were, buttoned them in by its protrusion further down. The efficiency of the omentum as a plug is still further proved by the amount of exertion which patients have taken after sustaining an injury of this sort. In Mr Key's case (No. 4 in the table), the patient walked a quarter of a mile, and in Mr Nunneley's case (No. 33 in the table), three miles, after being wounded; and in the case of an American officer, recorded by Assistant-Surgeon Sternberg (No. 36 in the table), the patient had travelled 100 miles in a cart over a very rough road. What would have happened in these cases, had the protrusion been small intestine in place of omentum, is probably that the whole of the tube would have gradually found its way outside, as occurred in a case of gore in my own experience. The changes which take place if the protrusion is not returned into the abdomen, are as follows:— Inflammation is set up in the protruded mass, which does not appear to extend beyond the neck of the flask or tongue-shaped tumour. Here adhesions form to both lips of the peritoneal wound, and so the occlusion or sealing up of the abdominal cavity is permanently secured. The result, as regards the portion of the tumour lying outside the wound, depends on the degree of constriction to which it is subjected at its neck. If this is very tight, the mass sloughs off; if less so, partial sloughing takes place; and if the constriction is not sufficiently severe to deprive the mass of vitality, the omentum becomes solidly matted together by inflammatory effusion of a plastic kind, resembling, as the cases already cited prove, pancreas; granulations spring up on the surface, which become covered with pus; the tumour gradually wastes away and appears to be retracted within the abdomen; and finally, after a lapse of about 50 days, the wound contracts and cicatrizes. Surgeon-Major Williamson, in his work on *Military Surgery*, gives an instructive case, in which an opportunity occurred of observing the state of matters five and a half years after a soldier had sustained a penetrating abdominal wound. The man was shot through the abdomen at the battle of Ferozeshah, on the 22d of December 1845. Very slight symptoms followed, so that it was supposed that the ball had coursed round the cavity and had not penetrated. He mentioned having passed some blood in his stools after receiving his wound. The ball had escaped near the spine, having

entered in front. He recovered slowly but perfectly, except that he continued subject to bowel complaints. He finally died of cholera on the 13th of May 1851. The details of the post-mortem examination, as recorded by Dr Taylor, are as follows:—"Cicatrix of a gunshot wound in the left linea semilunaris, about four inches above the crest of the ilium, and on the same plane posteriorly, another cicatrix an inch to the left of the spine. Omentum firmly adherent to the internal surface of anterior cicatrix, and gathered into a fold or knot." The intestines were not adherent. The report goes on to describe certain interesting appearances in the intestines, which do not concern us here.[1] The case is a typical illustration of the conservative or protective function fulfilled by an unprotruded omentum. I have tabulated four cases in which omental protrusions were left to themselves. The first formed the subject of a memoir, presented by M. Hippolite Larrey to the Royal Academy of Medicine of Paris in 1845. Patient had sustained a penetrating wound of the abdomen, from which an omental protrusion had taken place. This could not be returned. It was covered with a poultice. Considerable inflammation followed for the first day or two. On the 4th, a suppurating surface was established; on the 10th, the protrusion began to be withdrawn; and, on the 36th, it was reduced to the level of the skin. In 46 days the patient was cured. The case is quoted by Guthrie in his excellent lectures on *Wounds and Injuries of the Abdomen*, where he gives a parallel case of his own. A Spanish soldier had been wounded by a musket-ball passing through the abdomen from side to side. He was seen four days after the injury. A mass of omentum as large as an orange protruded from one of the orifices. "The protruded omentum gradually diminished in size, and was at last drawn into the wound in the abdomen and covered by granulations." The third case by Baron Larrey is precisely similar. The fourth is related at length by Mr Aston Key in the first volume of Guy's Hospital Reports (for 1836). The tumour partly sloughed off and the remainder gradually wasted away and disappeared. Complete recovery took place in 42 days. Irritation of the stomach (vomiting) was a prominent symptom in this case. The omentum was found to be quite insensible, and no pain was felt when the protrusion was pricked or squeezed. As a natural consequence of omental protrusion, dragging down of the viscera has been observed, as in some cases of hernia related by Travers.[2] Such a condition might occasion intestinal disturbance, and several authors allude to uneasy feelings caused by this dragging. Indeed, Ravaton goes so far as to aver that omental lesions may be diagnosed by the peculiar sensations experienced by the patient. These sensations most probably depend on the traction to which the other viscera are subjected, and this traction and its effects

[1] This case is quoted by Macleod in his work on *Surgery of the Crimean War*, from which I have taken the description of it.

[2] *Op. cit.*, page 363.

are worthy of being borne in mind in the management of such cases.

IV. The treatment of omental protrusions depends mainly upon the time which has elapsed since the protrusion took place; but the nature and position of the wound, the amount and condition of the protruded mass, the degree of constriction to which it is subjected, and the symptoms which have arisen, are also points of importance. The cases practically arrange themselves into seven classes.

1. When the omentum is sound, the injury recent, and the protrusion reducible.

2. When the omentum is sound, the injury recent, but the protrusion cannot be reduced by manipulation.

3. When the injury is recent but the omentum is much bruised, lacerated, or very ragged in consequence.

4. When the omentum is, in a comparatively recent case, congested or strangulated.

5. When the omentum has become inflamed and matted, its neck being adherent to the lips of the peritoneal wound.

6. When the protruded mass has undergone suppuration.

7. When the tumour has become gangrenous in part or in whole.

On each of these heads I shall offer a few remarks; but, before doing so, I would point out that, in the table which I have drawn up, I have arranged the 57 cases in four categories, according to the treatment which has been adopted, namely—A, cases in which nothing was done; B, cases in which the protrusion was reduced; C, cases in which it was cut off; and D, cases in which it was ligatured.

1. In recent and reducible cases where the omentum is uninjured, surgical authorities[1] are agreed that the hernia should be reduced after washing, if necessary, and the wound carefully stitched up. Guthrie (*op. cit.*, page 12) recommends that the omentum when reduced should be left between the edges of the peritoneal wound, "in order," to quote his words, "that by its retention it may more readily adhere to these edges, and thus form a more certain barrier against the extension of inflammation than is likely to take place by some accidental contact, when moving at liberty in the cavity of the abdomen, however closely it may be supposed to be applied to the inner surface of its paries." Erichsen lays down a similar instruction. Dupuytren is the only surgical authority of eminence who disapproved of reduction, because, he urged, the handling necessary in applying the taxis might inflame the organ. *A fortiori*, he discountenanced reduction under any other circumstances, fearing hernia from enlarging the wound, bleeding from retrenching the organ, and constriction from ligature. There can be no doubt that leaving the protrusion as it is, even when recent and reducible, is a perfectly safe and successful practice. Assistant-Surgeon J. S. Billings of the United States Army,

[1] Boyer, Ravaton, Larrey, Samuel Cooper, Guthrie, Ballingall, Syme, Chelius, Gross, Hamilton, Erichsen, Pollock, Otis, Bryant.

submit, the only justifiable modes of interference in such cases.

5. When the protrusion has become pancreatised (so to speak) by inflammation, and adhesions have united its layers together, and its neck to the lips of the wound, it may either be left alone or removed. If left alone the case is more tedious. Removal by knife or string, or both, are safe enough, but should be done antiseptically. The mass has now become practically extraperitoneal, and interference is not so dangerous as in recent cases. Perhaps, if it is decided to remove the mass, the knife or ecraseur is preferable to the ligature, bleeding vessels being of course carefully secured.

5. When suppuration has taken place in the hernial mass, incision is obviously indicated. The case should, in fact, be treated as any other abscess, or, if adhesions have formed and the matter is outside of the abdominal cavity, removal of the whole mass may be practised.

6. When the protrusion is gangrenous in part or in whole, if adhesions have cut it off from the abdominal cavity, it may be either left to nature, or removed by knife, ecraseur, or ligature. If there is any doubt as to adhesions having formed, the propriety of interference is more than doubtful. If removal is practised, antiseptic precautions should be adopted.

From all the evidence and considerations which I have now adduced, I think that the protective function of the omentum in penetrating abdominal wounds, and to a more limited extent in intestinal lesions, has been fully established. I would submit that, in cases of wounds perforating the abdominal parietes, surgeons would act wisely in not interfering too rashly with the beneficent operation of this organ, or even in endeavouring, if practicable, to interpose between the hollow viscera and the parietal wound, that organ which normally covers them, and seems intended to shield them from the baneful effects of extrusion or inflammation. The mortality of penetrating abdominal wounds is so great that any measure giving promise of reducing it is worthy of very special attention. The subject of omental protrusions has attracted more notice in France and America than in this country, and the fact that it is entirely omitted in many British text-books of surgery, would, if the interest of the matter did not of itself do so, justify me in entering into it in such detail.

APPENDIX No. I.—TABLE OF CASES.

A.—*Cases in which nothing was done.*

No.	Sex.	Age	Nature of Injury.	Site and Character of Wound.	Protrusion.	Treatment.	Result.	Reporter.	Reference.
1	?	?	Penetrating wound.	Abdomen.	Omentum.	Poulticed.	Protrusion gradually drawn into abdomen. Patient recovered in 46 days.	H. Larrey.	Guthrie on Wounds and Injuries of the Abdomen, p. 13.
2	M.	Ad.	Bullet wound.	Side of abdomen.	Omentum, size of an orange, 4 days out.	Left alone.	The protruded omentum gradually diminished in size, and was at last drawn into the wound in the abdomen and covered with granulations.	Guthrie.	Do., p. 13.
3	M.	26	Sabre wound.	Not stated.	Omentum formed a tumour.	Left alone.	Gradually diminished in size, and returned within abdomen.	Larrey.	Mem. de Chir. Mil. et Camp., 1817, t. iv. p. 436.
4	M.	18	Stab. Walked ½ mile immediately after with little difficulty.	Left side, immediately above the centre of the crest of the ilium.	4 inches of omentum.	Left alone.	Part sloughed off in 8 days. Tumour gradually wasted and disappeared. Recovered in 42 days.	Key.	Guy's Hospital Reports, vol. i. 1836, p. 580.

B.—*Cases in which the Protrusion was reduced.*

No.	Sex.	Age	Nature of Injury.	Site and Character of Wound.	Protrusion.	Treatment.	Result.	Reporter.	Reference.
5	M.	17	Stab, 5½ inches long.	Immediately above umbilicus.	Omentum.	Protrusion reduced, left lying against wound of peritoneum. Wound sutured.	Recovered in a week.	Guthrie.	Wounds and Injuries of the Abdomen, p. 12.
6	M.	Ad.	Lance wound.	Right side and lower part of belly.	Omentum.	Reduced; on which epigastric artery bled freely and was ligatured. Wound sutured.	Recovered.	Do.	Do., p. 13.

in a report on the treatment of diseases and injuries in the army of the Potomac in 1864 (*Med. and Surg. Hist. of the War of the Rebellion*, part i. page 202), writes:—"When the protrusion consists of a small knuckle of omentum only, some medical officers prefer to leave it *in situ* to act as a natural plug for the wound." As a field practice this was safe and convenient, but the cure is tedious in such cases, and recorded cases show that reduction is devoid of danger, and procures a speedier recovery. The cases which I have tabulated amply prove this. The only risk of the procedure is that of introducing septic matters into the peritoneal cavity; and, to avoid this, I would thoroughly wash the hernia with an antiseptic solution (warmed) before putting it back, and treat the abdominal wound strictly according to the principles of antiseptic surgery. Ablation and ligature of the mass, though both plans have been practised without injury, are unnecessary, and cases have been recorded in which death has been the apparent result of cutting off or tying recent protrusions. The very strong objection urged against these practices by Ravaton and Dupuytren are still entitled to respect.

2. As regards recent irreducible cases, authorities are not so unanimous. The obstacle to reduction is the constriction caused by the wound, and the practical question comes to be, Should the wound be enlarged to permit of reduction or not? Reduction is not, it will be observed, so imperatively necessary as in the case of the hollow viscera, or even of the other solid organs. Boyer, Erichsen, and Pollock ("On Injuries of the Abdomen," in Holmes's *System of Surgery*) reply in the affirmative, and Robert, Dupuytren, Larrey, Chelius, and Bryant, in the negative. Guthrie points out that the obstacle to reduction is in the wound of the skin and aponeurosis, and not in that of the peritoneum, and he advises enlargement of the former, proceeding afterwards as in reducible cases. The risk of enlarging the wound is, of course, hernia. Against this may be placed the painful symptoms that may be caused by strangulation, for whose relief some surgeons counsel enlargement of the wound without subsequent reduction. Again, some authors make the size and amount of the protruded mass a ground of difference in dealing with it. A small protrusion does not take so long to disappear when left to itself as a large one, and in the latter there is greater risk of a bit of intestine being enclosed, or of uneasy sensations or functional disturbance of stomach or bowels being caused by dragging. Pollock would remove by double ligature and excision a small protrusion, but would widen the wound and reduce a large one. Chelius, following Larrey, would leave a small hernia alone, but return a large one if possible. Hamilton (Dr F. H.) would excise a very large protrusion, tying the vessels if they bled; and Cooper (Samuel) would free, and, if possible, reduce a large hernia where stomach symptoms existed. Although enlargement of the wound and reduction have been practised with success, I should incline in this class of cases

to leave the protrusion alone, unless suspicion of a piece of bowel being included existed, or uneasy symptoms appeared, in which circumstances relief of the constriction or enlargement of the wound and reduction might be advisable. Guthrie's plan seems a sensible one, and he gives cases demonstrating its success. Both ablation and ligature, separately and combined, have been put in practice, and have resulted in cure; but Bryant's advice to wait for two weeks until adhesions shall have formed at the neck of the tumour, and then remove by single or double ligature or excision, or by both methods combined, commends itself as judicious and consistent with what we know of the pathology of this condition. If reduction is practised, antiseptic precautions are certainly indicated; and, even if Bryant's plan be followed, their adoption would, to say the least, do no harm. A prolonged and violent taxis would in any case be obviously improper and unsafe.

3. When the omentum is bruised or lacerated, its reduction in this condition would obviously be wrong, and the question remains whether it should be left as it is, the torn and bruised parts removed, and the rest reduced, or the whole excised with or without previous ligature, or simply ligatured at the level of the wound. Each plan has been practised successfully. Chelius, Bryant, and Gross advise ablation, bleeding vessels being secured if necessary. Dupuytren, Guthrie, Hamilton, and Otis recommend its being left alone, and Pollock advises removal after single ligature of the base, or transfixion and double ligature. Deliberate strangulation by string of a recent omental protrusion is not good practice. Pipelet gives instances of mischief caused by it; and Travers and Sir Astley Cooper, both high authorities, disapprove of the practice in the closely analogous case of epiplocele. Ablation by knife, though a sounder practice, seems unnecessary; and, on the whole, I should either, with Larrey, Guthrie, and others, leave the case to nature, clipping away ragged pieces if needful, or, with Bryant, wait till adhesions had formed, and then ligature at the neck, and excise the mass beyond under the antiseptic system.

4. In a congested or strangulated recent protrusion, the alternatives are—(*a*) to leave the case to nature; (*b*) to relieve the stricture; or (*c*) to excise or ligature. Returning a congested or semi-strangulated omentum into the abdominal cavity is out of the question. Relieving the constriction by enlarging the wound would only be necessary or advisable when symptoms existed demanding such relief. Incising the congested mass might give ease, and perhaps prevent extension of inflammation inwards. The same objection obtains to ablation or deligation, as in any other recent case where adhesions have not so far placed the mass outside the peritoneum. Syme, in condemning the practice of ligature in the parallel case of hernia, pithily remarks, that it would amount to relieving the patient from the effects of one stricture and exposing him to those of another still tighter. Relief of the stricture or removal after adhesion had formed, are, I would

B.—*Cases in which the Protrusion was reduced*—continued.

No.	Sex.	Age	Nature of Injury.	Site and Character of Wound.	Protrusion.	Treatment.	Result.	Reporter.	Reference.
7	M.	Ad.	Stab.	Right side of abdomen, near and below umbilicus.	Portion of omentum, size of a small orange.	Reduced after enlarging wound of skin and aponeurosis.	Recovered.	Guthrie.	Wounds and Injuries of the Abdomen, p. 12.
8	M.	Ad.	Not stated.	Not stated.	Omentum.	"Gradually returned."	Recovered in a few months.	Larrey.	Mem. de Chir. Mil. et Camp., 1817, t. iv. 278.
9	?	?	Not stated.	Size of a small crow-quill.	Omentum, size of a walnut.	Reduced.	Recovered.	Syme.	Principles of Surgery. 5th ed. p. 327.
10	M.	Ad.	Shot.	Abdomen.	Omentum protruded from both wounds.	Reduced.	Recovered.	Baudens.	Clin. des Plaies d'Armes a feu, 1836.
11	M.	5	Fell on a glass shade.	Abdomen.	Omentum and large intestine.	Reduced.	Recovered.	Fayrer.	Clinical and Pathological Observations, p. 376.
12	M.	21	Punctured wound.	Right side of abdomen.	Omentum.	Wound enlarged and omentum returned.	Recovered.	Earle.	London Medical Gazette, 1829, vol. iii. p. 27.
13	M.	10	Fell on a broken wash-hand basin.	Abdominal wall.	Greater part of small intestine, transverse colon and omentum.	Reduced.	Recovered in 25 days.	Blacklock.	Monthly Journ. of Med. Sci., 1852, vol. xv. p. 32.
14	M.	41	Stab.	Left umbilical region.	Omentum.	Returned.	Recovered.	Love.	Med. and Surg. Reporter, 1860, vol. iv. p. 271.
15	M.	23	Stab.	Left side of abdomen.	3 inches of omentum.	Returned.	Recovered in 3 weeks.	Heckford.	Lancet, 1860, vol. ii. p. 129.
16	M.	Ad.	Stab.	Below umbilicus.	6 inches of omentum.	Returned.	Rapid recovery.	Dillon.	Med. and Surg. Reporter, 1871, vol. xxiv. p. 382.
17	M.	Ad.	Bullet wound.	Abdomen.	Stomach, transverse colon and omentum.	Returned.	Recovered.	Lépine.	Bulletin de l'Acad. Royal de Med., 1843–44, t. ix. p. 146.

B.—*Cases in which the Protrusion was reduced*—continued.

No.	Sex.	Age	Nature of Injury.	Site and Character of Wound.	Protrusion.	Treatment.	Result.	Reporter.	Reference.
18	M.	24	Wound.	3 or 4 inches to right of umbilicus.	Omentum.	Returned.	Recovered in 2 weeks.	Cheesman.	New York Journ. of Med., 1841, vol. iv. p. 117.
19	M.	16	Gore by horn of cow.	2 inches below umbilicus.	2 feet of ileum and lower border of omentum.	Returned.	Recovered.	Crawford.	Med. and Surg. Reporter, 1870, vol. xxviii. p. 525.
20	?	?	Incised wound.	?	Intestine and omentum.	Replaced and wound sewn up.	Recovered.	Assist. Surg. E. A. Koerper.	Circ. 3, S.G.O. 1871, Case 316 p. 95.
21	M.	Ad.	Accidental wound by a knife.	By the side of the umbilicus.	Omentum.	Returned by means of a bougie.	Patient recovered without a bad symptom.	Poland.	Guy's Hospital Reports, vol. iv., second series, p. 73.

C.—*Cases in which the Protrusion was cut off.*

22	M.	Ad.	Stab with a knife.	Left side of abdomen.	Omentum.	Cut off without ligature.	Recovered in 2 months.	Forestus.	Obs. et Cur. Chir. Francofurti, 1611, lib. vi. obs. 7, p. 13.
23	M.	50	Stab.	Left hypogastrium.	Omentum.	Cut off without ligature.	Recovered.	Richter.	Eph. Nat. Cur. 24 Dec. ann. vi. 1697, obs. 198. p. 295.
24	M.	Ad.	Sabre wound.	Right inguinal region.	Omentum.	Protrusion extirpated.	Recovered in 6 weeks.	Larrey.	Mem. de Chir. Mil. et Camp., 1812, t. iii. p. 261.
25	M.	30	Blade of a scissors.	2 in. above ant. sup. sp. pro. of left ilium.	4 inches of omentum.	Cut off; arteries tied.	Recovered in 14 days.	Ackerley.	Obs. on Wounds of the Abd., Lond. Med. Gaz., 1837, vol. xx. p. 549.
26	M.	15	Stab.	Abdomen.	Large piece of omentum.	Cut off 2 hours after injury.	Recovered in 4 weeks.	Homberg.	Richter's Chir. Bibliothek, Gottingen, 1779, p. 152.
27	M.	14	Stab.	Between 9th and 10th ribs.	3 inches of omentum —ragged piece.	Cut off.	Wound healed in 10 days.	Coates.	Med. Gaz., N. S. vol. v. 1847, p. 953.
28	M.	30	Suicidal wound.	Abdomen.	The man seized the protruding omentum and cut off a portion measuring 144 square inches.	Cut off.	Recovered in 4 weeks.	Gushee.	Boston Med. & Surg. Jour., 1847, vol. xxxv. p. 80.

C.—*Cases in which the Protrusion was cut off*—continued.

No.	Sex.	Age	Nature of Injury.	Site and Character of wound.	Protrusion.	Treatment.	Result.	Reporter.	Reference.
29	F.	30	Gore.	2 inches above and to right of umbilicus.	Bowels, stomach, and lacerated omentum.	Bowels reduced, and omentum removed.	Recovered in less than 3 months.	Colegrove.	Boston Med. & Surg. Jour., 1836, p. 249.
30	?	?	Stab.	Not stated.	Torn and bruised omentum.	Cut off.	Recovered.	Willard, Garretson.	Med. & Surg. Reporter, 1870, vol. xxiii. p. 281.
31	M.	23	Stab.	Above and a little to left of umbilicus.	Portion of omentum much lacerated.	Lacerated parts clipped off—one being 12 × 4 in.	Recovered in 2 weeks.	Howe.	American Med. & Surg. Jour., Aug. 1855, vol. vii. p. 331.
32	M.	?	Shot.	Not stated.	Omentum.	Removed.	Recovered.	Bandens.	Clin. des Plaies d'Armes a feu, 1836, p. 346.
33	M.	Ad.	Stab by clasp-knife.	Under cartilage of 7th rib, left side.	Protrusion of omentum immediately, walked 3 miles after it.	Removed.	In a fortnight able to walk 3 miles, and was well in less than a month.	Nunneley.	Med. Times & Gaz., 1860, vol. i. p. 432.
34	M.	12	Gored by a bull.	Not stated.	10 inches of colon and a portion of lacerated omentum.	Shreds of omentum clipped off, and viscera returned.	Recovered.	Bertolet.	Med. Examiner, 1851, vol. iii. N. S. p. 489.
35	M.	17	Knife.	3 inches to left of umbilicus.	3 inches of omentum.	Cut off.	Recovered in 3 weeks.	Surg. J. J. Wright.	Med. & Surg. Hist. of the War of the Rebellion, part ii. p. 35.
36	M.	Ad.	Bullet.	Left hypochondrium.	Omentum—after 3 days' rough journey of 100 miles.	Adhered to orifice. Removed by wire ecraseur.	Recovered. Specimen weighed 3 oz.	Assist.-surgeon Sternberg.	Circ. 3, 1871, S. G's O., U.S.A., p. 250.
37	?	4	?	Above and to the right of umbilicus.	6 inches of omentum.	Cut off.	Did well in 4 days.	Guthrie.	On Wounds and Injuries of the Abdomen, p. 13.
38	F.	9	Penetrating wound.	2 inches to left of umbilicus.	1 inch of omentum.	Cut off. Bled, leeched, blistered, cal. and op.	Died in 3 days. Pleurisy, right side. Slight peritonitis, omentum plugged wound.	Key.	Guy's Hospital Reports, N. S. vol. ii. 1844, p. 475.

D.—*Cases in which the Protrusion was ligatured.*

No.	Sex.	Age	Nature of Injury.	Site and Character of Wound.	Protrusion.	Treatment.	Result.	Reporter.	Reference.
39	M.	7	Penetrating wound by a pointed bamboo.	Left hypochondrium, below cartilage of ribs.	A fleshy mass, 3 in. broad by 1 in. long.	Ligatured at base.	Protrusion sloughed off, wound granulated. Patient recovered.	Baboo Gopal Chundee Deb.	Indian Medical Gazette, August 1866.
40	M.	18	Spear wound, 16 days before admission.	Epigastrium.	A fleshy mass 5 in. × 1½ in.	Ligatured at base.	Protrusion sloughed off. Patient absconded.	Surgeon F. Odevaine.	Do., July 1866.
41	M.	20	Stab by a cockree.	Midway between ensiform cartilage and umbilicus.	Solid mass, 3 in. × 1½.	Ligatured at base.	Recovered in 16 days.	Surgeon A. Taylor.	Do., September 1866.
42	M.	36	Bullet wound, 10 days before admission.	Left side of the thorax.	A fleshy mass the size of the fore-finger.	Ligatured and cut off.	The wound healed quickly. Patient recovered.	Surgeon-Major W. B. Beatson.	Do.
43	F.	60	Gored by a cow, 5 days before admission.	In front of crest of left ilium—5 in. × 2 in.	Tongue-shaped mass, 5 in. × 1½.	Ligatured at base.	Protrusion separated and wound healed quickly.	Dr Greene.	Do.
44	F.	12	Gored by a cow, 4 hours before admission.	The region of the liver.	2 in. of omentum.	Ligatured and removed after 14 days.	Discharged well in 1 month and 4 days.		Do., September 1871.
45	M.	50	Stab with a sword, 3 days before admission.	Above and to right of umbilicus, 2½ in. long.	Semi-gangrenous omentum, 5 × 3 × 1½ in.	Ligatured and removed.	Patient absconded in a few days.	Surgeon D. O'C. Raye.	Medico-Legal Experience in Bengal, by K. M'Leod, p. 60.
46	M.	?	Sword wound.	Not stated.	Almost the whole of the omentum.	Ligatured and removed.	Sanatus est cito.	Galenus.	Omnia quæ extant Froben, 1567, t. i. lib. iv. 1244—"de usu partium."
47	M.	Ad.	Stab.	1½ in. below navel.	2 in. of omentum.	Ligatured and cut off.	Recovered in a few weeks.	Neuman.	Am. Med. Intelligencer, 1841, p. 164.
48	F.	21	Stab.	Left groin.	Omentum, size of palm of hand.	Double ligature and ablation.	Recovered in 6 weeks.	Hewson.	Med. Examiner, 1857, vol. vii. p. 567.

D.—*Cases in which the Protrusion was ligatured*—continued.

No.	Sex.	Age	Nature of Injury.	Site and Character of Wound.	Protrusion.	Treatment.	Result.	Reporter.	Reference.
49	M.	Ad.	Stab.	Left iliac region, 1½ in. above ant. sup. sp. pro.	6 in. of omentum severely lacerated.	Ligatured and cut off.	Recovered in 1 month.	Milner.	New Orleans Journ. of Med., 1869, vol. xxii. p. 177.
50	M.	10	Fell on an iron railing.	Not stated.	A piece of omentum considerably lacerated.	Ligatured and cut off.	Recovered.	Kloman.	Phil. Med. Times, 1872, vol. iii. No. 55, p. 691.
51	?	?	Knife wound.	Left hypochondrium.	Omentum.	6 inches ligatured and removed.	Recovered in 40 days.	Gibbes.	Am. Journ. of Med. Sci., 1873, vol. cxxix. p. 200.
52	F.	31	Sword wound.	Hypogastric region, right side.	Omentum and 7 in. of ileum.	Ileum returned. Omentum ligatured and removed.	Wound healed. Woman died subsequently of diarrhœa.	Saviard.	Nouveau Recueil d' Observ. Chir., Paris, 1702, p. 102.
53	M.	Ad.	Cut with a hunting knife.	Umbilical region.	Omentum.	Ligatured.	Separated in a few days; wound healed. Patient recovered.	Ravaton.	Chirurgie d' Armie. Paris, 1768, p. 497.
54	M.	27	Stab.	Left iliac region, 1½ in. above and internal to ant. sup. sp. process.	Omentum.	Ligatured.	Separated in a few days; wound healed in 3 weeks.	Birkett.	Lancet, 1867, vol. ii. p. 9.
55	M.	19	Incised wound.	Abdomen.	Omentum.	Ligatured and poulticed.	Protrusion sloughed off and wound healed.	Surgeon R. T. Stratton.	Medical and Surgical History of the War of the Rebellion, part ii. p. 83.
56	?	?	?	Abdomen.	Omentum.	Ligatured on 5th day.	Ligature separated in 3 days; discharged in 12.	Hargrave.	Guthrie on Wounds and Injuries of the Abdomen, p. 13.
57	M.	32	Shot wound.	1 in. above crest of right ilium.	About 6 inches of omentum.	Tied and replaced.	Ligature came away in 6 days. Recovered.	Assist.-Surg. W. A. Bradley.	Med. and Surg. Hist. of the War of the Rebellion, part ii. p. 175.

APPENDIX No. II.

Table showing the State and Position of the Omentum in 20 postmortem Examinations.

No.	Sex.	Age	Bodily Condition.	Disease Causing Death.	Distance of Lower Border of Stomach from Xiphoid Cartilage.	Distance of Lower Border of Transverse Colon from Umbilicus.	Distance of Lower Border of Omentum from Symphysis Pubis.	Condition of Omentum.	Viscera Exposed by Reflection of Anterior Wall of Abdomen.
1	F.	57	Fat. Anasarcous.	Cirrhosis. Kidney disease.				7½ in. deep, ¾ in. thick; reached down to symph. pubis on being stretched.	Liver and stomach superiorly. Colon on each side. Small intestine entirely concealed by omentum.
2	M.	21	Extremely emaciated.	Phthisis pulmonalis.				3 inches deep. Membranous.	Small intestine entirely uncovered.
3	M.	45	Pretty well nourished.	Cancer of rectum.				Slightly loaded with fat.	Small intestine entirely uncovered below level of umbilicus.
4	M.	46	Fairly nourished.	Disease of heart and kidneys.	8½ in. below x. c.	3 in. below umb.	4 in. above s. p.	Membranous. Could be drawn down to symph. pubis.	Liver and stomach superiorly. Transverse colon visible below them. One coil of small intestine seen below lower border of omentum.
5	F.	45	Emaciated.	Phthisis pulmonalis.	7 in. below x. c.	Level of umb.	1 in. above s. p.	7 in. deep. Could be drawn down to symph. pubis. Crumpled up to right, and at middle line.	Liver seen superiorly. Stomach not visible. Transverse colon seen at level of umbilicus. Small intestine concealed by omentum.
6	M.	50	Emaciated.	Phthisis pulmonalis.	6 in. below x. c.	¼ in. below umb.	4½ in. above s. p.	Contained very little fat. Crumpled up.	Liver and stomach superiorly. Band of omentum below them, and several folds of small intestine below it.
7	M.	50	Emaciated.	Aortic aneurism.	4½ in. below x. c.	½ in. below umb.	Reaches to s. p.	Membranous. Could be pulled down below symph. pubis.	Liver and stomach superiorly. Transverse colon below them. Small intestine concealed by omentum.
8	M.	40	Extremely emaciated.	Phthisis pulmonalis, perforating tubercular ulcer, peritonitis.	2 in. below x. c.	2 in. above umb.	3½ in. above s. p.	Slightly loaded with fat. Could be pulled down below symph. pubis. Bands adherent to outside of tubercular ulcers.	Liver and stomach superiorly. Latter contracted. Transverse colon partially concealed, and small intestine entirely so by omentum.
9	M.	30	Emaciated.	Typhoid fever, perforating intestinal ulcer, peritonitis.	1 in. below x. c.	2 in. above umb.	4 in. above s. p.	Membranous. Very little fat. Adherent to surface of small intestines by peritonitis.	Liver and stomach superiorly. Small intestine seen on right side; concealed on left by omentum.
10	M.	30	Emaciated. Anasarca.	Disease of heart and kidneys.	5 in. below x. c.	½ in. below umb.	5 in. above s. p.	4 inches deep. Membranous. Adherent to lower border of liver. Crumpled up. Capable of being pulled down to halfway between umbilicus and symph. pubis.	Liver and dilated stomach visible superiorly. Lower half of transverse colon and whole of small intestine uncovered.

Table showing the State and Position of the Omentum in 20 postmortem Examinations—continued.

No.	Sex.	Age	Bodily Condition.	Disease causing Death.	Distance of Lower Border of Stomach from Xiphoid Cartilage.	Distance of Lower Border of Transverse Colon from Umbilicus.	Distance of Lower Border of Omentum from Symphysis Pubis.	Condition of Omentum.	Viscera Exposed by Reflection of Anterior Wall of Abdomen.
11	M.	51	Emaciated. Anasarcous.	Disease of heart and kidneys.	6 in. below x. c.	Level of umb.	9 in. above s. p.	3 in. deep. Shrivelled between stomach and transverse colon. Could be pulled down to half way between umb. and s. p.	Liver visible superiorly. 2 inches of stomach below its lower border, part of transverse colon, and whole of small intestine exposed to view.
12	F.	45	Well nourished. ½ in. of subcutaneous fat in abdominal cavity.	Pneumonia.	4 in. below x. c.	Level of umb.	6 in. above s. p. in middle line.	3 to 5 inches deep. Extended down to s. p. on left of middle line. Shrivelled up on right.	Liver and stomach visible superiorly. Transverse colon seen below them. Small intestine uncovered on right side and concealed by omentum on left.
13	M.	24	Well nourished. ½ in. of fat in abdominal wall.	Fracture of skull.	5 in. below x. c.	3 in. above umb.	5 in. above s. p.	10 to 16 inches deep. Slightly loaded with fat. Extended down to s. p. on left side.	Liver and stomach visible superiorly. Transverse colon partially obscured by omentum. Small intestine covered by omentum on left side; uncovered on right.
14	M.	50	Well nourished. ½ to 1 in. of fat in abdominal wall.	Fracture of skull.	2½ in. below x. c.	4 in above umb.	6 in. above s. p.	9 inches deep. Could be pulled down to symph. pubis. Contained a good deal of fat.	Liver seen superiorly. Stomach and transverse colon concealed by omentum. Small intestine covered by omentum superiorly; uncovered below.
15	M.	73	Emaciated.	Chronic bronchitis, ascites.	2 in. below x. c.	4 in. above umb.	9 in. above s. p.	8 × 12 in. Membranous. Could be pulled down to s. p.	Liver visible superiorly. Stomach, transverse colon, and omentum concealed by folds of small intestine, which were uncovered.
16	F.	40	Emaciated.	Heart disease.	5 in. below x. c.	3 in. below umb.	4 in. above s. p.	3 in. deep. Adhered below to sac containing fœtus.	Liver and stomach visible superiorly. Tumour in right iliac and hypogastric region. Small intestine uncovered to left of them.
17	F.	46	Well nourished. ½ in. of fat in abdominal wall.	Cirrhosis. Ascites.	5½ in. below x. c.	1½ in. below umb.	5½ in. above s. p.	12 × 18 in. ½ in. thick. Could be pulled down beyond s. p.	Liver and stomach seen superiorly. Transverse colon seen below them. Small intestine partially covered by omentum.
18	M.	66	Emaciated.	Syncope.	3 in. below x. c.	2 in. above umb.	3 in. above s. p.	2 to 6 in. deep × 9 to 12 broad. Moderately loaded with fat. Could be pulled half way down to s. p.	Liver seen superiorly. Small intestine uncovered below it, concealing stomach, transverse colon, and omentum.
19	M.	46	Emaciated. Anasarca.	Chronic Bright's disease.	3 in. below x. c.	1 in. above umb.	6 in. above s. p.	5 to 6 in. deep × 9 or 10 broad. Deeper on left side. Moderately fatty. Could be pulled down to s. p.	Enlarged liver visible superiorly, concealing contracted stomach. Transverse colon almost concealed by omentum. Small intestine uncovered.
20	F.	22	Well nourished. ½ to 1 in. of subcutaneous fat.	Tubercular meningitis.	Level of x. c.	½ in. above umb.	About 12 in. above s. p.	7 in. × 12 in. Symmetrical, moderately loaded with fat. Could be pulled down to near symph. pubis.	Small bit of liver seen superiorly. Transverse colon, cæcum, and sigmoid flexure, and a few folds of small intestine, all quite uncovered, exposed. Omentum shrivelled and crumpled beneath dilated transverse colon.

NOTES

ON

ANTISEPTIC SURGERY

IN

WAR.

BY

SURGEON-MAJOR H. MELLADEW,

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PREFACE.

THE notes embodied in the following pages are the result of visits to Berlin, Dresden, St. Petersburgh, Moscow, Vienna, Munich, Paris, and other cities during my winter leave. The journey was undertaken to obtain information concerning foreign military hospitals, and to gather hints about Antiseptic Surgery as applied to service in the field.

I was most hospitably received and most kindly assisted in my object by everyone I had the pleasure to meet, and take this opportunity to offer my sincere thanks, especially to the following gentlemen :—

The British Military Attachés at Berlin, St. Petersburgh, Vienna, and Paris.

To Surgeon-General Dr. Roth, 12th (Royal Saxon) Corps.

His Excellency von Koslov, Chief Medical Inspector-General of the Russian Army.

His Excellency von Ritter, in medical charge of the St. Petersburgh District.

Dr. Wywodzoff, Surgeon to the Imperial Russian Court.

Sanitätsrath Dr. Grimm, at St. Petersburgh.

Hofrath Dr. Unterberger, at St. Petersburgh.

Dr. Scott, at Moscow.

Oberstabsarzt Dr. Neudörfer, at Vienna.

Generalstabsarzt Dr. Ritter von Nussbaum, Munich.

Surgeon-General Dr. Lotzbeck, Munich.

Oberstabsarzt Dr. Port, Munich.

H. MELLADEW,
Surgeon-Major Royal Horse Guards.

LONDON, *9th March*, 1881.

NOTES ON ANTISEPTIC SURGERY IN WAR.

ARE wounds to be treated antiseptically in war, and, if so, how is the treatment to be carried out? are questions argued out in every possible form on paper, in lectures, and in conversation in nearly every country in Europe. This subject stands first for discussion in the Military Section of the International Medical Congress to be held in London in August next. Having, during the winter, visited many hospitals, both civil and military, in Paris, Germany, Russia, and Austria, perhaps it may not be out of place to put on paper some of the impressions I have brought away of what I have seen myself, of what I have heard in conversation with many of the first surgeons of the day, as also of what I have read on this most important subject. The matter is of the most vital importance, not only to the individual soldier, but also to the State. Such an urgent question has it become, talked of, experimented upon everywhere with a view to its final settlement, that its choice as one of the subjects at the Congress could hardly have been omitted, and its position as No. I. shows the importance which is attached to it.

The first part of the question may be got rid of at once; there seems to be but one answer everywhere, that Antisepsis in some form should certainly be the mode of treatment for wounds in war. After its brilliant results in civil hospitals, its steady triumphant progress from Glasgow and Edinburgh,

carrying blessings with it all over the world, there can be no doubt that the benefits it confers should also be extended to the unfortunate wounded on the battle-field. How well it has there already answered, under the most unfavourable circumstances, is shown by the numerous reports of the surgeons employed during the Russo-Turkish War, at the head of which stand those of Bergmann and Reyher. The wounds dressed antiseptically on the battlefield, on arrival at the hospitals, after a transport often of many days, over the worst possible roads, in the hottest weather and the thickest dust, were all healthy, some already healed, others nearly so. In the hospitals the necessary examinations could be made under spray without danger, fresh antiseptic dressings applied, and convalescence greatly hastened thereby. It has been my good fortune to meet many surgeons employed in the late wars in Bulgaria, Bosnia, and the Herzegovina, who all speak in the highest terms of antiseptic treatment. It was put in practice wherever possible, in as perfect a form as circumstances allowed; some attempt at it was made nearly everywhere. However small the means at disposal, and however defective consequently the practice, the results were always satisfactory as compared with those obtained before antiseptic treatment was heard of. There seems to be a general conviction that Antisepsis must be introduced on to the battle-field and into all war hospitals: ideas vary merely as to the necessity or not of spray, and in minor details; the best means of carrying it out remain only to be settled. Of course, strict Listerism is impossible on the battle-field, in consequence of the great disproportion between wounded and surgeons. One objection to its general introduction into military surgery is its cost. In the present enormous strength of armies, anything likely to add to the expense of equipment, except it be to increase its destructive power, is not easily sanctioned. Any extra cost to the State incurred

by fitting out each soldier and every hospital of war with the means of thoroughly carrying out antiseptic treatment, would most certainly be repaid many times over by the numbers of lives which would be saved, by the more rapid healing of wounds, and thereby hastened convalescence, which would enable men to rejoin the colours in a comparatively short time—men who, in former wars, would, if not succumbed under the various forms of blood-poisoning, at all events have been prevented by a much slower convalescence from again meeting their comrades in the ranks until after the end of the war: all the more would this be the case during the present quickly-fought campaigns. Antiseptic treatment, again, would save many a limb—and invalid pension afterwards—which, without it, would probably be lost; by it the work of the military surgeon would be greatly lightened, not only at the dressing stations, but also at the different war hospitals; many wounds, once closed with the antiseptic pad, would not require further attention for many days, and time be thus saved—time so urgently needed for those absolutely necessary operations at the dressing stations, as the arresting of severe hæmorrhage, the immobilising fractured limbs with splints or plaster-of-Paris, applied on strips of linen, bandages, or stockings, after antiseptic dressing had been applied to the wounds. It would confer another benefit, admitted by all army surgeons as of the first importance: it would permit rapid evacuation of the wounded from the front, the sending them away as far as possible and as soon as possible. Wounded men, as a rule, bear transport well, and in the better air and more cheerful society far from the battle-field, perhaps at or near their homes, they have undisturbed rest, are better fed, better nursed, and convalescence is much more rapid than if kept crowded together in the different hospitals near the theatre of war. Room so much needed is gained for others. The writings of Pirogoff and others show how enormously the rate

of mortality decreased whenever rapid evacuation could be carried out during the Russo-Turkish War. I add some statistics which speak for themselves :—

RUSSO-TURKISH WAR.

Surgeons.		Deaths.
Esmarch	Of 57 gunshot wounds of knee not treated antiseptically in the first instance ...	44·5 p. cent.
,,	,, 15 ,, ,, knee-joint with fracture treated antiseptically primarily	6·6 ,,
,,	,, ,, ,, ,, ,, ,, not in first instance ...	66·6 ,,
Reyher ...	,, 78 ,, ,, ,, ,, not treated antiseptically at all	77·4 ,,
,, ...	,, 62 ,, ,, ,, ,, ,,	7·6 ,,
,, ...	,, gunshot wounds of soft parts, treated primarily antiseptically	18·1 ,,
,, ...	,, ,, fractures, ,, ,, ,, ...	13·1 ,,
,, ...	,, ,, wounds of joints, ,, ,, ,, ...	21·5 ,,
,, ...	,, ,, ,, soft parts, not treated antiseptically in first instance ...	35·5 ,,
,, ...	,, ,, fractures ,, ,, ,, ,, ,, ...	61·5 ,,
,, ...	,, ,, wounds of joints ,, ,, ,, ,, ,, ...	14·7 ,,
Gross ...	,, 70 gunshot fractures { 34 treated strictly antiseptically	13·8 ,,
	{ 36 ,, not on strict antiseptic principles	19·5 ,,
,, ...	,, 66 ,, ,, into joints { 19 treated antiseptically in first instance ...	13·8 ,,
	{ 36 ,, ,, not ,, ,, ...	9 ,,
	{ 11 ,, not strictly on antiseptic principles	
Krause* ...	publishes a list of 23 gunshot wounds treated in Professor Volkmann's Klinik in Halle, treated on strictly antiseptic principles :—	
	Of 7 gunshot wounds of soft parts, 2 with injury to large vessels—none died.	
	,, 2 ,, fractures ... ... none died.	
	,, 4 ,, ,, of joints ... ... none died.	
	,, 3 perforating gunshot wounds of skull ... ... one died { Brain extensively lacerated.	
	,, 6 ,, ,, ,, of chest ... ... none died.	

* Langenbeck's Archiv., 24th volume.

Professor Esmarch places the "schwerpunct" (all-important point) of antiseptic treatment on the battle-field itself; although, as a rule, an "exact" antiseptic dressing there cannot be applied, the work of surgeons should be governed by the principle of Antisepsis. As experience teaches that even very severe gunshot wounds can, and do, heal aseptically under an occlusive dressing—that is, without inflammation and suppuration—if only they are not subsequently infected, it is very necessary that no examination on the battle-field with dirty fingers, probe, &c.,

take place, and that the protective dressing laid on the wound do not consist of poisonous material—as dirty charpie—but of an antiseptic preparation which forms with the blood in the wound a dry aseptic crust.*

Professor Dr. von Nussbaum, of Munich, puts the question in his book,† "Must this antiseptic treatment be carried out in war also?" and continues :—The fact "that the fate of a wounded man lies almost altogether in the hand of the surgeon who first dresses the wound," should necessitate the greatest efforts to be made to introduce Antisepsis, not only into the war hospitals, but also to render it possible on the battle-field itself. Septic wounds cannot always be made aseptic; it should therefore be the great aim of the surgeon at the help and dressing stations to prevent septic wounds being sent into the field hospitals.

Most of the wounds in battles of the present time are by rifle bullets; they bleed but little, and are especially fitted for at once closing with antiseptic pads. A firm, impermeable protection is given to the wound, which heals rapidly without any disagreeable symptom, if not previously infected by unclean fingers, probes, or afterwards by needless disturbance or examination, or by the shifting of the antiseptic dressing. The men of the Sanitäts detachment should, if possible, close the wound at once with the antiseptic pads proposed to be carried by each soldier. If not closed on the battle-field by the wounded themselves, or by the bearers, the wounds are to be dressed at the help and dressing stations. If the contents of the soldier's packet are not sufficient, they can be supplemented by those in the bearer's pouch or from the dead around. It may be that too much weight is put on this immediate closing of the wound, and that perhaps it

* Langenbeck's Archives, volume 24, heft 2.

† "Guide to Antiseptic Treatment of Wounds," third edition, 1879.

would be better not to allow the bearers to lose too much time in dressing the slighter wounds on the battle-field, but to order them to make all haste to submit the wounded men to the surgeons at the first help and dressing stations. Wounds would thus escape one danger, that of infection by the bearer's hands, which cannot possibly be clean. However, there can be no doubt that the fewer slight wounds the surgeon is asked to attend to the better; the more thoroughly can he give his time to the more serious ones, and make them fit to be moved to field and other hospitals, to which the slighter cases, already dressed on the battle-field by the bearers or wounded themselves, at once pass on, unless circumstances should have arisen requiring further attention.

Professor v. Nussbaum recommends, should the wounds appear septic on future examination in the hospitals, their thorough syringing out with an eight per cent. solution of chloride of lime, and their further washing with a five per cent. carbolic acid solution. Should this treatment not suffice, should the wound remain septic after repeated washings and dressings even, "we stand again on the uncertain ground of former sad times;" all may go on well, but blood-poisoning in one of its many forms may result. The great importance, that the entire hospital staff should have a thorough knowledge of the practice of Antisepsis, is therefore evident: "The fate of the wounded man lies in the hands of the surgeon who attends him during the first few hours." Of course it is impossible to employ strict Listerism—dressing and spray—on the battle-field, and, fortunately, it is not required; Esmarch's advice to close every wound at once with an antiseptic pad being sufficient for the time; afterwards in a hospital it can be dressed at more leisure on stricter Listerian principles. Rather than apply to the wound the charpie of the packet which the soldiers of most armies still carry—a material made, perhaps, of dirty, infected linen by

unclean fingers—Professor Nussbaum advises, in the absence of the antiseptic pad, to leave the wound open, when the secretions will become thickened, form a scab, and thus prevent the development of bacteria. Charpie is not to be used on any account; it has to answer for the great mortality in former years from pyæmia, hospital gangrene, and erysipelas, &c., following wounds which under antiseptic treatment would have healed quickly and almost without fever. He recommends that every soldier should carry a pad of salicylic cotton-wool, enclosed in salicylic gauze and bandage. Oberstabsarzt Dr. Neudörfer, of the Austrian Army, one of the most experienced military surgeons of the day, recommends a similar treatment of wounds in war. After hæmorrhage has been arrested, the wound is to be cleaned and washed with some antiseptic solution and by position or drains the escape of any secretions ensured. A thin layer of salicylic acid in powder is next to be applied to the wound surface, protected by cotton-wool or organtin in eight or ten layers, and afterwards secured by a three-cornered bandage. This would apply to the first help and dressing stations and field hospitals. Such a dressing can, under favourable circumstances, remain undisturbed for three to four weeks. If to be changed, Dr. Neudörfer allows no water to be used. This dressing is very simple, requires little time, and would for that reason be well adapted for field and other hospitals. This surgeon differs from Professor Lister, in believing that "in the pus and in the albuminous secretions of the body a chemically acting ferment is formed as 'contact body,' which is slightly soluble in serum and other animal fluids, insoluble in alkohol, ether, and aromatic acids, but very soluble in water,"* and not that the poison enters wounds from the air. Under certain conditions this ferment can be absorbed in the

* "Behandlung der Wunden im Kriege."

wound by the fluids and blood of the body, and, there acting as a poison, produce the different forms of wound diseases. He therefore aims at—

First, to prevent the formation of this "contact body" in the secretions; and, secondly, if already formed, to arrest its absorption. Water consequently is not to be used, but some application which renders the poisonous ferment insoluble, and prevents its absorption into the system, such as carbolic acid, which forms with the albumen a "constant" compound. Dr. Neudörfer rests his convictions upon the good results obtained by him in the campaigns of 1848, '59, '64, and '66—before, of course, antiseptic treatment was known as such—and upon his experience since in hospital practice. The strict Listerian system was given a full and fair trial, but did not seem to possess any great advantages over the former mode of treatment, besides consisting of a great deal which is unnecessary; thanks to its complicity therefore and expense, it is not adapted for war, and luckily not necessary.

He, however, allows one great merit in Lister's treatment—the extreme cleanliness insisted upon—and lays down that hands and instruments should be scrupulously cleansed, not on account of bacteria, but to get rid of the smallest particle of dirt, which, by contact with the wound secretions, would cause their poisonous degeneration. Then the escape of blood, serum, &c., must be prevented, or, if this cannot be done, their decomposition not permitted. This, especially when the fluids cannot escape freely, is accompanied by their coagulation or precipitation by one of the many Antiseptica which prevent their decomposition, as carbolic, boracic, salicylic, and benzoic acids; creosote, thymol, alkohol, and camphor.

If we can do without spray, the carrying out of antiseptic treatment in war would be much simplified.

Spray diffusers are cumbersome to carry about, take up

much room in the waggons, and require each the whole attention and time of one man, whose hands are so urgently required elsewhere. When it is considered how many wounded the spray would be required for at the same time, and the number of diffusers therefore necessary, after a hard-fought action, it is easy to understand what a saving in material, space in the waggons and of hands, apart from the expense, its non-employment would cause. On the Continent spray seems to be gradually falling into disuse, some of the greatest surgeons having discontinued it. Professor Billroth, whose division at the General Hospital in Vienna can show a constant succession of the most serious and extensive operations, works without spray, and the results—to a casual observer—could not be more excellent or more satisfactory. Its use was discontinued about eight months ago, I believe, and although no statistics are at present published, the results now without spray compare satisfactorily with those formerly obtained under it. I have seen as many surgeons operate without it as with it, and if only that most important condition of Professor Lister's treatment—that of scrupulous cleanliness of hands, instruments, &c.—is faithfully carried out, the results obtained appear as good. Most thorough and frequent washing and rinsing-out of the wound by a stream of carbolic or any other disinfectant solution should, it would appear, more thoroughly get rid and kill any bacteria—if that theory be a true one—which have found their way into the wound from the air, than a very minutely-divided stream—which, after all, spray is—falling very gently and lightly, with insufficient force to remove anything adhering to the wound surface. Although conclusive statistics, with a long list of cases, are perhaps still wanting, thoroughly comparing the two methods—spray or no spray—it is, at all events, certain that the many great surgeons now operating without it after a previous thorough trial of spray would hardly continue to do

so were the results obtained not at least equally satisfactory.

It seems, therefore, probable that, given perfectly clean hands, instruments, antiseptic washings and dressings, and, above all, clean sponges — which are rarely sufficiently cleansed, and, therefore, become the agents frequently of poisonous changes in the wounds—we can be as successful in the treatment of wounds without spray as with it, which, if true really, would remove one of the greatest stumbling-blocks in the way of the practice of Antisepsis in war, and free a number of hands urgently wanted for work elsewhere. Whether spray would be beneficial in much-crowded hospitals, with older cases and septic wounds, is perhaps probable. As these, however, are generally far away from the front, in permanently established buildings or tents, the means to obtain spray would be comparatively easy.

We may divide Antiseptic treatment of wounds in war into that proposed for

1st. The battle-field itself, to be carried out by the men of the Army Hospital Corps and by the wounded themselves;

2nd. For the first help and dressing stations; and

3rd. For the field and other war hospitals.

It seems to be generally recognised that a wound received should as soon as possible be closed antiseptically, either by the wounded man himself, by a surgeon, or by a soldier of the Army Hospital Corps, with the dressing material carried on the man himself, which, if not sufficient, is to be supplemented by that from the bearer's pouch or from any dead at hand. As no water is likely to be found in the immediate vicinity of the wounded man, it is necessary that the antiseptic occlusive dressings should be used in a dry state. The greater majority of wounds is by bullets, and bleed little; they are as easily closed by an antiseptic as by

the old charpie pad. There should be no previous examination of the wound, except for most urgent reasons, by either finger or probe. Dressing stations are, if possible, to be established near water; as this, however, is not always to be had, it is very desirable that antiseptic dressing materials to be employed there should be dry also. Similar material would therefore be applicable to both battle-field, first help and dressing stations. At the latter place, also, examination of the wounds should, if possible, be avoided, bullets and splinters not be removed, nor any operation performed unless urgently required, such as the separation of a limb hanging but by a shred, or the necessary interference in extensive shell lacerations with tearing and crushing of the large blood-vessels, cutting off the blood supply to the parts below, &c. It is well to remember, though, that even the most extensive wounds heal when primarily closed with antiseptic pads. Surgery should be as conservative as possible.

Coloured cards should be carried in the pouches or havre-sacks of the bearers, as in those of the surgeons, employed at the theatre of war, of different colours with different meanings, say, for example:—

A red card, attached to the button of a wounded man's tunic, or hung round his neck, signifies:—
Slightly wounded, able to bear immediate transport, further attention unnecessary.

A blue card—unable to bear long transport, requires further examination, &c., at field hospital.

A green card—severely wounded, to remain at nearest hospital.

The German war medical regulations order white tickets for wounded requiring immediate hospital attention; red for those who are able to bear transport.

On these tickets the nature of the wounds is to be noted

as shortly as possible, such as Dr. Köcher, in "Sanitätswesen at Plevna," recommends:—

Fract. fem. d. $^1/_3$, meaning fractura femoris dextra in upper third; $^2/_3$, middle; $^3/_3$, lower third, &c.; and more details as to dressing, operation, &c., as time would allow.

The Austrian "diagnosis" tickets carry the numbers one, two, three, each signifying different degrees of transportability; the surgeon strikes out the two not applicable.

The slightest glance at a man on arrival at the dressing station or at the field hospital would show whether the wound requires further attention; those men with red cards would at once pass on and give room to others requiring further surgical assistance; while an immense amount of time would be gained for absolutely necessary operations, and for the putting up of fractures, &c., in plaster-of-Paris or in splints, so as to render them fit for transport.

Soldiers having had their wounds attended to at the first help and dressing stations, their fractured limbs put up, as Professor Esmarch recommends, before reaction has set in; having been divided into groups by the coloured tickets according to the degree of their transportability, pass on to the field hospital, where the necessary operations are to be performed. Those with red tickets only pass through further to the hospitals in the rear. The forming of the wounded into easily distinguished groups prevents any confusion; a great deal of time is gained, and no wound once dressed is disturbed unnecessarily. Watraszewski during the Russo-Turkish War employed carbolised rags, covered with ordinary cotton-wool, to the wounds with good results. Of oakum he speaks highly, as also of a five per cent. solution of carbolic acid as a hæmostatic agent.

Opinions vary greatly as to the material best adapted for antiseptic dressings in war, also as to the best way of carrying it there.

Very necessary conditions are, that the material chosen should be:—

1. Sufficiently antiseptic.
2. As lastingly so as possible.
3. Cheap.
4. Easily and rapidly prepared; and
5. Easily carried.

An immense variety of dressing materials rendered antiseptic in a great many different ways has been proposed. It may be divided into two classes:—First, that intended to be carried by the soldier on his person, by the men of the Army Hospital Corps, and in the surgeons' pouches; and, secondly, that to be placed in the hospital waggons for use in the different hospitals.

1. *The soldier's packet.* It is recommended to be carried in future sewn into the lining of the left breast of the tunic, instead of, as formerly, in the infantry at all events, loose in the left trouser pocket—a most objectionable place, for many reasons, one being that it is often lost by accident, or thrown away as taking up room which is considered by many more advantageously filled by tobacco or articles of food. The antiseptic agent which has most votes for this purpose is probably salicylic acid, but differences of opinion exist as to the best means of carrying it in the dressing packet.

To prepare material for antiseptic dressings in the quantities required to supply the present enormous armies at the last moment would be next to impossible; it must be done in time of peace and stored in readiness for sudden war, at all events in sufficient quantity to provide the first outfit. Just as uniforms, articles of equipment, powder, cartridges, &c., are stored, and those longest in the magazine taken out and used, to be replaced by fresh, so can these dressing materials be prepared, collected, used, and the numbers again made complete, if, should any volatile material be used, means can

be devised to reduce that volatilisation to a minimum. Volatile agents have the advantage, if the infection from air theory is the true one, of keeping the surrounding atmosphere aseptic.

The problem of providing an envelope sufficiently protect ing the antiseptic properties of the soldier's dressing packet has probably been solved by Oberstabsarzt Dr. Port, of the Bavarian Army. His packet, which he had the kindness to show me, is flat, about three inches by five, oblong, like a well-filled envelope. It contains either two little papers, each enclosing a small quantity of pure salicylic acid in powder, wrapped in layers of salicylic cotton-wool of equal size, or two pads made of a square piece of filtering-paper, covered with gauze and soaked in a solution of carbolate of lime, and afterwards dried. A short gauze bandage is folded round the two pads in each case, wrapped up again in parchment paper, and then enclosed in an envelope of tin. The metal is cut into a long strip, is folded across, the projecting edges of the one turning over the other and closing the case. This is finally wrapped up in paper soaked in a spirituous solution of asphalte, which, on drying, becomes perfectly air-tight. One of these little pads is intended for each wound, inlet and exit; the cotton-wool to keep, in the one case, the powder in its place; the parchment paper, torn in half, is applied over the pads again, and the bandage over all. The tin case spread out would be found useful as a makeshift splint, or a means of giving temporary support to the wounded limb.

Now this packet certainly has the one great advantage—that of enclosing the disinfectant in an air-tight envelope, which protects it from all rough usage; it, as a consequence, can be stored for a considerable time without serious deterioration or loss of quality; it is very flat; takes up but little room, and can be manufactured, I am assured by a most competent authority, very cheaply. There is every probability of its

being adopted in, at all events, one army. It does not contain a triangular bandage, which is a disadvantage perhaps; it seems to me also that the loose powder is very apt to get lost, upset by shaky fingers of the wounded, or blown away by the wind. Again, if struck by a bullet, particles of the tin envelope might be carried into the wound and become a possible serious complication. The hard tin case with sharp edges would perhaps become an annoyance during a long march, if pressed upon by straps, &c.

Professor Esmarch recommends for the soldier's packet:—

1. A three-cornered bandage, with safety-pin.
2. A starched gauze bandage about 2 yards long, 4½ inches wide, also with a safety-pin.
3. Two antiseptic pads of salicylic tow, wrapped up in salicylic gauze.

The whole, enclosed in strong parchment paper, will be 5 inches long, 3½ inches wide, and nearly 1 inch thick. This packet should, however, not be carried in the trouser pocket. Oberstabsarzt Dr. Münnich,* in an elaborate article on the various materials recommended for Antisepsis in war, gives his vote for chloride of zinc, with bleached tow or cotton-wool; this salt does not crystallise or dust-out, thanks to its hygroscopic qualities. Cotton-wool lies closer than ordinary tow, but has not many advantages over the bleached article. With a triangular bandage it is to be enclosed in parchment paper. The material is easily prepared; it is cheap, and does not lose much of its disinfectant property by rapid drying. If freshly made, and with a ten per cent. solution of chloride of zinc, as by Professor Bardeleben at the Charité in Berlin, it is too caustic and irritating to be long left in immediate opposition to the wound. Pro-

* "Deutsche Militärärztliche Zeitung," 2 heft, 1880.

tective silk is, therefore, placed between it and the wound surface. This, however, would probably be unnecessary in war, when the material has not been so recently prepared.

Professor Nussbaum recommends salicylic cotton-wool and gauze. He says* :—"In war, as in peace, a fresh wound must not be examined by either probe or finger without antiseptic precautions; it must be at once thoroughly closed with a pad—salicylic cotton-wool or tow wrapped in gauze." "Under such protection the wound heals like a subcutaneous one, or retains its aseptic condition for days, until time and opportunity allow of its further antiseptic treatment."

As we have already seen, Dr. Neudörfer, of the Austrian Army, also advises salicylic acid in powder, cotton-wool, and gauze. These are some of the chief materials recommended for the soldier's dressing packet. The necessary conditions are that it should be as small and as portable as possible, able to withstand plenty of rough usage, yet contain sufficient dressing material thoroughly to guard and protect the wound, this material to be antiseptic, and to be kept so as long as possible by some envelope impervious to damp or other hurtful influences.

Any loose powder, such as salicylic acid, would seem to be exposed to too many risks on the battle-field; but, being an excellent and stable disinfectant, it might be fixed by glycerine in cotton-wool, which itself is a perfect filter for anything contained in the air.

I have had a soldier's packet made which seems to me to answer all requirements. I hope still further to perfect it, and to make some experiments with it, before the Congress meets. Its contents at present are the following:—

Two pads of carbolised cotton-wool, thirty grains in weight,

* "Leitfaden," &c.; see above.

one for each wound, wrapped up in a piece of carbolised gauze one foot long and three inches wide.

A piece of gutta-percha tissue (or paraffin paper), folded round the above and closed air-tight by means of a solution of gutta-percha. This again lies in a triangular bandage, and the whole in a vulcanised india-rubber cloth envelope, itself thoroughly closed by the above solution. This packet is five inches long, 3 inches wide, and $\frac{1}{2}$ inch thick; weight, $1\frac{3}{4}$ to 2 ounces; price, if made wholesale in large quantities, $3\frac{1}{2}$ to 4 pence. The protection to the antiseptic material seems as perfect as can well be obtained; the packet, if sewn into the tunic lining in the hollow below the clavicle, would not be noticed much, and, by its softness, cause no pain or discomfort should belts pass over it. It contains everything needful for the dressing of wounds on the battle-field.

Boric acid cotton-wool would be cheaper, as, of course, also tow; but the former is not reliable, the acid not being equally diffused and apt to crystallise out in patches, while the other does not lie closely enough. Boric acid cotton cannot be applied directly to the wound, requiring an intermediate piece of protective material.

The material chosen for the soldier's packet would probably also be found best adapted for the surgeons' dressing pouches, as also for those of the men of the Army Hospital Corps; it is required mainly to supplement the former, to render the wound fit for carriage to the field hospital. The surgeons employed at the first help stations and the men of the bearer companies might, perhaps, be supplied, before the commencement of an action, with disinfectant dressing material, freshly made or "vivified" at the field hospitals by some means, such as solutions of carbolic acid or of other disinfectant agents, by Port's or Bruns' powder, &c., the composition and preparation of which are given further on.

Carbolised olive-oil on lint or cotton-wool I found a most excellent dressing for wounds requiring transport during the Franco-Prussian War; but whether this is sufficiently portable for general use on the battle-field and help stations is doubtful. At the dressing station, where the surgery and store waggons are to be drawn up, abundance of antiseptic dressing materials could easily be at hand.

Now come the questions, in what form is disinfectant dressing material to be supplied to the field and other hospitals in war, and how is it to be carried?—questions for which a great variety of answers has been suggested. Are simply ordinary cotton-wool, tow, gauze, &c., to be taken, as now, in the waggons, to be freshly-made antiseptic as required by solutions of carbolic acid, chloride of zinc, &c.; or are the materials to be prepared beforehand and packed in air-tight boxes in the waggons? The two surgery waggons of our bearer company contain 5 lbs. each of carbolised tow; but it is not stated whether it is to be freshly prepared on mobilisation.* Carbolised catgut is in the equipment, as also in the waggons of the Austrian and German Armies. The Sanitäts waggons of the bearer companies of the latter, besides, carry two spray diffusers; those of the field hospitals, two more; while at the reserve hospital depôt twenty-four are found.

The German War Sanitäts Regulations of 1878 hand over the antiseptic preparation of the various dressing materials carried in the waggons to the field hospitals; adding that, as at the chief dressing station the antiseptic treatment cannot be fully carried out as a rule, the Sanitäts detachment is provided with the necessary materials for it, beyond the dressings always carried, in order to have them at hand if desirable, and if time and circumstances permit, as also, in special cases, to allow the complete method to be carried out.

* It is made by Savory & Moore, by passing carbolic acid vapour through tow.

The first equipment of the field and other war hospitals would have to be drawn from stores of ready-made dressings; these, which probably will have been prepared some time before use, might be afterwards freshened up by solutions of the various disinfectants carried in a concentrated form in the surgery waggons. Subsequent supplies would be sent freshly made to the front by the contractors, or rendered antiseptic at the theatre of war by other contrivances.

Oberstabsarzt Dr. Port, of the Bavarian Army, proposes to carry carbolised tow and other disinfectant dressings in packages containing about 1¾ pounds, one hundred of which are to be stowed away in the waggons of a field hospital. The tow is prepared by being soaked in a solution of carbolate of lime, made with carbolic acid and quick-lime, a little glycerine to prevent hardness and roughness after drying, and to fix the salt, being afterwards added. This material can be quickly dried, losing only two per cent. of the acid in the process, much less than if it alone had been used. This tow, to the weight of 1¾ lbs., is pressed into square boxes, made entirely of iron wire covered, to prevent rust, with a coating of a spirituous solution of asphalte. The wire is woven loosely, in open meshes about half-an-inch apart, into long pieces about three inches wide; out of these strips lengths are cut as required to make the cases, and united by wire or pieces of tin or sheet iron; and, when filled with the tow, wrapped up in a thick paper soaked in the asphalte solutions and thereby made air-tight. Now, not only are the contents of these wire cases of use for the treatment of wounded in the field hospitals, but the cases themselves are of the greatest value in field surgery. At the garrison hospital at Munich I had the opportunity, thanks to the inventor, of seeing the variety of useful splints of most excellent quality which can be made with the wire strips of these cases with very little difficulty. The pieces are joined together in the

most ingenious way, strengthened by means of strips of sheet-iron or tin and a few nails, and the result is every conceivable splint of any length, firm, cheap, and adapted for any emergency. Pieces of tin are almost everywhere to be found, or, if not, preserve-tins would supply the needed material. A class of military surgeons at Munich undergoing a course of operative surgery employs its time, when bodies fail, as during my visit, in making these splints, and have attained a wonderful proficiency in their present work, as shown by the large number of splints, from the largest to the smallest, from the most complicated to the simplest, lying ready for use. Excellent splints are also made there of thick cardboard soaked in the above asphalte solution; they are roomy enough to take in not only the bandaged limb, but also the trousers, which, in default of other material which cannot always be carried about, constitute a very useful cushion.

Professor P. Bruns* very strongly recommends carbolised gauze. The gauze is soaked in an alkoholic solution of carbolic acid, to which is added colophonium and castor-oil; the first fixes the acid, the oil prevents the material becoming first sticky and then hard. The place of the oil can be taken by glycerine or stearine. Dr. Bruns has succeeded in making this mixture for impregnating the dressing materials into a very concentrated form, like an extract almost. It is easily soluble in alkohol, and could be readily carried in bottles, jars, or boxes in the waggons, taking up but little room. About 125 yards of gauze would only require 3½ pints for thorough impregnation of the alkoholic solution. But to carry about the large quantity of alkohol might not be found very easy. This gauze can be prepared in half an hour and less, is much cheaper than Lister's gauze, and said to be more permanent. It adapts itself better to the inequalities of the body surface,

* Langenbeck's Archiv., volume 24, heft 2.

and causes no skin irritation; it also takes up less room than tow. Dr. Bruns has been able, under machine pressure, to pack into the same space double as much gauze, by weight, as tow. Then, again, with 2½ pounds of carbolised gauze, equal to 29 square yards, twelve amputations of the thigh can be dressed; with 2½ pounds of carbolised tow, only five.

The preparation of the concentrated mixture for war purposes is given as follows by Dr. Bruns:—

Take 400 grammes of colophonium in the finest powder, and add to it successively 100 grammes of spirit of wine and of carbolic acid, also 80 grammes of castor-oil (or 100 melted stearine). The mixture is to be stirred until it has acquired an even, powdery extract consistence, when it is at once placed in a vessel and closed air-tight. For use, this mixture is dissolved in 3½ pints of spirit by constant stirring. The gauze to be impregnated is spread out—2½ pounds—roughly, not in layers, in a large flat vessel; the mixture is poured over it and rapidly absorbed. In order thoroughly to soak the gauze, it is to be wrung out 2-3 times and replaced. Finally, the gauze is hung up to dry, but for as short a time as possible—that is, only so long that the spirit will have nearly evaporated—in summer and in the open air perhaps in 5, in winter and in a moderately warmed room in 10-15 minutes. The gauze is now ready for use; it is best stored in a closed tin box, where it will keep unchanged for months.

If the gauze is to be freshly used, the castor-oil or stearine may be replaced by an equal quantity of glycerine.

Dr. Münnich* has tested this gauze, prepared according to the above formula, with the following results.

* "Deutsche Militärärztliche Zeitschrift," February, 1880.

Time of Testing.	Castor-Oil Gauze.		Glycerine Gauze.		Stearine Gauze.	
	Lying Free.	In Parchment Paper.	Lying Free.	In Parchment Paper.	Lying Free.	In Parchment Paper.
Immediately after Preparation	6·1 per Cent.		6·3 per Cent.		6·5 per Cent.	
After 3 days	4·4	5·8	4·3	5·9	5·1	6·4
„ 1 week	3·1	5·7	2·5	5·9	4·5	6·4
„ 2 weeks	2·2	5·6	1·9	5·6	3·0	6·2
„ 3 „	1·5	5·5	1·6	5·4	1·3	6·0
„ 4 „	1·4	5·1	1·1	5·0	0·8	5·2
„ 5 „	1·0	3·8	0·9	3·9	0·6	3·5
„ 6 „	0·7	3·1	1·0	3·1	0·3	2·6
„ 7 „	0·8	2·0	0·6	2·7	0·4	2·4
„ 8 „	0·5	1·4	0·3	1·6	0·2	1·9

These differently-prepared gauzes do not differ to any great degree; there is a rapid loss of carbolic acid after the fifth week in all those packed in parchment paper. Further experiments with these gauzes when exposed to the heat of the body applied to wounds speak highest for that made with stearine. The following tables show the loss of carbolic acid in three antiseptic dressing materials when stored, as also when applied to a wound (Dr. Münnich):—

Loss—Packed in Parchment Paper.

Period.	Fixed Carbolised Tow.	Simple Carbolised Tow.	Bruns' Castor-Oil Gauze.
After 2 weeks ...	0·2 per Cent.	4·1 per Cent.	0·5 per Cent.
„ 4 „ ...	0·5 „	4·9 „	1·0 „
„ 6 „ ...	0·6 „	5·0 „	3·0 „
„ 8 „ ...	1·0 „	5·1 „	4·7 „

As Dressing on Wounds.

Period.	Fixed Carbolised Tow.	Simple Carbolised Tow.	Bruns' Castor-Oil Gauze.
After 4 days... ...	1·4 per Cent.	4·8 per Cent.	5·0 per Cent.
„ 7 „	3·9 „	5·8 „	5·4 „

This shows that the "fixed" tow has a decided advantage, not only when packed, but also when on wounds, over Bruns' gauze. This "fixed" tow is made by Dr. Münnich as follows:—

Tow	2½ lbs.
Colophonii	100 grammes.
Spiritus	1200 „
Acid Carbolic	100 „

The solution is poured over the tow and mixed with it as thoroughly as possible; the tow is then opened out for drying, and is ready for packing away in about half an hour. This dressing material is still further improved by using cleaned and bleached tow. It contains, after drying, eight per cent. of carbolic acid, and lost, packed in parchment paper, after a quarter of a year, only 2 per cent.; after half a year, only 4½ per cent.

The one objection to tow, especially to the ordinary, is that it does not lie close and is apt to loosen, and thus but imperfectly protect the wound. The above tow, however, is cheap, and was used with excellent results in General Zimmermann's corps during the Russo-Turkish War.

A "simple" carbolised tow is prepared by Dr. Münnich as follows:—One pound of the raw material is soaked in a mixture of 50 grs. of carbolic acid and 550 grs. of spirit, opened and spread out on a table; the drying is complete in half an hour to two hours, according to the season. Experiments as to its lasting antiseptic property follow.

Immediately after preparation the tow contained 8·7 per cent. of carbolic acid.

Periods.	Packed in				Lying Open.
	Tin Box.	Parchment Paper.	Writing Paper.		
After 3 days .	8·3 per Cent.	8·1 per Cent.	7·7 per Cent.		6·1 per Cent.
„ 1 week	8.0 „	7·6 „	7·3 „		4·2 „
„ 2 weeks	5·6 „	4·6 „	2·9 „		1·1 „
„ 3 „	4·6 „	4·2 „	2·1 „		0·8 „
„ 4 „	4·5 „	3·8 „	1·9 „		0·3 „
„ 5 „	4·3 „	3·8 „	1·7 „		—
„ 6 „	4·4 „	3·7 „	1·6 „		—
„ 7 „	4·4 „	3·7 „	1·1 „		—
„ 8 „	4·2 „	3·6 „	0·7 „		—

Simple carbolic tow, therefore, packed in parchment paper, remains fit for use for twelve days.

The preparations of tow have two properties which fit them particularly for use in war hospitals—their cheapness and easy and rapid mode of preparation. Packed in air-tight tin boxes, they would be especially adapted to form part of the first equipment of the surgery waggons proceeding on service.

Salicylic cotton-wool as a dressing material for war hospitals would probably be too expensive, and cannot be altogether depended upon.

Dr. Münnich closes his interesting article with the following recommendations:—

1. For the soldier's packet, for the regimental surgeons, and for the bearer companies, chloride of zinc, cotton-wool or bleached tow.

2. For the chief dressing station he advises "fixed" carbolic tow, which is especially adapted, covered with gauze bandages, soaked in carbolic acid, for wounded to be sent away.

This tow is to be prepared during the days of mobilisation, and, after pressing, to be packed in parchment paper. Fresh supplies can be easily made during the first rest days. In addition, carbolised gauze would be very desirable. For field hospitals the "fixed" carbolic tow would seem best adapted as a first equipment, for permanent hospitals the same, or the "simple" carbolic tow, which might be prepared every eight days or so, supplemented by carbolic gauze. Dr. Laué,* in the German military medical journal, after an extensive trial of Münnich's carbolised tow, speaks very highly of it:—"It protects absolutely against pyæmia, septicæmia, and hospital gangrene, tends to prevent erysipelas, renders the healing process more rapid, decreases or prevents suppuration; its use is unattended by any secondary hurtful influence, and it equals in its action Lister's gauze." For war Dr. Laué considers Münnich's carbolised tow well adapted, being without difficulty prepared in twenty-four hours by hospital servants from the raw material; it is easily kept, cheap, and portable. Of twelve amputations, six of which of the thigh, one died from already existing pyæmia; of four joint resections, and as many compound fractures, none died treated with this carbolised tow.

Dotter,† on the other hand, prefers Bruns' carbolised gauze; complete dressings of a thigh amputation he states as only about a halfpenny dearer than if Münnich's tow is used; it is more rapidly prepared; an equal quantity of gauze takes up but a quarter of the room of the other. For field service he especially recommends Bruns' concentrated mixture, the preparation of which has already been described.

Instead of the crystallised, it would be better to carry the liquified form of carbolic acid, such as Calvert's (one of water

* Dr. Roth, "Jahresbericht," 1880.
† Idem.

to nine of acid). If the bottles were marked in ounces on the outside, dilution of the contents could be more rapidly carried out afterwards.

The *St. Petersburgh Weekly Medical Journal* of 20th November, 1880, contains an article on the Neuber-Lister permanent (Dauer) dressing, by Dr. Unterberger, of the Russian Guards, who gave it a trial when in charge of the surgical division of the hospital at Krassnoje Selo during the last manœuvres of the Russian Guard Corps. The hospital, thanks to its proximity to the manœuvre ground, may be considered as a chief dressing station; and antiseptic treatment was thus, according to Esmarch's advice, pushed on to the battle-field. The surgical division, which contained a daily average of fifty-five patients, was in tents pitched in the hospital garden, with the operating-room in a hut. The strictest antisepticism was practised, and the opportunity not lost of trying the modified Lister dressing—the Neuber-Lister permanent dressing, in which the gutta-percha are replaced by decalcified bone drains. Dr. Unterberger thinks that there is a great future in military surgery for these bone drains, especially at the dressing station. If the bone drains are not able to displace altogether the older ones, there is a large field for their use; by making Lister's dressing into a permanent one, they would save much time and labour in war.

The great objection against them, their too rapid absorption often, may perhaps be got rid of by using drains made from chicken-bones, as recommended by Dr. Macewen in the *British Medical Journal* of February 5, 1881; but their price would, no doubt, prevent their general use in war.

Ordinary catgut is not always very reliable, which probably also applies to that carried in the surgery waggons of the English, German, and Austrian Armies. To avoid any

risk, therefore, it might be advantageous to provide in future only that prepared by Professor Lister's method, as given in the *British Medical Journal* of February 12, 1881, for which he claims such decided advantages over that treated on the older plan. He dissolves 1 part of chromic acid in 4000 parts of distilled water, and adds to the solution 200 parts of pure carbolic acid or absolute phenol; into it is put catgut equal in weight to the phenol. At the end of forty-eight hours the catgut is taken out of the solution and dried, and, when dry, placed in 1 to 5 carbolic oil; it is then fit for use.

The antiseptic dressing material would probably be most safely carried in the waggons, in air-tight tin cases holding one to two pounds each, or in Dr. Port's wire cases, which have so many advantages over all other contrivances. It is to be sincerely hoped that these will be exhibited during the International Congress.

The following axioms laid down by the great Russian military surgeon, Pirogoff, for the general treatment of wounds in war, are very interesting.

They are taken from Surgeon-General Dr. Roth's translation (German) of Pirogoff's work on the Russo-Turkish War.*

1. War is an epidemic of injuries.

2. The condition of wounds, the mortality, and recovery depend in the main on the kind of weapon used.

3. Not medical or surgical treatment, but administration, plays the chief part in assistance rendered to the wounded and sick at the theatre of war.

4. Not quickly-performed operations, but a properly organised and conservative treatment, is the chief object of surgical and administrative activity at the theatre of war.

* Roth, "Jahresbericht," 1880.

5. A disorderly crowding together of wounded and sick at dressing stations and in hospitals is particularly to be avoided.

6. For the same reasons, the severely wounded are to be removed as far as possible from the theatre of war.

7. Separation of the wounded, thorough ventilation, and especially the divided, and as much as possible isolated, position of the sick, are the true means of avoiding the spread of traumatic infection diseases.

8. Well-organised grouping at the dressing station and in the hospitals is the best means of ensuring proper surgical assistance.

9. Immediate removal of bullets, and the performance of primary operations, are not as necessary in present battles as was formerly ruled, and are but rarely necessary if life is not in danger. Sieges are an exception to this rule.

10. The examination of fresh gunshot wounds with probe or finger, opening them further with instruments, and the removal of bone splinters are generally hurtful, and ought only, in exceptional cases, be undertaken under surgical supervision.

11. The application of immobilising dressings, &c., especially of those with plaster-of-Paris, takes the place, in the great majority of wounds, of primary operations (amputations and resections) at the dressing station. All wounded with gunshot fractures are only to be moved with properly-applied plaster-of-Paris splints.

12. Secondary resections are to be preferred to primary, especially if an expectative treatment in gunshot wounds of joints still gives hope of success.

13. As amputations of the thigh offer but slight chances of success, all attempts at conservative treatment in these gunshot fractures, as also in those of the knee-joint, are to be considered as a progress in military surgery.

14. Suppuration in external injuries easily causes infection, especially when severely wounded are crowded together under one roof; it then becomes a danger, not only to the wounded man himself, but also to his neighbours.

15. Puriform infection not only spreads by the air, which becomes a source of danger when large numbers of wounded are crowded together in closed rooms, but also by the surroundings of the wounded—linen, bedding, walls, floors, and above all by the attendants.

16. Good ventilation of the sick-rooms does not itself prevent puriform infection when severely wounded are collected together in great numbers. Only isolation, and as far as possible separation of the houses, are, with attention to cleanliness and antiseptic treatment, a safe remedy against the spread of the various forms of puriform infection.

17. In treating gunshot wounds, the main points are—rest to the injured part by immobilising dressings and suitable position, and also prevention of decomposition changes. Cold, antiphlogistics, and low diet suit in exceptional cases; all lowering treatment is hurtful to the soldier, especially towards the end of a long war.

18. The use of anæsthetics is important, not only for operations, but also for the application of dressings, and is only contra-indicated by shock.

19. Statistics of war surgery are not reliable, and do not offer the surgeon true indications. The only conclusion which can be drawn from them is, that every injury and every operation have a certain minimum mortality, which even the modern improvements in medical science cannot further reduce.

20. Voluntary nursing forms a very important independent assistance to the medical service in the field.

Lühe* lays down the following conditions as necessary for the carrying out of primary Antisepsis in war:—

* Lühe, "Primary Antisepsis in War."

1. Occlusion of the wound by an antiseptic dressing immediately after it has been received, without any probing by finger or instruments.

2. The same treatment to be pursued at the dressing station, if the skin wound is small (bullet), even when of a joint or with fracture, if the wound does not gape or contain septic material, pieces of clothing, &c., as shown on examination without probing.

3. Bullets and bone splinters are not to be extracted at the dressing station.

4. The limb is to be dressed with some antiseptic material, after cleansing the part surrounding the wound.

5. Immobilisation is to be secured in gunshot wounds of joints by the necessary contrivances, in wounds of the soft parts by simpler means, position, &c.; the splints, &c., to be secured from wound secretions by a layer of impermeable material.

6. In large skin wounds (shell, &c.), dressing at the dressing station according to Volkmann (dry); if necessary, primary resection or amputation, with—

7. Immobilisation over the antiseptic dressing.

8. The best surgeons to be employed at the dressing station.

9. The wounded to remain as long as possible in the hands of the same surgeon.

Lühe can only recommend two first dressings—1. Esmarch's antiseptic salicylic tow pad; 2. Port's powder—and prefers to combine the two, in dressing the wound covered with the powder, with an antiseptic pad and three-cornered bandage.

This powder is prepared as follows:—

200 parts of carbolic acid, 400 of colophonium, 250 of alkohol, and 150 of glycerine. The colophonium is dissolved in the alkohol, with the aid of slight heat; the carbolic acid and

glycerine added after cooling. The powder is made from this mixture by taking one part of it and adding eight of precipitated chalk and mixing it thoroughly in a mortar. Bruns recommends the dusting of this powder over the wound, covering it with a thin layer of tow, which is also dusted over, another layer of tow, and then a piece of paraffin paper—a gauze bandage to fix the whole. Every soldier is to carry on his person half an ounce of tow, a gauze bandage, and a piece of paraffin paper.

These are some of the chief antiseptic dressing materials recommended: only the next war will set the much-disputed question at rest, as to which are best adapted for a campaign, most suited there to meet all the varied requirements, best able to bear with the least loss in antiseptic quality the rough usage, exposure to heat, cold, and wet, be most easily carried, most quickly prepared, most efficient, without adding very considerably to the cost of equipment of the individual soldier, as also to that of the various war hospitals—an outlay, however, which would be repaid many times over by the many benefits the antiseptic system would confer on the soldier, as indirectly also on the State.

The following order from the German Army Medical Department, referring to antiseptic dressing material for Army purposes, has just been published (Berlin, 8th February, 1881):—

"In consequence of reports received concerning the use of dry carbolic-spirit tow, the Army Medical Department is convinced that it has proved itself a useful antiseptic dressing, not only for use in time of peace, but especially on field service.

"The reports agree as to its easy mode of preparation,

its softness, ready adaptability in dressings, also as to its being an antiseptic material which can be depended upon when freshly prepared. As disadvantages are mentioned—its instability with the present method of packing, its aptitude as a dressing to become loose and insufficiently protect the wound, and its not soaking up the discharges equally.

"In consequence of the favourable results obtained, and the yet unsettled question as to what antiseptic dressing is best adapted for the field, the Army Medical Department considers the further employment of carbolic-spirit tow desirable, particularly as it is so readily prepared in large quantities for war purposes.

"In order to do away with the above-named disadvantages, the material is to be used as freshly prepared as possible, the method of applying it to the wounds improved, and the attempt to be made, by the use of parchment paper or mackintosh, to ensure a more equable absorption of the wound secretions. It is to be more simply prepared in future, by pouring over a pressed cake of tow, 1 kilogramme (2¼ lbs.), a solution of 2 ounces of spirit, and then wrapping it up in parchment paper. The cakes are thoroughly soaked through in ¼ to ½ hour, and ready for use.

"This method of preparation does not increase the bulk of the material, it requires but little time, the tow is thoroughly impregnated, the quantity of spirit used is small, and loss of carbolic acid by evaporation prevented, which are all points in favour of carbolic-spirit tow as a dressing in the field."

Further experiments are to be made with it, and reports sent in on 1st March, 1882.

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CASES

OF

FEMORAL AND POPLITEAL ANEURISMS.

BY

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1875.

CASES

OF

FEMORAL AND POPLITEAL

ANEURISMS.

CASE I.—*Double Popliteal Aneurism associated with Femoral Aneurism of the Right Side; Ligature of the Right External Iliac Artery; Left Popliteal Aneurism Cured by Compression.*

G. F. H., aged twenty-eight years, of healthy appearance, has suffered twice from primary venereal sores, once accompanied by a bubo in the left groin, which suppurated. Has never had rheumatism or secondary syphilis. At one time he smoked tobacco very freely. Family history not satisfactory, both parents being very delicate.

He gives the following history of himself:—Early in November, 1874, for the first time, noticed a swelling behind the left knee, accompanied by pains during the night, which disappeared towards morning. About the end of November noticed a similar swelling behind the right knee, but cannot fix upon any particular dates. On one occasion he felt considerable pain in these swellings when kneeling on a high stool in church, which position pressed the thighs down upon the calves of the legs. Did not pay any particular attention to his condition until the 5th February, 1875, having taken his usual amount of walking exercise (which was considerable) up to that date without inconvenience, when both swellings became very painful, and being on board a steamer coming

from Canada he consulted the surgeon of the vessel, who at once pronounced them to be popliteal aneurisms. While on board the steamer flexion of both limbs was carried out with a hope of consolidating the tumours, but he was unable to bear it for any length of time.

On the 20th February, having rejoined his regiment at Aldershot, pressure was commenced on both femorals, by means of weights and artery compressors, which treatment was kept up till the 24th, when he came under my observation at Netley Hospital suffering from two large popliteal aneurisms, one in each leg.

Pressure was now established on both femoral arteries by means of Carte's and Signoroni's instruments, and continued till the 4th of March with but slight intermission; by this time the tumour in the left leg was somewhat consolidated. Read's compressor, third series, was now applied over the left femoral, alternating with Carte's lower down the thigh, and on the 6th of March pulsation had quite ceased in the tumour.

At this date there was no difference in the condition of the right popliteal aneurism, though compression had been kept up very steadily on the femoral vessel since admission into Netley Hospital on the 24th of February. On the 13th of March it was observed that there was dilatation of the right femoral artery for about two inches below Poupart's ligament, which complicated matters very seriously, there being as yet no attempt at consolidation in the popliteal aneurism. Digital pressure was therefore established against the bri[illegible] of the pelvis, immediately above Poupart's ligament, and pressure by Signoroni's and Carte's instruments on the thigh; this was kept up without any benefit till the 2nd of April, when flexion of the thigh on the body and the subcutaneous injection of Langenbeck's solution of ergotine in immediate neighbourhood of popliteal aneurism were commenced. By flexing the thigh on the body as far as possible pulsation was completely stopped both in the femoral dilatation and in the popliteal aneurism. This treatment was carried out till the 26th of April without any benefit, when 20-grain doses of iodide of potassium were administered three times a day; but no benefit having been derived from this course, it was decided, in consultation with Professor Longmore and Sir James Paget, to ligature the right external iliac artery, which operation (with the valuable assistance of the above gentlemen) I performed on the 10th of May.

The operation consisted in making an incision through the

abdominal walls of five inches in length, extending from one inch above anterior superior spinous process of ilium to one inch above the centre of Poupart's ligament. The usual precautions were carefully observed; the vessel, reached with but little difficulty, was found healthy, and tied by a strong silk ligature, both ends of which were left out of the wound.

The wound was closed at the upper end with silver wire sutures, the lower end being left open, and the whole dressed with lint saturated with carbolic oil.

The limb was wrapped in cotton wool, everted, semi-flexed, and placed on a soft pillow. Recovery gradually ensued without any unpleasant symptoms, with the exception of obstinate constipation and flatulency.

The ligature came away on the thirty-fifth day, and the patient was out driving on the sixtieth day. He left Netley for Canada on the 10th of August, or three months after the operation.

Professor Parkes, F.R.S., was good enough to examine this patient's circulatory system a few days before the operation. The following is an extract from his report:—

"Heart in natural position; impulse extremely feeble. At apex first sound very short and feeble, second sound well heard. No murmur. Midway between sternum and nipple a very slight but quite undoubted diastolic murmur, not carried to apex, though it can be heard a little down the sternum. At the third left and second right cartilage it is hardly heard and often undetectable.

"Both radials rather tortuous; slightly locomotive.

"Brachials not apparently changed.

"Nothing decided about abdominal aorta; a good deal of pulsation.

"Right femoral dilated. Popliteal aneurism."

This case may be considered of some interest in giving encouragement to surgeons in ligaturing large arteries, notwithstanding undoubted co-existent disease of the general arterial system.

Case II.—*Right Popliteal Aneurism.*

Private T. C., 67th Regiment, age thirty-three years, and sixteen years service, was admitted to the Royal Victoria Hospital, Netley, on the 30th April, 1875, for aneurism of the right popliteal artery.

The following is a brief history of the case:—

While serving in Burmah, in November last, he first became troubled with sharp lancinating intermittent pains, shooting from

the inside of the right thigh down to the foot, and which gradually increased in intensity.

On or about the 1st or 2nd of December, he became conscious of a swelling in the popliteal space, which he could not account for in any way, as he felt nothing rupture suddenly in that region, nor had he previously passed through any violent exertion. The swelling or tumour soon increased in size, and the pains in the limb became more severe. On the 4th of December, according to his statement, he reported himself to his surgeon, and was received into hospital, where he remained until the 16th, during which period the treatment adopted was unsuccessful, and he was then invalided to England.

On his way home he was admitted into the hospital of the 45th Regiment, at Rangoon, and remained there from the 20th December to the 19th of January, during which period digital compression of the femoral artery was tried, besides flexion of the limb, producing, he states, a very sensible diminution in the size of the tumour, which became reduced from the size of a hen's egg to that of a walnut. On his way home from Bombay the "*flexion*" method was again adopted for nine days without any good result.

The patient, on admission to Netley, was in very good general health, had a good family history, with the exception that he stated that a brother of his was seized with illness suddenly while at dinner, became insensible, fell off his chair, and expired in the space of two hours. There was no history of syphilis or rheumatism. The day after his admission the circumference of the joint over the tumour exceeded that of the sound side by half an inch, and the tumour itself on being felt appeared to be about the size of a hen's egg.

On the 2nd of May the patient was placed in position in bed; the groin shaved and sprinkled with French chalk, the limb slightly flexed and everted, and Read's tourniquet, third series, applied, so as to compress the femoral artery against the brim of the pelvis, and when the skin became painful, alternated by Carte's tourniquet placed over the artery in the middle of the thigh. The pressure was so adjusted that a very slight pulsation was permitted in the tumour, and kept up from 10 a.m. until 7 p.m., under the superintendence of various medical candidates, and then left off for the night. He was also placed on half diet without stimulants or other extras.

On the 3rd of May, at 7 a.m., the tourniquets were again applied, substituting Signoroni's for Carte's, but on account of the pain

produced by the former, the latter had to be re-applied, and pressure was kept up to 7 p.m. After the removal of the tourniquets he experienced a pricking sensation on the inside of the joint, but did not suffer any other inconvenience. The instruments were re-applied the next day (4th May) under the same conditions, and at 6.45 p.m., while changing the tourniquets, pulsation was strong and distinct in the tumour; but on removing them at 7 p.m. *pulsation had quite ceased*. While the tourniquets were applied considerable pain was produced by pressure; and the pricking sensation previously complained of in the inside of the knee-joint continued after their removal. The next morning at 7 the swelling was found to be much smaller, quite hard and devoid of pulsation, and on measuring the limb it was found that the circumference had increased half an inch, probably from the pressure used. The temperature in the limb diminished somewhat, and it was in consequence wrapped in cotton wool and kept in position.

On the 5th of May the instruments were re-applied for occasional pressure for four hours, and then removed, but pain was still complained of over the knee-joint, and down the course of the posterior tibial artery as far as the foot.

8th May.—He was able to get up and sit by the fire, but was not allowed to walk about for three weeks afterwards.

The comparatively rapid cure in this case was, no doubt, assisted by the patient's strict attention in carrying out the instructions for his treatment, though, of course, it was mainly due to the well-regulated pressure kept up by the instruments employed in the case. The pressure was continued over three days, and altogether for thirty-three hours, as shown by the following analysis. The pressure on the 5th of May was only supplementary, and exerted no influence on the case:—

Date	Pressure Applied	Pressure Removed	Instruments used	Hours per Day
1875	A.M.	P.M.		
May 2	10 0	7 0	Read's and Carte's alternately - -	9
„ 3	7 0	7 0	Read's, Carte's, and Signoroni's - -	12
„ 4	7 0	7 0	Do., do., do. - -	12
			Total,	33

Case III.—*Left Popliteal Aneurism.*

A. M., 32nd Regiment, aged thirty-two, service ten years, a tall, well-developed man, of temperate habits, has had primary syphilis, but no secondary symptoms. There is no history of rheumatism.

The present disease is traceable to an injury he received at King-williamstown in January, 1875. While walking along a bad road at night he tripped in a rut, and was sensible that something had given way at the back of the thigh, but as he found nothing wrong nor suffered pain, he thought lightly of the matter.

Early in the following March he began to feel pains in the left knee and calf of leg, which obliged him to seek relief, and when in the act of rubbing in some liniment for these pains, he became cognisant of the fact that there was a pulsating tumour behind the knee-joint. He reported the circumstance, and was admitted to hospital, where he states he was kept at rest, given low diet, and ordered large doses of the iodide of potassium. Treatment by flexion was commenced, and continued (during the day-time) for eight days; but as this did not succeed, digital compression was resorted to, and kept up for twenty days, with no better result than an apparently slight diminution in the size of the tumour, which is stated to have been about as large as a hen's egg.

He was then invalided to England, and arrived at Netley on the 22nd June, 1875. The following was his condition on admission:—Patient extremely nervous and excitable, heart's action rapid, and every artery in the body pulsating strongly.

In the left popliteal space a pulsating tumour, about the size of a hen's egg, was found. Upon placing the hand over it, a strong, heaving, and expansile pulsation was communicated to it, and upon auscultation a bruit was heard. Firm pressure on the femoral artery completely arrested the pulsation.

The patient was ordered to observe perfect rest for a few days, given low diet, and grs. xv. of iodide of potassium three times daily.

On the morning of the 26th of June treatment by compression was commenced.

The groin being shaved and well dusted over with French chalk, Read's compressor, third series, was applied over the femoral artery, below Poupart's ligament, and at the lower part of Scarpa's triangle Carte's tourniquet was adjusted. These instruments were used alternately, and the patient instructed how to change them whenever one or other of them began to cause pain from pressure,

and in such a manner as not to completely arrest the flow of blood in the vessel—the object being to cause coagulation in the sac. The instruments were removed at 7 p.m., having been on for seven hours and three quarters. A dose of Battley's sedative was given at bed-time, and the man was allowed a good night's rest.

On the 27th, pressure was continued in the same manner from 7 a.m. until 7 p.m., the patient being easy and cheerful the whole time.

June 28th.—The tumour feels harder and the pulsations less forcible; slight pains in the knee and tumour during the night. Pressure re-applied and continued as before. Towards evening the limb became slightly œdematous; but this disappeared when the pressure was removed.

29th.—Shooting pains in the tumour and knee complained of. Pressure put on at 7 a.m. At 10 a.m. the groin was so painful that Read's instrument was removed, and Signoroni's applied lower down on the thigh; but not being found to answer, it was taken off in about an hour, and the former instrument re-arranged with a soft pad under it. A vessel can be seen and felt over the inner condyle. Instruments taken off at 6.30 p.m., when the sac was found to be very hard, and the pulsation in it almost gone. A good deal of pain, of a burning character, was experienced until midnight, when it was found that all pulsation had ceased in the tumour. The pain gradually wore off, and he slept well until morning.

30th.—All pulsation stopped; collateral circulation established; limb rather cold and numb. To be wrapped in cotton wool and flannel. Pressure moderately applied during the day.

Date	Pressure Applied	Pressure Removed	Instruments used	Hours per Day
	A.M.	P.M.		H. M.
June 26	11 15	7 0	Read's and Carte's alternately every quarter of an hour	7 45
„ 27	7 0	7 0	Do.	12 0
„ 28	7 0	7 0	Do., and Signoroni's for an hour only	12 0
„ 29	7 0	6 30	Read's and Carte's	11 30
			Total hours,	43 15

31st.—Treatment discontinued. Patient to remain in bed for some days.

Mr PORTER ON FOREIGN BODIES IN THE BLADDER.

SURGICAL REPORTS.

BY

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SURGICAL REPORTS.

I.—TIGHT ORGANIC STRICTURE OF THE MALE URETHRA; A LARGE PORTION OF A CATHETER LODGED IN THE BLADDER; THE STRICTURE FORCIBLY DILATED; THE FOREIGN BODY EXTRACTED; RECOVERY.

II.—A PORTION OF A GUTTA-PERCHA BOUGIE BROKEN OFF IN THE BLADDER, CALCULOUS FORMATION AROUND IT; A MODIFICATION OF "ALLARTON'S LITHOTOMY;" RECOVERY.

III.—NINE INCHES AND A HALF IN LENGTH OF A GUM ELASTIC CATHETER LODGED IN THE MALE BLADDER, SURROUNDED BY PHOSPHATIC DEPOSIT; A MODIFICATION OF "ALLARTON'S LITHOTOMY;" RECOVERY.

IV.—PATIENT AGED TWENTY-SIX YEARS; A SMALL LITHATE CALCULUS, WITH AN OXALATE OF LIME NUCLEUS; LITHOTRITY; RECOVERY.

V.—PATIENT AGED FIFTY-ONE YEARS; SMALL LITHATE CALCULUS; LITHOTRITY; RECOVERY.

ALTHOUGH substances of the most varied forms and composition have been found in the female bladder, demanding surgical interference for their removal, few foreign bodies are met with in the

male viscus, with the exception of portions of bougies, or catheters. The extraction of these from the female is an easy procedure compared with their withdrawal from the male patient. The explanation of this is simple, when we consider the anatomy of the urethra in women, its great dilatability, and its short and slightly curved course. The length of time a foreign substance has remained in the bladder influences, in a great measure, the surgeon in bringing his practical knowledge to the aid of the sufferers. The knife, or lithotrite, is usually required when the substance has continued long enough to become the nucleus of phosphatic deposit. When called upon to relieve a male patient suffering from a foreign body in the urinary bladder, the practical surgeon must earnestly consider all the features of the case before rashly proceeding to any operation. He should endeavour to ascertain the nature of the substance, whether tough or brittle, its shape, the length of time it has lodged in its unnatural situation, and, lastly, the condition of the bladder as affected by its presence. No doubt, in some cases where the urethra is large, and the foreign body small, it may at once be seized by a lithotrite and taken out, or if friable it may be broken up, and allowed to be washed away with the urine, like a calculus. This is on the supposition that the state of the bladder is healthy. But, on the other hand, if it be tough, and incapable of being acted upon by the lithotrite, and that it is lying in a viscus inflamed and irritable, then the surgeon should not delay to remove it by the knife. For the removal of foreign bodies which cannot be extracted along the urethra, lithotomy through the middle line of the perinæum is, *par excellence*, the operation to be chosen. The highest authorities agree on this point. Mr. Erichsen says—"This is more safely done by the median than by the lateral operation of cystotomy." Ashurst states, when treating of foreign bodies in the bladder—"In the male, however, it is usually necessary to resort to lithotrity (if the nature of the body admit of its being crushed), or to lithotomy, the *median* being in such a case the preferable operation." Again, Mr. Bryant writes—"In adults, possibly, the median operation may be chosen when the foreign body is not large."

There is a difference of opinion among surgeons as to the best way of reaching the staff, and by it the bladder, in central lithotomy. Allarton plunges his knife at once, with its back to the rectum, into the membranous portion of the urethra, placing the point of the bistoury half an inch in front of the anus. In the old "marian"

operation a cut was made on the right or left of the raphe upon the staff, beginning below the scrotum, and terminating an inch above the anus. Sir Henry Thompson says—"Respecting the modes of making the incisions, I prefer dissecting from the skin inwards to the staff, as in other operations, to the method by transfixion." He does not mention where he begins his incision through the skin.

Mr. Erichsen recommends a rectangular staff, resembling Buchanan's, to be used, and considers that "the surgeon can judge of the exact point where to enter the knife (directing it so as to open the groove just below the angle) which he cannot do with the curved staff."

I consider the recommendation of Sir H. Thompson to cut from the skin inwards most useful, particularly when the perinæum is deep. In such a case the whole blade of the knife would be swallowed up if driven to the staff, according to Mr. Allarton's advice, and the heel of the blade would be brought to bear on the tough skin of the raphe; whereas the free division of the integument, in the first instance, facilitates the further separation of the structures to be cut through.

The operation I have four times successfully performed, and which I take the liberty of suggesting to others, is a combination of Allarton's and Thompson's; but differing slightly from both. It is briefly described as follows:—The staff, grooved on its convexity (and as large as possible) having been introduced, kneeling in front of the patient, I insert the forefinger of my left hand into the rectum with its palmar surface upwards, and fix it against the staff at the apex of the prostate gland. I then push Allarton's bistoury about a quarter of an inch in depth with its back to the bowel, half an inch anterior to the anus in the raphe, along which I then cut one inch and a half in length towards the root of the scrotum, freely dividing the skin, and superficial fascia. This having been done, I return the point of the knife to its first starting place, and quickly plunge it to the staff at the apex of the prostate, making sure that I have freely opened the urethra, by moving the bistoury in the groove of the staff. I then cut forwards to the extent of the superficial incision. Still keeping the staff steadied with the point of the finger in the bowel, I introduce a long steel probe along the groove of the instrument into the bladder. Then having withdrawn the finger from the rectum, I introduce it on the probe, and dilate the prostate by insinuating the finger with a rotatory motion.

This being accomplished, the forceps is passed in on the finger, the foreign body caught, and at once extracted. The following cases of foreign bodies in the bladder, and the surgical exertions for their removal, may be interesting to the profession:—

CASE I.—*Tight Organic Stricture of the Male Urethra; a large Portion of a Catheter lodged in the Bladder; the Stricture forcibly dilated; the Foreign Body extracted; Recovery.*

T. C., aged forty-three years, a married man, had suffered for three years from tight organic stricture of his urethra about four inches from its orifice. It was caused, apparently, by an injury received nine years before. He had it dilated on several occasions by different modes of treatment, but it always showed a great tendency to close again. He had been three times attacked with complete retention of urine, and was with great difficulty relieved. He was in the habit of introducing bougies himself, and allowing them to remain in the passage for some hours, and for the last two years felt satisfied with his own treatment without applying to a surgeon. He was never able to force a bougie larger than No. 4 through, or rather into the stricture.

On the 28th June, 1872, he came under my observation, and gave the following details:—He had cut off about five inches and a-half of a French bulbous catheter, which he forcibly pushed into the stricture, where he left it so tightly grasped, that he thought it impossible for the instrument to come out, or, on the other hand, to glide backwards into his bladder. He went out for a drive of two hours' duration, and on returning home discovered to his great alarm that, instead of remaining as he had placed it, the piece of catheter had worked its way through the stricture, and lay partially in his urethra and bladder. I thus found him in a state of great anxiety, complaining of acute pain in the region of the bladder, with a constant desire to pass water, which dribbled away on each exertion to avoid it. Pressure on his perinæum gave intense agony. Having endeavoured to calm his mind, I had him placed immediately in a warm bath, and administered a draught containing tincture of hyoscyamus, liquor potassæ, laurel water, and camphor mixture. After remaining in the bath for twenty minutes I had him removed to bed, and told him I would endeavour to extract the piece of the catheter early the following day. He was desired to repeat the warm bath in the meantime, as also the draught at ten o'clock p.m.

Here was obviously a most anxious case, and one that called for the best consideration prior to the adoption of operative measures. The stricture forbade the introduction of a forceps or lithotrite, to seize the foreign body, and the same difficulty was a bar to my passing a fair-sized staff, on which I might cut into the bladder. After having carefully weighed all the difficulties, I determined in the first instance to dilate the stricture, and then with a lithotrite catch and extract the piece of the catheter; but should I fail in this, at once to cut into his bladder and take it out.

June 29th.—Assisted by my friends Mr. Wharton and Dr. M'Cormick, I had the patient placed fully under the influence of chloroform; I then introduced "Holt's Dilator," and burst the stricture; on the withdrawal of the dilator only three or four drops of blood followed. I then passed a No. 9 silver catheter, to be certain that a way sufficiently large was made for a lithotrite, and also to push on the foreign body fairly into the bladder, should it be in any degree lodged in the prostatic portion of the canal. The stopper was allowed to remain in the catheter, to prevent the urine escaping, and as I passed the metallic instrument I distinctly felt the offending substance. I then rapidly withdrew the catheter, and replaced it by a small-sized lithotrite, which was with the greatest facility introduced. With this I felt the piece of the catheter lying to the left side of the patient's bladder, and having opened the blades of the lithotrite, I was fortunate enough to grasp it and extract it slowly, but without difficulty. I then drew off his urine, introduced a quarter grain morphia suppository into the rectum, and placed him comfortably in bed, with warmth to his feet.

June 30th.—He had a quiet night, and was free from vesical irritation. From this date he made a rapid recovery, and was about his business the sixth day after the operation. Probably no case could be presented more suitable to the "immediate plan" of treating stricture, and certainly the scientific course to adopt was the one which I pursued previous to resorting to the extraction of the foreign body by a cutting operation. The method I adopted was crowned with the most gratifying success. Plate IX., Fig. 2, gives a good representation of the piece of the catheter removed.

CASE II.—*A Portion of a Gutta-Percha Bougie broken off in the Bladder; Calculous Formation around it; a Modification of "Allarton's Lithotomy;" Recovery.*

J. M., aged twenty-five years, was admitted into the Meath Hospital under my care, May 8th, 1870. He had suffered from organic stricture for more than twelve months, and had been in the habit of treating himself by introducing bougies. About seven months previous to admission he had constructed a bougie from a piece of gutta percha, and on the first occasion that he passed it into his bladder a portion about three-quarters of an inch in length broke off when he was withdrawing the instrument, and this subsequently became the nucleus of a stone. When he came under my observation he was suffering from all the symptoms of a calculus in the bladder, and his health was greatly broken down. He got very little rest, his urine was loaded with pus, and contained blood. A No. 5 sound was the largest that the stricture would admit, and with this the foreign body was easily found. The contracted state of the urethra, and the highly inflamed condition of the bladder in a man so debilitated, prohibited the idea of removing the stone and foreign body with a lithotrite. I determined to cut it out.

Operation.—May 13th.—His rectum having been cleared out by a tepid water enema, at 10 o'clock a.m. he was brought into the operation theatre, and put fully under the influence of chloroform. He was quickly tied, and held in the usual position for lithotomy. A No. 5 staff grooved on its convexity was introduced, and firmly held by my colleague, Mr. Wharton. Then, kneeling, I inserted the forefinger of my left hand into his rectum with its palmar aspect upwards, and its point steadily pressing on the staff at the apex of the prostate gland. I then pushed Mr. Allarton's knife with its back towards the bowel a quarter of an inch deep, striking the perinæum half an inch in front of the anus. I then cut forwards along the raphe to the extent of one inch and a half, dividing the skin and superficial fascia. I next fixed the point of the bistoury where I commenced my incision, and plunged it (with the cutting edge still away from the rectum) deeply until I lodged its point fairly in the groove of the staff at the membranous portion of the urethra. Having made certain that I had safely entered it, I cut forwards to the extent of the superficial incision. I then laid aside my knife, but still keeping my left forefinger in the bowel,

I passed in through the wound a long steel probe which I caused to glide along the groove of the staff into the bladder, proving that I was in the proper course by striking the end of the probe sharply against the stop at the extremity of the staff. And here let me recommend in the strongest manner the advantage of using a staff constructed with this abrupt resisting point in every mode of lithotomy, whether the surgeon hits it with the point of his knife in the lateral operation, or with the end of the probe as in the procedure under consideration; it proclaims that he has travelled the direct route, and is safely in the bladder. I then had the staff removed, and at the same time withdrew my finger from the rectum. Still firmly holding the probe with my right hand, I inserted my left forefinger (which had served whilst in the bowel to direct the knife in safety to the staff) along the probe into the bladder, and by rotating it two or three times I largely dilated the prostate. I felt the foreign body lying at the bottom of the viscus, below the level of my finger. I then took out the probe, and on my finger insinuated a small lithotomy forceps with which I quickly seized the stone, and brought out a large portion; it broke, however, under the grasp of the forceps, so that I was obliged to remove the remainder of it by a second introduction of the instrument. I was aided by a careful washing out of the bladder by means of a strong syringe. The patient was now placed in a well-warmed bed, and had a morphine suppository passed into his rectum, and directions were given to keep him as dry as possible by placing folded sheets under his buttocks as often as required. The amount of bleeding was remarkably trifling, and the wound needed no dressing.

May 14th.—He had a most refreshing night, was free from the constant calls to micturate which disturbed him before the operation; his urine flowed freely through the wound, and his pulse counted only eighty; he did not suffer from any febrile symptom.

May 16th.—He expressed himself as greatly relieved, all feeling of bladder irritation had subsided, and the urine passed constantly through the wound. It would be tedious to relate the daily condition of the patient; he improved in health rapidly, the urine began to flow through the urethra on the sixth day after the operation, the wound closed in the most healthy and satisfactory manner, and he left the hospital cured four weeks after the removal of the foreign body.

A very good sketch of the piece of the bougie surrounded by the calculous formation is given in the lithograph, Plate IX., Fig. 3, by Forster & Co., drawn by his accomplished artist, Mr. Tomsohn.

CASE III.—*Nine inches and a half in length of a Gum Elastic Catheter lodged in the Male Bladder and surrounded by Phosphatic Deposit; a Modification of Allarton's Lithotomy; Recovery.*

J. T., aged thirty-eight years, by trade a plumber, was admitted into the Meath Hospital, under my care, November 28th, 1872. He had suffered from stricture of the urethra for two years previous to his admission into hospital, and had himself been in the habit of passing bougies and catheters occasionally during the last eighteen months of that period. Nine weeks prior to coming under my notice, in withdrawing a gum elastic catheter (No. 6), which he had introduced, it broke across, leaving one inch and a half of the instrument between his fingers and thumb. The larger portion he immediately tried to take out of his urethra, but failed in the attempt, and, in about an hour afterwards, it slipped into his bladder. For two days subsequently he felt no annoyance, and was able to follow his usual occupation. On the third day, however, after the accident, he felt great pain during, and at the termination of, micturition in his glans penis; his urine was bloody, and any sudden motion increased his suffering. He placed himself under the care of a surgeon who commenced to dilate the stricture before attempting the removal of the foreign body; but the patient became anxious for speedy relief, and left the hospital while he was undergoing treatment. He then applied to my friend Dr. Trimble, of Castlebellingham, who detected the foreign substance in his bladder, and recommended him to come to me.

On admission to the hospital, I found him suffering from frequent calls to make water, so often as ten times during the night. His urine was bloody, and contained pus and ropy mucus. One small piece of the exterior of the catheter had been expelled, and numerous fine fragments of phosphatic deposit also came away.

November 29th.—I sounded him with Sir Henry Thompson's sound, and soon felt the catheter which, on being struck, gave the sensation as if it were more or less encrusted with calculus matter. I ordered him to remain in bed, and prescribed sedative medicine, with diluent drinks. In consultation with my colleagues, it was considered a case not suitable for an attempt to extract the foreign body by the urethra. The catheter, it was considered, might break (if caught in a lithrotrite) into pieces, which could not be washed away with the flow of urine, but remain to form the foundation of other deposits. And even if it did not

thus become disintegrated, it could not be drawn through the urethra in its coated condition without lacerating the canal to a dangerous extent. The highly inflamed state of his bladder also prohibited such instrumental interference. I therefore decided upon performing the operation which I have above described, and which I strongly recommend. I operated on the morning of December 4th, and removed the portion of the catheter, a faithful delineation of which from the pencil of Mr. Tomsohn, and lithographed by Forster & Co., is to be seen, Plate IX., Fig. 1.

The operation was done under the influence of æther, which was kindly administered by my colleague Mr. Smyly, in a very efficient manner.

The patient made a very satisfactory recovery. The urine passed through the penis on the fifth day, and he left the hospital, with the wound healed and in fine health, on the 7th January, 1873.

Remarks.—The accident of a catheter or bougie slipping backward into the bladder is by no means so uncommon as is generally supposed; and every surgeon should carefully examine instruments prior to using them. Age renders them brittle and unfit; and, even when their condition is sound, the utmost precaution should be adopted (when allowing them to remain in the urethra) to secure them in such a manner that they cannot recede, or glide forwards from the canal. When a portion of a soft bougie or catheter is for a short period lodged in the bladder, it is not an easy procedure to feel it with a sound; and this fact is alluded to by Mr. Fleming, a gentleman who has devoted much attention to injuries and diseases of the urinary organs, in his description of a case in which a bougie—which had been introduced and left in the urethra—escaped into the bladder.—*Dub. Hosp. Gazette,* Sept. 1, 1858. A very interesting case is reported by my friend, Mr. William Stokes, Professor of Surgery in the Royal College of Surgeons, in the first number of the *Irish Hospital Gazette.* This was a case of the removal of an *entire* gum elastic catheter from the bladder by Allarton's operation.

CASE IV.—*Patient aged Twenty-six Years; a small Lithate Calculus, with an Oxalate of Lime Nucleus; Lithotrity; Recovery.*

H. B., a cavalry officer, aged twenty-six years, consulted me November 15th, 1871. He had suffered for fourteen months previously from great pain after micturition, and irritability of the

bladder. Whenever he rode on horseback his urine became bloody, and even when blood was not apparent to the unaided eye the fluid had a smoky hue, and was turbid. Prior to this date he had enjoyed excellent health, and was able to take the most active exercise, in addition to the ordinary duties of his regiment. He was now obliged, however, to go continually on the sick list, feeling himself disabled for duty, and his sufferings were so great that he contemplated throwing up his profession. He had been under the care of three different surgeons, who from time to time prescribed medicines calculated to alleviate the irritable condition of his bladder, and he had been sounded by each of them without detecting the presence of a stone. He had a capacious urethra, and bore very well the introduction of an instrument. I passed Sir Henry Thompson's sound into his bladder with ease, and after turning its beak from side to side without striking the calculus, I at last hit it on turning the point of the sound down behind the prostate close to the neck of his bladder. The click which was given led to the supposition that its formation was hard, but that its size was not considerable. In this examination I was ably assisted by Surgeon-Major Gilborne, who had given his opinion that the gentleman suffered from stone. And here I may say a word respecting Sir H. Thompson's sound, which I consider every surgeon who treats urinary diseases should have in his possession. I believe many stones have been discovered by its use which would have escaped the search made with the ordinary sounds constructed with large curves. A calculus of small size may lie undiscovered beneath the long curve of an instrument, while the short bend of Thompson's sound can hardly fail to find it when turned completely round in the bladder. I should add that the cylinder attached to the handle not only gives a most convenient hold of the instrument, but intensifies the noise if it strikes the foreign body, even in the most gentle manner. I told the patient to remain in the recumbent position as much as possible, and I prescribed a mixture containing tincture of hyoscyamus, four drachms; laurel water, two drachms; camphor mixture eight ounces—to take an ounce every third hour; whilst I desired that he should drink every day a pint of decoction of triticum repens.

November 18th.—*First Sitting.*—Having placed him on a low bed, with a hard hair mattress under him, two assistants held his legs flexed in the manner usually fixed for lithotomy. I introduced Sir Henry Thompson's flat-bladed lithotrite, and soon caught the

stone, which measured half an inch in the diameter which was seized. I crushed it once, and withdrew the instrument. He bore the operation well. I then put a quarter grain morphine suppository into his rectum, and gave him a glass of champagne. He was instructed to remain on his back in bed, and to make water in that posture; to have plenty of oatmeal tea as a drink, and chicken broth during the day. I visited him on the evening of same day, and was told that he felt relief, and was less frequently obliged to micturate. The urine was not bloody, and no detritus had come away. I then ordered him a draught containing twenty grains of hydrate of chloral.

November 19th.—He had passed a restless night, not from pain or vesical irritation, but he felt nervous and wakeful, and a small quantity of *debris* had been expelled, weighing five grains. He was ordered to remain quiet in bed, and to partake of the same diet as the day before, the chloral draught to be repeated at bedtime.

November 20th.—He had a good night, and felt greatly refreshed. This morning I washed out his bladder with Clover's apparatus, but with little effect, the amount of detritus being almost *nil*.

November 22nd.—*Second Sitting.*—I used the same instrument as at the first crushing, and caught two fragments, each measuring about a quarter of an inch in diameter, and I broke them with ease. The lithotrite on this occasion brought out between its blades a large quantity of pulverized stone. On visiting the evening of this day I found that a large amount of debris, weighing twenty-two grains, had passed out. The greater portion came away with a rush on his making water, about two hours after the operation. The chloral draught was taken again this night.

November 26th.—He felt very much relieved, having slept soundly, but no fragments passed away.

November 27th.—I explored his bladder with Thompson's sound, and was unable to feel any piece of calculus. I then permitted him to sit up in an arm-chair, and to have some chicken for dinner, with a glass of champagne.

November 28th.—He had a good night, and no detritus came away.

November 30th.—He was so well, and free from all irritation, I allowed him to go to Bray for change of air, where he remained for ten days.

December 11th.—He returned to town greatly improved in

general health, but his urine was not clear, and he felt a sharp pain at times whilst expelling the last drops of water. I then sounded him again under the influence of chloroform, assisted by my friends Mr. Wharton and Surgeon-Major Gilborne. On this occasion I used a flat-bladed lithotrite in sounding, as I could with ease, and at once, crush any fragment, and as I should escape, by its means, the trouble and consequent irritation of introducing the second instrument. I was unable to find any piece of calculus.

December 19th.—*Third Sitting.*—Up to this date he progressed very well, was able to drive out every day without pain, and retained his water for four hours and a-half. There was no hæmaturia, but the urine continued rather opaque, and he noticed occasionally a sudden check in the stream. I made him empty his bladder, and then introduced a small flat-bladed lithotrite, and almost instantly caught a small fragment, and reduced it to powder. This piece measured one-eighth of an inch, as indicated on the scale of the lithotrite, and was brought out in the jaws of the instrument. From this date the urine became perfectly transparent, all symptoms of irritation ceased, and my patient made a rapid recovery. In a letter dated January 4th, 1871, he says, "I feel now perfectly well, and rode out with my regiment for two hours this morning." The weight of all the detritus preserved was twenty-eight grains. A small triangular nucleus of oxalate of lime was found in the debris passed on the 22nd of November.

Case V.—*Patient aged Fifty-one Years; Small Lithate Calculus; Lithotrity; Recovery.*

W. S., aged fifty-one years, had suffered from frequency of passing water for three months previous to coming under my observation. The first symptom of irritation he perceived was immediately after a sudden exertion in pursuing a run-away horse. He then expelled bloody urine, and felt great uneasiness in his bladder, with pain at the extremity of his penis. He was healthy-looking, and passionately fond of horse exercise, but latterly was unable to ride for the space of half-an-hour without dismounting to micturate. His urine had a smoky tinge when the red colour subsided.

November 7th, 1871.—Assisted by Mr. Wharton, I sounded him, and detected a calculus, which from the click imparted by striking Sir Henry Thompson's sound against it led us to infer that the stone was tolerably hard, but of small size.

November 9th.—*First sitting.*—I placed him in the position for lithotrity, on his bed, with his buttocks raised by means of a pillow. He had been directed to retain his water for two hours. I then slowly introduced Thompson's flat-bladed lithotrite, slightly heated and well oiled, and soon felt the stone lying at the right side of his bladder. I immediately separated the blades of the instrument, easily caught the calculus, and crushed it. I found it so difficult to screw home the male blade that it was evident a large quantity of detritus lay between the blades, and, without attempting to break a second piece that day, I withdrew the instrument. In so doing a good deal of pain was produced by its increased size, particularly at the orifice, which I was obliged to slightly lacerate in extracting the beak of the lithotrite. I placed a quarter grain morphine suppository in his rectum, ordered him a glass of champagne, and to have oatmeal tea to drink *ad libitum*, with chicken jelly every third hour.

November 10th.—He passed a good night; no fever; several small fragments came away with the urine, which he expelled in the recumbent position.

November 11th.—Very little debris found, but he had a tranquil night.

November 12th.—He had more frequent calls to make water, and a small amount of detritus got rid of. Ordered to have a chloral draught at night.

November 13th.—He slept well, and felt most refreshed. A good deal of debris, chiefly in fine particles, passed off. From this date to the 17th he was able to sit up, and had but slight inconvenience; his urine, however, was opaque, and he felt occasionally at the end of micturition a sharp sting.

November 17th.—*Second sitting.*—I passed a small-sized flat-bladed lithotrite, and seized a fragment measuring a quarter of an inch. I reduced it to powder, and brought it out between the blades.

November 25th.—I washed out his bladder with Clover's instrument; but the result was but a trifling amount of debris.

November 27th.—*Third sitting.*—I passed in the flat-bladed lithotrite, and after searching for a minute or two, I seized a fragment of stone by turning the curve of the instrument downwards. It measured one-eighth of an inch. I screwed home the lithotrite, and brought out the disintegrated particles in the female blade.

November 29th.—The patient felt very much improved in

health. Since the last crushing he had passed a considerable quantity of detritus, along with two large fragments, one weighing four grains, the second two grains and a half.

December 1st.—He was allowed to go out for a short walk. On his return, in less than an hour, he felt great urgency to pass water, and whilst so doing two large pieces of calculus were expelled with force. It should have been noticed that up to this date his urine never became perfectly limpid.

December 9th.—*The fourth and last sitting.*—I introduced the flat-bladed lithotrite again, and grasped a fragment which the index of the lithotrite marked to be less than one quarter of an inch in the diameter caught. It was immediately reduced to fine detritus, and brought out between the blades of the instrument.

After this operation his urine daily became transparent. He lost all vesical irritation, and left Dublin quite well.

On the 8th January, 1872, I received a letter from him in which he states—"I never was better in all my life, no stony symptoms of any sort remaining, and the water still continues perfectly clear. I have given it a fair test, having been constantly hunting since." The aggregate weight was thirty-six grains. A remarkable feature in this case was that although the stone had increased to a considerable size, still the patient had only been suffering from irritation of his bladder for two months previous to its detection.

CLINICAL LECTURES

ON

ORTHOPÆDIC SURGERY,

DELIVERED AT THE PHILADELPHIA HOSPITAL,

BY

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Nos. I, II.

THE ETIOLOGY, MORBID ANATOMY, VARIETIES AND TREATMENT OF

CLUB-FOOT.

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THE ETIOLOGY, MORBID ANATOMY, VARIETIES, AND TREATMENT OF CLUB-FOOT.

LECTURE I.

GENTLEMEN: Every observant person, whether within or without the profession of medicine, must be impressed with the prevalence of various deformities in individuals of the human family, and their very existence must cause him to reflect upon the nature of their being, their chronic and progressive character, and the apparent difficulty of arresting them. Much of this has been due to lack of knowledge of the mechanical factors which enter into their etiology, and the very general neglect the subject of malformations has received, from an educational standpoint, in our medical schools. Though twenty-five years have elapsed since the establishment of orthopædic surgery as a legitimate special branch of surgical art, its science and practice, noticeably in this city, are permitted to occupy a very subordinate position, and its principles practically are untaught. In this clinic, which will initiate the course of lectures upon orthopædic surgery in the Philadelphia Hospital, which it will be my privilege to deliver before you this Spring, no better subject could be chosen than one descriptive of *club-foot*, a condition which you will frequently meet in practice, and of which many examples may be constantly observed in the nervous, obstetrical and surgical wards of this hospital.

We may define club-foot, or talipes, which latter designation was first employed, about thirty years ago, by William J. Little, of London, as a deformity of the foot, caused by paralysis, permanent spasm, or structural shortening of the

muscles, contractions of fasciæ or ligaments, and resulting in an alteration of the normal relations of the tibio-astragaloid articulation, or between the bones of the tarsus proper. Under the generic term club-foot, or talipes, we include all deformities of the foot which occur on an antero-posterior or transverse plane, and which are characterized by flexion, extension, inversion or eversion.

To obtain a clear conception of the deformities under consideration, it is best to divide the foot into an anterior and a posterior portion, the former, the "*pes*," or foot proper, and the latter, the "*talus*," or ankle. These portions articulate at Chopart's joint, which is formed by the astragalus and os calcis behind, and the scaphoid and cuboid in front. For purposes of clinical study, club-foot is most conveniently separated into two classes, composed of the simple and the compound forms. Of the former there are four varieties, two between the tibia and foot, namely, *equinus*, in which the heel is raised, the foot being held in the extended position, the patient walking upon the ball of the foot; and *calcaneus*, its opposite, in which the patient walks upon the heel, the foot being drawn into the position of flexion. There are also two lateral deformities: *varus*, in which the internal border of the foot is elevated, the sole directed inward, and the anterior portion of the foot adducted; and *valgus*, its opposite, in which the outer side of the foot is raised, and the sole everted. Any combination of these simple varieties will present compound forms, such as *talipes equino-varus*, *equino-valgus*, *calcaneo-valgus*, etc.; some authors have added others: for instance, *talipes cavus*, in which the arch of the foot is increased, and *talipes planus*, or spurious valgus, in which the foot is flattened, the arch resting upon the ground. Recently, Shaffer, of New York, under the title *non-deforming club-foot*, has described a class of cases in which there is little or no deformity, but which are very important on account of the inconvenience they occasion the sufferer, and the results to which they give rise, coupled with the liability of being overlooked, unless care be taken in the examination of the patient.

The varieties of club-foot may be classified as follows:—

TABLE NO. I.

- Varieties
  - Simple, ...
    - Antero-posterior { Equinus. Calcaneus.
    - Lateral . . . . { Varus. Valgus.
  - Compound, . .
    - Equino- . . . { Varus. Valgus.
    - Calcaneo- . . . { Varus. Valgus.
  - Other forms, . . { Cavus. Planus. Non-deforming.

As previously mentioned, these simple forms, or their combinations, constitute the deformities which you will meet with, and a knowledge of their relative frequency is of interest and importance. Much difficulty is experienced in the investigation of this subject, owing to the difference in nomenclature employed by various authors, similar conditions being spoken of under different names. Duval has recorded 1000 cases, of which 574 were congenital; 364 of these were in males, and 210 in females. His statistics as to relative frequency are valuable, and are as follows:—

TABLE NO. II.

	Cases.	Boys.	Girls.
Equinus and equino-varus, ...	417	215	202
Varus, ...	532	302	230
Valgus, ...	22	14	8
Calcaneus, ...	9	6	3
Extreme calcaneus, ...	20	13	7
Totals, ...	1000	550	450

I have compiled the following statistics, shown in Table No. III, from the records of the New York Orthopædic Hospital, and the Orthopædic Dispensary of the University of Pennsylvania:—

TABLE NO. III.

	Congenital.	Acquired.
Equinus,	5	87
Calcaneus,	3	31
Varus,	73	66
Valgus,	29	236
Equino-varus,	95	68
Equino-valgus,	3	9
Calcaneo-varus,	0	2
Calcaneo-valgus,	5	34
Totals,	213	533

Lannelongue has collected the statistics of the Maternity Hospital (Paris), covering a period of ten years, from 1858 to 1867, inclusive. In 15,229 births, 8 children were born with club-foot, a proportion of about 1 case in 1963 births.

The condition may be present as a congenital or an acquired deformity, and the relative frequency of the two forms may be seen by reference to Table No. III, from the cases treated in the New York Orthopædic Hospital, and the Orthopædic Dispensary of the University of Pennsylvania, in which are recorded 746 cases, of which 213 were congenital, and 533 acquired. Tamplin's deductions, shown in Table No. IV, covering 764 cases of congenital talipes, show the relative frequency to be as follows:—

TABLE NO. IV.

CONGENITAL.

Talipes varus,	688	cases.
" valgus,	42	"
" calcaneus,	19	"
" varus of one foot and valgus of the other,	15	"
Total,	764	"

ACQUIRED.

Talipes equinus,	401	"
" valgus,	181	"
" equino-varus,	162	"
" calc. and calc.-valgus,	110	"
" equino-valgus,	80	"
" varus,	60	"
" varus of one foot and valgus of the other,	5	"
Total,	999	"

Adams states the proportion between the congenital and acquired forms to be as 2:3, and the tables already referred to show the large preponderance of cases in which the deformity has been acquired. Giving due weight to the statistics which have been alluded to, we may conclude that club-foot occurs more frequently in males than in females; that cases in which inversion and adduction of the foot, either accompanied or not by elevation of the heel, or the varus types, are oftener met with, and that the right foot is more frequently deformed than the left, but that many more cases of double club-foot occur than of single; and that the primitive forms, pure equinus, calcaneus, varus or valgus, are rare.

The etiology of congenital talipes is veiled in obscurity. The difficulty of studying pathological changes occurring during intrauterine life is self-evident, as the fœtus cannot be subjected to any direct scientific method of investigation. Comparative physiology, embryology, and the changes and diseases which occur subsequently to birth, give us data of comparative value, but all such investigations have resulted in much speculation, many theories, and but few facts. The theory that diseases which produce the acquired forms have their prototypes during intrauterine existence has its supporters, notably Little. But microscopical research has not yet shown the existence of changes in the fœtal brain and spinal cord analagous to those found in cases of the acquired paralytic forms. Voluntary muscular control is retained in congenital cases, while it is lost in the acquired varieties referred to, and the electrical reactions are markedly different; so that this theory has no foundation to rest upon, except the similarity in the appearance of the deformities.

Hereditary influence, with its transmission of peculiarities of face and form, of various tendencies, of traits of character, etc., has some weight as an etiological factor.

Another theory of causation is that of arrest of development, and although cases occur in which co-existing deformi-

ties, such as spina bifida, harelip, cleft palate, etc., are also present, the feet show no evidence of arrest of development, the only alteration being that of the direction of the planes of the feet which is characteristic of the deformity. Adams and Hüter, it is true, have described changes in the bones involved, consisting of alteration of form and relative position of articulating facets, but these changes are by no means constant, and whether they be causative or secondary to the altered relation of the bones, is a matter regarding which there is much difference of opinion. Personally, I incline to the latter view, although the theory has many eminent supporters, including A. Lücke.

The theory which has, perhaps, the greatest number of votaries, is that which ascribes to abnormal intrauterine pressure, and deficiency of amniotic fluid, the influence productive of club-foot; the foot being permanently fixed in the abnormal position during intrauterine life. Although numbering among its supporters such names as Volkmann, Kocher, Banga and Parker, I do not think the assumption tenable, for the following reasons: Were this deformity the result of pressure, it is reasonable to believe that in many cases deformity of other members would co-exist, having been exposed to the same pressure-influence; such, however, is not the case, combinations of this kind being of rare occurrence. Again, in children who have been born with club-foot, and in which the mother had previously given birth to several healthy children, no appreciable difference in the quantity of amniotic fluid discharged during the various labors can be made out. Further, I have recently seen a case of double equino-varus in a twin, the other child showing no deformity whatever.

Dr. H. W. Berg, of New York, in a series of investigations which are commendable for their originality, ascribes congenital equino-varus to a failure of rotation during intrauterine existence. In his studies at the New York Hospital and Wood's Museum at Bellevue Hospital, he has followed the changes which occur in the position of the lower extremities at different periods of fœtal life. At first, the entire leg is rotated outward, and the feet are in a position of marked varus, and, subsequently, of equino-varus. Later, rotation inward takes place, gradually diminishing the amount of varus; but even after this rotation has been completed some varus remains, and, in a very slight degree, is the normal position of the foot in the newborn. Dr. Berg found, in some instances, equinus to be present in fœtuses of two, three and four months, the condition disappearing in the process of normal growth, and he reaches the conclusion, that in early fœtal life equino-varus or varus is physiological, and that its disappearance is coincident, and keeps pace with the normal rotation of the limb. When, from any cause, rotation is retarded or arrested, club-foot results.

To summarize the theories to which I have alluded, and which constitute the principal ones advanced in explanation of the causes of congenital talipes, I have reduced them to the following: that which would ascribe club-foot to pathological changes occurring in the fœtus, similar to post-natal diseases; that which assumes, as a cause, the action of mechanical forces upon the child in utero; then the theory of heredity, with its influences but little understood; the theory of arrest of development; and, lastly, the theory promulgated by Dr. Berg, which would make club-foot dependent upon the absence or retardation of rotation. The last mentioned possesses the merit of being demonstrable by embryological research, and in the present state of our knowledge it has, in my opinion, greater claims to recognition than those which are based upon similarity of post-natal conditions, or those which rest upon even a more fanciful basis.

Turning our attention now to the consideration of the etiology of acquired talipes, we do not find the path of investigation beset with the difficulties we met with in the study of the causation of the congenital types. We may divide the causes into six groups: 1st. Infantile spinal paralysis. 2d. Spastic contractions due to an irritative lesion of the spinal

cord. 3d. Contraction of aponeuroses. 4th. Traumatism. 5th. Rachitis. 6th. Hysteria.

By far the greater number of cases of acquired talipes are due to infantile spinal paralysis—"poliomyelitis anterior." This is essentially a disease of childhood, usually occurring at the period of dentition, its invasion being, as a rule, sudden, marked by fever, gastro-intestinal disturbance, sometimes ushered in by a convulsion, and immediately followed by muscular paralysis, more or less extensive. Recovery follows rapidly in many of the muscles affected, but is rarely, if ever, complete, a certain amount of residual paralysis remaining permanently, in one or both of the lower extremities. Atrophic changes now take place, and are characterized by wasting of the muscles of the limb, loss of electro-contractility, especially to the faradic current; later by reactions, when stimulated by galvanism, characteristic of degenerative change, and deformity, of which the most frequent is club-foot.

It has been thought that the deformity in these cases was due to the loss of equilibrium between the muscles of the limb; one set being paralyzed, their antagonists drawing the foot into the deformed position; but Hüter has shown that the weight of the limb, in the position assumed in paralysis, is the cause of contractions, and that these were due to atrophy and arrest of growth, and were not in any sense muscular. In some cases, resulting from poliomyelitis, the deformity is due entirely to the force of gravity, the foot dropping into the position of equinus, and the anterior portion being adducted by its own weight. In these cases there is little, if any, contraction, and the deformity is readily reduced by manual pressure, but, of course, returns immediately upon the removal of the hand. Volkmann, also, has directed attention to the fact that, owing to the weight of the body, the limb assumes an abnormal position, which eventually becomes permanent, being due, not to contraction, but to abnormal growth.

The "spastic paralysis" of Erb is also productive of club-foot. This condition has been called by Adams, "paralysis

with rigid muscles," and by Seguin, "tetanoid paraplegia." It is well illustrated by the case I now present.

Case I. *Tetanoid Paraplegia, producing double Talipes Equino-varus.*—Barney, æt. six years. No record or information could be obtained regarding previous history. Having stripped him, it will be noticed that the thighs are adducted and slightly flexed upon the pelvis. The legs are held firmly at a moderate degree of flexion at the knee joint. The feet are extended and inverted in the position of pronounced equino-varus. All muscular groups of the lower limbs are in a condition of spasmodic rigidity. You will notice these contractions may be temporarily overcome by firm and continuous pressure, but immediately reappear upon the removal of the opposing force. Locomotion, with assistance, is accomplished, with difficulty, by a swinging, discordant gait, typical of the disease, the patient walking only upon the ball of the feet and toes, the weight of his body not being sufficient to overcome the contraction and bring the heels to the floor. The other symptoms characteristic of the central lesion—deficient intelligence, strabismus, exaggerated muscular reflexes, and general rigidity and spasm of the muscular system, are all present. As I propose to operate upon this patient, I will defer a further consideration of his *feet* until my next clinic. The condition appears to be due, in some instances, to retarded development in the motor tract of the brain; in others, to a lesion in the same position, followed by secondary changes in the lateral columns of the cord. The researches of Rupprecht, of Dresden, not only show that tenotomy is followed in some of these cases by improvement in the position of the feet, but that the mental state is also appreciably benefited by the operation. His article has been published in Volkmann's series of clinical lectures, and constitutes an important and valuable contribution to our knowledge of this interesting class of cases. Various spinal diseases, acute compression, syphilis, tumors, caries, etc., are frequently productive of a similar condition. Other diseases of the nervous

system should be mentioned as causes of club-foot. In rare cases, pseudo-hypertrophic muscular paralysis, and post-hemiplegic contractions produce the deformity, but neuro-mimetic conditions, which of late years have attracted much attention, are more frequently the cause of it. A careful elimination of other possible etiological factors in a given case, coupled with a proper appreciation of the general condition, will usually lead to correct conclusions in the cases of the latter kind.

Sayre has advocated the view that paralysis due to reflex irritation is, in many instances, productive of talipes, and has reported cases in which he claims that functional disturbance of the nervous system can cause spasm of muscles, which, if sufficiently prolonged, while healthy growth continues in their antagonists, becomes the cause of a permanent deformity. Much discussion has taken place concerning this condition as a cause of club-foot, but I do not consider Dr. Sayre's theory as in any way tenable.

Talipes equinus sometimes occurs as a concomitant of the paraplegia of Pott's disease of the spine, but disappears upon recovery from the paralysis; joint diseases of the lower extremity are also potent factors in the production of club-foot. In ankle-joint disease the deformity may follow osteitis of the articulation, and remain as a permanent condition, due to ankylosis of the joint in the position of extension. In hip-joint disease, it would be due to prolonged malposition during the period of growth. I have lately seen a case of this disease in which the limb upon the affected side was shortened but one inch, and in which there was a marked equinus accompanied by contraction of the plantar fascia. Occupations requiring long-continued standing in one position can be also considered causative agents; printers, bakers, blacksmiths, and those engaged in kindred trades may be mentioned as the principal sufferers. The enforced position and the weight of the body are the factors in the production of this variety of the deformity, which is most commonly a valgus. The same

remarks will apply to the valgus of adolescence, due probably to rapid growth, and increased weight of the body, without, however, a corresponding development of the muscles, aponeurosis, and ligaments of the feet. The influence of long-continued decubitus is further shown by the case reported by Volkmann, in which an equinus was found, after prolonged typhoid fever, so resistant that it required a year's treatment to restore the feet to their normal position. As to traumatism, it will be only necessary to allude to the possibility of wounds, burns, rupture of tendons, etc. The former may result in the production of deep cicatrices, which, by their contraction, tend to draw the foot into a deformed position. Spurious valgus, or splay-foot, is frequently the result of rachitis, although, as before mentioned, occupation is often an important factor in its causation. All these forms may be simulated by hysteria, and this class of cases frequently taxes the knowledge and ingenuity of the surgeon; their recognition lies in a thorough understanding of general morbid conditions and a careful diagnosis by exclusion.

Before closing, I desire to call your attention to the morbid anatomy of club-foot. I shall, however, touch upon it only sufficiently to give you an idea of the muscles involved in the production of the various deformities, and will illustrate my remarks by reference to the following classification.

TABLE NO. V.

Extension (equinus)	Gastrocnemius. Soleus. Plantaris. Peroneus longus.
Flexion (calcaneus)	Tibialis anticus. Peroneus tertius. Extensor longus digitorum.
Adduction (varus)	Tibialis anticus. Tibialis posticus. Flexor longus digitorum.
Abduction (valgus)	Peroneus longus. Peroneus brevis. Peroneus tertius.

Dividing the muscles into three groups, which move the foot in four directions, as shown in Table No. V, we have a posterior group, the calf muscles, the gastrocnemius, and the soleus, and two anterior groups, the tibial and the peroneal. In the normal condition, an equilibrium is maintained between these muscles, and the correct anatomical relation of the parts is preserved; but should spasmodic contraction or paralysis of one or more of these groups occur, the balance is destroyed, and deformity takes place. As has been remarked, the purely primitive forms of club-foot are extremely rare, and this statement will apply to these deformities, whether they be congenital or acquired. A brief consideration of them, however, is necessary in order that a clear understanding of the compound forms which are encountered most frequently in practice may be obtained. They are *equinus*, *calcaneus*, *varus*, and *valgus*; the two former being antero-posterior deformities; the two latter occurring upon a transverse plane.

In *talipes equinus* the heel is raised, the patient walking upon the ball of the foot. Here we find the posterior group of muscles, consisting of the gastrocnemius and soleus contracted and shortened, the tendo-Achillis being felt as a tense band. In the opposite condition, *talipes calcaneus*, the anterior groups of muscles, tibialis, anticus, posticus, and peronei, are at fault, and being shortened, maintain the foot in the position of flexion, the patient walking upon the heel. *Talipes varus* manifests itself by inversion and adduction of the foot, the deformity taking place anterior to Chopart's joint; in it, the sole is turned inward and raised, and the anterior portion of the foot adducted, the tibialis anticus and posticus and flexor longus digitorum being contracted. In *valgus*, on the contrary, the sole is turned outward, and its outer border raised, the peronei being the muscles at fault. In this deformity, however, the plantar fascia is involved, the arch of the foot being diminished by its relaxation.

In all these varieties, changes occur, not only in the muscles, but also in the ligaments, fasciæ, and in the bones themselves, whether as causes or effects; but we shall defer the study of them until our next meeting, my object in briefly mentioning the primitive deformities now, being merely to impress upon you the character of the changed relation of the parts from an anatomical rather than a pathological standpoint, which latter condition can be best considered when we come to speak of the most frequent of all the forms of club-foot, namely, *talipes equino-varus*.

LECTURE II.

Gentlemen: Our last meeting closed with a brief description of the primitive forms of club-foot. We now pass to the consideration of the treatment of talipes in general, such modifications as may be necessary to correct the deformity in any special case which may come before us being deferred until we discuss the compound forms. Properly to cope with these conditions, it is essential that you should thoroughly comprehend the factors, pathological and mechanical, which produce them. Because of the lack of exact knowledge upon the subject by the profession, many cases of deformity remain uncured, and scores of children who could otherwise be relieved are left to the care of inconsiderate instrument makers. It is only by the intelligent application of measures fitted exactly to the case that success can be achieved; and the knowledge requisite to do this is not possessed by the mechanician in any greater degree, than is the knowledge necessary properly to care for a medical case a part of the education of the apothecary. Nor has the training of the general practitioner been such as to make him an adept in this branch of surgery, and when it is considered how few are his opportunities of seeing many such cases, it is not strange that extreme deformities are frequent, and that their existence and progression should be an opprobrium. It is only by the dissemination of knowledge by clinical teaching, and the

establishment of institutions dedicated to the care of these special cases, that a better state of things may be hoped for, and the importance of such measures cannot be over-estimated.

The object of treatment in club-foot is not only to remove the existing deformity, but to restore to the foot its functions; and to do this many procedures have been resorted to, which have been in turn discarded. We may consider the methods now in use as *mechanical* and *operative*. First among the former is manipulation, applied so as to stretch the contracted tissues, passive motion, massage, shampooing, and electricity being used the while, to aid in the restoration of function. The hand, if pressure and traction to the contracted tissues could be continuously applied by it, would, no doubt, constitute the best instrument for the relief of club-foot; the apparatus which is best adapted to take its place is that which should be relied upon in the mechanical treatment of the deformity.

Massage and electricity serve, in paralytic cases, to restore, as far as possible, the functional activity of the paretic muscles, and should always be employed as adjuvants in such cases.

Tenotomy for the division of contracted tendons, called aponeurotomy when performed upon fasciæ and aponeuroses, was resorted to first by Delpech, of Montpellier, France. It was not, however, generally employed, until Stromeyer, of Hanover, rendered it popular. Little introduced the operation into England, and Dickson, of South Carolina, first performed it in America. To Deltmold, of New York, and Mütter, of this city, however, is due much of the credit of making the operation popular in this country. Opinions differ as to the indications for tenotomy and the proper time for its performance; whether, for instance, in cases of congenital talipes, it should be done prior to the time at which the child is able to walk, or subsequently. No difference of opinion can exist as to the advisability of early operation in cases in which the nature or extent of the deformity renders correction by mechanical means alone impossible; but as experience

is the only guide to discrimination, mechanical appliances should always be granted a fair trial before resorting to operation. Rigidity, or the reflex spasm caused by point pressure mentioned by Sayre, is not in itself a safe criterion, nor does excessive deformity, taken alone, furnish a reliable indication.

Retentive dressings, such as splints of silicate of soda or plaster-of-Paris, are used, either alone or after tenotomy, serving to retain the foot in the position acquired after manipulation or operation. The rubber muscle advocated by Richard Barwell, of London, and extensively used by Sayre, may be employed to take the place of paralyzed muscles; or Scarpa's shoe, as variously modified, may be applied to fix the foot and exert traction.

As I mentioned to you at our last meeting, the type of club-foot with which you will most frequently meet is talipes equino-varus. The principles of treatment appropriate to the mechanical conditions present can be applied to any of the other forms. This deformity is well illustrated by the case I now show you.

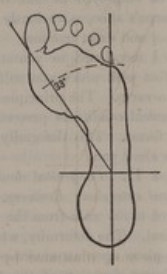
Case II. *Congenital double Talipes Equino-varus: Mechanical extension; Recovery.*—Richard C., æt. four months, referred to my care from the Obstetrical Department of this Hospital. The deformity, which in this case affects both feet, takes place upon an antero-posterior and a transverse plane, combining elevation of the heel, *equinus*, and inversion of the foot, with elevation of the internal border of the sole, *varus*. The os calcis is drawn upward by the contraction of the gastrocnemius and soleus, and rotated in such a manner that its posterior border is turned outward and its anterior border inward. The bones of the tarsus, following the direction of the os calcis, are inverted, and the inner sole raised by the action of the tibialis anticus. The altered relation of the bones of the tarsus leads to change in form, especially of the articular facets; and some have considered these alterations as causative. This is by far the most frequent of the

congenital forms of club-foot, and it has been argued that arrest of development in bones and muscles is the principal etiological factor. As the various theories on the subject were discussed in our former clinic, they need not detain us here.

The lateral deflection of the anterior portion of the foot, as compared to that of the normal imprint, is well shown in the following cuts. The outline tracings are from impressions of the feet of patients suffering from various deformities, obtained after the method advocated by Rohmer (*Les Variations de Forme Normales et Pathologiques de la Plante*

Fig. 1.

Fig. 2.



du Pied, Thèse, Nancy, 1879) consisting of first covering the plantar surface of the foot with lampblack, which leaves a correct impression of the sole upon white paper, on which the patients are then requested to walk. To obtain a correct basis of measurement, and still further to carry out Rohmer's researches as a guide to treatment, I selected the medio-tarsal joint as a base line of measurement; erecting upon it a perpendicular corresponding to the long axis of the os calcis. As they are comparatively stable structures in all deflections from the normal condition of the foot, the *position* and character

of deformity could be readily determined by a comparison of the degrees of variation.

In the thirty-two normal feet measured, I have found the angle of deflection, which is represented by an imaginary line passing through the head of the metatarsal bone of the great toe, to range between 26 and 37 degrees (*average*, 20 males, 34.8 degrees; 12 females, 31.5 degrees); typical examples may be seen in Figs. 1 and 2, *males*, and Figs. 3 and 4, *females*.

In valgus, on the contrary, the angle of internal deflection is reduced to from 12 degrees in moderate cases, to 5 degrees in

Fig. 3.

Fig. 4.

extreme ones, illustrated by Figs. 5 and 6, which represent the imprint of patient's feet suffering from acquired "flat-foot" of rachitic origin. From an examination of seven cases, I have ascertained the average deviation from the perpendicular to be about 8.2 degrees.

The adduction of varus has in two instances reached an internal rotation of 63 degrees. I consider all feet that have an internal deviation in excess of 40 degrees as abnormal. An examination of fourteen cases of varus yields an average of 51 degrees.

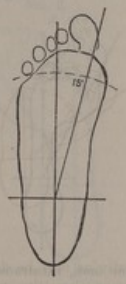
This method of measurement I believe to be of importance, as furnishing us with an excellent and accurate guide to the

amount of deformity, as well as affording an opportunity of determining the improvement that may follow any plan of treatment instituted.

In the correction of equino-varus, as in that of the other compound forms, it is best to divide the process of rectification into two stages, the object being to overcome one of the factors of the deformity before attacking the other. The reason for this will be sufficiently obvious, when it is considered that the altered relations of the tissues of the foot take place upon

Fig. 5.

Fig. 6.



two planes at right angles to each other. Our endeavor, then, should be directed first to the lateral or varus element of the deformity. Manipulation should be sytematically used, and while sufficient in very mild cases, is of great service as an adjuvant in severe ones. It should be applied several times daily, and in the following manner: The heel is firmly grasped by one hand, while with the other the anterior portion of the foot is gradually and steadily brought into a position of valgus, and held there for a few moments, then allowed to return to its abnormal position. After the manipulation has

been repeated several times at short intervals, the foot may be placed in any light dressing. This splint will retain the foot in its corrected position, and may be modified from time to time to suit the lessened amount of varus. It may consist of material suited to the case. In the milder degrees of the deformity, adhesive plaster wound around the foot and attached to the fibular aspect of the leg answers the purpose, but when greater strength is required splints made of leather, gutta percha, or hatters' felt may be moulded to the parts, and secured by a roller bandage. These have the advantage over fixed dressings of plaster-of-Paris in allowing inspection as

Fig. 7.

Fig. 8.

frequently as may be desired, together with the application of massage, electricity, etc.

But the majority of cases of varus cannot be cured by such simple means. As in this case, which is typical of congenital equino-varus, we have absolute deformity to overcome; tendons and muscles are shortened, and the tissues structurally altered. The so-called "mild measures" will not avail, and time occupied in the trial is wasted. Nothing will be of benefit except the application of instruments, which by their accuracy of construction and power will appropriately stretch the tissues involved, or, after a fair trial of these, operations which will divide the resisting structures.

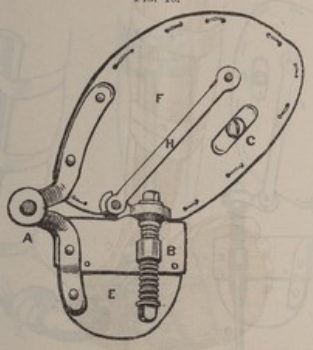
In such cases the shoe which I now show you (Fig. 9) is of the greatest service. It is a modification of Taylor's ankle support, and in its original form was devised by Shaffer, of New York. To this brace I have given more power by substituting in the sole plate, for his extension bar a triple thread screw worked by a key at "B," and by throwing the

Fig. 9.

centre of motion further to the outer side of the sole at "A" (Fig. 10). The instrument consists of a steel trough fitted to the inner side of the leg, extending from the upper part of the tibia to the internal malleolus. A hinge at "C" (Fig. 9), the direction of which is such as to allow pressure exerted upon it at right angles to operate upon the anterior or lateral deformity, connects this trough with a continuation, or foot

portion, which is joined by a plate to receive the foot by an antero-posterior joint, so that the shoe may be accurately adjusted to the "equinus" element of the deformity. The endless screw which I show you at "A" (Fig. 9) is operated by a key, and acts through this hinge upon the anterior portion of the foot. The sole is divided opposite the medio-tarsal joint, and by means of the screw "B" (Figs. 10, 11) acting upon the centre of motion at "A" allows of extreme and

Fig. 10.



powerful abduction of the anterior part of the foot. The apparatus having been applied to fit the deformity, and secured by a bandage ("F, F"), the foot is thrown into a position of valgus by means of the screw "A" (Fig. 9) acting upon the hinge "C," and this is supplemented by applying the force of the screw in the sole plate "B" (Fig. 10), which still further acts upon the anterior deformity. It is better to use the apparatus by stretching the tissues several times in succession, and after allowing them to relax, to adjust the

brace to the corrected position. Having by this method overcome the lateral deformity, as illustrated in Fig. 12, our attention must be directed to the antero-posterior or equinus element.

To correct this deformity by mechanical means, it is necessary to apply an instrument which, through the tendo-Achillis, will elongate the contracted posterior muscles of the calf. To

Fig. 11.

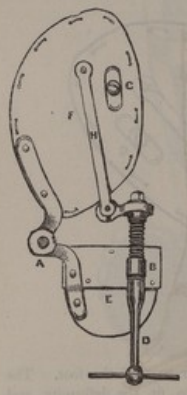
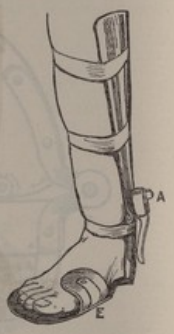


Fig. 12.



accomplish this, many modifications of Scarpa's shoe have been devised. They consist of two steel uprights extending from the upper part of the tibia to the ankle-joint, and are attached to a heel-cup and sole, to hold the foot, the heel being strapped in its place by means of a band of webbing, a bandage, or similar material passing over the instep. The sole may, or may not, be divided opposite the medio-tarsal junction. At

first sight, such an apparatus would seem to fulfill the indication of applying a force sufficient to flex the foot and stretch the tendo-Achillis, but in practice we find that as the necessary power is exerted, the centre of motion in the instrument being opposite the ankle-joint, the heel-cup slips away from the os calcis, and the posterior border of the foot is found resting upon the top of the heel-cup. To obviate this, Shaffer has in his extension shoe, which I now proceed to apply to

Fig. 13.

this patient (Fig. 13), divided the sole of the brace opposite Chopart's joint, and attached to the anterior portion or sole an extension bar which is worked by a key introduced beneath the heel-cup at "B." The shoe having been applied *extended to an angle corresponding to the angle of deformity*, and the heel secured in its place by a strap passing over the instep "E," the os calcis is further secured by a strap "D" passing around it posteriorly and attached to the buckles upon

either side of the anterior portion of the sole plate. When flexion is made by the key at "A," which acts upon the endless screw opposite the ankle-joint "C," the tendency of the heel, as you see in Fig. 14, is to slip away and rest upon the upper border of the heel-cup, and the degree of flexion of the foot does not correspond to that of the brace. If now, we insert the key below the heel-cup at "B," and throw the anterior portion of the sole forward, the os calcis is dragged

Fig. 14.

upon by the strap passing over it at "D," and the centre of motion is transferred from a point opposite the ankle-joint, to a point represented by the centre of the strap "E" which passes over the instep, and the heel descends until it rests upon the extension bar. The tendo-Achillis is thus thoroughly put upon the stretch, and may be felt as a tense band (see Fig. 15). The operation is repeated several times at each sitting, and the amount of flexion thus gained is held by readjustment of

the brace in the acquired position. No danger need be apprehended from interference with the circulation, if proper precautions be observed; the pressure is not continuous, being rather a *momentary overstretching*, followed by relaxation. The foot should be inspected daily. After the treatment has resulted in bringing the foot to a right angle with the leg, a retention-shoe with stop-joint should be worn, to keep the foot in the corrected position, and a similar apparatus must be

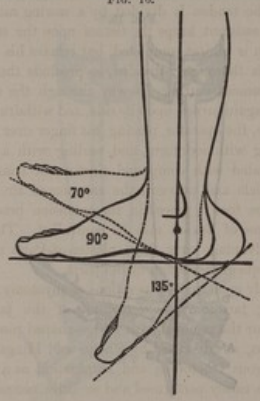
Fig. 15.

applied during the night throughout the treatment. Here let me remark that a cure is not effected when the amount of possible flexion of the foot forms a right angle with the tibia.

Referring to the diagram (Fig. 16), you will observe that the normal foot in extreme flexion forms with the leg an angle of about 70 degrees, and at the ankle joint motion is possible, in the normal condition, through an arc of which the extremes are represented by 135 degrees of extension, and 70 degrees of

flexion. Our endeavor in the treatment of talipes equinus should be to make the acquired flexion reach this amount as nearly as possible. In connection with this subject, I wish to call attention to the existence of cases of *incomplete equinus*, designated "non-deforming club-foot." In this condition flexion is impossible beyond 90 degrees, and the deformity is amenable to the treatment just described.

Fig. 16.



Case III. *Acquired double Talipes Equino-varus from Tetanoid Paraplegia; Tenotomy of each tendo-Achillis.*—You will recollect the case presented at our last meeting, which comes before us now for operation. In performing tenotomy, much of its success is due to attention to detail. Two tenotomes are necessary; one sharp-pointed, with which to puncture the skin, and the other probe-pointed, which is introduced through the puncture, and beneath the tendon or fascia to be divided. The parts having been rendered aseptic by cleansing

with a solution of 1 : 2000 of bichloride of mercury, and put upon the stretch by flexing the foot, the puncture in the integument is made over the central portion of the tendon in such a manner that the incision in the skin and that of the deeper tissues shall not correspond after relaxation of the parts. Through this puncture the probe-pointed tenotome is introduced flatwise beneath the tendon, and as close to its deep surface as possible. The cutting edge being now turned toward it, the tendon is divided by a sawing motion of the knife. An assistant keeps the tissues upon the stretch until the operation is almost completed, but relaxes his hold before the tendon is thoroughly divided, to preclude the possibility of the instrument cutting its way through the skin. The tenotome is again turned upon its side, and withdrawn through the puncture, the operator placing his finger over its point of exit, dusting with iodoform, and sealing with a pledget of cotton saturated with compound tincture of benzoin, which forms a pellicle and prevents the entrance of air. After the operation, the foot is placed in the extension brace, in order that its degree of flexion may be controlled. This possesses the great advantage over fixed plaster-of-Paris dressings of allowing frequent inspection of the parts.

I shall only mention the operations of myotomy, or division of muscles; tarsotomy, or osteotomy of the tarsal bones; tarsectomy, or the removal of a wedge-shaped piece of bone; open incision, as advocated by Phelps and Hingston, instead of subcutaneous tenotomy; and amputation as a last resort. These are so rarely performed and so little necessary, that it is only essential that you should know that such procedures have been devised.

The next case I have to show you is one of acquired talipes calcaneus.

Case IV. *Acquired single Talipes Calcaneus from Infantile Spinal Paralysis; Application of Barwell's Rubber Muscle; Improvement.*—Maggie B., æt. ten, first presented for treatment in the Orthopædic Dispensary of the University of Pennsylvania. She has kindly consented to appear before

you to-day. You will notice the characteristic deformity, the foot being flexed by the action of the anterior groups of muscles, the patient walking upon the heel. In this condition, no treatment does so well as the application of elastic force, advocated by Barwell. The rubber supplies the place of the paralyzed gastrocnemius and soleus, and should be applied as you see in this case. To the shoe are attached two uprights with an antero-posterior joint opposite the ankle. It is important that this joint should be so arranged that while it will permit flexion to any degree, it will stop extension at a right angle. The posterior rubber muscle is attached above to a band which passes around the upper part of the calf and below to the heel of the shoe. Should there be much contraction of the anterior muscles, their tendons may be divided in the manner described, before the application of the apparatus. An operation has been devised for exsection of a portion of the tendo-Achillis for the radical cure of this condition, and consists in the removal of a portion of the tendon, and the stitching together of the divided ends.

The next case I have to exhibit illustrates a very important principle in treatment.

Case V. *Acquired single Talipes Equino-varus; Mechanical extension, Aponeurotomy; Recovery.*—Joseph F., æt. ten years, applied to the Orthopædic Dispensary of the University Hospital for relief from a congenital talipes equino-varus with pronounced *cavus*. The case was treated by mechanical extension, as described when speaking of talipes equino-varus, and resulted in the perfect reduction of the equinus and varus. There remained, however, marked cavus caused by contraction of the plantar fascia. The extension-shoe was applied with the hope of relieving this condition, but without result, when aponeurotomy was resorted to. Several operations were performed, the knife being entered beneath the plantar fascia, and the resisting tissues nicked, and the extent of the division was regulated by the degree of relaxation of the plantar arch resulting from each operation. As you see, the boy has made a perfect recovery.

I wish to draw your attention especially to the inefficacy of mechanical means in cases of cavus with marked contraction of the plantar fascia, and the reason will be readily understood if we consider the structure and function of the arch of the foot. In those forms of talipes which depend upon contraction of muscles, mechanical force applied through the tendon will act upon muscular tissue and elongate it. The plantar arch, on the contrary, is constructed with the view of supporting the weight of the body, the tissues entering into its formation are of the most unyielding character, *i. e.*, plantar fascia, and no amount of mechanical power which can be safely applied will suffice in cases in which it is markedly contracted. Aponeurotomy is necessary, and performed tentatively, nicking a little, and repeated as often as necessary, yields the best results.

The last patient to which I shall call your attention to-day illustrates a condition the opposite of cavus, the essential element being relaxation of the plantar tissues.

Case VI. *Acquired double Talipes Planus ("Flat-foot") from Rachitis; Plantar Springs; Improvement.*—John B., æt. thirteen years. This case shows the deformity in a marked degree, the plantar arch being relaxed and flattened, the internal border of the foot resting upon the ground. These cases, in which there is much pain, have received the appellation of "inflammatory valgus." Besides rachitis, the other causes of this deformity are paralysis, ankle-joint disease and rheumatism. It also occurs in growing children, and in those whose occupation necessitates long standing in one position. In mild cases, the most efficient means at our disposal for its relief are the plantar springs, which have been applied in this case, with the resulting improvement which you notice. They are made as follows:

A tempered steel spring is placed inside the shank of the shoe, moulded in such a manner as to support the relaxed tissues of the arch, and overcome the tendency of the foot to eversion. In cases of greater severity, it should be supple-

mented by an ankle support having a pad which will make pressure upon the internal malleolus.

All forms of club-foot may be simulated by the neuromimetic or hysterical conditions. In cases of this kind, the pedal deformity may be accompanied by contractions in other regions, or it may be the only symptom outside the general condition, and the dependence of the local trouble upon the neurotic state may be very difficult to discover. Here, as in neuromimetic affections in other regions, contractions and contractures may so counterfeit their organic prototypes as to render positive differentiation well nigh impossible. In making a diagnosis, the general condition and surroundings of the patient, the hereditary history, together with any fact as to previous mimicry or simulative tendency, should be carefully weighed. The local condition alone is not a reliable guide: the contractions are often as unyielding as in the real deformity; the muscles do not relax during sleep, and the condition may be very persistent. It is only by a careful consideration of each case, and a diagnosis by exclusion, that a correct opinion can be formed.

The care of this condition taxes the patience and ingenuity of the surgeon to the utmost. In few words, the treatment is that of the general neurotic state, coupled with the absolute avoidance of all local manipulations and mechanical contrivances suited to similar organic deformities, and which would here direct the attention of the patient to the affected member. Despite the most careful general treatment, the deformity may persist for months, as shown by Dr. S. Weir Mitchell,[1] of this city. In this case, hysterical single talipes equinus in a young lady of fifteen had continued for two years, notwithstanding the fact that treatment had removed all the more general symptoms of the hysterical state; and it was not until division of the tendo-Achillis, which I performed after consultation with Dr. Mitchell, that the deformity finally disappeared.

[1] Lectures on Diseases of the Nervous System, especially in Women. By S. Weir Mitchell, M.D. p. 129. Philadelphia: Lea Bros. & Co., 1885.

OBSERVATIONS

ON

SOME POINTS

CONNECTED WITH

LATERAL LITHOTOMY,

BY

ASSISTANT SURGEON F. P. STAPLES,

ARMY MEDICAL STAFF.

In an article lately published on the above operation, the author pronounced it to be the great handiwork of British Surgery, and doubtless we all remember the solemnity which attended "cutting for stone in the bladder" at the smaller metropolitan hospitals at home, where such proceedings were of rare occurrence.

In this country, on the other hand, surgeons do not accord it the first place in capital operations, and possibly the difference between the attached importance in the two countries may be found in the facilities afforded to the great body of practising surgeons in India for its repeated performance.

Familiarity is said to breed contempt however, and a long series of successful cases is very apt to make us think little of the difficulties that properly belong to lithotomy. But my object in writing is not to say so, but to place on record the results of a tolerably large experience for the

benefit of those who have not personally experienced the difficulties of the operation.

I have been further induced to offer these remarks to the profession from repeated disappointments while referring to text-books for comparison with cases under treatment. In these manuals it would appear that the descriptions are either written from operations upon the dead subject, where, for obvious reasons, difficulties cannot present themselves, or if from the experience of the writers, they lead one to presuppose a freedom from disappointment that possibly does not attend the practice of the authors.

The following observations have reference only to the lateral operation, performed with the ordinary staff, and which appears to be deservedly more popular with surgeons than the median, bilateral, or the method proposed by M. Borelli of Milan* in 1867, and are mainly the result of what has occurred in the practice of the writer.†

PART I.

ENTERING THE BLADDER.

THE surgical rules laid down for our guidance in order

* In the method referred to, Median Lithotomy is performed as follows :—A centrally grooved staff is passed into the bladder, and pressed upon so as to make it project in the centre of the perineum. Instead of then dividing the tissues layer by layer, M. Borelli traverses them all in a single thrust with a long bistoury having a narrow blade, so as at once to reach the groove in the catheter, taking care not to wound the bulb of the urethra. Dividing the tissues with the point of the instrument to an extent sufficient for the admission of the finger, he dilates the wound by its aid to admit the forceps, and seizing the stone employs the necessary degree of force to ensure its extraction.—Vide *Medical Times and Gazette* for 30th November 1867.

† On reading over this para., I find I forget to recognise the ingenious method of Doctor Buchanan of Glasgow, *viz.*, the method by a rectangular staff, and beg to do so now in this foot-note.

to effect this procedure admit of no exception as far as our own surgical literature is concerned; and I don't think in the whole range of operative measures there is another regarding which testimony is so concurrent as to the proper method of manipulative interference.

With such a statement before me, it is not to be wondered at that I feel diffident in announcing my intention, so to speak, of setting up a schism against this surgical dogma in the case of children; and the only plea I can urge in apology for so doing is expediency, or, in other words, the method which I shall bring to your notice, permitted, in my hands, of a more feasible entry into the bladder than that commonly taught and practised.

For the sake of comparison, I shall now quote from an elementary work* the directions for this stage of the operation: —"The finger is fixed upon the staff, and the structures covering it are divided upon the point of the knife, which must be directed along the groove towards the bladder, the edge of the knife being carried outwards and backwards, dividing in its course the membranous portion of the urethra and part of the left lobe of the prostate gland to the extent of about an inch. The knife is then withdrawn, and the forefinger of the left hand passed along the staff into the bladder. The staff having been withdrawn, and the position of the stone ascertained, the forceps are introduced over the finger into the bladder."

A surgeon following these instructions in an operation upon the adult subject could scarcely fail to perform a successful lithotomy; but in the class of cases abovementioned, *viz.*, young children, experience has forced the conviction upon me, that an adherence to those surgical dogmas,

* *Gray's Anatomy.*

viz., pushing the finger into the bladder along the groove of the staff, or introducing the forceps either along the staff or finger, is productive of a liability to cause the following accidents :—

1. Rupture of the urethra.
2. Passage of the finger into the recto-vesical pouch.

A consideration of the very small size of the membrano-prostatic portion of the canal in patients of a tender age, of this limited channel being already filled by an unyielding instrument, of the primitive nature of the left lobe of the prostate gland in children, and the limited incision which can be made with safety in that structure, seems to go far towards proving my assumptions, *viz.*, that the introduction of the finger into this limited wound, which contains already the unyielding staff, tends to produce the accidents above enumerated ; and if it be granted that there is not room in the wound in children for two such instruments as the staff and finger, it will be easily understood that the space is insufficient for the staff and forceps, or still more so for the finger and the latter instrument.

In a great many of the operations which I have seen,* which might have been described as tedious and difficult, and eliminating such causes as hœmorrhage or fracture of the stone and a resort to the scoop, &c., the delay has arisen either from difficulty in introducing the finger upon the staff, or from an attempt to introduce the forceps upon the finger.

In some of my own cases, before I altered my practice, I remember that my efforts to dilate the wound by the introduction of the finger upon the staff was attended with a

* This statement has reference to adults as well as children, but in a less degree.

degree of force inconsistent, in my opinion, with a scientific practice ; and the same observation may be held to apply to the introduction of the forceps upon the finger, and in a less degree to the introduction of that instrument upon the staff.

It struck me that the obstacle to the easy introduction of the forefinger into the bladder in children, for the purpose of dilating the wound, was the staff itself, or, as it is called in surgical works, "the guide" ; and that in the same class of cases, or, speaking from my own experience, in all cases, the chief impediment to the easy introduction of the forceps upon the forefinger was the digit itself. Although loth to depart from a practice, the success of which is based upon a strict adherence to those two points, I thought I saw, in the avoidance of the rude manipulation abovementioned, and in a lesser probability of causing the accidents already enumerated by another method of operating, a reason sufficient to warrant the departure.

The method here recommended, is performed as follows :—

When the prostatic incision has been made and the knife has been withdrawn, the left forefinger is pushed on, with the nail feeling the groove in the staff, to that point where the staff and undivided gland prevents its further passage into the bladder without the use of force.

The surgeon should now take the staff in his right hand and proceed to withdraw it, making it, in doing so, feel or rather ride upon his finger nail, keeping up at the same time a firm but not a forcible pressure with the point of the finger. The staff may now be completely withdrawn, and as the nail clears the groove, the finger will be found to occupy the prostatic wound, and a little onward pressure will gain its admittance to the bladder.

Dilatation may now be made with the finger, and the size and position of the stone having been ascertained, it may be withdrawn, and the forceps introduced to complete the operation.

Operators who have always looked upon the staff as a guide to the forefinger, and upon either the staff or forefinger as a guide to the forceps, as points essential to success in lithotomy, will object no doubt to the method recommended, on the grounds that they are asked to relinquish the great landmarks of the operation, and that to them the chances of passing the forefinger or forceps into the recto-vesical pouch appear greater from the loss of these landmarks in the new operation.

Now, with regard to these objections, I will remark that the staff in my method is looked upon as an essential guide to the knife only, and that its removal at the precise point I have indicated, although differing considerably from the method commonly practised, does not amount to the abstraction of an essential landmark.

At that period of the operation when I have directed the removal of the staff, the nail and point of the forefinger will be well within the prostatic wound, and as this wound extends into the bladder, it seems improbable that the finger should take any course other than through it to that viscus.

If, however, an unyielding instrument* is allowed to remain in this limited wound, I think it will be apparent, even in theory, that the introduction of a second instrument† must be difficult, and practically I have always found it so.

* The staff.

† The instrument here referred to is the finger.

With regard to the second point of the assumed objection, which claims either the staff or forefinger as essential for the safe guidance of the forceps into the bladder, I shall be a little more plain-spoken. The necessity of such a proceeding, which is taught to students as an incontrovertible dogma, appeared questionable to me after I had performed lithotomy but a few times; and, in the case of children, to force into the wound already filled by the forefinger an instrument so little calculated to glide (under the circumstance) as a lithotomy forceps, struck me not only as of questionable value, but unscientific.

The point sought to be gained by using these consecutive guides to the bladder was the avoidance of the instrument passing into the recto-vesical pouch; but supposing, as in the operation which I have recommended, the forceps is introduced without either of these directors, what are the chances of their passing into the space abovementioned?

Answering that question from my own experience, I can assure my professional brethren that I have failed to observe a tendency to the accident under notice, and I will remind lithotomists that the bottom of the wound which communicates with the bladder at this period is greater than the circumference of the finger, and greater than the circumference of an ordinary forceps; and for this reason it appears inconceivable that the latter instrument, gently introduced, should (provided the parts behind the triangular ligament have not been rudely disturbed) take any other course than through this wound into the bladder.*

* There are some surgeons who make a practice of condemning anything new, calling proposals of that nature by the name of "innovations." To these I will merely say, that before committing critical remarks to the columns of the press, it would be in accordance with fair-play to at least make a trial of the operation proposed.

PART II.

SEIZURE AND EXTRACTION OF THE STONE.

INEFFECTUAL efforts to grasp the stone fairly are not unfrequently observed at lithotomy operations.

Partial seizure of the calculus is also of common occurrence, and is often followed by the great disappointment of its slipping from between the blades during extraction, necessitating another search,—an increased manipulation in the wound and bladder; while the chances of a repetition of the accident remain the same.

Fracture of the stone, with its attendant results, *viz.*, laceration of the wound, increased rough treatment of the wound and bladder from the fragments, scoop, and surgeon's fingers, may be also placed amongst the accidents arising from partial grasping of the stone.

All these accidents have happened in my own practice, and I would willingly attribute them to a want of acquired skill in using the forceps, but that the operations I have seen performed by others have not been remarkable for a greater freedom from their occurrence.

I have for some time attributed the frequency of those accidents to the orthodox method of using the forceps, a description of which I shall quote here from a practical work on surgery,* in order to better illustrate what I shall say hereafter :—

"The forceps on entering the bladder are not to be opened, but should be employed as a sound, and moved gently about to find the stone, and having come in contact with

* *Skey's Operative Surgery.*

it,' then the blades may be expanded upon it and the stone seized."

Now, if it be recollected that the stone at the precise moment, we are advised to act, as in the above quotation, is surrounded by the fluid contents of the bladder, it will be easily understood, I think, how the points of the forceps, used as a sound, are liable to cause a change in its position in the fundus, either in a direction posteriorly or laterally, and practically further from the perineal wound and the operator.

And if it be granted that the effect of using the forceps as a sound is a liability to cause this recession of the stone, it follows that, if immediately upon a knowledge of the contact, the blades are quickly opened and as quickly shut to catch the body already set in motion, that there is a liability of their closing only upon a part of the receding calculus, if not of missing the latter altogether.

There is one other circumstance in connection with the usual mode of using the forceps which appears to me to help in causing partial seizure of the stone, and this remark is more especially applicable to calculi of large size.

I allude to the method of using the forceps when searching for and seizing the stone with one hand only.

At first this statement may appear of trivial importance, but any surgeon can convince himself that such is not the case by taking a lithotomy forceps out of a case of instruments, and trying how far he can separate the blades whilst using them as he would a pair of scissors, *viz.*, with the right hand only. It will then be found that in a pair of lithotomy forceps, owing to the long arm of the lever being behind the joint, that to separate the blades to an extent sufficient to seize a tolerably large stone is a matter of some difficulty

due regard being had to the fact that the thumb and index finger can be separated so as to render them unsteady, when the adductor muscles are suddenly called upon to close the forceps upon the stone.

I am afraid I have not succeeded in making my meaning upon this point as clear as is my own conviction, but I hope it will be better understood from what follows.

I have long practised a method of using the forceps different from that generally recommended, and from which I believe I have had better results as regards the following points, *viz.*,—

1. Finding the stone.
2. Fairly grasping the stone between the blades.
3. Ability to use greater force in extraction without the danger of fracture.

And for these reasons beg to recommend it to the notice of practical surgeons.

I have already described the method of introducing this instrument into the bladder; and supposing that previous to their introduction the stone has been felt with the left forefinger, as is generally the case, let the operator proceed as follows:—

Take a handle of the instrument in each hand, and having separated them to their full extent, raise them so as to make the blades press upon the fundus of the bladder, and then bring them together, when, in normal cases, the calculus will be fairly grasped, and extraction can then be proceeded with.

It will be observed that the main points of difference between the old method and that I am advocating are—

1. That in the method recommended the forceps are not used as a sound.

2. That both hands of the surgeon are used in manipulating them while within the bladder.

With regard to the first point, I will remark that it seems unnecessary for the surgeon to reassure himself of the position of the stone by using the forceps for that purpose as a sound, as it is to be presumed that he has derived an accurate knowledge upon that important point from his forefinger, previous to their introduction; while the chances of his moving the stone from its already ascertained position by pushing such a heavy instrument against it, appear to me to be greatly increased, and that such a contingency would be productive of failure in seizing it afterwards seems, as I have before pointed out, more than probable.

Regarding the second point, I think it is apparent that the greater sweep of the blades insured by using the forceps with both hands, and the gradual closing of them along the fundus of the bladder where the stone is found, in normal cases, almost prevents the possibility of missing it or of grasping it otherwise than fairly; and, owing to the invariable success which has attended the method in my hands, I beg most strongly to recommend it to practical surgeons as worthy of their notice.

It will no doubt be urged as an objection against this method, that the chance of grasping the stone at the precise moment when the great gush of urine from the bladder takes place is lost,* and to those who have not made the said gush an object of study, the objection may appear a valid one.

Theoretically we are taught that if we are very dexterous

* See any of the text-books for the significance attached to this point.

we shall have the forceps in the bladder and the blades open in time to receive the calculus which the outrushing urine is to leave between them; but, it may be asked, is this the case in actual practice?

I am inclined to think that it is not; and, generally speaking, even with the most dexterous, the great gush of the contents of the bladder through the wound will have occurred before the blades of the forceps have been sufficiently expanded to seize the stone.

There are two other points, although not strictly coming under the heading of this para., which I shall notice here, *viz.*:—

1. Selection of forceps.
2. Abnormal position of the stone.

As regards the first point, it has been very truly said by Mr. Skey, in his work on Operative Surgery, that "the lithotomy forceps should be adapted to the size of the organs operated on."

Many other surgeons insist on the necessity of using as small a forceps as possible; but the soundness of the advice seems to me open to question.

'Tis true that greater facility of introduction is obtained by using a small instrument where the forceps are conducted into the bladder upon the finger, but it appears to be a matter of opinion whether such advantage is not overbalanced by the following results of operating with a small instrument:—

1. The greater difficulty of seizing the stone when at all large.
2. The delay from having to change the instrument from the same cause.
3. The increased difficulty of extraction, under any circumstances, on account of the wedge shape of the open blades of a small pair of lithotomy forceps.*

Regarding the second point, it occasionally happens that calculi are not found at the fundus of the bladder, but occupying a position above the pubis.†

When such is the case, the necessity of using a curved forceps is obvious; and it is also essential that the blades should be as long as possible, to avoid difficult extraction or fracture of the stone, as was the case in the instance referred to in the foot-note, from, as was then pointed out, the unavoidable use of a short-bladed instrument.

PART III.

WOUND OF THE RECTUM.

THAT this accident does sometimes occur is certain,‡ but it would appear that a partial incision of the gut is of more frequent occurrence than is generally supposed.

The nature of the wound in lithotomy, and the rapid infiltration of its edges, prevents our acquiring a knowledge of this fact and its consequences in the living; but I lately had an opportunity of satisfying myself of its frequency in the dead subject.§

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* On this point *vide* Sir William Fergusson's *Surgery* for very practical remarks.

† For details of a case of this nature, see one published by the writer in the *Medical Times and Gazette* for October 28th, 1865.

‡ *Vide* Lithotomy in *Cooper's Surgical Dictionary*, page 935.

§ In a School of Surgery which I attended.

In some of the cases where this accident had occurred I observed that the incision had not divided the mucous membrane, although the cellular and muscular layers had been cut; and in one operation I noticed that the epithelial layer only prevented the division from being complete;* and had the subject been a living one, the accident could not have resulted otherwise than in recto-vesical fistula.

Many circumstances increase the liability to wound the rectum in lithotomy, such, for instance, as using a centrally grooved staff to expedite the first stage of the operation; a broad-bladed scalpel, either from choice or from want of a proper lithotomy knife; an unemptied state of the gut itself; the tendency of the edges of the wound to cause the knife, in making the prostatic incision, to cut parallel to the wound in the perineum; careless holding of the staff by assistants; and the want of lateralising the knife when operating.

Each of the circumstances mentioned no doubt plays an important part in individual cases in the causation of the accident under notice, but the last two are the most important.

With regard to the first of these, *viz.*, careless holding of the staff, I may mention that there is every probability of the rectum being wounded in the following manner:—The operator, either from want of practice, or some other cause, becomes confused in the deep dissection, or perhaps tries to feel the groove before he is within the space between the accelerator urinæ and erector penis muscles, and fails. He becomes a little flurried perhaps from the

* A good way of explaining this accident to students is to pass the fore-finger, after the operation is over, into the rectum, and if they have divided the latter, the consequence of so doing can be made very apparent to them.

disappointment, and the staff is brought to his assistance by being made to press outwards in the perineum.

In this way the groove is reached; but if the assistant is not careful to again bring the concavity of the staff against the pubis before the operator pushes on his knife through the prostate, it is obvious that there will be the utmost danger for the rectum.

It would be difficult, in a case such as I have described, to say with whom the blame should rest; but as the operator is certain of being credited, not only with his proper share of it, but also with what may be due to the carelessness or want of skill of his assistant, it is always well for the latter to understand that he should keep the concavity of the staff firmly under the pubic arch; and that it is no part of his duty to keep constantly looking into the wound, as there is ever a liability, in directing his attention elsewhere, of his relaxing his hold upon the instrument, and, in this way, of tending to make his chief commit the serious mistake under notice.

Regarding the other important point, *viz.*, not sufficiently lateralising the knife in making the prostatic incision, I think I may safely state that the cause of surgeons not doing so is a fear of wounding the pudic vessels.

At least such would appear to be the paramount one with commencing lithotomists, and is due, as far as my observations have enabled me to judge, from the necessarily impractical teaching of the professors of demonstration and dissections, who are accustomed to give such an importance to the probability and effects of wounding the artery in question, as to interfere seriously with the future practice of their pupils as regards lateralising the knife in making the prostatic incision in lithotomy.

I shall inquire further into this subject when I come to the subject of hœmorrhage, and return to wound of the rectum.

Surgical literature is not as rich in records of this accident as might be expected, and doubtless for obvious reasons; but there is the very high authority of Mr. Samuel Cowper and of Mr. Key as to the absence of serious results attending it.

It is presumed that such statements are intended to have a comparative value with other dangers in lithotomy, for the result of a complete rectal wound is very self-evident, and I cannot but think that a partial division of the gut, involving the cellular and muscular layers as described above, admitting, as it would, of the entrance of urine and other discharges between them, is a cause very potent in retarding the subsequent healing of the wound.

There is one other circumstance which may be noticed in connection with this part of the operation, and which I find I have omitted from the list of circumstances tending to cause a wound of the rectum,—I allude to prolapsus during the operation.

I have amongst my notes the record of a case of this kind which occurred to me in 1865 while stationed at Jullundur, in the Punjab. The prolapsus was complete, and at first I was puzzled to know how to act, as the tumor to a great extent hid the perineum from view. However, I returned the tumor with a towel, and keeping my left forefinger in the rectum to support it, was enabled to complete the superficial incision. The spasmodic action of the gut soon ceased, and during the subsequent stages of the operation I had no further trouble from the prolapsus.

It would appear that in cases of vesical calculus accompanied by irritability of the rectum, there is a great tendency to prolapse during the spasmodic stage of the anæsthesia; but when the insensibility is complete, the spasmodic contraction will have passed away, and if the tumor is then reduced, there is not much fear of the surgeon being embarrassed by its return.

PART IV.

HŒMORRHAGE.

The loss of blood in lithotomy is divided anatomically thus—

A. From the superficial perineal vessels.
B. From the artery of the bulb or its branches.
C. From the prostatic plexus of veins.
D. From the internal pudic artery.
E. From either the artery of the bulb, internal pudic, inferior hœmorrhoidal, or dorsal artery of the penis, when distributed anomalously.

In a surgical operation, however, no such division is admissible, and the following answers all practical purposes, viz., superficial and deep, or, to be more minute, that which takes place previous to the knife striking the groove in the staff, and that which follows the prostatic incision.

I shall however treat of it upon the anatomical division, owing to the extreme diversity of opinion which obtains upon the subject.

With regard to the first of the sources above indicated,

experience would seem to prove that they are as frequently cut as the reverse in a lithotomy operation. Practically, however, their division would be of little moment were it not that the blood from them obscures the dissection; and to obviate this inconvenience it is as well to secure them.

I have frequently seen in the Medical Journals recorded cases where surgeons have applied ligatures to one of these superficial vessels; but in my own practice I have never found it necessary to do so, and the plan I have found to answer as well is torsion with the finger and thumb nails of the left hand.

I have no desire to place this method of dealing with the superficial perineal vessels before the safe plan of ligature as a surgical proceeding; but as I have found it uniformly successful, I am anxious to bring it to the notice of surgeons, as well for the latter reason, as by its employment the time taken to adjust the ligature is gained.

The operation consists simply in pinching the cellular tissue and mouth of the bleeding vessel between the nails of the finger and thumb, and two or three applications of these human forceps are generally sufficient.*

Hœmorrhage from the second of the sources mentioned is the subject of much difference of opinion,—some holding, with the late Mr. Key, that these vessels of the bulb are the most frequent cause of loss of blood in lithotomy, while others consider that the artery of the bulb will be seldom divided in that operation, and that the hœmorrhage is, as a rule, venous.

* These means are equally well suited for other operations, where bleeding from subcutaneous vessels is obscuring a dissection; and I may mention that I have had excellent results from it in an operation for restoration of the nose, when applied to the numerous arterial branches which furnish the hœmorrhage in that operation.

The experiments of Mr. Key upon the dead subject were formerly looked upon as mainly proving the correctness of his views, but for some reason they are not now generally accepted as conclusive; and some of those gentlemen who have dissecting-room opportunities would advance the cause of science were they to repeat them.

In practice it has never appeared to me an easy matter to say definitely from whence hœmorrhage from the deep part of a lithotomy wound is proceeding. The vessel furnishing it will, I presume, be cut after the groove is reached, and when the surgeon is making the prostatic incision; and an operator, as a rule, does not wait at this stage to attend to hœmorrhage, the extent of which is obscured by the gush of urine from the bladder. Nor does it appear necessary that he should do so, as the pressure he is called upon to make in the next stage of the operation—with his finger, the forceps, and the stone, is good treatment, and not unfrequently effectually checks the bleeding.

It sometimes happens, however, that after the removal of the calculus the hœmorrhage does not cease; but the surgeon and his colleagues perhaps agree that it is not necessary to interfere with it. By the time, however, that the reaction from the chloroform is complete, it will have assumed an alarming character; and if the operation has been a prolonged one, the patient is tolerably certain soon to become collapsed;* and as it is then scarcely possible to see from whence the blood is issuing, there is some danger for the hospital staff to become confused, if they have not been in the habit of dealing with such cases.

* This case is drawn from one which happened within the experience of the writer, but in which he was not the operator.

Let us now examine the different methods at our disposal for the control of such a hœmorrhage, *viz.*, removal of clots, plugging, the ligature, and the application of cold.

The first should be most scrupulously attended to, as a dangerous bleeding may go on behind a clot, and until the latter is removed it is impossible to reach the vessel from whence it issues. Of the permanent means, pressure is the most easily applied, and if the surgeon makes up his mind to resort to it, he should apply it by means of a plug, using for the purpose a thick catheter, with lint or whatever material may be at hand; and before he commences, it will be well to place the patient in the lithotomy position.

I have given this proceeding the first place on this list of our means to check a hœmorrhage in lithotomy in deference to high authority, but personally I have a strong objection to cause the wound to undergo the rude treatment which it entails.

I have never been obliged to resort to it; and about two years ago, in the most alarming case I ever saw, I dealt successfully with the hœmorrhage as follows:—Having cleared the wound of clots, I took a triangular, or rather wedge-shaped piece of ice, and having smoothed its edge, I introduced it gently into the wound, and held it there with a towel.

The effect was most reassuring, and in a few minutes I had the satisfaction of seeing the hœmorrhage cease, and the patient recover from that most distressing condition—hœmorrhagic collapse.

A case may occur, however, where neither plugging nor the application of ice will control the bleeding from the divided artery of the bulb, and the surgeon will then be called upon to resort to the ligature. To do so he is advised

in some of the surgical treatises to enlarge the skin wound and seek for the vessel; but whether this advice is based upon practical experience or not appears doubtful. The nature of a lithotomy wound, its depth, the fact of its being constantly filled with blood, rendering it next to impossible to see the mouth of the bleeding vessel, and the propensity of all arteries beneath strong *facsiæ* to retract, seem somewhat confirmatory of the above doubt; but nevertheless it is not impossible to follow the advice given and delegate the artery.

To carry a ligature around the vessel by means of a strong curved needle appears to me to be better practice, because it is likely to be more expeditious in result, and in the instance of the vessel under consideration, where anatomy so clearly indicates its position, there would be little difficulty in performing such an operation.*

The third anatomical source I have given above is the prostatic plexus of veins, and is, according to some of the most reputed authorities, the chief source of hœmorrhage in lithotomy; while, as before mentioned, others deny this, and affirm that they have failed to notice that dilated condition of these vessels which would confirm the opinion.

The fact appears to me, and for reasons already given, that it is a matter of extreme difficulty to define the anatomical source of hœmorrhage from the deep part of lithotomy wounds; and the fact of the blood being mixed with the fluid contents of the bladder adds at first to the difficulty.

In my own experience the appearance of the blood has

* For my own part I am strongly of opinion that this means of controlling hæmorrhage has fallen into greater disuse than it deserves, as I have frequently been successful with it in cases of bleeding from the temporal and facial arteries, and where to apply a ligature in the orthodox way would have been almost impossible.

always been arterial, but in a case which occurred to me in 1865, I was strongly of opinion that the prostatic plexus furnished the hœmorrhage, and in a published account of the case* I endeavored to prove this; but now I am inclined to think my diagnosis was not correct, and that it was the artery of the bulb from which the bleeding took place.

The hœmorrhage occurred 42 hours after the time of operation, and came from the orifice of the urethra. The wound at this period was inflamed and closed by infiltration of its edges, and as it did not appear advisable to subject it to further manipulation, cold was applied over the pubis and perineum, and astringents given by the mouth.

No arrest of the bleeding took place from this treatment, and a catheter was introduced into the bladder and 20 ounces of urine (slightly bloody) drawn off, when the hœmorrhage completely ceased.

Its having done so perplexed me not a little, and I then accounted for it by supposing that the distended bladder had kept patulous the prostatic veins, and that when the urine was drawn off the contraction which followed constricted the divided vessels. But the blood (à urethra) was decidedly arterial in appearance; and had it been from the source under consideration, I am now inclined to think it would have been mixed, to a greater degree than was the case, with the urine which was subsequently evacuated from the bladder.

The temporary arrest of the hœmorrhage for 42 hours was not an incurious feature in the case, and was attributable doubtless to the torsion exercised by a broken calculus in passing through the wound.*

* Vide *Medical Times and Gazette* for October 1865, page

The sudden arrest of the bleeding when the urine was drawn off from the bladder is also worthy of notice, and is the only circumstance that would seem to disprove the theory now offered in explanation of the case, *viz.*, that the hœmorrhage proceeded from the artery of the bulb.

I regret that the circumstance which induced me to pass a catheter in this case is not mentioned in my notes (probably it was physical evidence of distension); and although the result of the operation was in every respect satisfactory, I have no desire to recommend its repetition in hœmorrhage from the prostatic veins, but beg to refer the reader to my remarks on the treatment of this condition from the deep part of a wound under the last para.

The internal pudic artery must be a very rare source of hœmorrhage in cutting for the stone, although doubtless in the days of the gorget it was in great danger. Operating with the ordinary knife it would seem difficult for a lithotomist to divide it, and yet students are so alarmed by their dissecting-room teachers at the probability of doing so, that it takes a considerable experience of the operation to rid them of the bugbear.

A good way to assure students of the comparative safety of the pudic vessels in lithotomy would be as follows:—Take a male pelvis and pass a string from one tuber ischii to the other, and let the triangle formed by this and the rami of the pubis be bisected by another cord. The latter will represent the position of the staff; and as the back of the knife will be carried to the bladder in apposition with that

* The stone was unavoidably broken during the operation.

instrument, the student will find that to make the edge of his knife touch the ramus of the pubis, the margin of which will represent the internal pudic vessel, he will have to cut either horizontally, or out of all proportion to the limits proper for a prostatic incision.

Instances are on record however of this vessel having been divided, but in the book from which I am quoting,* mention is not made of the instrument with which the accident was caused.† It is also stated to have been ligatured successfully by Dr. Phisick, of New York, and by Sir Benjamin Brodie.

The operation was doubtless necessary in the cases recorded; but it appears reasonable to hope that such a proceeding would seldom be required, and that successful control of the hœmorrhage would result from some of the other methods indicated,—from a curved acupressure needle, or from direct pressure over the ascending ramus by means of a piece of cork, or, what I have found still better in other cases of hœmorrhage from deep vessels, *viz.*, the edge of a shilling. This brings me to the last of the anatomical sources mentioned, *viz.*, an abnormal distribution of either of the following vessels,—artery of the bulb, arteria dorsalis penis, inferior hœmorrhoidal or internal pudic; and on this subject I shall quote a distinguished author‡ who says—"The surgeon is not to blame in the event of a vessel being accidentally divided when taking an abnormal direction, which it is impossible to become acquainted with until after the accident has occurred."

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* *Surgical Dictionary.*

† Gorget or knife.

‡ *Science and Art of Surgery*, by Mr. Erichsen.

The control of the hœmorrhage would engage his attention however, and he would probably be successful with some of the following means, which I beg leave to recapitulate:—

1. The insertion of a wedge-shaped piece of ice into the wound.
2. Pressure with the edge of a shilling or piece of cork over the left side of the perineum.
3. The acupressure needle.
4. The application of a ligature by means of a curved needle.
5. Plugging.
6. Enlarging the wound, and direct deligation of the artery.

PART V.

SELECTION OF CASES.

THE great importance of this part of the subject may be overlooked by young surgeons on first arrival in this country in their very natural desire to become lithotomists, and most probably the first serious reflections upon it will be induced by the rude shock of an unsuccessful case.

Independently of the very old, against whom we are warned in the text-books, there are two classes of patients who present themselves at our dispensaries imploring relief of their maladies, but whose cases are only too apt to end fatally if subjected to operation.

The type of class 1 is an oldish man who has had a stone for many years, but who from favorable circumstances has been able to bear up against the attendant suffering till within the last few days, when, from some cause, probably a debauch, a blow or fall, or the rude passage of an instrument, he has been seized with intense pain, and, as soon as he can, presents himself at the dispensary expressing an eager anxiety to be cut at once.

Should his request be acceded to, the termination of the case will possibly be as I have indicated ; and an examination of such a case *post-mortem* would doubtless reveal not only cystitis, but inflammatory complication of the peretoneum as well, and the lesson to be learned from such a case is to recollect that in patients where severe pain is superadded to the other symptoms of long existing stone in the bladder, it may be due to inflammation, and not to some trivial circumstance connected with it as a body foreign to the interior of that viscera.

It appears almost needless to say that practice will be determined by the diagnosis.

Class 2 is represented by a patient a little beyond the adult period of life, who says he has had stone in his bladder for some years, and that for the last 12 months or so he has been losing health and flesh rapidly, and his anxiety to be cut has been determined by the latter circumstance, and not by any extra distress caused by the calculus.

Cases, however, of which this is the specimen selected, progress unfavorably ; the wound resulting from the operation soon becomes glazed over ; granulations are not formed ; and the death of the patient, which soon follows, would appear to be due to hectic fever, until an autopsy throws light upon the hitherto obscure case, and accounts for the previous 12 months loss of flesh, by revealing extensive tuberculous infiltration of the lungs.

Yet this condition might be easily overlooked, particularly in those cases of quiescent tubercle, and where our attention is not specially directed to the chest ; but in stone patients, whose histories are marked by loss of flesh, it would always be well to remember that such a condition may not be due to the disease for which relief is sought, but to the more serious complication, *viz.*, pulmonary tubercle ; and the course to be followed in such a case is obvious, *viz.*, to refuse operative interference under any circumstances.

There is one other class of case which may be mentioned under this heading, *viz.*, very young children ; but it seems inadvisable to put down any limit, and say that a child under this age should not be cut, as experience generally nullifies any attempt at the fixation of dogmatic laws in this, or in fact in any, surgical operation ; but I may mention, for the benefit of those who have not had an opportunity of gaining experience upon the subject, that amongst my recorded cases is that of a child who was cut successfully at the age of $2\frac{1}{2}$ years.

CONCLUSION.

I have now concluded the observations which my experience of lithotomy suggested to me, and beg to recommend the changes I have introduced into the operation to the notice of the profession.

I would also beg to take this opportunity of expressing an opinion as to the simplicity of lithotomy as an operation; and I think it would be well if surgical teachers were less in the habit than they are, of investing the proceeding with a halo of alarming difficulties, the majority of which will be found upon experience to have existed only in their imaginations.

PUCHMURREE, CENTRAL INDIA; *July* 1872.

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EXPERIMENTAL ENQUIRIES

INTO CERTAIN

WOUNDS OF THE SKULL:

BY

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[Reprinted from the British and Foreign Medico-Chirurgical Review, of July 1st, 1864].

Experimental Enquiries into certain Wounds of the Skull. By W. F. Teevan, B.A., F.R.C.S., Surgeon to the West London Hospital, &c.

[Reprinted from the British and Foreign Medico-Chirurgical Review, for July 1st, 1864].

When a foreign body penetrates the skull, externally, the inner table will invariably be found more injured than the outer; and, as nearly all violence to the skull is applied from without, the above fact has been the cause of the establishment of an imperative rule in operative surgery. Now, as this rule was not determined without much discussion, and, as the remedy seemed so severely disproportionate to the apparent amount of damage, it can scarcely be wondered at that, at different times, surgeons should have inquired why it was that so trifling an external injury should be attended with such unseen internal destruction of bone, as to necessitate the performance of an usually fatal operation. To the military and civil surgeon all wounds of the head are necessarily interesting, but, to the latter, they often possess an additional interest from their importance, forensically.

The following results given are those of a series of experiments made on the skull, with different instruments, to determine, firstly, what, generally, are the comparative varying forms of the apertures of entry and exit; and, secondly, to inquire into the causation of a fact ascertained in the course of experiment—the always relatively greater size of the aperture of exit.

The experiments, 125 in number, were made in the dead-house of Westminster Hospital on the skulls of persons ranging in age from 16 years to 60 years, within one month after their deaths. The instruments used were spherical bullets, conical bullets, pick-axes, crowbars, nails, and bricks.

In giving the results of such inquiries it is perhaps better to reverse the process of elucidation employed to obtain facts and the deductions to be made from them. *When a foreign body passes completely through any part of the skull—it matters not what the direction may be—the aperture of exit is always larger than the aperture of entry.* It is here necessary to remark, that, in order to gain the same results on the dead body that we should on the living, it is requisite to place the head in the same conditions as during life, when it is maintained temporarily fixed, in varying positions, by the contractility of the muscles; whereas, so soon as the rigor mortis has passed away, this force no longer exists, and the head dangles about like a flail when even very slight force is applied to it. Hence, if a dead man's skull be fired at, a very much larger wound will be made than if a similar amount of force were applied during life: the reason why such should be the case will hereafter be explained. It must also be borne in mind, that, when the living body is struck, the muscles of the part are always, instantaneously, involuntarily, contracted, thus still more increasing the difference between an ante-mortem and a post-mortem wound. To arrive, therefore, at the same facts as would be and are seen during life, it is necessary that the head be fixed.

The number of gunshot experiments made was ninety. Spherical bullets were used in twenty-three instances, flat-headed bullets in eleven cases, and conical bullets in the rest. If a bullet be fired,

with a full charge of powder, close, and at right angles, to the part to be struck, a wound is produced which, for practical purposes, may usefully be called a typical wound; and it will render the descriptions of all other wounds presenting different appearances more easily understood if they be regarded as wounds altered from the typical wound by some modifying condition or fact.

Now, if a shot be fired at the outside of a skull, under the conditions already laid down, it will be found that the aperture of entry in the external table is cleanly cut, and of exactly the same shape and size as the circumference of the bullet, the opening merely sufficing to admit the bullet, and looking very much as if it had been made by a trephine; there is never any splintering or fissuring about the edges. If the aperture of exit in the internal plate be examined, it will be found considerably larger than the aperture of entry; its circumference will generally be irregular, though rarely having splinters attached to, or fissures radiating from, its edge. The average diameter of the aperture of exit exceeds that of the aperture of entry by about one-third. The average irregularity in the aperture of entry seldom equals a line, whereas the irregularity in the aperture of exit generally varies from one-eighth of an inch to half an inch.

The size and shape of the aperture of entry made by a shot, fired under the conditions already laid down, never vary, whether the skull be thick or thin, hard or comparatively soft; not so with the aperture of exit, which attains its maximum size and irregularity in thick or hard skulls, and its minimum size and irregularity in soft or thin skulls.

If a head be decapitated, fixed, and a shot be fired into it through the foramen magnum, the above descriptions will equally apply and be found to be true, if, instead of aperture of entry in the external table we read aperture of entry in the internal table, and, if, instead of aperture of exit in the internal table we substitute aperture of exit in the external plate—thus clearly proving that the aperture of exit of a bullet is always larger than that of its entry. In no instance was there a single exception to this rule, neither have I been able to find one in the specimens in the different museums.

Spherical bullets fired from the smooth-bore arm produced much larger apertures of exit than conical bullets fired from the modern revolver; but the sum total of all the damage that can be done with the latter is very much greater than with the former, for they will traverse a greater amount of structure, and thus rarely lodge in the skull: the aperture of entry of a spherical bullet is generally circular; whereas that of a conical bullet is often oval.

The round bullet fired from the old firearm would seem to be retarded in its velocity according as whether the integuments of the skull, and the brain and its membranes, were present or not; but these structures seem to have no material influence in lowering the highest velocity of a conical bullet fired from a revolver: in compound gunshot wounds the aperture in the scalp is generally smaller than the aperture in the bone, and the eversion or inversion of its edges will be determined by the direction in which the shot is fired, whether from within or from without.

That bullet whose velocity can be reduced to the lowest, compatible with its penetrating the skull, will produce the greatest amount of damage. Hence, if the distance be gradually increased, or the amount of the charge of powder be gradually decreased, a correspondingly increased amount of damage will be produced. Bullets going at low velocities generally make apertures many times their own size, irregular in shape, with fissures radiating from, and fragments of bone adherent to, their edges. They have also a great tendency to present depressed fractures, consisting of three or four triangular pieces of bone driven in at their apices, but still usually adherent, more or less, by their bases, to the skull; in

fact, they produce very similar wounds to those that are made by a hammer, whose striking surface is about the size of a shilling; and it is in these cases that the dura mater so often protects the brain, and entirely prevents the fragments being driven into that organ. It is remarkable what a slight resistance will often stop a nearly spent bullet. Three times the skull was struck obliquely with the old bullet, and in each case it glanced off, merely wounding the scalp. Eight times the skull was hit with the conical bullet slantingly, and in one instance only it failed to penetrate. When the bullet strikes in the above-mentioned manner the wounds are very much greater than when the ball hits at right angles, for there is a tendency for the bullet to be cut in two by the sharp edge of the bone, or to be otherwise altered in shape, thus producing more laceration of structure. In the above seven instances in which the skull was perforated, at an acute angle, by a conical bullet, the aperture of exit was in every case larger than the aperture of entry.

In eleven instances the skull was penetrated, at right angles, with flat-headed bullets, with a full charge of powder. They produced the cleanest cut apertures I have seen; the difference between the openings being less than in any other experiments. All the above results were found to ensue, equally, whether the shots were fired from the outside or inside of the skull.

If a nail, pickaxe, or any metal rod tapering to a point, be driven into the skull, either from within or from without, the aperture of entry will be found cleanly cut, and only sufficiently large to allow the instrument to pass, whereas the aperture of exit is the largest, in proportion to the aperture of entry, that can possibly be created by any penetrating force; its circumference is very irregular, and sharp triangular spiculæ of bone are generally found detached, some completely, others still adherent by one end to the aperture. The reason why in this variety of fracture, termed *punctured*, there is so little apparent injury at the aperture of entry, and such comparatively great destruction at the aperture of exit is, that the instrument acts, not only as a penetrating body, but also as a wedge, thereby giving rise to vibrations which destroy, to a great extent, the cohesion of the atoms of bone around the path traversed, and thus a greater separation of particles ensues.

The skull may be often struck with a brick or hammer with a considerable amount of force and yet no fracture occur; and extensive comminuted depressed fracture may be produced by either of the above two instruments without any injury whatever to the scalp beyond a bruise. The apertures of such fracture are always very large, with irregular edges, and have generally triangular pieces of bone adherent by their bases to the wound, their apices being depressed, either inwards or outwards, according to the direction of the force. When the great size of the openings is considered, it will be seen what little difference there is between them, the diameter of the aperture of exit not exceeding that of entry by more than half an inch.

All that has hitherto been related applies to bodies which penetrated the skull either from within or from without. When the instrument which strikes the skull does not itself pass through the bone then occasionally modified results ensue. For instance, when a person is thrown out of a carriage and the head strikes the ground, a portion of the skull is often driven inwards in a cone-shaped manner, and it will sometimes be found that the line of fracture in the external plate is much more extensive than the corresponding line of fracture in the internal plate; this is easily understood if it be remembered that the body which the head struck against did not itself pass into the skull, but merely depressed, or drove in, a portion of bone. This case, therefore, is one of non-passage into the skull of the fracturing body and does not come under the proposition I have laid down. It was related to me that a case occurred in which the aperture of entry was

actually larger than the aperture of exit. I stated that such a fact was a physical impossibility in all cases where complete passage of the instrument took place; and, it was then explained that the weapon, a tapering metal one, was impacted in the wound. It will thus be seen that this was a case of incomplete passage, and that the diameter of that part of the instrument in the aperture of entry exceeded the diameter of that part which had progressed further, and, in all probability the skull was soft and porous. I have never seen a similar wound in any museum specimens, nor have I been able to produce one. The use of the expression *aperture of exit*, in all such cases, is incorrect, as the instrument never made its exit.

It occasionally happens that there is an aperture of entry only, the foreign body, generally pointed, perforating one table and then sinking into the diploë. Five times I produced depressed fracture of the external table without any injury to the internal table, by striking the skull externally with a tapering crowbar, and three times I made depressed fracture of the internal table only, without any injury to the external table, by striking the inside of the skull with the same instrument.

Although displacement of bone inwards constantly occurs when the blow is from without, yet, displacement outwards may also occasionally happen, from violence acting in the above direction. However, it can only result when the penetrating body acts as a lever; for instance, a bar of iron may penetrate the skull, and then, by its own weight, tilt up a portion of bone; in fact, depressing one margin of the aperture, and elevating the opposite margin. When a man falls head-foremost on to the spike of a railing, it sometimes ensues, that, the head being fixed, the body describes part of a circle, and thus the spike acts, passively, as a lever, and elevates a portion of bone contiguous to the aperture.

The above experiments, therefore, prove, that, when a foreign body passes completely through any part of the skull, it matters not what the direction may be, the aperture of exit is always

larger than the aperture of entry, and they, moreover, show that the supposed greater brittleness of the internal table has nothing whatever to do with causing an aperture of exit in that plate to be larger than an aperture of entry in the external table.

Independently of any surgical interest the above facts may have, they are of great importance forensically, inasmuch as they enable us to state precisely, in nearly every case, what were the direction and nature of the fracturing body; and I will shew, that, in certain cases, we can diagnose, simply by reference to the bone only, whether the wound was made before or after death.

I now proceed to enquire into the causation of the comparatively greater size of the aperture of exit.

If the American, British, French, and German, surgical works be examined, it will be found they all state, that, when a foreign body penetrates the skull from without, the inner table is more injured than the outer one, and they assign as the cause one or other of the following reasons:

1. Because the internal table is more brittle than the other.

2. Because the internal table is not supported, and, therefore, that it suffers more than the external table which it supports.

3. Because the penetrating body loses part of its momentum in passing through the proximal plate and diploë, and, therefore, as it strikes the distal table with diminished power, it inflicts greater injury on it.

The following experiments, which I made, will show that the cause is not to be found in any of the explanations given, but is to be sought for in an, as yet, unnoticed fact. When it is considered that nearly every foreign body which penetrates the vault of the skull does so from without, and, that, as the result of this, the internal table is certainly damaged more than the external one it will at once be seen nothing was more likely than that surgeons

observing two bodies struck by the same power, and one always suffering to a greater extent than the other, should also come to the conclusion that it was the more brittle of the two. No one appears to have ever thought of reversing the conditions to see if like results followed. Professor Erichsen was, I believe, the first to show, in the earliest edition of his work published in 1853, both clinically and experimentally, that, if the direction of the force be from within outwards, the aperture of exit in the external table will be larger than the aperture of entry in the internal table, and gave as the reason of such fact the loss of momentum experienced by the penetrating body, in passing through the proximal table and diploë, causing it to strike the distal table with diminished power, and thus inflicting a larger wound.

Professor H. Meyer, in an article published at p. 85 of the second volume of Langenbeck's 'Archiv für Klinische Chirurgie,' for 1862, assigns two reasons for the greater damage done to the inner table when a foreign body penetrates the skull from without; firstly, the loss of momentum; and, secondly, that, as the skull is composed of two concentric hollow spheres the inner one has, necessarily, a less diameter than the outer one, and therefore, a segment from the outer table must be flatter than a segment from the inner table; consequently, as the result of this, when both tables are struck, the inner one suffers greater injury than the outer one, as its segment undergoes a greater change of form than the external one. Had Professor Meyer reversed the conditions of his experiments he would never have given the second reason.

I now lay down and proceed to establish the following proposition: *The aperture of entry is caused by the penetrating body only, whilst the aperture of exit is larger than the aperture of entry inasmuch as it is made by the penetrating instrument plus the fragments of bone driven out of the proximal table and diploë.*

When a bullet strikes the external plate from without it is the

only body which makes and passes through the aperture of entry in that table; whereas the bullet and the pieces of bone which it cuts out of the external table and diploë are all driven in upon the internal table, fracture it, and, finally, pass through it.

Fig. 1 is a horizontal section of the skull made through the plane of the gunshot holes, and it will explain the following experiment: If a bullet be fired with a full charge of powder from a revolver, close, and at right angles, to the external and lower part of the right parietal bone, A, it will pass through that bone, traverse the brain and its membranes, and, finally, emerge from the head at a corresponding point in the left parietal bone, D. There are, therefore, four openings in the skull, the apertures of entry, A, and of exit, B, in the right parietal bone, and the aperture of entry, C, and of exit, D, in the left parietal bone.

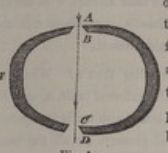

Fig. 1.

By some it would be stated that the aperture of exit, B, in the right parietal bone, is larger than the aperture of entry, A, in the same bone, because the bullet had lost some of its momentum in passing through the external table and diploë, and, therefore, that, as it strikes the internal plate with diminished power, it makes a larger opening. Now, if this explanation were true, then, *à fortiori*, the aperture of entry, C, in the left parietal bone, will be larger than the aperture of exit, B, in the right parietal. But what are the actual facts of the case as seen in the experiment? Why, the aperture of entry, C, in the left parietal bone, although made by the same bullet, is actually smaller than the aperture at B; not only is it smaller, but it is clean cut, and regular. This, therefore, conclusively proves that one supposed cause, loss of momentum, can have nothing whatever to do with causing the aperture at B to be larger than at A.

It is manifest, also, that the greater size of the aperture B, in the internal plate, is not caused by its greater brittleness, for it is seen that when the bullet crossed over to the other side it made a small clean-cut hole, C, in the internal plate, resembling that in the external plate, A.

It may be stated that B is always larger than A, that D is always larger than C, that C is always smaller than B; sometimes C is the same size as A, now and then a little larger than it.

Now, why is B larger than A, and C smaller than B? When the bullet cuts out the pieces of bone from the external table, A, and the diploë, it not only drives them forward, but it also tends to separate them, and cause them to exercise lateral pressure on all the bone contiguous to their paths, for, as the *vis viva* of the bullet is very much greater than the *vis viva* of each of the fragments, it must follow that the bullet, in its endeavour to outstrip the fragments, not only presses them forwards, but also pushes them to either side; and thus it is, that, the inner table being struck by a disc, composed of fragments of bone, whose surface is larger than that of the bullet, has a larger hole made in it than that in the external table which was struck by the bullet only. C is smaller than B because it is made by the bullet only. This is how it is that the aperture of entry in the left parietal bone is smaller than the aperture of exit in the right parietal, for the fragments of the external table and diploë of that bone make the aperture of exit in the internal table larger than the aperture of entry, but they have no influence whatever on the second aperture of entry in the left parietal bone The smaller the hole the bullet makes in the proximal table, the larger, comparatively, will be the aperture in the distal plate, for, the narrower the path the bullet makes for itself, the greater pressure it will exercise on all before it. Thus it is that there is but little relative difference between the openings of the wound made by a bullet whose velocity is low, although that wound may be

very large; in fact, it will generally be found that the larger the wound the less difference in size between its apertures. The experiment above related was repeated seven times, and, in order to obtain the results described, it is necessary that the head be fixed, that the skull should be perforated at right angles, by the same bullet, at two points where the bones are exactly the same as regards relative thickness, position, &c., and also that the bullet should not alter in shape in passing through the first side, for should it become flattened the conditions are at once altered, and hence, the second side being struck by what is now an entirely different weapon, as regards shape, we cannot draw any correct inference from such results: the use of iron bullets almost entirely prevents the above fallacy from taking place. The experiment can be very well exemplified on two well-planed planks of dense wood, each about one inch thick (fig. 2). If one plank be fixed a few inches behind the other, and a shot fired through both of them, it will be found that the aperture of entry, M, in the first plank, is small and regular, whereas the aperture of exit, N, in the same plank, is very much larger and ragged.

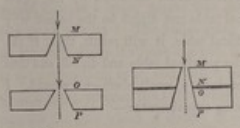

Fig. 2.

Fig. 3.

The aperture of entry, O, of the same bullet, into the second plank, will be found cleanly cut and very much smaller than the aperture of exit in the first plank. This results from the fact that the fragments driven out of the first plank have no influence whatever on the second plank, which is separated from it. If, however, both planks be brought together, firmly braced, (*see* fig. 3), and a shot fired through both of them, we shall have different results, for, the second plank, being close to the first one, is acted on by the fragments driven out of it, and, it will therefore be found that the

aperture of entry, o, in the second plank is now large and irregular, clearly shewing that the fragments cut out of an aperture of entry are the active agents in making the aperture of exit larger and irregular.

It is stated, by some, that an aperture of exit, in either table of the skull, is larger than the aperture of entry, because the distal table supports the proximal one, and, that, therefore, the latter suffers less. If this were true, it ought to result, that if the distal table be removed, and a shot fired through the now unsupported table, a large irregular hole will be made. I accordingly cut away one table and the diploë, with a trephine whose diameter was three times as great as that of the bullet, and fired through the now unsupported plate—the wound was as small and as clean cut as ever. This experiment I tried six times, equally on both tables, with always the same results.

If it be true that the loss of momentum sustained by the bullet in passing through the proximal table and diploë be the reason why it makes a larger hole in the distal table, it will result, that, if the bullet has not to cut through the proximal table and diploë, and thus lose no momentum, it will produce a small clean-cut wound. I therefore performed the following experiments:—

At three points on one side of the median line of the skull—one point in the frontal bone, another in the parietal, and a third in the occipital—I cut through the external table and diploë, with a trephine of the same diameter as the bullet, and thus isolated, but not removed, a button of bone, and, at corresponding points on the other side of the median line, I cut through and removed, with the same trephine, a button of bone out of the external table and diploë. Each spot was then fired on to successively, and the result was, that, in each case where the button of bone had been removed, the aperture in the distal table was scarcely larger than the bullet, whereas, in each instance in which the button of bone had been isolated, but not removed, the

aperture of exit was just as large and irregular as usual. The differences between the apertures on one side of the median line and those on the other were very marked.

The above procedure was followed on the outside of two skulls, and on the inside of one skull, in each instance with the same results.

In fact, if the proximal table and diploë be removed with a trephine from any part of the skull, either inside or outside, and a shot be fired through the remaining plate, the aperture will be almost the same size as the bullet, and tolerably regular; but, if, previously to firing, the button of bone be replaced after removal, then the result will be entirely different—the wound will be irregular and very much larger than the bullet; thus clearly showing that the bone cut out of one table and driven on to the other is the cause of the greater size of the aperture of exit.

If a bullet penetrate the skull, at a low rate of velocity, why should it make a larger wound than that made by a bullet travelling at a great velocity? The bullet going at full speed affords no time for the part, which it strikes and puts into motion, to transmit motion to the surrounding bone, and, hence, merely cuts out a piece of bone its own size; whereas the bullet whose rate of velocity is low allows time for the part which it strikes to communicate force to, and set in motion, a considerable portion of the contiguous bone not struck; and, consequently, the lower the rate of velocity, the larger will be the wound, for, as more time is given, more particles will be set in motion. I have already stated that I have observed there is a gun-shot wound of a peculiar kind produced after death, and, so far as I have been able to ascertain, it cannot be made during life. If a ball penetrate, at right angles, both sides of the skull of a living person, the first aperture of entry will be of exactly the same diameter as the ball, regular, and free from any splinters or fissures; whereas, if both sides of a dead man's skull be pierced by one ball, then the first aperture of entry will be found rather larger than the

ball, irregular, and generally complicated with fissures, thus differing entirely from what is seen in life. The rationale of the above is as follows—when a bullet strikes the living head with great velocity, the cranium, being fixed, resists the blow, and the bullet passes through without impressing any motion on it; but, after death, the head, being no longer fixed, partly yields to the impetus, and, therefore, as the time of the bullet's impact is longer, a greater number of atoms of bone are set in motion—thus causing a larger wound than is produced during life. If a bullet can perforate both sides of the cranial cavity it shows that it has sufficient power to go through the proximal table without making a larger wound than is necessary for its passage, consequently, if the aperture of entry be found large, irregular, and fissured, the cause of the altered facts must be sought for, not in any supposed diminution of the bullet's velocity, but, in the cessation of muscular contractility. If however, a dead man's head be fixed, either artificially or by the rigor mortis, we can produce the same effects as in life. There is, therefore, a particular kind of gunshot wound which can be produced after death, but cannot be made during life. The converse of this, however, does not hold good.

In another communication I intend to make some further remarks on the subjects of this paper, and, also, to give some observations of surgical interest.

AN INQUIRY

INTO THE

CAUSATION, DIAGNOSIS, AND TREATMENT

OF

FRACTURE OF THE INTERNAL TABLE OF THE SKULL.

BY

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An Inquiry into the Causation, Diagnosis, and Treatment of Fracture of the Internal Table of the Skull. By WILLIAM FREDERIC TEEVAN, F.R.C.S., B.A., Surgeon to the West London Hospital, &c.

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It is, firstly, necessary to establish the existence of fracture of the internal table only, resulting from violence applied to the exterior of the skull, inasmuch as it is denied by some, doubted by many, and admitted by but few, English Surgeons. I will, therefore, adduce not only many well recorded cases, but, also refer to such pathological specimens, illustrative of this particular injury, as I have been able to discover during a personal examination of all the pathological museums in London, the museum at Netley, and the Musée Dupuytren in Paris.

It has, by some, been supposed that this variety of fracture was known to Hippocrates; this, however, is incorrect, as a careful examination of his works will shew. One of the earliest writers who was aware of this lesion was Jacobus Berengarius Carpinensis, who stated, at page 6, of his work, "De Fractura Cranii," published in Bologna, in 1535: "Est alia species in qua os frangitur tantum inferius versus panniculum, et dicitur Marusis ab Haly, in Pantœgni vero Monesis vel Marusis. Hanc speciem credo ego esse plicaturam ossis, quando os plicatur, et intra tantum rumpitur et non extra." However, the earliest recorded case I have been able to find is that by Ambrose Paré, at page 225, of the tenth volume of his works, published in 1652: "Ce que j'ay veu aduenir à vn gentilhomme de la compagnie de monsieur d'Estapes, lequel fut blessé sur la brèche du chasteau de Hedin, d'un coup d'arquebuse qu'il reçeut sur l'os pariétal, ayant vn habillement de teste, lequel la balle enfonça sans estre rompu, ny pareillement le cuir, ny le crâne extérieurement, et le sixième jour mourut apoplectique. Done

aduint que pour l'ennuie que j'auois de cognoistre la cause de sa mort, je lui ouuris le crâne auquel trouuai la seconde table rompue, avec esquilles d'os qui estoient insérez dans la substance du cerueau, encore que la première fust entière. Ce que pareillement atteste auoir veu et monstré à messieurs Chapelain, premier médecin du roy, et Chastelan, premier de la reyne, à vn gentilhomme qui fust blessé à l'assaut de Roüe." Saucerotte, in his essay on "Contre-Coups," at page 415, of the fourth volume, of the Memoirs of the Royal Academy of Surgery, mentioned instances of this fracture which occurred to Tulpius, Mery, Le Dran, and Soulier. Pott, at page 273, of the second edition, of his work, "On the Injuries of the Head," related two cases which fell under his own notice. In Velpeau's well known work, "De l'Opération du Trepan," at page 29, of the edition of 1834, two examples are recorded which happened to Bilguer. In the Handbuch der Praktischen Chirurgie, by V. Bruns, page 297, vol. 1, references are given to twenty cases; and, in the "Archiv für Pathologische Anatomie," vol. 22, page 80, two instances are given. Mr. Guthrie, at page 329, of the fifth edition of his "Surgical Commentaries," gave an account of an interesting case which occurred to Mr. Dense. In the "Archiv für Klinische Chirurgie," page 547, of the second volume for 1862, there is an article entitled, "Ueber isolirten Bruch der Glastafel," by Dr. B. Beck, who brings forward an instance which fell under his care at the Battle of Vicenza, in 1848, but it is not a strictly correct specimen of the fracture as there was a fissure in the outer table. I shall, however, allude to his explanation of the cause of the fracture at another page. A very interesting case, which has been referred to by many continental surgical writers, occurred to Mr. S. Cooper, at the Battle of Waterloo, and was narrated by him, at page 1270, of the seventh edition, of his Dictionary of Practical Surgery. Very recently, an instance, in which fracture of the internal table produced laceration of the middle meningeal artery, and fatal extravasation, happened to Mr. Edwards, and is recorded, at page 191, of the eighth volume of the Edinburgh Medical Journal. But, perhaps, the most interesting examples, inasmuch as the crania are preserved,

occurred, respectively, to M. Denonvilliers in Paris, and Dr. Cowan in the Crimea. In the first instance, a young man was struck on the head by a bullet, which wounded the scalp, but not the bone. He remained well for a fortnight, when symptoms of encephalitis came on, and M. Denonvilliers trephined him, on the 25th day, at the spot struck. The disc of the external table came away in the trephine, and a piece of the internal table was then seen lying on the dura mater. The detached portion had to be broken in pieces before it could be withdrawn through the trephine hole. Temporary relief only followed the operation, for the man died a week after its performance. Dr. Cowan's case is thus related in Holmes' System of Surgery, vol. 2, page 47: "Fissured fracture of the inner table may also occur from the action of a ball without external evidence of the fracture. Such a case occurred in the 55th Regiment in the Crimea. The soldier had a wound of the scalp along the upper edge of the right parietal bone. The ball in passing had denuded the bone, but there was no depression. The man walked to the camp from the trenches without assistance, and there were no cerebral symptons on his arrival at the hospital; but five days afterwards there was general œdema of the scalp and right side of face, the wound became unhealthy, and slight paralysis appeared on the left side. The next day, hemiplegia was more marked, convulsion and coma followed, and he died on the thirteenth day after the injury. Pressure from a large clot of coagulum, and extensive inflammatory action, were the immediate causes of death; but a fissure confined to the inner table, running in line with the course of the ball, was also discovered. A preparation of the calvarium in this case was presented, by Dr. Cowan, 55th Regiment, to the museum at Fort Pitt." I could refer to many other undoubted cases of fracture of the internal table only, but it would be superfluous to do so.

I have only been able to find two pathological specimens, illustrative of this variety of the fracture of the skull. The first specimen, numbered 29 A, in the Musée Dupuytren, at Paris, is the calvarium taken from the man who was trephined by M. Denonvilliers, and is a very well marked case of a detachment

of the internal table only. The second specimen, which is in the museum at Netley, is the skull cap taken from the soldier who was under Dr. Cowan's care, and it shows a straight fracture a few inches long, with slight separation of its edges, affecting the internal table only, of the right parietal bone. In each instance there was no fracture nor fissure of the external table, and in each the condition of the bones was quite normal. I have purposely excluded all references to those specimens of fracture of the internal table in which there were any fissures in the external as being imperfect illustrations, for, if the case be one of fracture of the internal plate with a fissure in the outer, it is, in reality, a complete fracture of both laminæ. It will often be found, in complete fractures of both tables, that there are certain fissures or fractures in one table without any corresponding ones in the other; but they have no bearing whatever on the present subject.

Thus, it will be seen that the occurrence of fracture of the internal table, without the slightest injury whatsoever to the outer table, from violence applied to the exterior of the skull, is placed beyond all doubt.

It is here necessary to make some remarks regarding the physical properties of the skull. It may be, temporarily, depressed at a spot without any fracture being produced, as is well seen when a dry skull cap, being allowed to fall on a stone floor, rebounds without any fracture taking place. This can only result from the elasticity of the bone, which implies that there was a certain amount of depression, or flattening, of the part struck, at the moment of impact—it is the act of the depressed bone in recovering its former position which causes the rebound. It is imagined that, on account of the arched form of the skull, the effects of violence on the inside of the cranium must be very dissimilar to those on the outside. Now this is certainly true regarding those bodies which act over a large portion of the head, but small bodies, such as bullets, which only strike a very limited part of the skull, produce exactly similar effects, whether they act on the inner or outer surface of the calvarium, supposing always the amount of force similar, inasmuch as every point on the inside or outside of the

skull is virtually a flat surface. It must also be premised that, with the exception of those places where there are sinuses, there is, physically speaking, no such thing as a distinct and separate inner or outer table. The two laminæ and intervening diploë are inseparably blended together, and are one and indivisible.

It is asserted, in most surgical works, that because the inner table of the skull is more dense than the outer, it is necessarily more brittle. Now it is a fact, in physics, that if a given body is more dense than another, it does not consequently follow that it is more brittle, and I have already shown, and will still further show, in this enquiry, that the inner surface of the skull is not more brittle than the outer—the fact implied in the term "lamina vitrea" is founded on an assumption which cannot be supported.

What is the causation of this fracture?

Most of the French surgeons regard this fracture as an example of contre-coup, resulting from the greater brittleness of the inner table, as will be seen by the following quotations. "Premier cas. La table externe percutée, peut résister, tandis que la table interne se fracture immédiatement au-dessous parcequ'elle est plus cassante, aussi l'a-t-on appelée vitrée."* Velpeau assigns a similar reason: "Plus mince, moins étendue en surface, plus irrégulière, plus dense que la table externe, la couche vitrée éclate et se fendille sous un effort manifestement moindre que la précédente."† It would, indeed, seem as if most of the French writers had adopted the views of Saucerotte, who regarded the fracture as a variety of fracture by contre-coup, and in the following words gave his explanation of it: "Qu' un instrument contondant soit appliqué, avec violence, sur un os de la tête où les deux tables soient distinctes l'endroit frappé, de convexe qu'il étoit, deviendra concave; par conséquent il y aura un grand déplacement dans ses parties intégrantes: car une voute menace de la plus prochaine destruction, lorsqu'un corps quelconque, qui, par sa masse ou sa vitesse surpasse sa résistance, tend à l'énfoncer. Or, il n'est plus étonnant que la table vitrée se rompe, parce qu'elle est

* Vidal, de Cassis, Pathologie Externe, tome, 2, p. 545.
† De l'Opération du Trepan, p 29.

mince, sèche, et fragile, quoique l'externe, par sa souplesse et son élasticite, prête à l'effort du coup.* Legouest, who is one of the most recent writers on military surgery, considers it an instance of direct fracture, at the same time he ascribes its occurrence to the greater brittleness of the inner table: "Lorsque l'os frappé est épais, resistant et à diploë solide, la table interne, plus mince et plus friable que l'externe, se rompt quelquefois, cette dernière restant intacte."†

The German surgeons are of opinion that fracture of the internal table occurs from its greater brittleness, and Dr. Bernhard Beck, in the article I have already alluded to, assigns an additional reason—the shortness of the inner table. He states that the outer table has a much larger superficies than the inner, and, therefore, a greater faculty of extension from the shorter and more brittle tabula vitrea, and, consequently, when a projectile hits the outer table obliquely and sets it in vibration, it suffers a stretching and displacement of its tissue: the shorter brittle table follows the process in like manner, but not quickly enough, and is, therefore, broken. It is the shortness and brittleness of the inner, not any depression of the external, table, which cause it to break. He also tried many experiments on the dead body, striking the head with bullets, and often produced fracture of the inner table, when the outer was neither depressed nor fractured. According to his experiments the outer table can be depressed from half-a-line to one line without breaking.

Those English surgeons who have admitted the existence of this fracture state that it occurs from the greater brittleness of the inner table. Sir B. Brodie's words express the English belief: "The greater elasticity of the outer table of the skull, and the greater brittleness of the inner table, seem to afford the only reasonable solution of these phenomena."‡ It will thus be seen that all surgeons assign, as the cause of this fracture, the greater

* Mem. sur les sujets proposés pour le Prix de L'Acad. Royale de Chir. tome IV, p. 374.

† Traité de Chir. d'Armée, p. 283.

‡ Med. Chir. Trans. vol. 14, p. 331.

brittleness of the inner table, and Dr. B. Beck gives an additional explanation—its shortness.

I will now show that the cause of this fracture is not to be sought for in any of the reasons given, but that it occurs in obedience to a well known physical law—that fracture commences in the line of extension not that of compression.

If it were true that the shortness and brittleness of the inner table were the cause of its fracturing, then it would follow that, if violence were applied to the inner surface of the skull, it would be impossible to produce fracture of the external table only, without any injury whatsoever to the inner. Now what are the results of my experiments on this point? Why, they show that, if the inside of the skull be struck the external table can be fractured without any injury whatsoever to the internal table—thus proving that the alleged shortness and brittleness can have nothing to do with the causation of this fracture. I give the following details of an experiment, which any one can, with a little practice, perform. A skull cap, stripped of all its soft parts, with a wet cloth inside it, is to be laid with its convexity in the palm of the left hand, which is to be protected with several layers of a moist cloth, to obviate an inconvenient amount of pain. If the inside of the skull be now struck by a hammer, with a slight degree of force, fracture of the external table will be produced, without any fracturing or fissuring of the inner. I have performed this experiment several times, and exhibited some of the specimens at the Pathological Society. *I would here refer to a specimen, which is quite unique,

* Since the above was in type I have met with another case:—"Mr. Trevan exhibited a calvarium, showing a fissured fracture of the *external table only*, produced by a bullet striking the *inside of the skull*. The skull cap was taken from a Swiss, who shot himself a few weeks ago, and died in one of the London Hospitals. The bullet entered the right parietal bone, traversed the brain and its membranes, struck the inside of the left parietal bone, and remained imprisoned in the cranium. There was no fissure or fracture at the spot on the inner surface of the left parietal bone where the bullet struck, but at the corresponding spot in the outside of the bone there was a starred, fissured fracture of the external table only. Mr. Trevan remarked, that this variety of fracture had never been described, and that he brought forward the specimen to corroborate his own experiments on this subject, and in support of the statement which he had made to the Society on a former occasion—that, whether the inside or outside of the skull be struck, fracture of the distal table only, without injury to the proximal one, can be produced in either case, and that such fracture occurred in obedience to a well-known physical law—that fracture commences in the line of extension, which is the distal side, and not in that of compression."—*Lancet, Nov. 8th*, 1865.

of a certain fracture of the skull, which has never been described by any surgical writer. In Guy's Hospital Museum, No. 1082[29], there is the calvarium of a man who committed suicide by shooting himself. The bullet entered the skull in the right temporal region, traversed the brain and its membranes, struck the inside of the left part of the frontal bone, and remained imprisoned in the cranial cavity. At the spot on the inner surface of the left frontal bone, where the bullet struck, there was a black mark, but no fissure nor fracture, but, at the corresponding point outside there was a starred, fissured fracture of the external table only. Here, then, was produced, accidentally, a similar kind of fracture to that which I had caused experimentally.

Thus, therefore, whether the inside or outside of the skull be struck, fracture of the distal table only, without any injury to the proximal table can be produced in either case.

I will show the causation of the above facts.

If a stick be bent across the knee, until it commences to break, it will be found that the fracture begins, not at the spot where the knee is applied, but at a point exactly opposite on the other side, and the fracture commences there in obedience to a well-known physical law that, when pressure is applied to a body till it breaks, the fracture commences in the line of extension, not that of compression. Now, when a stick is bent, the atoms along the proximal curve at which the pressure is applied are brought nearer together or compressed, and the atoms along the distal curve are separated or extended; therefore, if the pressure be continued till the stick breaks, it follows that the rent or fracture must commence at that spot in the distal curve where the greatest extension is going on, which point will be found exactly opposite to where the pressure is applied.

The annexed diagrams show the *rationale of Fracture of the Internal Table only, produced by a blow on the outside of the skull.*

Fig. 1.

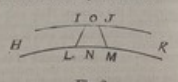

Fig. 2.

Let A B (Fig. 1) be a section of the skull. Draw two vertical lines, C E and D F, parallel to one another. Now, if pressure be applied at G, temporary depression takes place, and the bone assumes the shape of H K (Fig. 2), and the lines C E and D F, are no longer parallel to each other, but converge towards each other at the upper surface, I L, J M, so that the distance from I to J is less than that from C to D, but the distance from L to M is greater than that from E to F, signifying that the atoms of bone in the upper surface from I to J have been brought nearer to each other, or compressed, whilst the atoms of bone in the lower surface from L to M have been extended or separated from each other; therefore, if any fracture take place, it is clear it must do so in the line of extension L M, and at that point in the line where the greatest extension is going on, which is at N, exactly opposite the spot O, where the pressure was applied.

Proof: Take a cane, slightly bent, say A B (Fig. 1), and insert two pins or wires, C E, D F, vertically, and parallel to each other: the more the pins project at each surface, the more manifest will be the result. Exert pressure at G till the cane is made flatter, H K. It will now be found that the wires are no longer parallel to each other, but converge along the upper surface, so that the distance between them from I to J, is less than that from C to D; but the distance from L to M is greater than that from E to F, showing clearly that the atoms along the lines I J have been compressed and brought nearer to each other, whilst those along the line L M have been extended; consequently, if any fracture take place, it must commence at N. If the pressure on the cane be continued till it breaks, it will be found that it commences to break at the point N.

A familiar instance of a fracture accidentally occurring in nature, similar to fracture of the internal table only, is when the ice cracks under pressure. It will often be seen that there are cracks in the under surface of the ice and none in the upper, and it will always be found that when a crack takes place it commences in the under surface, thus illustrating the fact that the fracture commences in the distal side, which is the line of extension.

The next diagrams show the rationale of *fracture of the External Table only, by a blow from within the skull.*

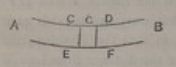

Fig. 3.

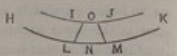

Fig. 4.

Let **A** B (Fig. 3) be a section of the skull. Draw two vertical lines parallel to one another, C E, D F. When pressure is applied at a point, G, on the inside of the skull, the bone is temporarily depressed very slightly and assumes the shape H K (Fig. 4), and the lines C E, D F, are now no longer parallel to each other but converge at O, so that the distance from I to J is less than that from C to D, but the distance from L to M is greater than that from E to F; therefore, the atoms of bone from I to J have been brought nearer to each other by being compressed, whilst those from L to M have been extended. If any fracture should take place it must do so in the line of extension L M, and at the point in the line where the greatest amount of extension is taking place, **N**, which is in the outside of the skull.

Proof: Take a cane and bend it slightly, A B, (Fig. 3.) Insert two pins or wires, C E, D F, vertically, so that they may be parallel to each other—the longer the pins, the more manifest will be the result. Exert pressure on the concavity at the point G, so that the cane assumes the shape H K (Fig. 4). It will now be found that the wires are no longer parallel, but converge towards each other at O in the upper surface, and that the distance from I to J is less than that from C to D, but that the distance from L to M is greater than that from E to F, showing that the atoms along the line I J have been brought nearer to each other by being compressed, whilst those along L M have been extended. Therefore, if any fracture takes place, it is clear that it will commence along the line of extension, L M, and at that point, **N**, in the line where the greatest extension is going on. Consequently, if the pressure at O be continued till the stick begins to break, it

will be found that the fracture commences at **N**. It is stated by Dr. Beck, that fracture of the internal table only occurs in those parts where there is but little diploë. I, on the contrary, could only produce the fracture where the diploë was abundant, and, certainly, nearly all the cases recorded, and also the pathological specimens, shew that hitherto the fracture has nearly always taken place on the inside of one of the parietal bones, which, as is well known, contain a thick diploë. It is very difficult to produce an incomplete fracture of a thin body, for, if the pressure be sufficient to cause fracture, that fracture will not be limited to one surface, but will affect both; whereas, if the material be thicker, it will be found, although a much greater pressure will be required to effect fracture than in the former case, yet, that fracture, limited to the distal surface, may be brought about. Hence, fracture of the internal table only is, in reality, an incomplete fracture of the skull, for, as fracture always commences in the distal table first, whether the violence be applied to the inside or the outside of the skull, it follows that, if the force exerted be not sufficient to cause a complete fracture of both tables, the effects of the violence are spent on the distal table.

Legouest states: "Ces fractures de la table interne, mises hors de doute aujourd'hui, ne peuvent être produites que par l'action obliquement dirigée d'un projectile, ou par le choc médiocre d'un corps à surface étendue, plane et régulière," and Dr. B. Beck also states that when the fracture takes place it is produced by a body striking obliquely. It is manifest, however, that it is not necessary that a bullet should hit obliquely in order to fracture the internal table only. What is necessary is that the bullet should not strike with much force. Now, a bullet striking at right angles, when at full speed, does not bend the part it hits, but carries away bodily all that bone which is immediately in front of its path, whereas, when it hits slantingly, it acts with but little force on the point of impact, and very slightly depresses the bone, temporarily; consequently, if a spent bullet strike, at right angles, it may cause this fracture. In every case in which I produced it, it was by hitting the skull at right angles with but little force. Hence, the kind of

violence likely to cause fracture of the internal table only is that resulting from a small stone, spent bullet, stick, or some body acting with a slight amount of force on a limited part of the skull; merely temporarily depressing or bending the part struck.

I now proceed to the diagnosis and treatment of the fracture in question. This lesion is not necessarily followed by any bad consequences, but should it cause damage to the cranial contents, it would be in one of the following ways:—

1. The spiculæ, or jagged edges of the fracture, may irritate, or lacerate, the brain and its membranes, and thus cause acute or chronic encephalitis. This is the most likely result to follow, inasmuch as the edges of the bone are very thin and sharp, and an entire piece of the internal plate is sometimes detached, and may thus irritate, but not compress. It will be observed that the symptoms of this fracture are more likely to resemble those following the *punctured fracture* than any other.

2. The line of fracture may cross the course of the middle meningeal artery, lacerate it, and so allow extravasation of blood to take place. The sinuses may also be torn in the same way.

3. Part of the inner table may be depressed in such a manner as to cause compression of the brain. Thus, this injury may cause inflammation or compression of the brain, and the symptoms may therefore vary in different persons. Now, as this fracture can never be seen during life, it is obvious that its diagnosis must often be doubtful, and sometimes impossible, for it is only by the occurrence of certain symptoms, after a certain injury, that its existence can even be suspected. It is, in the first place, requisite to pay attention to the kind of instrument which may have struck a person in a given case, for as I have already pointed out, this fracture has hitherto been caused by a small body, such as a stick, stone, or bullet, striking with but slight force, and it must be remembered that there is, generally, no injury whatsoever to the part of the external table which is struck, and that the fracture always occurs at a point in the internal table corresponding to the spot struck externally. Concerning this last statement, I may mention that all evidence is conclusively confirmatory, and notwithstanding Saucerotte's assertion that the fracture in the internal table may occur at some distance from the point struck. Thus, as every fact shows that should fracture of the internal table happen, it will be found at a point corresponding to the spot struck externally, it follows that should the trephine be required, it must always be applied to the spot which was struck.

I will enumerate those symptons which are sometimes diagnostic of the occurrence of fracture of the internal table only, but it is very rarely, indeed, that its existence can be absolutely determined.

If a person, after receiving a blow of the description I have already mentioned, should, in the course of some days, or weeks, begin to complain of a fixed pain at the spot struck, and be finally attacked with encephalitis, we may conclude, if on examination no injury can be detected to the bone struck, that some spiculæ of the inner table, or the ragged edges of the fracture, are scratching the membranes, and giving rise to an amount of mischief which will very probably terminate in intra-cranial suppuration. No acute symptoms may perhaps arise in a given case, but it may degenerate into chronic cerebral irritation. In those cases where the symptoms of the formation of pus are well marked, with paralysis of the side opposite to that struck, the diagnosis would be very probable.

If a person should show the symptoms of compression, with paralysis of the side opposite the injury, a few hours after receiving a blow in the neighbourhood of the middle meningeal artery, and on examining the bone struck we can detect no injury to it, although there may or may not be a scalp wound, we may infer that the vessel has been torn by a fracture of the internal table running across the direction of the artery, and that extravasation of blood is going on.

When compression of the brain is caused by a depression of the internal table alone, the patient may become insensible on the receipt of the blow and remain so. There would be paralysis, more or less, of the side opposite to that struck, but generally the symptoms of compression are not well marked. There might, or might not be, a scalp wound, but there would be no injury whatsoever

to the external table; if there was a fissure the case would be one of complete fracture of both tables, with depression of the inner one only. Now, if the patient was insensible from the first, it is evident that the above symptons might result from an intra-cerebral extravasation of blood on the side on which the blow was received and hence a diagnosis would be impossible. But it must be recollected that the compression produced by depressed bone is rarely so complete as that caused by an extravasation of blood, and that when the internal table only is depressed, the symptoms of compression will not be nearly so strongly marked as when both tables are driven into the brain. Hence, the less definite the symptoms of compression, the greater the reason to believe that they are caused by the internal table only.

There are two cases in which the diagnosis may be made with almost certainty. Firstly; when a person recovers immediately after the blow, but finds there is paralysis of some part of the body opposite to the side struck, and examination fails to detect any injury to the bone. Secondly; when, after the blow, no evil consequences arise at first, but, in the course of time the patient begins to complain of fixed pain in the part struck, and all the symptoms of chronic cerebral irritation show themselves, although the surgeon cannot find any injury to the external table.

Inasmuch, therefore, as fracture of the internal table only, can never be seen during life, and can only be diagnosed by the occurrence of certain symptoms after a certain injury, it is evident that its treatment can never be preventive.

There can be no doubt that there is, both at home and abroad, a yearly increasing dislike to resort to the use of the trephine, resulting not only from the very great mortality which attaches to every violence, whether surgical or accidental, to the head, but also from the fact that the patient will often die, although the trephine may have effected the end proposed, and it is very certain that, in the present day, lives are saved, which, in time past, would have been lost. But, inasmuch as the treatment now pursued is chiefly one of expectancy, it is necessarily fraught with much danger in certain cases. Surgeons of the present time are

unanimous in stating that no such success follows the use of the trephine in their hands as attached to its employment by surgeons of the past, and they explain the discrepancy by alleging, what is no doubt perfectly true, that the operation was often unnecessarily performed on persons who had nothing the matter with them. If, therefore, that were so, it would clearly prove that the use of the trephine, on a healthy subject, is not, *per se*, the very dangerous operation it is stated to be. All surgical experience shows that the mortality which will attach to an operation performed on parts before they are in a state of inflammation, must be very much less than that which will follow interference with parts acutely inflamed; consequently, the use of the trephine as a preventive means is widely different from the employment of the same instrument as a *dernier ressort*. In the present day surgeons rarely apply the trephine at once except in cases of punctured fractures. Now, I have already stated that the effects produced by a fracture of the internal table, are most likely to be similar to those caused by the punctured fracture, but we cannot thereupon follow the treatment adopted in the latter case, inasmuch as fracture of the internal table can never be seen on its immediate occurrence, but only suspected at a later period by the advent of certain symptoms. Hence, the urgency, or persistency, of the symptoms can alone justify the use of the trephine in suspected instances of fracture of the internal table; consequently a surgeon would be warranted in trephining, on the spot struck, if all the symptons of intra-cranial suppuration were well marked, and there was paralysis of the side opposite to the injury; so also if the violence had been applied in the course of the middle meningeal artery, and the symptons of compression were urgent and persistent, together with paralysis of the opposite side; and, likewise, if the patient had been insensible from the first, with obstinate symptoms of compression and paralysis of the side opposite. It happens sometimes that a patient is never rendered insensible by the blow, but finds he has paralysis of some part of the side of the body opposite to that struck. Now, if, in the course of some weeks, the paralysis does not disappear, the use of the trephine, to the part struck, would be indicated.

A man occasionally receives a blow on the head, from a stick, or stone, which causes no inconvenience at first, but, in the course of some days, or weeks, he begins to complain of pain in the part struck, and is finally attacked with chronic cerebral irritation. On examination, no injury can be detected to the part hit, and if, in the event of there being no scalp wound, an exploratory incision be made down to the spot no fissure nor fracture be found in the external table, but the symptoms persist, and the patient continue to suffer from fixed pain in the part struck, we may suspect that there is a fissure or fracture implicating the internal table only. Now, if, after a persevering use of those means fitted for such a case, the symptoms above described do not disappear or abate, the use of the trephine, to the part struck, will become necessary. A labouring man, suffering from chronic cerebral irritation, must be considered virtually dead, for, not only is he completely incapacitated for supporting himself and family, but he is unable to enjoy life, and becomes a burden to himself and others. The question ought to be, not whether, in a solitary case, the trephine can effect a cure, but whether, out of one hundred cases, it can restore some. No answer can be given to the first proposition, but the second can be answered, and that, too, affirmatively.

HOW WERE THE FRACTURES OF THE ORBITAL PLATES OF THE FRONTAL BONE OF THE LATE PRESIDENT LINCOLN PRODUCED?

To the Editor of THE LANCET.

SIR,—In the record of the post-mortem examination of the late President Lincoln, drawn up by Acting Assistant-Surgeon Taft, of the United States Army (*vide* THE LANCET, June 17th), it is stated that the bullet—a round one—entered the occipital bone one inch to the left of the longitudinal sinus, and was found imbedded in the right anterior lobe of the brain, immediately behind, but not in contact with, the orbital plate. The following facts are given:—

"The hole made through the occipital bone was as cleanly cut as if done with a punch. The orbital plates of both orbits were the seats of comminuted fracture, the fragments being forced inward, and the dura mater covering them remaining uninjured. The double fracture was decided to have been caused by contre-coup."—*Philadelphia Medical Reporter.*

For the production of fracture by contre coup, it is absolutely necessary that force be transmitted from the spot struck; and if no such transmission took place in the case under consideration, it is clear that the fractures cannot be regarded as examples of fractures by contre coup. Now it is stated that the aperture of the bullet's entry was only half an inch in diameter, and as cleanly cut as if done with a punch. These facts resulted from there having been no transmission of force. The following words of mine (*British and Foreign Medico-Chirurgical Review* for July, 1864, p. 213) explain the reason why:—

"The bullet going at full speed affords no time for the part which it strikes, and puts into motion, to transmit motion to the surrounding bone, and hence merely cuts out a piece of bone its own size; whereas the bullet whose rate of velocity is low allows time for the part which it strikes to communicate force to, and set in motion, a considerable portion of the contiguous bone not struck; and consequently, the lower the rate of velocity, the larger will be the wound, for as more time is given, more particles will be set in motion."

Consequently the fractures of the orbital plates could not have been produced by contre-coup, as there was no transmission of force.

Prof. Longmore, in a communication to THE LANCET of June 17th, considered that the fractures were caused by the brain being driven, by the impetus communicated to it from the bullet, against the orbital plates.

I, however, am of opinion that the fracture of the right orbital plate was produced by the direct action of the bullet, and that the left orbital plate was broken by an extension of the fracture into it from the adjoining plate. The

fact of the dura mater covering the orbital processes not being injured proves nothing, inasmuch as it is, I should suppose, well known that when a spent bullet strikes a bone protected by a cloth, skin, or membrane, the bone is often fractured, whilst the covering remains uninjured. It was assumed, that because the bullet was not found in contact with the orbital plate, it had, therefore, never struck that bone. The bullet, however, was discovered in the very position I should have supposed; for I have ascertained from repeated experiments on the dead body, that when a round ball is fired from a pistol, at the distance of only a few feet from the head, it makes a clean-cut aperture of entry, traverses the brain, strikes a spot on the inner surface of the skull, immediately opposite the aperture of entry, and then recoiling into the brain, remains imprisoned in the cranium. The conical bullet, however, fired at a short distance with a full charge of powder, from a revolver, not only generally enters the head, but also makes its exit from it. Inasmuch, therefore, as the injuries sustained by the late President Lincoln were exactly similar to those which follow the use of a similar weapon on the dead body, I am unwilling to believe that there was any departure from well-ascertained causes and results, and that any apparently inexplicable circumstance arose from some imperfection or want of minuteness in making the post-mortem examination, rather than from any deviation in the action of physical laws.

I am, Sir, your obedient servant,

W. F. Teevan, F.R.C.S., B.A.,

Surgeon to the West London Hospital.

Portman Square,
July, 1865.

CASES IN SURGERY

ILLUSTRATIVE OF A

NEW METHOD IN APPLYING THE WIRE LIGATURE

IN

COMPOUND FRACTURES OF THE LOWER JAW.

CASES IN SURGERY

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BY

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LONDON:
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CASES IN SURGERY

ILLUSTRATIVE OF A

NEW METHOD IN APPLYING THE WIRE LIGATURE

IN

COMPOUND FRACTURES OF THE LOWER JAW.

In the following pages I purpose to explain a new method of applying wire ligature in fractures of the lower jaw. Some of the cases here reported have been already communicated to the profession through one of the medical journals, but the mode of operating is now published for the first time. Experience of this method, extending over several years, has shown me that it can be more easily and quickly applied, with less irksomeness to the patient, and I am so convinced of its superiority that I believe it has only to be known to be adopted, and to supersede the complicated and painful appliances hitherto in use.

It is not necessary for me to enter into any of the difficulties of the numerous methods now in practice; the mechanics of this injury have been so well illustrated by Hamilton, Packard, and Heath. I make no claim to originality in the use of wire ligature in fractures of the jaw, as it has been used, though only in exceptional cases, by Dr. Buck, of New York, in 1847; Kinloch, of Charleston, 1859; Hamilton, of New York, 1858; and Dr. Wheelhouse, of Leeds, 1864. In 1863 I operated successfully in a case where a portion of the lower jaw, including two incisor teeth, had been removed by a direct blow from a capstan bar. Since then I have on every occasion practised this plan.

The instruments used are the drill (fig. 3, Pl. I) and its bow (fig. 2), or the steel-rod (fig. 4), covered with india-rubber, to place in the groove A (fig. 3) to drive the drill; the key (fig. 5) for twisting the ends of the wire, a tubular straight needle (fig. 6), and a watchmaker's broach. Fig. 1 is for the purpose of broaching at right angles between the molar teeth. The operation is advo-

cated for compound fractures of the jaw only; and by compound fractures of the lower jaw I mean fractures in which the periosteum and surrounding tissues are lacerated and permitting some degree of primary displacement. Simple fractures seldom require the aid of the surgeon. Having applied in the early part of my practice the wire ligature with the ordinary tie or cross-twist, I could not avoid noticing that, however firm the fracture may be fixed on the day of operating, it becomes relaxed on the second or third day, and that the wire with a cross twist will not bear the strain of several extra turns without breaking. This necessitates a second application at a stage so painful that it is often objected to by the patient. To obviate this difficulty I devised the twist or coiling which is detailed in the following cases, and which enables the surgeon to tighten, as often as he judges the case demands, without pain to the patient and in a few seconds of time.

The drill and bow, as the watchmaker's drill, is the best instrument to use in the hands of

an expert, as it enables the operator to have one hand at liberty to steady the bone, while an assistant works the bow of the drill. An Archimedian drill (used by clockmakers) is more manageable when the operator is not practised in drilling. The wire used should be full 1-24th inch silver, or even stronger.

The first case illustrates one method of applying the wire.

T. S—, while engaged in a street brawl, April 5, 1866, received a blow on the lower jaw. On making an examination the following morning I found a compound fracture of the lower jaw at the symphysis, with great mobility of the fracture. With the patient's consent I prepared to fix the fracture. Having directed an assistant to steady the head and another to evert the lower lip, I passed the drill through the bone on both sides of the fracture at the reflection of the mucous membrane, care being taken not to injure the teeth. Fig. 3, Pl. II, illustrates the mode followed in this case. The silver wire was then

passed through the opening at A. Next, the tubular needle was passed through B, into the open end of which the return end of the wire was introduced. Then the tubular needle was withdrawn, and with it the wire. The use of this needle is to act as a director to the internal opening of the aperture at B, and to obviate a second of delay in searching for the entrance from behind forwards of either openings A and B. Afterwards the end of the wire at A was inserted into the slit of the key (fig. 5, Pl. I) and twisted in three or four coils, the same being repeated with the end of wire at B, until the fracture was fixed. On the fifth day it became slackened and required the use of the key in the coil at A or B—either would do—though it is best to choose the smallest coil for tightening, and to notice first if the cross-piece of the wire A (fig. 5) is well into the slit B (fig. 5). This case required tightening every three or four days. In twenty days the fracture was firm and united. The patient, from the commencement, expressed his ability to masticate, which I did not permit.

Afterwards he informed me that he had disregarded my veto in this respect occasionally. The application in this situation, at or near the symphysis, need not occupy more than three minutes in its performance.

Case 2.—T. B—, ship carpenter, was struck by a piece of heavy timber on the face, which caused him to fall seventeen feet from a work stage. On examining him, one hour after the accident, I detected a compound fracture of the lower jaw on one side at the situation of the first and second molar teeth, which had been removed by the force of the blow. With one exception I never before witnessed so much mobility in fractures of this part. The remaining teeth were firm *in situ*. Agreeably to my instructions, my assistant exposed the injured portion by drawing aside the cheek, and the third molar tooth was steadied with a piece of wood directed across the mouth from the side opposite to the fracture. Then using, on this occasion, the Archimedian drill, a hole was drilled from without backwards

and inwards across the third molar tooth below the enamel, this tooth being firm in the posterior portion of the fracture. The wire was then passed through the hole in the molar tooth B (fig. 4, Pl. II) from without inwards, and brought forwards between the bicuspid and canine teeth A (fig. 4). As these latter teeth were close set in the anterior fragment of the fracture, the broach was used between them to enable the thick wire ($^1/_{24}$ in.), to pass. Finally, the ends of the wire were coiled with the key, an operation which was repeated from time to time as was required. In three weeks there was union; on the fourth week the bone was firm, and the wire removed. The patient was with difficulty restrained from using the jaw in mastication.

In this case the bone was not drilled; but had the bicuspid and canine teeth not been firm, I should have drilled at a point between A and C (fig. 4). A metallic amalgam was easily introduced into the aperture in the third molar with the aid of the steel syringe (fig. 8, Pl. I).

Case 3.—W. T—, during a street riot, Feb. 18, 1867, was severely injured about the face. On making a careful examination next morning I found a compound fracture of the lower jaw, half an inch to the right side of the symphysis, and also at the left angle of the jaw, accompanied with great swelling of the surrounding tissues, &c. I operated on the fracture at the symphysis, repeating the method as in Case 1. The fracture at the angle was supported with bandage and adhesive plasters. On the fifth day the parts over the injured angle of jaw became the seat of inflammation and abscess, which, combined with an attack of pneumonia on the eighth day, made it impossible to assist the fracture at the angle with mechanical aid. The fracture at the symphysis was tightened at periods, and on the expiration of six weeks it was firmly united, at which time there was no union at the angle. The patient now returned to his native town.

Case 4.—W. H—, while at work, fell into the dry dock, March 8, 1867, and received a com-

pound fracture of arm, fracture of base of skull, and severe compound fracture of the lower jaw at the symphysis. From the very serious nature of his other injuries I did not think it prudent to interfere with the injured jaw in this case, during the first week. On the 15th of March, the condition of the patient being much improved, the fractured portion of the jaw was separated by an interval of one quarter of an inch, and no aid whatever had been applied. It was now wired by the method followed in Case 1. The treatment was the usual tightening of the coils. There was perfect recovery in four weeks after the operation. This patient made use of the jaw in mastication, abstaining only from flesh.

Case 5 is illustrated by fig. 2, Pl. II. Capt. T— applied to me on November 11, 1867, to fix a fracture of the lower jaw. The fracture was compound, and situated between the right bicuspid and canine teeth; there was also a fracture of the left ramus, from which I removed, at a later stage, a portion of necrosed bone. To

fix the fracture it was drilled through the anterior fragment at B (fig. 2), and a broach passed between the bicuspid and molar teeth. The wire was then passed through the drill-hole at B, and then backwards and outwards between the molar and bicuspid teeth. The ends were then coiled and tightened with the key, but I could not reduce and fix the displacement until I had removed the canine tooth, which had been displaced and hindered proper adaption of the fracture. The fractured ramus was aided with bandage and plasters. At the expiration of seven weeks the parts were firm and united. On the patient's recovery the gap between the bicuspid and incisor teeth did not exist, nor was there any trace of the site of the removed canine.

Case 6.—April 16, 1868, J. P— was struck a severe blow on the jaw by a pugilistic acquaintance, with such effect that there resulted a compound fracture of the left side, between the first and second molars, and a simple fracture of the right of the symphysis. The molar teeth at the

site of the fracture were large and firm, which induced me to operate, as illustrated in Fig. 1, Pl. II, by the method C, E, D. Had the simple fracture required any treatment, I should have followed the method represented by A and B (fig. 1). The wire was tightened occasionally. The simple fracture was firm in three weeks. The compound fracture was united at the end of the seventh week.

PLATE 1

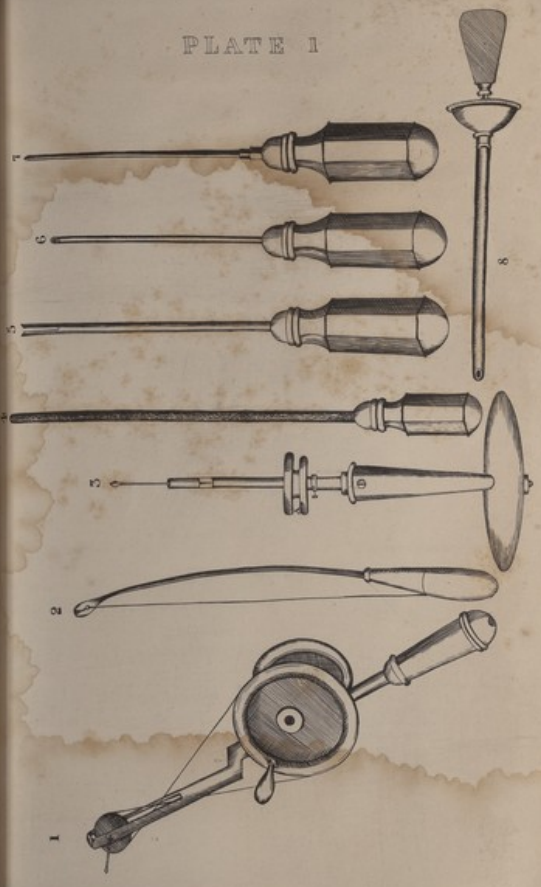

PLATE 2

FIG 1

FIG 2

FIG 3

FIG 5

FIG 4

ON THE

TREATMENT

OF

ANEURISM BY COMPRESSION,

WITH

TWO CASES OF ITS SUCCESSFUL APPLICATION.

BY

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COMMUNICATED TO THE MEDICO-CHIRURGICAL SOCIETY, 2D MARCH 1869.

EDINBURGH: OLIVER AND BOYD, TWEEDDALE COURT.

MDCCCLXIX.

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TREATMENT

OF

ANEURISM BY COMPRESSION.

Case I.—On the 12th of September 1867 I was consulted by J. G., æt. 32, on account of swelling of the left limb, extending as high as the knee, accompanied with a varicose state of all the superficial veins of the foot and leg, and a sense of pain resembling rheumatism affecting the heel. These symptoms at once led to my examining the popliteal space, with the expectation of discovering a popliteal aneurism. It was fuller than natural, but there was neither a tumour nor any unnatural pulsation of the artery. The pulse, in fact, from the knee downwards, was scarcely discernible. There was no want, however, of free arterial circulation in the limb, as was obvious from the colour of the extremity, and the rapidity with which the veins filled when emptied by pressure upwards. The view that the venous engorgement was purely due to varix, was further encouraged by the immediate subsidence of all venous fulness on Mr G. assuming the recumbent posture. His history was one of rheumatism, with two attacks of rheumatic fever. He had, furthermore, an aortic bruit with the first sound, but no evidence of any cardiac structural alteration beyond the lesion of the aortic orifice. Excluding the idea of an aneurismal affection, these symptoms appeared to point to an extensive varicose dilatation of the veins of the limb, deep as well as superficial; the distended condition of the deep veins producing the neuralgic pain in the heel and along the course of the anterior and posterior tibial nerves. I accordingly recommended bathing the limb with warm water, and the employment of an elastic stocking.

I heard nothing further of my patient from this date until the end of February 1868, except that he was well in every respect, but that the limb continued to give rise to uneasiness.

I had an opportunity of again seeing him on the 1st of March 1868, when I heard from himself that the limb had continued swollen since I saw him before, the swelling having decidedly increased of late, while the pain had commensurately incommoded him. On again examining the popliteal space, I now found it occupied by a pulsating tumour. This extended from beneath the gastrocnemius, as high as above the upper extremity of the adductoro-vascular canal, and bulged between the ham-string tendons, forming a rounded prominence posteriorly. On applying the stethoscope there was a double bruit in the tumour; the pulsation was powerfully distensile; compression of the femoral and common femoral arrested these last symptoms and diminished the tumour to fully one-third; but when the obstructing pressure was removed, these symptoms were at once, and more characteristically reproduced. Firm pressure over the tumour diminished its bulk, but not so characteristically as when obstructive pressure was employed to the common femoral. The limb was fully one-third larger than before, and nearly twice the circumference of the healthy limb. The surface of the limb below the knee was of a leaden colour, and the venous return was seriously compromised. The leg could be extended completely, but could only be bent to a right angle upon the thigh. These changes in the limb were traced to an accident which occurred two months before when in Ireland. He stated that, at that period, when coming out of a house in the country after a heavy fall of rain, he had slipped upon the moist and polished surface of a flight of limestone steps, and had strained himself, as he expressed it, in checking his fall and regaining his footing.

The aneurismal nature of the swelling, its large size, its acute activity, the absence of any considerable fibrinous clot, the serious venous engorgement of the limb, were all points of manifest importance in determining upon the course of treatment to be adopted. The natural impatience of the patient to recover as soon as possible, to escape confinement—to be well, at all events, in time to return to Ireland to be present on the occasion of the Prince of Wales's visit to Dublin—had also to be considered. It was plainly out of the question that he should delay his treatment till after the visit to Ireland, and it was also impossible for him safely to return to his ordinary active mode of life until at least three months had elapsed; as under any plan of treatment it was impossible, in a shorter period, that the absorptive removal of the tumour could have so far progressed as to render him free from the risk of suppuration of the sac should he begin incautiously to move about. I desired him to confine himself strictly to bed, to lay the limb upon a pillow with the knee flexed, to keep to a restricted diet, and take 10 grains of iodide of potassium three times a day. I also ordered a belt and shoulder-straps connected together, and with a slipper, by means of which continued flexion of the knee might be maintained according to the method of Mr E. Hart. I further desired a bag of shot, 12 lbs. in weight, to be placed upon the hollow of the groin.

9th March.—The flexion and shot-bag compression of the groin has now been maintained for five days. He complains of the forced flexion being excessively irksome, but says the pressure of the shot-bag in the groin gives him no uneasiness. The general swelling of the limb is much diminished; the pulsation in the tumour and tension of sac is less marked. Bruit still double and distinct. The shot-bag to be increased in weight to 36 lbs., and to be laid partly on the groin, partly on the lower part of the abdomen. Iodide of potassium increased to 1 drachm three times a day.

11th March.—The shot-bag of 36 lbs. occasions no uneasiness, and keeps the pulsation in the tumour still more distinctly in check, but does not entirely command it. The shot-bag, attached by hooks to the rail of a cradle placed over the limb, so that the extremity of the bag compressed the artery on the brim of the pelvis, seemed more efficiently to control the circulation, but not to arrest it entirely.

16th March.—The pelvic portion of the Carte's apparatus was employed, the compressing pad having been removed, and an 8 lb. weight, with a channel through the centre, adapted to slide upon the screw-rod, being substituted for the elastic compression effected by the screw. This completely controlled the circulation through the common femoral.

17th March.—The pelvic saddle occasions great annoyance. He also complains of the weight hurting him over the bone. The weight shifted to the site of the bifurcation of the common femoral.

18th March.—He complains of the discomfort of the saddle as perfectly intolerable, and explains that the pressure on the groin can quite easily be borne, but that the saddle is what he cannot tolerate. The whole apparatus removed, and the shot-bag substituted.

19th March.—The thigh-piece of the Carte's apparatus applied with a 12 lb. weight instead of the screw pressure. The extremity of the weight is applied so as to act upon the bifurcation of the common femoral.

21st March.—The weight has been constantly kept on, except when the surface of the thigh has been dusted from time to time with fuller's earth. The tumour is less, and when the weight is accurately applied, there is no pulsation or bruit in the sac, and no tension in the tumour. The weight rises and falls with each systole and diastole.

22d April.—The compression, by means of the 12 lb. weight, has now been kept on for a month; and although the circulation is absolutely commanded during the waking hours, he expresses himself as very doubtful what takes place during sleep, as he has several times found himself lying on his face on coming to consciousness in the morning.

Vespere, 9 P.M.—The apparatus, applied as before, is directed to

be watched night and day by two of his relations—one hand being constantly upon the weight, the other upon the tumour, so as to make certain that no displacement can take place without the compression being at once adjusted to check it.

24*th April*, 3 P.M.—All pulsation in the tumour and all bruit has ceased. The superior internal and external articular arteries ramifying over the condyles, the anastomotica magna, and the long branch of the external circumflex, can be felt beating distinctly in the neighbourhood of the knee. The pressure has been maintained so as to prevent all pulsation in the sac for forty-two hours. He complains of pain around the knee and down the limb; the foot and leg feel colder than the sound limb; and markedly so than before. The limb was rolled in cotton-wool, and the compression desired to be continued till next morning.

25*th April*.—Weight removed; pain is much diminished. No trace of pulsation anywhere in sac.

26*th April*.—Last night he got out of bed and hopped into the drawing-room; as he did so, he complained of a sudden feeling of pulsation, experienced in the upper part of the sac. The pulsation, and a modified bruit, are quite distinct in the upper quarter of the sac; the increased pulsation in the anastomotica magna is quite distinct, but the vessels over the inner and outer condyles cannot be distinguished. Weight and apparatus reapplied; the 8 lb. weight, however, being substituted for the 12 lb.

6*th May*.—Pulsation has gradually extended till now; the whole sac pulsates as at first. There is, however, only a single bruit to be recognised. Patient says the 12 lb. weight is more comfortable and easily managed than the 8 lb. one. The 12 lb. weight to be applied, and constant surveillance of the compression maintained by day and night.

7*th May*.—The pulsation and bruit in the sac have ceased, and the vessels over the inner and outer condyles are again to be felt. The weight, with absolute compression, has been maintained for twenty-eight hours on this occasion.

12*th May*.—As a means of keeping the patient quietly in bed, the weight worn steadily till to-day, without, however, any watching, except what the patient himself was able to employ. The tumour is rapidly diminishing; and, instead of occupying the whole boundaries of the popliteal space, it is recognised as an ovoid hard swelling, quite fallen away from the outer and inner ham-string tendons, and can be traced upwards beneath the inner ham-string muscles as a fusiform mass, diminishing from below upwards.

4*th February* 1869.—I saw this patient to-day. There is no trace of the large tumour in the popliteal space, except some slight thickening of the tissues; and no pulsation can be felt below the crossing of the sartorius muscle, except that of the anastomotica magna branches, which, with the long branch of the external circumflex, anastomose with large arterial branches over the inner and

outer condyles of the femur. The varicose enlargements in the calf and leg are much as before the development of the aneurism, but the general swelling is much less, and all pain in the heel and limb has disappeared.

CASE II.—Peter Gorrie, æt. 50, was admitted to the Royal Infirmary on the 10th of July 1868, on account of a pulsating tumour in the left groin. He was recommended to my care by Dr Farquharson of Stockton on Tees, where he had been occupied as an engineer. I am indebted to Mr A. Bennett's notes of this case for the following particulars:—

History.—The swelling appeared in his groin six weeks previous to his admission into hospital. When first noticed it was about the size of a plum; but it has gradually increased in size, and has been attended with a considerable degree of pain and numbness along the inner side of the left thigh and leg. When first observed pulsation was recognised in the swelling. He has been abroad for several years, and has suffered from ague.

Present Condition.—Upon examination, a tumour the size of half an orange is seen to exist in the left groin about an inch and a half below Poupart's ligament in the line of the femoral artery, extending downwards for about two inches. It has a distinct visible pulsation, which is distensile in its character, separating the fingers from each other, when applied smoothly over its surface. On applying the stethoscope to the swelling, a distinct single systolic murmur is audible. Pressure applied to the common femoral or external iliac is attended by subsidence of the swelling and disappearance of pulsation and bruit, which are more strongly marked just when the controlling pressure is withdrawn. His pulse is irregular from an occasional intermission. Careful examination of the chest revealed no cardiac or aortic physical lesions.

11*th July*.—To-day at 12.30 compression by a 12 lb. weight was applied to the common femoral artery immediately above the tumour by means of the same apparatus latterly employed in Mr G.'s case. By this means all pulsation and bruit in the sac were immediately arrested. To have 30 grains of iodide of potassium every four hours. The dressers to take it in turns to sit by the patient controlling the position of the weight by the indications afforded by the tumour.

Vespere.—Patient complains of pain caused by the pressure of the weight. To have 40 minims of the solution of morphia.

12*th July*.—Slept tolerably during the night. There is still pulsation felt in one portion of the sac on the removal of weight, but this is limited, the rest of the swelling feeling hard and pulseless. Weight continued.

Vespere.—Has slept for several hours during the day, and makes no complaint of pain.

14*th July*.—Having been absent from town since the 11th inst., I saw this patient now for the first time since applying the pressure.

On removing the pressure of the weight, I at first imagined the pulsation in the tumour still continued, but more careful examination showed me that the sac was consolidated, and the pulsation corresponded only to the line of the superficial femoral artery where it ran over the anterior surface of the tumour. The scarf skin was slightly ruffled where the weight had compressed the artery. Compression has been maintained for seventy-two hours in all. The pressure and apparatus removed, and fuller's earth dusted over the surface.

20*th July*.—The tumour rapidly disappearing. The superficial vessels in the groin and abdomen pulsating distinctly. Still complains of pain down the thigh as far as the knee.

1*st Aug*.—The patient has been out of bed for some days; the tumour can hardly be recognised.

4*th Aug*.—Dismissed cured.

I have brought these two cases of aneurism, treated by compression, before the Society, because I believe them to be the first cases in which this method of treatment has been successfully employed in Edinburgh, and because in the treatment of them I have employed a new modification of compression by weights, which it appears to me is more easily borne by the patient than any other plan which as completely commands the circulation through the main trunk towards the aneurismal sac.

So far as I know, in modern times, compression was never attempted in this city in the treatment of aneurism, except in three cases by the late Professor Miller. Two of these were cases of popliteal aneurism, one of axillary aneurism; in all the compression was abandoned for ligature. The apparatus employed by Mr Miller was that of Dr Carte, and leaden weights laid over the trunk of the afferent vessel. I am informed that some years ago a case of popliteal aneurism occurred in a soldier at Piershill, in which the surgeon, educated in the Dublin school, successfully employed compression. The apparatus used was that of Dr Carte.

I had occasion once, some years since, to make trial of compression by Carte's apparatus with elastic bands in a case of popliteal aneurism in a female, but was forced to abandon its use in consequence of the suffering experienced by the patient, who, before anything else was employed, died suddenly from the bursting into the pericardium of a small aortic aneurism situated close to the origin of the right coronary artery.

It may, perhaps, seem strange beyond belief that, in a surgical school such as that of this city, the first cases of the successful treatment of aneurism by compression should occur fully twenty-six years after this method of practice became an established procedure in Dublin, and after it has become generally accepted by the whole civilized world.

I believe that two causes have chiefly operated to prevent the more general employment of compression in the treatment of

aneurism by the surgeons of this school. 1*st*, The comparative rarity of cases of popliteal and femoral aneurism, since the introduction of railways throughout the kingdom has done away with posting and post-boys. 2*d*, The determined opposition, on purely speculative grounds, afforded to the employment of compression by those whose practical opportunities were greatest.[1]

Accordingly, upon commencing the treatment of Mr G.'s case, I had personally no such practical experience of compression as could serve to guide my procedure, and worked somewhat in the dark. The experience thus gained at once afforded a standard by which I could direct the management of Gorrie's case; and I feel confident that, had I Mr G.'s case to treat again, I could secure complete consolidation of the tumour within forty-eight hours. In Mr G.'s case the flexion treatment, combined with the partial compression effected by the bag of shot, undoubtedly did some good; though I am quite ready to admit that perfect repose in bed, with bandaging the limb, and the elevation of it upon pillows, might very possibly have effected as much.

The compression during the first month, when the method of applying the 12 lb. weight was adopted, without constant watching by an assistant, although calculated to keep the aneurism in check, was quite unsatisfactory, so far as the induction of consolidation was concerned, and might, I believe, have been continued for months in the same fashion without any further benefit accruing from its employment. The compression effected by the same apparatus, but under the constant supervision of assistants who watched the tumour, the weight, and the position of the patient sleeping and waking, so as to secure a uniform interruption of the circulation through both the superficial and deep femoral, speedily produced a very different result. This continuous and accurate compression was commenced on the 22d of April at 9 P.M., and within eighteen hours afterwards the pulsation and bruit were both so modified as to indicate a marked change in the condition of the contents, and within forty-two hours from its commencement the consolidation was complete. Indeed, the assistant in attendance for the last six hours of that time told me that it was impossible to recognise whether the compression was complete or not from anything to be felt in the sac, and that the application of the fingers along the line of the superficial femoral below the situation of compression could alone be relied upon as affording any certainty as to the efficiency of the compression. We may, I think, reasonably regard these forty-two hours as the essential period during which consolidation

[1] "So long as it is my sincere persuasion that ligature of the artery is preferable to pressure for the cure of popliteal aneurism, I shall deem it my duty to pursue this method, though it may not perhaps be the best suited for the lowest capacity of surgical skill. Let every man act according to his powers; but let no one who feels it necessary to choose inferior means throw blame upon those who are able to practise a higher exercise of their art."—*Syme, Edinburgh Monthly Journal*, 1847, pp. 569, 570.

occurred, and incline to the belief that the previous intermittent compression was of no material importance in conducing to a curative result. I think this view of the matter is strengthened by a consideration of the sequel, for after twenty-four hours more of the weight, during which no pulsation or bruit could be recognised, an unwary movement of the limb in hopping from one room to the other was followed by a partial recurrence of the aneurismal symptoms, which, in the course of the next ten days, extended themselves over the entire sac in spite of the employment of interrupted compression of the femoral by means of an 8 lb. weight; while the constant compression, when again maintained for twenty-four hours under the surveillance of an assistant, secured for a second time the complete and permanent consolidation of the sac.

Acting upon this impression, I had the compression carefully and sedulously watched in Gorrie's case from the very commencement, so as to avoid the possibility of any direct circulation through the sac —of sufficient strength, at all events, to produce a palpable impulse to the fingers of the assistant. And the result here quite justified my anticipations, for in twenty-four hours after the application of the weight the consolidation of the sac was complete; the pulsation which led to the maintenance of the compression for forty-eight hours longer proving to be limited to the superficial femoral artery, where it ran over the sac. I believe, therefore, as the result of my experience in these two cases, that the compression, to prove quickly successful, must not be intermittent but persistent, and must continually act upon the main trunk leading to the aneurism with sufficient strength and force to interrupt all circulation through that vessel as completely as if it were secured by a ligature.

This does not, however, imply a return to the views of the older surgeons, who seem to have imagined, in the use of compression, that inflammatory cohesion of the coats of the artery at the point of compression should be aimed at. This, most assuredly, is not what is required, nor should it be desiderated; all that is necessary to secure consolidation is the absolute interruption to the direct circulation of blood through the aneurism until sufficient blood clot has been deposited to occupy the entire sac. No doubt it may be argued, that while continuous compression, sufficient to maintain such a degree of interruption of the circulation, is very desirable, it is not absolutely essential; and that the partial deposition of fibrine effected in an hour or two, repeated as frequently as the comfort and convenience of the patient will permit, will secure in the long-run an equal favourable result; or, as Mr Jolliffe Tufnell puts it in his Clinical Lectures on the Treatment of Aneurism by Compression:—

"When, however, the patient may be particularly irritable, and consequently not inclined to bear a degree of pressure sufficient to control pulsation altogether, no effort to enforce this should be made, but a wave of blood should be permitted to pass through the sac; for, although consolidation of its contents may be thereby somewhat

retarded, still experience proves that total obstruction to the direct flow of blood is not absolutely necessary for the cure of an aneurism, and *that it may be induced by a degree of pressure upon the trunk of an artery sufficient to diminish without entirely interrupting the current of blood through the sac.*"[1]

It certainly may be so, but assuredly Mr G.'s case cannot be held to support that opinion; for what was observed there after all circulation throughout the sac had ceased upon the 24th? Two days afterwards, a sudden strain of the limb in some way permitted the circulation to recommence through the upper part of the sac, which ten days of interrupted compression did not prevent from becoming general throughout the sac, and which only yielded to the continuous maintenance of uninterrupted compression for a renewed period of twenty-eight hours.

It may perhaps be well to consider the changes which led to the reproduction of the circulation through the sac in Mr G.'s case after consolidation had apparently become complete. It will be recollected that the aneurism was originally, in its upper part, fusiform in its outline, and that it extended upwards as high as the upper extremity of the adductoro-vascular canal. In fact, when I first detected the aneurism in Mr G.'s case, from a certain degree of want of definiteness of outline, which the sac seemed to present at its upper and inner margin, there was a doubt in my mind whether or not the aneurism had become diffuse, and the blood had sought its way up along the course of the vessel. I believe, however, that this was due to a considerable amount of serous effusion, and formed part of the œdema which affected the limb more completely and generally below the knee, for it disappeared within the earlier period of treatment, when the compression was effected by means of the shot-bags. To my mind there is no doubt that the origin of the anastomotica magna was involved in the commencement of the sac, and I am therefore inclined to think that the re-development of the symptoms of aneurism in the upper part of the sac should be referred to the movement of the limb in hopping, giving rise to the displacement of the coagulum within the sac, and the insinuation of blood from the superficial femoral into the anastomotica magna. This at first comparatively limited establishment of circulation between the coagulum and the sac speedily gave rise to further extension of the aneurismal symptoms within the old limits, and this too in spite of the employment of some degree of pressure—a degree, however, which failed to prevent the modified current finding its way through the normal channel.

I believe, accordingly, that to treat an aneurism successfully by compression in as short a period as possible, the pressure applied to the main trunk must suffice entirely to interrupt the circulation through that vessel, and that this interruption to the normal circula-

[1] Practical Remarks on the Treatment of Aneurism by Compression, by Jolliffe Tufnell, M.R.I.A., etc., etc. Dublin, 1851. P. 71.

tion through the sac should be as continuous as possible. Mr Tufnell's original statement seems, in cases of aneurism manifesting any degree of activity, to be the safe one as a guide in practice: "Make your command over the circulation complete, but do so with the minimum amount of pressure by which this object can be gained."[1]

Here a further subject is brought up for consideration in the amount of pressure which will be required to determine a complete temporary arrest of all circulation in the vessel at the point of compression. The mode of applying the pressure will modify this quantity more materially, so far as practice is concerned, than anything in the part or circulation. We saw in Mr G.'s case that a 36 lb. bag of shot modified the flow of blood through the vessels in the groin, without, however, entirely arresting the circulation, while an 8 lb. weight, accurately adjusted by means of the arch and rod, sufficed completely to check all pulsation in the sac. There was, therefore, a great disparity of effect: 8 lbs. advantageously adjusted, was sufficient; 36 lbs. lying over the groin, and thus losing its effect by diffusion, was insufficient. The advantage enjoyed by the 8 lb. weight chiefly consisted in the smallness of the extremity, by means of which the compression of the artery was effected; but what it gained in this respect it lost in being much less easily tolerated by the patient than the diffusely-acting 36 lb. shot-bag, or than the same weight with a larger surface of application. When employed, however, with such a surface for its compressing point as made it quite comfortable to the patient, the 8 lb. weight proved insufficient to control completely the circulation through the artery lying beneath it. There was, therefore, an obvious relation between the power of tolerating compression effected through surfaces of varying extent and different forms.

To avoid detail, I may mention that, after making several experiments upon this subject, I became convinced that the curve, of the form of the larger extremity of an egg, turned from a mass of lead 3½ inches in its short or transverse diameter, was perfectly tolerable when weighted to 12 lbs., which was more than sufficient to check all circulation through the common femoral or superficial femoral in any part of the groin above the crossing of the sartorius, and this quite independently of any ordinary amount of adipose tissue overlying the course of the artery.

The situation in which this compressive weight acted most efficiently was, of course, over the horizontal ramus of the pubes, or over the common femoral below that point; but in the former position it was irksome, in the latter it was more difficult of retention than at a lower point corresponding to the bifurcation of the common femoral, where naturally the hollow of the groin is deepest, when the thigh is slightly flexed, abducted, and rotated outwards; or, in other words, when the patient lies on his back with the knee

[1] Op cit., p. 71.

slightly flexed, and laid upon its outer side upon pillows. Here the artery under pressure is forced into the angle between the pectineus muscle and the neck of the bone, while the anterior crural nerve is less exposed to compression than at any lower point. This practical observation differs somewhat from the statement of Mr Tufnell: "The weight appears a simple instrument, and so it is; but do not on this account let it be lightly regarded. It is a most efficient means of controlling pulsation, and has no drawback of which I am aware, unless its being applicable only to one spot of the artery—viz., that where it crosses the pubes, and where a bony point for counter-pressure exists—can be regarded as such."[1]

In the second case, that of Gorrie, the compression admitted of no variation from the one spot immediately above the upper border of the tumour practically corresponding to the lowest part of the external iliac and commencement of the common femoral artery. Though there was no opportunity for moving the pressure from point to point in Gorrie's case, I found no occasion in Mr G.'s case to resort to this, except during the first day, when compression was applied, the pressure occasioning really no intolerable annoyance in either patient. Certainly, in both there was a slight degree of uneasiness of the cutaneous tissues produced by the pressure, but nothing, they both assured me, to what might be experienced from a corn; and even this subsided within six hours after it had commenced. In Mr G.'s case, the cool season, the shaving of the surface, and the diligent dusting with purified fuller's earth, saved him from any abrasion; in Gorrie's, there was slight ruffling of the scarf skin, chiefly due to the extreme heat of the weather at the time, and the very copious diaphoresis of the surface throughout the whole seventy-two hours while the pressure was continued, which rendered the effect of the powder less satisfactory than it should have proved.

In Mr G.'s case, when commencing the use of compression, I felt somewhat uneasy as to the effects it might have upon the venous circulation of the limb, but was gratified to find throughout the whole progress of the treatment a steady improvement shown in the diminishing turgescence of the varicose veins, and also in the improved colour of the limb. There was some very appreciable lowering of the temperature of the foot and leg for fully a day, on both occasions, when consolidation of the sac became complete, and the blood commenced to seek its way to the leg and foot through the anastomosing vessels alone; but the degree of paling and coldness did not approach to what I have observed after ligature of the femoral artery. In Gorrie's case, where the aneurism was obviously developed from the profunda femoris at its origin from the common femoral, there was, of course, no such change in the vascular supply as to present manifest symptoms or to afford ground for any anxiety.

The high point to which the aneurism in Mr G.'s case ascended

[1] Op. cit., p. 51.

upon the vessel, involving the origin of the anastomotica magna, made me particular in examining the condition of the limb both after the consolidation became complete, and also more recently, when I had an opportunity of examining the patient. The superficial femoral could be traced on both occasions with facility as far as the crossing of the sartorius. Below this point there was nothing more than the pulsation of a vessel, about the size of a crow-quill, which could be recognised, but the branches of the anastomotica magna could be felt over the vastus internus and in the adductor magnus muscle. There was also some puffiness of the lower and middle thirds of the thigh. There was, however, none of the redness of the surface over the anterior aspect of the thigh, which I have seen after ligature of the superficial femoral. I could trace no vessel lying along the surface of the tumour—a point insisted upon by Mr Tufnell[1] and Mr Erichsen—though I could feel the anastomosis of the long branch of the external circumflex with the superior external articular, and what I presumed to be the superior internal articular, communicating with the anastomotica

[1] "And here I may . . . refer to the uniformity with which these anastomosing vessels become enlarged. One of very considerable size runs down the centre of the tumour, apparently beneath the integument; a second on the inner side of the patella; and a third less constantly felt outwards over the head of the fibula. The artery which takes its course over the centre of the tumour is sometimes as large as the radial, and is apparently, from its direct position, the main medium of circulation after obliteration of the trunk. Judging from injected preparations, where the femoral artery has been tied in Scarpa's space, the principal supporter of the circulation would appear to be the branch that accompanies the sciatic nerve, which becomes tortuous and greatly enlarged; but this occurs in consequence of the transmission of blood, by the superficial femoral being suddenly stopped, and a corresponding increase thrown upon the internal iliac and profunda. Under treatment by pressure, on the contrary, the femoral artery remains pervious down to the very sac, and from careful dissections I am rather inclined to believe that the vessel which, in cases cured by compression, naturally at first assists the circulation, is one that, although constant, has not hitherto received a name, but which, arising from the posterior aspect of the femoral artery five inches above the head of the fibula and three above the head of the joint, gives a branch to the semi-membranosus, another to the biceps muscle, and then runs down the centre of the popliteal space, accompanying the external saphena vein. By close observation of the course of the arteries surrounding the joint, I am also led to conclude that the collateral branch, which generally is found to pulsate on the inside of the patella, is not the internal articular, but a branch of the popliteal given off at least an inch above it, which, instead of running under the tendon of the adductor magnus between it and the bone, passes superficial to the tendon immediately beneath the integuments, parallel to, and in company with, the tendon of the gracilis muscle. There is also a third arterial branch, which I look upon as being closely connected with the subject of compression. This is a vessel of considerable size, which, leaving the popliteal a little above the origin of the articular arteries, runs down immediately connected with, if not actually in the substance of, the communicans peronei nerve. It is this artery I consider that gives rise to the burning pain experienced at the period of cure; the sudden distention of the vessel, and its consequent pressure on the nerves, causing the painful sensation so generally complained of."—*Tufnell*, pp. 83–85.

magna; though I see Mr Tufnell is inclined to dispute that these are the vessels. Whatever they were, certain it is that, upon both the first and second consolidation of the sac, their activity was both palpable, and, to some extent, visible, while in the interval none of them could be recognised.

In Gorrie's case, with the exception of the pulsation of the superficial epigastric circumflex ilii and external pudic, nothing manifestly marked the change in the circulation effected by the obliteration of the profunda femoris.

The result in Mr G.'s case appears to me to open up an interesting question as to the comparative efficacy of compression and ligature. Perhaps at first sight one would be inclined to say that the recurrence of the aneurismal symptoms, after consolidation had become apparently complete, indicates that ligature would have afforded a more satisfactory result; but the freedom of the anastomosis in this case through the anastomotica magna leads me, on more mature consideration, to the opposite conclusion. Owing to this free anastomosis, cases are not wanting in the records of surgery where a popliteal aneurism, treated by ligature of the superficial femoral, has only ceased to pulsate until the collateral circulation became established; and in all these cases the directness of inosculation between the branches of the profunda and the anastomotica magna accounted for the free circulation from the common femoral into the superficial femoral above the aneurism. Such a case is represented in the Fasciculus of the Fort-Pitt Museum, illustrative of Arterial Disease, and another is given by Professor Porta of Pavia, in his great work upon the Effects of Ligature and Torsion.[1] In my own museum I have a preparation illustrative of the same result in a female, the subject of popliteal aneurism, in whom ligature of the superficial femoral had been performed by the late Dr Richard Mackenzie. The pulsation returned so soon as the collateral circulation was established, and continued till the period of her death, which was occasioned by the bursting of an aneurism of the aorta.

In all these cases the free and direct circulation into the superficial femoral above the aneurismal sac determined the persistence of the disease even after the upper portion of the superficial femoral had been obliterated by the successful application of a ligature in Scarpa's space.

Now, had these cases been treated by compression of the common femoral, so as to render the circulation of the extremity and in the sac still more indirect and circuitous, a successful result might have been at once attained, without resorting to a secondary

[1] Delle Alterazione patologiche delle Arterie per la Ligatura e la Torsione, Esperienze ed Osservazioni di Luigi Porta, Professor di Clinica Chirurgica nell'i, R. Universita di Pavia, 1845, p. 378, Tavola 12 and 13.

Cooper, British and Foreign Med. Review, vol. vi. p. 67.

Roux, Popliteal Aneurism, Bollettino etc., Bologna, An. xiii. serie ii. vol. xi. p. 185.

Monteath, Popliteal Aneurism. Treatise on Aneurism, etc., by Ant. Scarpa. Translated by J. Henry Wishart. 2d edition, Edinburgh, 1819. Appendix, p. 540.

operation, whether ligature of the vessel just above the sac or of the external iliac artery. I incline to believe, therefore, that in this case of Mr G.'s, had deligation of the superficial femoral been employed, it would have been another instance added to the list of failures after ligature. For in it the great facility for a reproduction of the circulation in the sac was manifested in connexion with the patent condition of the anastomotica magna, which continued even after consolidation did become permanent, the pulsation of this vessel almost up to its origin being manifest both immediately and after the lapse of several months.

It must be obvious that compression accurately applied to the common femoral must command the arterial circulation of the limb far more completely than compression or ligature of the superficial femoral. In compression also the power of altering the situation of the vascular obstruction must afford a facility in regulating the degree of interruption of the free flow of blood through the sac proportioned to the activity of the collateral circulation, which cannot be claimed for the ligature; for the ligature, once applied, is not susceptible of any modification to suit the requirements of each case according to the effects it is found to induce.

I have no wish to argue against the employment of the ligature in the treatment of popliteal aneurism as fraught with great danger to life and limb, because, speaking from personal experience, I should incline to believe the contrary, and to regard evil results when they occur as largely due to a want of care in applying the ligature, or rather, I should say, in clearing the artery for that purpose. But it appears only a reasonable conclusion, if compression affords a higher certainty of success in curing the aneurism than ligature of the superficial femoral, that it should throw an enormous weight into the balance in favour of an operation which has the great advantage of no wound to add to its risks, nor operative procedure to increase its horrors in the mind of the patient.

It is a curious subject for observation, as yet unsettled in the history of compression in the cure of aneurism, to determine the nature of the occluding substance in the sac. The old view was, that it consisted of concentric fibrinous layers adherent to the sac and to each other, which formed a solid mass, and occluded the sac and its afferent and efferent vessels. In cases of compression, this view was adopted apparently without investigation, from the analogy of the process of consolidation of aneurism when it occurs spontaneously or after ligature. Anything else, in fact, than the formation of an adherent fibrinous laminated clot was believed to be fraught with danger to the patient in the form of either gangrene of the limb or suppuration of the sac. In so very modern a work as the fifth edition of Mr Erichsen's admirable work on Surgery,[1] we find him saying, "If it (the current) be suddenly arrested, coagulation of the blood

[1] Science and Art of Surgery, being a Treatise on Surgical Injuries, Diseases, and Operations, by John Eric Erichsen, etc., vol. ii. p. 24.

which happens to be in the sac may then take place, filling it with a large, dark, soft clot; the sudden formation of which is indeed, like a foreign body, apt to induce suppuration and sloughing of the sac and neighbouring tissues, and hence is rather prejudicial than otherwise." Recent writers and observers seem to doubt whether or not this slow deposit of concentric fibrinous layers is the mode by which consolidation is effected, at least in those cases where it takes place rapidly, and sometimes almost suddenly. In Mr G.'s case, we certainly could hardly suppose that it was the breaking-up or solution of such fibrinous layers "hard as a cricket-ball" which permitted the reproduction of the symptoms of aneurism. It appears more probable, therefore, to suppose that the coagulation of the fluid blood in the form of an ordinary clot is the means by which the obstruction through the sac is in the first instance effected. This view tallies with that of Dr Wm. Murray, the accomplished lecturer on Physiology in the College of Medicine, Newcastle on Tyne, in whose hands a case of abdominal aneurism was consolidated under pressure in five hours. Nor could any other mode of occlusion of the sac serve to account for the results obtained by Mr Heath of Newcastle on Tyne, in his famous case of Aneurism of the External Iliac, where the compression effected consolidation in *one* hour. The same also may be said of the case occurring in the practice of Dr E. D. Mapother,[1] of St Vincent's Hospital, Dublin, where absolute compression was continued for only four and a half hours, when consolidation suddenly became perfect.

Another feature of these aneurisms, in which a simple clot forms in great measure the occluding medium, is the rapid subsidence of the sac, and speedy disappearance of the tumour—features observed in both the cases I have narrated.

If further evidence were needed in proof that an ordinary coagulum may play an important part in the consolidation of an aneurism, we see it in the collateral fact, that where an aneurismal sac has given way externally, or upon a mucous surface, such as the trachea, the coagulum which plugs the opening not only temporarily occludes the aperture, but permanently arrests the hæmorrhage; and, as I have more than once observed, in the case of a large sacculated thoracic aneurism, this process is followed by partial consolidation of the sac, shrinking of the tumour, and healing of the opening. It was a change of this kind in one portion of the sac which led to the pro-

[1] Med. Press, March 29, 1865, part i. pp. 298–300. This view, although not usually entertained by modern surgeons, was undoubtedly enunciated by the late Professor Porter of Dublin, who at page 110 of his work on Aneurism, published some years before the treatment of aneurism by compression was employed, says: "The great object to be accomplished is the removal of the impulse of the heart from the blood contained within the sac for a sufficient time to allow of this reservoir becoming slowly and gradually filled with blood, and for that blood to become firm and coagulated. . . . If there could be any other means devised for removing the impulse of the heart during the required period, a ligature need never be applied for the cure of aneurism."

longation of Mr Liston's life after the aneurism had opened into the trachea. If it be once admitted that the cure of an aneurism is due to the mere coagulation of its contents, and extension of this process into the vessel upon the proximal and distal side of the sac, it encourages us to expect a larger amount of success in the treatment of aneurism by the employment of compression upon the distal in addition to that on the proximal side of the sac. In such circumstances, the almost complete severance of the blood in the sac from the general circulating fluid would favour coagulation not only by repose, but also by diminishing the vital repulsion of the blood for the parts with which it is in contact to such a degree as to probably determine a rapid consolidation. The consequences observed where a ligature has been applied to the continuity of an artery in two situations, at a moderate distance from each other, and where no coagulation has taken place in the fluid generally, or in fact at all, except where the deligation has cut through the inner coats at the point of application, must not be regarded as at all invalidating this view of the matter; for although the blood has little tendency to coagulate so long as it is in contact with the internal coat of a living artery, the interior of an aneurism cannot be regarded as possessing the same vital repulsion for blood which is presented by the internal tunic of a healthy vessel.

The proposal to employ simultaneously distal as well as proximal compression in the treatment of aneurism was first suggested to me by my kind friend Mr Edward Ledwich, of Mercer's Hospital, Dublin. "A case," he says, "occurred in our Hospital a short time ago where pressure was employed for some time without success. It was a fusiform aneurism in the popliteal space. I was called by one of my fellow-surgeons to look at it. I advised him to apply pressure to the vessel below as well as above the tumour. He did as I suggested, and in the course of a few hours it became perfectly consolidated." I observe also that Dr Mapother expresses himself strongly in favour of this procedure: "The pressure on the cardiac side should be preceded and accompanied by pressure on the artery leading from the sac. This step was suggested to me by my distinguished colleague Dr O'Ferral."[1]

In both the cases of aneurism I employed the Iodide of potassium in large doses, on the recommendation of Dr Geo. W. Balfour. In Mr G.'s case, while small doses of 10 grains, which were given for the first three or four days, threatened to induce coryza, the large dose at once checked this, and was unattended by any symptoms of iodism throughout the whole period of its administration. In the second case, large doses produced within twenty-four hours severe

[1] Med. Press, March 29, 1865, p. 300.
Treatment of Aneurism: Advantages of completely arresting the Current through the Sac, by E. D. Mapother, M.D.
Notes on the Rapid Pressure Treatment of Aneurism, by W. Murray, M.D. —British Med. Journal, Oct. 5, 1867.

iodism, which lasted for nearly a fortnight, though the medicine was given up as soon as the consolidation of the sac was recognised to have occurred. It is a little difficult to see in what way the iodine conduces to the good result and affords such marked relief as recent experience of its employment has shown it to possess in thoracic aneurisms. I have sometimes been tempted to believe that iodine only benefits those cases of aneurism where mercurio-syphilis (*i.e.*, syphilis treated with mercury) has been the predisposing cause of the aneurismal condition; and just as iodine and its compounds benefit syphilitic phthisis (pulmonary gummata), that so iodine relieves syphilitic aneurismal disease.[1] At the same time, as there can be no doubt that in some anæmic cases the employment of iodine increases the proportion of globules to the other elements of the blood,[2] it may act in them by affording substance for the formation of a more bulky red clot out of the same quantity of fluid blood. Whatever effect the iodine may possess upon the aneurismal changes, certainly in the first case no obvious influence was traceable to its administration, unless, indeed, we were to presume that the ultimate coagulation occurred just when the condition of blood most favourable for the formation of a clot had been reached. In the second case, the irritant influences of the iodine were very manifest; but we have still to learn that such physiological effects bear any proportion to its curative influences in aneurism. Furthermore, it is, I think, an interesting subject for observation and experiment how far the large doses of iodide of potassium are influential, like those of bromide of potassium, in stupifying the cerebral structures, and how much may be expected from the employment of either remedy as an adjuvant to compression in making the patient tolerant of the compressing agency.

In treating the first case, I employed the flexion method of Mr E. Hart, without, however, the preliminary and systematic bandaging of the limb which he deems essential to its success. I did not employ the bandage because I feared, in the condition of the circulation of the limb, lest the use of the bandage should have induced gangrene when flexion of the knee was superadded to the compression of the tumour in the groin. I have always considered this flexion method of treatment advocated by Mr Hart the perfection of simplicity, and in lecturing upon the subject have taken occasion to impress upon my pupils the facility in application it affords and the good results which theoretically it has appeared to promise. The apparatus I employed consisted of a waist-belt and shoulder-straps, to which a slipper was attached by means of a strap and buckle. I

[1] Is the frequency of aneurismal disease in soldiers due to the tightness of their tunics and stocks, and the badly-adjusted knapsacks they wear; is it not rather due to the frequency of syphilis among them, and the almost constant employment of mercury for its cure?
[2] Leçons sur le Chancre, professées par le Docteur Ricord, p. 143 et seq. Maunder's translation of the same, p. 102 et seq.

found that no degree of flexion which I could enforce upon the patient had any perceptible influence upon the circulation in the sac as gauged by the pulsation and bruit. I should not, however, have readily relinquished further efforts to enforce its employment had it not been that the patient insisted that, come what might, he neither could nor would tolerate the flexion any longer.

The employment of the shot-bags was only a temporary measure until I could have the Carte compression apparatus modified to carry ovoid weights. If, however, they exercised no real beneficial influence on the progress of the aneurism, they certainly seemed to accustom Mr G. to the pressure of weight in the groin, and thus to render the cutaneous tissues there more amenable to the use of the 8 and 12 lb. weights afterwards.

The employment of weights in the treatment of aneurism by compression has, I am aware, nothing novel in it. They were early employed in the history of compression by the surgeons of Dublin, and in imitation of their methods I had several weights made in 1853, for the use of the late Professor Miller. They, like the leaden weights then usually employed, were simply applied over the line of the artery, and retained in position by means of straps surrounding the limb, without in any degree girthing it.

More elaborate weight-compressors had also been invented both by Carte and Reade.[1]

The instrument which I employed, and now show you,[2] is simply the Carte's circular compressor for the thigh with the metal arch

[1] *Mr Carte's apparatus for applying the weight.*—"This apparatus consists of three parts—viz., a belt of peculiar shape, a receiver in connexion with it, and metal weights of different sizes. The belt, which is made of leather lined with chamois, is shaped so as to fit accurately the hip, buttock, and upper part of the thigh; it almost surrounds the limb, and is secured on the outside by straps and buckles, one strap passing round the pelvis. Immediately over the artery a circular hole is left in the belt, about an inch or a little more in diameter. The receiver (which is termed a 'hopper' from its resemblance to this part in some machines) is made of very strong leather, and has the shape of an inverted cone, the small end being attached to the edge of the circular hole in the belt. The weights, which are of lead and of different sizes, have a conical shape, so as to fit easily into the receiver, and are so shaped that the smaller end will pass through the opening in the belt, and rest upon a compress or pad laid upon the artery underneath."

Messrs Reade's apparatus for applying the weight.—"This apparatus consists of a cradle composed of two light flat metal hoops or rings, joined together by a connecting flat piece of iron, into which the upper part of the thigh fits; and the hoops are so constructed that their size can be increased or diminished as occasion may require. A ball and ring similar to that of the 'aneurism compress' comes off from the upper hoop; but, instead of the screw, a smooth cylindrical rod moves in the ball, and is connected with a pad below. The apparatus is intended for the compression of the artery in the groin, which is effected by passing flat leaden weights having a hole in the centre down the rod connected with the pad; and their number may be increased until the necessary degree of pressure is made."—*Observations on Aneurism and its Treatment by Compression*, by O'B. Bellingham, M.D., pp. 92, 93.

[2] See plate facing title-page.

heightened, the pad removed, and the elastic element withdrawn. There still remains the rod, no longer, however, acting as a screw, but sliding through the ball-and-socket joint beneath so as to admit of the easy introduction of the weight. The weight is ovoid in form, and has a channel through its central long axis large enough to let the rod play easily without catching. The extremity of the weight by which the pressure is applied has the form of the larger end of an egg cut out of metal, 3½ inches in diameter. The inclination given to the rod by the universal ball-and-socket joint determines exactly the amount of weight and the situation at which the pressure is to be applied. A very slight variation in the inclination of the weight from the perpendicular materially modifies the actual amount of weight borne by the limb at the point of compression, so that the minimum amount of pressure sufficient to control the circulation through the artery can be arranged to a nicety, with the same mass of metal. The pressure is rendered accurate by the ball-and-socket joint, which is made immovable by a pinch-screw, or it is directed by the hand of an assistant, sitting at the bedside, steadying the weight or gliding it from side to side as his other hand, resting on the aneurism, may indicate. This apparatus has certainly the virtue of simplicity, as there is nothing connected with it which could not easily be constructed by a blacksmith or brassfounder. So far as I am aware, it has not been previously employed. In editing the last edition of the "System of Surgery" by the late Professor Miller, I briefly mentioned this apparatus: "The weight is perforated, and allowed to slide loosely upon the rod of the Carte's apparatus, its ordinary compressing-pad having been removed."[1]

The only notice I have been able to find of at all an analogous plan of employing weight-pressure in the treatment of aneurism is in the Report of the Proceedings of the Surgical Society of Ireland,[2] where Mr Samuel A. Cusack is reported as saying—"The pressure throughout was made by means of the weights now on the table, which are kept upright and in position by means of a stiff iron wire fixed in a ring in the cradle which is placed over the patient's body—a mode of applying them which I have found more convenient than any other contrivance in which the patient or an assistant is obliged to keep the weight in an upright position."

The weight employed as a compressing agency has a great advantage over the screw-compressor, whether with the improved elastic element of Carte or not, in so far that it yields with every impulse of the artery, without, however, allowing any blood to pass it. The weight will accordingly be seen to move slightly but quite perceptibly whenever the pressure is accurately applied, and completely arrests the circulation. This slight yielding makes the pressure far more tolerable than any method which affords no allowance for a certain degree of "giving." Further, there is no squeezing of

[1] A System of Surgery, by James Miller, F.R.S.E., etc., p. 421.

[2] Dublin Med. Press, February 29, 1860, p. 179.

the limb against the splint behind, as there must be in the screw-pressure; the counter support in the case of the weight is afforded by the whole limb, and diffused over every point of it which comes in contact with pillow or mattress.

Digital compression by the thumbs of professional assistants is considered by some as the *ne plus ultra* of excellence in compression. To this view I cannot accede. Digital compression is painful, usually unequally maintained, apt to be excessive, requires relays of skilled assistants, whose frequent shifts annoy the patient and prevent sleep, while, except in an hospital patient, they are practically unsuitable. The weight, on the contrary, once correctly set, needs no meddling, exhausts no strength, is unvarying in its degree of force, and requires only so much regulation and watching as can be afforded by a non-professional assistant, who, without fatigue, can easily maintain a constant and efficient superintendence for several hours at a time without occasioning the slightest disturbance of the patient's repose.

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THE

CONSOLIDATION

OF

INTERNAL ANEURISM.

BY

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JOHN FALCONER, 53, UPPER SACKVILLE-STREET.

1879.

THE

CONSOLIDATION OF INTERNAL ANEURISM.

In a comparatively recent number of the Dublin Journal of Medical Science* the following opinion has been advanced in reference to the cure of aneurisms—viz., "That the layers of lymph deposited in the sac of an aneurism are formed by the walls of the sac itself, and are not a deposit from the blood."

Sorry as I am to be obliged to differ from so distinguished a surgeon as the writer of this article, still I cannot admit the correctness of any one of the arguments which he has advanced in support of his views; and I must maintain and hold to the fact (as demonstrated in every pathological museum) that the consolidation and cure of an aneurism is not the result of any secretion from the sac itself, but a purely mechanical deposit—rapid when the sac is occluded by clot, and slow and progressive when consolidated by fibrin deposited in successive layers.

I affirm that in neither case is inflammation present, and that in no instance does lymph (the product of inflammation) act as the agent, or constitute the medium through which cure and recovery are brought about.

In the case of an aneurism treated by ligature, where the circulation is suddenly and almost entirely cut off, and rapid consolidation consequently follows—or of an aneurism cured in a few hours by merely compressing the main artery leading to the sac—here, in each instance, the material that fills the chamber is simply coagulum, firmer, or less firm, according to the plasticity of the blood of the patient, and the relative proportions of serum and

* Vol. LXIV., No. 70, October 1, 1877. On Aneurismal Sacs. By William Colles, Surgeon to the Queen in Ireland; Regius Professor of Surgery in the University of Dublin; Surgeon to Dr. Steevens' Hospital, &c., &c.

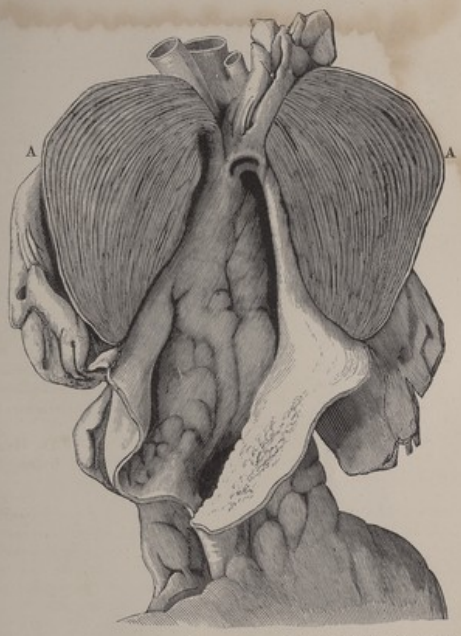
crassamentum in the blood of the individual in question; but in an aneurism (whether it be internal or external) where the process of cure is progressive, and consequently slow, here the fibrin has time to deposit, and it does so in regular successive layers, commencing at the circumference of the sac, and gradually filling it to the centre, thereby converting the whole into what may be rightly designated as a solid fibrous tumour. *This process, too, is purely mechanical.*

Recently an opportunity has been afforded to me of demonstrating the same, and I avail myself of it to show (by *facsimile* representation) how this consolidation takes place. A gentleman who a few months previously had been cured of an aneurism of the innominate artery by rest in the recumbent position, committed suicide and destroyed himself in a fit of insanity. The examination of the thoracic vessels revealed the aneurismal sac filled (as in the engraving annexed) in regularly successive layers,* and no clearer demonstration can, I think, be given of the way in which an internal aneurism is cured. To produce this end, however, all and everything tending to the production of inflammation must be studiously avoided, whether as regards the constitution of the patient or the sac of the aneurism itself. *By passively and mechanically deposited fibrin alone can the life of the sufferer from aneurism be saved*, where, as in the instance in question, the disease occurs in a situation that the surgeon's hand cannot reach.

The particulars of this case were so clearly given by Dr. Head, when laying it before the Pathological Society of Ireland in January last, that I beg to repeat them here. He says:—

> The case which I wish to present to the Society is one of an aneurismal sac that had undergone cure by the process upon which so much stress has been laid by Mr. Tufnell—viz., consolidation, by successive deposits of fibrin. The previous history is short. In July last, this gentleman, whose age was fifty-four, and who was leaving Ireland, came to thank me for my long attendance upon him, two years before, for a stomach affection. I had not seen him for these two years, and he told me that, during that time, he had suffered from intense neuralgia in the back and shoulder, along the back of the neck, and in the back

* The statement of the case from which the preparation was taken was laid before the Dublin Pathological Society by Dr. Head, President of the King and Queen's College of Physicians in Ireland, on the 26th of January, 1878, and published in the June number of the Dublin Journal of Medical Science. For permission to obtain the drawing from which the accompanying woodcut is engraved, I am indebted to the medical staff of Dr. Steevens' Hospital, Dublin.



A A.—Aneurism of the innominate artery cured by consolidation, the sections showing the fibrine of the blood as deposited in successive layers.—Case published in *Dublin Journal of Medical Science*, June, 1878, page 545.

The preparation itself is in the Museum of Steevens' Hospital, Dublin.

ANEURISM OF THE INNOMINATE ARTERY.

of the head. For this he had been treated in various ways, and he had used large hypodermic injections of morphia, which gave him a good deal of relief, but, after some time, he was obliged to give them up, from the intense itchiness of skin which they caused. He then appeared to be well, and had not suffered much from neuralgia for some time. He was a Presbyterian chaplain in the army, and was going over to take charge of troops at Shorncliffe, in England. He mentioned to me that he had a little mark on his chest, beneath the right collar bone—a dusky spot, about the size of a five-shilling piece, as if he had got a bruise, and asked me to look at it. Upon examining it, I detected a distinct pulsation underneath, and came to the conclusion that he had a thoracic aneurism. But for the pulsation, however, it would have been impossible to detect an aneurism. There was no sign of pressure—no murmur, no difficulty of deglutition, no dilatation or irregularity of any vessel. There was no sign of interference with either recurrent nerve, and his voice was natural. The impulse of the aneurism was, if anything, slightly in advance of the heart's impulse, as if the commencement of the contraction of the heart acted upon the aneurism before the apex of the heart struck the walls. This I have observed in two instances of aneurism of the aorta. There was no double impulse, and no murmur or bruit. He complained of little or no pain, except a burning sensation. I told him that I thought he was not fit for duty, and explained to him, to a certain extent, the nature of his illness—that an effort should be made to cure him; and I suggested a consultation with Mr. Tufnell, who concurred in my diagnosis that it was an aneurism, and most probably of the arteria innominata. As I considered that this gentleman was likely to be benefited by the treatment which Mr. Tufnell has so ably advocated, we explained to him the nature of the treatment. He said that he would be glad to adopt it. He assumed the recumbent position the last week in July, and continued it until the middle of October. I had to leave town myself at the beginning of August, and Mr. Tufnell was then kind enough to take charge of the case. The patient took little or no medicine, and we did not give him any iodide of potassium; but, when he was under my sole care, and, occasionally, when his heart beat a little fast, he got small doses of aconite, which reduced the frequency of the pulse a good deal. The principal treatment consisted simply of the horizontal posture, absolute rest, and a minimum of liquids, his food consisting as much as possible of solids, so as to diminish the quantity of blood, and at the same time keep up its healthy condition. While undergoing treatment, Mr. Tufnell and I came to the conclusion that the aneurism had undergone a great deal of consolidation, although to what extent exactly we could not tell, because there was still a strong impulse. It, however, gave the impression of a solid tumour striking against the sternum, and we could not feel any signs of lateral dilatation whatever.

The centre of the impulse was about the edge of the sternum, between the cartilages of the first and second ribs, extending about an inch and a half in each direction; and there was also marked dulness on percussion. The heart was slightly displaced and pushed downwards, the apex beating between the sixth and seventh ribs, a good deal to the left of the nipple and over a considerable surface, and there was also some amount of dulness on percussion over the region of the heart. We were of opinion that the heart was slightly enlarged. He bore his confinement very well; but, in the beginning of October, he began to show signs of great restlessness, and I thought it better not to confine him any longer. He began to get up, and gradually to go out; but became sleepless, and got into a state of great mental depression, fearing that he would be put on half-pay, and that he would be arrested for debts which really he did not owe. Mr. Tufnell and I now advised him to apply for additional leave of absence, thinking that, the aneurism being now very much consolidated, a little more rest might enable him to go back to his duty. He applied for additional leave, and on the very day before the occurrence of the unfortunate act which terminated his life, went before a Medical Board; the leave of absence recommended was granted, but of this he could not be made aware, as the proceedings are private. He was now advised to go to the country for change of scene, and his friends were taking him there, when, at the Railway Station at Kingsbridge, he became suddenly excited, ran away from them, and threw himself over the wall, which is twenty-four feet in depth, into the Liffey. He was not killed by the fall, but the water in the river was shallow at the time, and he was immersed in the mud. From this he was extricated as speedily as possible, and taken to Dr. Steevens' Hospital, and after lying there for about two hours, he died. I did not see him myself, but was informed that no impulse could be detected over the aorta, but he got a violent cough, which was characteristic of pressure. The Coroner directed a limited *post mortem* examination to be made, which gave the opportunity of ascertaining exactly the site of the aneurism and the result. The lungs were congested, and had a good deal of frothy fluid in them, such as is met with in the lungs of a person who has been drowned; and the patient, who was a heavy man (sixteen stone weight), when lifted, was found lying on his face. Upon further inspection a *solid* tumour was disclosed, occupying the entire mediastinum, and firmly attached to the under-surface of the sternum and the cartilages of the first and second ribs. A portion of the sac of the aneurism being adherent to the under-surface of the bones, it was supposed at first that the shock of falling twenty-four feet had burst the sac of the aneurism, but it was found that there had been no rupture whatever, nor were there any signs of extravasation of blood. The heart itself was covered with fat, and to some degree enlarged, flat, and flabby. On opening the left side of the

heart, the wall was found to be very thin and the cavity a good deal dilated; but the valves were perfect. When we opened the aorta, we found it very much dilated and altered by atheroma in a marked degree, with dilatation almost amounting to true aneurism. Upon slitting up the aorta, we found that the original aneurism occupied almost the entire of the arteria innominata. At the back of it the vessels were quite pervious—namely, the subclavian and the carotid on the right. Those on the left were also perfect; but the descending aorta was very atheromatous; and, at a distance of between three and four inches, there was another small aneurism, the size of a walnut. This we could not diagnose during life; but it, too, was all but filled up with fibrin. The principal interest in the case lies in the manner in which the aneurism was cured. The layers of fibrin are very firm and closely laminated, the layers of it being almost as thin as sheets of paper spread one over the other. It is a most interesting example of what may be done by the absolute rest and other items of treatment advocated by Mr. Tufnell—in fact, this aneurism was cured.

Such is the detail given by Dr. Head, and in every instance in which an internal aneurism is cured the mode of procedure is the same. Steadily, and persistently aided, nature can arrest the progress of a disease that otherwise must destroy life, and in some instances is capable of effecting this end in a comparatively very brief period of time—such, for example, as in a case of abdominal aneurism which was recently under my care, and which was successfully treated in seven weeks. The particulars of this case I subjoin:—

A tall, delicate young man, who had recently suffered from primary and secondary symptoms, and been under a mercurial course, consulted me, upon the 7th of April last, for "a painful beating in his belly." He was engaged in the victualling business, and the history which he gave of his case cannot, I think, be better detailed than in the words of the patient himself, as taken down at the time. He said—"Five weeks ago I was working in the shop when an explosion of gas took place in the cellar underneath, and I was blown up to the ceiling; I was stunned and a good deal hurt, but I went to work again after a day or two. A week after this I was shoving up a side of beef, a man being on a ladder to put a hook into the beef; I pushed up the beef as well as I was able, but it *came back upon me*, and I had to let it down again. I felt at the time greatly exhausted, and had to rest for a while; I then tried again, and at last, after a very great struggle, got up the side of beef upon the hook. I did not feel any great pain then, but I was quite faint and very tired. Some days after, as I was going to work, I

felt a great pain in my stomach, and a shivering came over me; I worked on, however, for a fortnight after this, until I was unable any longer to bear the pain. I now noticed the beating in my belly, and a throbbing, and it became very sore to the touch."

Upon examination of the abdomen, pulsation was evident to the eye, to the left of the median line, mid-distance between the umbilicus and cartilage of the ribs on the left side. Upon placing the patient on his back, a tumour, circular in form, with a distensile pulsation of two inches in each direction, could be almost grasped. The pulsation was accompanied by *bruit de souffle*, audible both to the unaided ear and by the stethescope when the patient was recumbent, but the bruit was totally lost as soon as he stood erect. Dr. Gordon, late President of the King and Queen's College of Physicians, saw him, in consultation, a day or two afterwards, and the condition at that date was precisely the same as on the 7th—the patient, in the meanwhile, having been kept quietly in bed. Regular recumbence was not, however, commenced until the 12th of April, by which date a water-bed had been procured, and it was now continued without the patient once moving from the horizontal position till the 26th of May, when he was allowed to sit up, and upon the 1st of June to go out for a drive, which he continued to do daily.

No medicine of any kind was taken during the period of recumbence, and the only medicament employed was a turpentine and assafœtida enema administered upon the 30th of April, which brought away a very large number of scybala, whose collection and retention in the abdomen were causing uneasiness to the patient.

The pain, so severe at first, and which was dependent upon the tension of the aneurismal sac, subsided very rapidly—indeed in a few days after lying horizontal. Upon the 9th of June the patient went out of town for change of air, but came in again upon the 14th for examination. No bruit or dilating tumour could now be found—upon the most careful auscultation and manipulation—by either Dr. Gordon or myself, and no aortic symptom beyond a fulness at the spot where the aneurism had existed. The origin of the aneurism I attribute to the intense strain put upon the coats of the aorta when endeavouring to push up the side of beef, the spine being then strongly bent backwards, and in the most favourable position to cause a tear of the inner and middle layers of the vessel, and I do not refer it in any way to the contusions following upon the explosion of gas.*

* This individual is at the present date—viz., January 15, 1879, steadily engaged at his trade, and has been so ever since the beginning of September last.

PAMPHLETS

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