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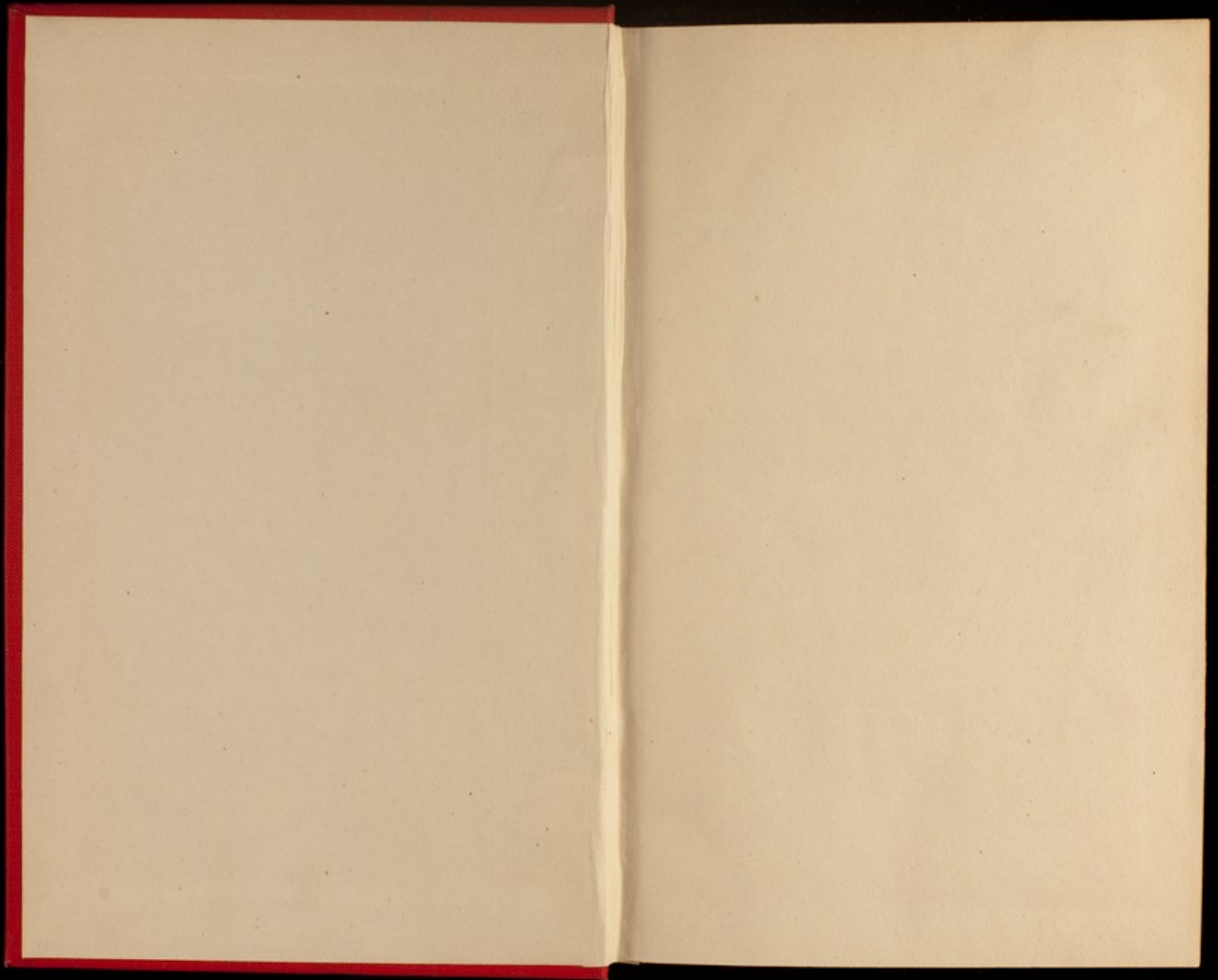
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THE
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ANIMAL PATHOLOGY.

By Mr. YOUATT.

LECTURE XXII.

The preventive Measures continued—Are there Medicines which will prevent the Development of the Disease?—The Box—Alisma Plantago—Belladonna—Scutellaria—The Danger of the use of Preventives.—The Treatment of Rabies—It is a State of excessive nervous Irritation—Can this be allayed?—Depletion considered—Injection of warm Water into the Veins—The transfusion of Blood—The injection of certain Fluids into the Veins—Medicines—The Box—Alisma Plantago—Belladonna—Scutellaria—Guaco—Veratrum Sevadilla—Ticunas Poison—Other South American Prophylactics—No instance of Cure in the Lecturer's Practice—Caution—Encouragement.

IF the present lecture should be somewhat unsatisfactory you will at least have this consolation,—that it will be the last with which I shall trouble you on the subject of rabies. The knife and the caustic are most valuable preventives, and thousands have been rescued by them from otherwise certain destruction. In the present state of medical knowledge, with relation to this disease, that surgeon would be altogether without excuse who hesitated to have recourse to them, or to apply them with all the severity which the case seemed to demand; and he who has unfortunately been bitten, should muster all his courage, and submit to the application of them. He will not find the operation half so painful as he feared.

But, let the knife or the caustic have been freely and skillfully applied, can the medical man be sure that every portion of the wound with which the virus could by possibility have come in contact has been destroyed? Can the surgeon, partial to the knife, be certain that inoculation of the old wound, or the surface of the new one, may not have been effected by the very

operation which he has performed? The plain matter of fact is, that both the knife and the caustic have failed in the most skilful hands, and the former oftener than the latter. Not many months ever pass without some record of a death from hydrophobia, although the patient had submitted to the usual and the best surgical treatment.

In many cases of severe and lacerated wounds, which bid defiance to excision, and can scarcely be fathomed even by the caustic, the humane and honest surgeon will often painfully reflect on the possible melancholy issue of the affair. He will identify himself with the feelings and apprehensions of the family and friends of his patient; and while he will place much confidence on the attempted destruction of the part, he will look anxiously around him for some subsidiary measure which shall make assurance doubly sure.

Are there any *preventives*? I know that I am treading here on dangerous ground. There are plenty of these preventives, if the advertisements of the quack or the firm belief of the peasant can be depended upon. Indeed, the principles and the practice of legitimate science acknowledge the possibility of their existence. We inoculate with vaccine matter, and we obtain, for awhile, an immunity from small-pox. The poison of measles has been received into the frame, and the system is fully impregnated with it. The patient, accidentally or by design, is inoculated with the variolous matter while the eruption of measles is thick upon him. The second poison is perfectly inert until the first has run its course, and then it assumes all its wonted activity. Then, are we able so to modify or change the connexion or action of certain parts, or can we bring the constitution generally into such a state that the influence of the rabid virus may be successfully resisted?

This is a very important inquiry. It is that on which the efficacy of every preventive measure is founded, and, to a certain extent, it is confirmed both by theory and practice.

Rabies is an affection of the nervous system. Is it possible, by the exhibition of some diffusible stimulant, whose power of excitement may be considerable and permanent, and its sedative effect slight and transient, and whose principal action is on the nervous systems—is it possible to maintain a constant excitement of them until the virus is worn out? or may a gradual change or modification of the nervous system be produced, so that it shall not be susceptible of the influence of the virus?

These were the questions that I was accustomed to ask myself, and which induced me to put to the test all the supposed preventives whose composition I could ascertain; and I

perfectly satisfied myself that many of them had power, and to a very considerable extent.

Mr. Blaine had satisfactorily ascertained that the common Box was the basis of the preventive drinks that had acquired so much reputation in Hertfordshire and Kent. Some rue entered into the composition of the drench, but the box was the principal ingredient. Its bitter and nauseous taste would induce the suspicion that it possessed some powerful medicinal virtue. It was evidently a narcotic. The human patient always experienced considerable giddiness for half an hour after taking it, and the dose for the dog was regulated by the degree of staggering and distress which it produced. Several of them were destroyed by an over-dose. The rue also is an acknowledged antispasmodic, and exerts its chief power on the nervous system. This compound was undoubtedly a preventive. Very few dogs bitten by a rabid dog, and no preventive measures being taken, escape; but the decided majority of those who were drenched with this mixture never exhibited the slightest symptom of rabies. Beyond this, however, we could not go. We saved the majority; but we had no effectual security.

The *Alisma Plantago* is a decided sedative. I experimented on it to a considerable extent, and was satisfied that it had effect, perhaps more than the box; but it was not the preservative which it was asserted to be.

The *Belladonna* followed, and it also had power, but no absolute dependence could be placed upon it. So it was with Dr. Spalding's *Scutellaria Lateriflora*. Its effects on the cerebral system were singular. I repeatedly took an infusion of it, and the consequence almost invariably was a feeling resembling the pleasing exhilaration of incipient intoxication, rapidly changing to a giddiness not quite so pleasing, and the whole passing off in eight or ten minutes, and leaving no exhaustion or depression. It was certainly more efficacious than either of the others; but instances of palpable failure too often occurred. I combined these medicaments in various ways; but I could never get any thing approaching to a specific.

The end of the whole was, that I gave up these experiments altogether. I could not discover any simple or compound medicine on which full dependence could be placed. I also began to feel that by these continued experiments I was exposing myself and my assistants to a degree of danger which could not be justified—I was likewise beginning to produce a dangerous state of mind in some of those about me. They saw that these medicaments had power—that, probably, we saved quite as many dogs as we lost; and they began to cherish a faith in them that threat-

ened to lead to a disinclination to have recourse to the far surer preventives, the knife or the caustic. In some persons, aware of the experiments which I was pursuing, and anxious to add the lesser efficacy of the drench to the more powerful one of the caustic, there began, after awhile, to arise a distrust of both. A state of nervous irritability likewise occasionally ensued, the possible consequences of which I dreaded to consider. I knew how much depended on the mind. I knew the importance of inspiring the patient with perfect confidence, and banishing from the memory, as much as possible, the record of that which had passed. The daily drenching precluded this; and I feared what dreadful power an irritated and unhinged mind might give to the minutest particle of the poison that might perchance lurk in the wound.

And now, gentlemen, I approach, and with diffidence and reluctance, the last and the most important division of our inquiry, *the treatment of rabies when it is unequivocally developed*. In the great majority of cases prudence and humanity will dictate the immediate destruction of the animal. There are but two motives that will justify any delay. If mischief has been done—if a human being or any valuable animals have been bitten, and it has become necessary to have recourse to painful preventive measures, it may, for the sake of the practitioner and of the patient, be advisable to commit the dog to close and safe confinement, in order that the appearances after death, corresponding with the symptoms during life, may justify these precautionary means. The reputation of the practitioner and the satisfaction of the patient may demand this.

The human or the veterinary surgeon may also be justified in delaying the destruction of the animal in order to experiment on the effect of certain medicines or operations. This is a noble but a fearful object. Ever would his memory be revered who discovered a cure for rabies; but his laurels would be won at extreme hazard, and the most perfect self-devotion to the cause of science and humanity could alone induce him to pursue his dangerous path. Having now retired altogether from veterinary practice, I may be permitted to say, that the countenance of the government of the country might be given to many less worthy objects than the attempted discovery of a cure for rabies. I do not think that there would be much difficulty in discovering the man who would honestly devote himself to such a pursuit, and, perhaps, I have him in my mind's eye.

The true nature of rabies has been sufficiently established in the course of our inquiries. *It is a state of excessive nervous irritation, commencing with the motor nerves of organic life,*

speedily involving the cerebral system, and rapidly exhausting the powers of nature. Then the object of the practitioner, whether human or veterinary, would be to allay, if possible, this dreadful state of excitation: to that point, and to that alone, his attention should be primarily directed.

The first weapon would be depletion, and carried to its fullest extent. The vessel selected would be the jugular vein in the quadruped: the incision would be large, and the blood would be suffered to flow not only until a manifest change was produced in the action of the heart and arteries, and syncope supervened, but, the circulation beginning to return, the bleeding would recommence, and a second and longer fainting ensue. It is painful, however, to record the little permanent effect which even decisive treatment like this produces.

A dog weighing about ten pounds, and evidently labouring under rabies, was subjected to copious venesection in my hospital. About six ounces of blood were first abstracted; when he fainted, and scarcely rallied again for many an hour. On the following day he was again bled to syncope: four ounces alone could be abstracted. On the third day there was manifest but deceptive improvement—he ate and drank, and was gentle and tractable; but towards evening the eyes closed and the head drooped, and the husky guttural sound in the throat returned, and he should have been bled again; it was delayed, however, until the following morning, when five ounces more of blood were abstracted. He did not rally after this depletion, but died in the evening.

This was the first experiment on the power of bleeding—others were afterwards made, with more system and with more decision. The result was uniform—the symptoms were invariably mitigated, but the dog died.

The same result has attended the adoption of venesection in the human being. It is of very ancient date, and it has been often resorted to in modern times. Bolingall mentions six cases in which the patients were bled *ad deliquium animi*, without any better result than temporary alleviation of the symptoms. Dr. Rutherford abstracted sixty-six ounces of blood from one patient, but he died.

It had been imagined that the injection of warm water into the veins might have a more decided effect. This was put to the test of experiment. A white terrier, undeniably rabid, was the subject of the experiment, and which was conducted in my hospital by Mr. Mayo. About seven ounces of blood were taken from the jugular vein, and nearly the same quantity of warm water injected. The dog struggled violently, so that the exact quantity abstracted and injected could not be ascertained; but it was

at least seven ounces. During the process of injection the animal heaved convulsively, and was very sick. The difficulty of breathing increased—it was painful to behold. In order to relieve it, and as another interesting experiment, the anterior portion of the third ring of the trachea was removed, but without any marked advantage. I would rather say, that the labour of respiration was aggravated. On being liberated, he appeared to be very much weakened—he sat on his haunches leaning against the wall, and his fore-paws continually slipping from under him. I watched him during two hours. He was gradually sinking, and he was found in the morning nearly slipped down, his head resting on the ground, and quite dead: he must have died without a struggle.

Magendie conducted a similar experiment. His patient was a large mastiff. He laid bare the jugular vein, and, making an incision into it, abstracted about a pint of blood, while at the same time, through another incision situated lower down in the vein, he introduced warm water. On the whole, beside the pure blood that was taken away, there escaped about ten or twelve ounces of mingled blood and water. The dog, that before the experiment had been exceedingly violent, was now quite calm, and on being liberated curled himself up as it were to sleep; but he did not close his eyes. Five hours after the injection he began to suffer great difficulty of respiration, which continued to increase for half an hour, and then he died. During the whole of the previous time he was altogether calm. He was destroyed, as was the poor terrier whose case I have just related, by infarction of the lungs, the whole tissue of which was filled with aqueous blood.

In one case I had recourse to the transfusion of blood. The patient was a large mastiff bitch, with dumb madness, and tolerably manageable. Eight ounces of blood were withdrawn, and replaced by nearly a similar quantity of that fluid from another mastiff that I happened to have at that time in my hospital. A slight shivering fit immediately followed the transfusion, and this was succeeded by increased brightness of the eyes, and, as I thought, a slight retraction of the tongue. The breathing was quickened—it was to a certain degree laborious; and the pulse was accelerated eight or ten beats in the minute. In less than six hours this had all passed over, and she was evidently weaker than before the experiment.

Ten hours after this experiment six ounces more of blood were withdrawn, and replaced by nearly an equal quantity from the dog that furnished the fluid for the first transfusion. The countenance again brightened, and the respiration and the pulse were

quickened; but this was evanescent, and on the following morning she was dead. I had anticipated this result, for it was too essentially a nervous disease to yield to the change of a small portion of the vital current.

The same experiment, and with the same ill success, had been made on the human being. Dr. Dieffenbach, of Berlin, had a patient evidently hydrophobous. The usual means had been tried, but these having no effect, and his state becoming worse, transfusion was resolved on. After a bleeding of twenty-four ounces twelve ounces of blood were at two different periods injected.

At each injection the pulse rose and became regular, and, after some time, the dread of fluids seemed to diminish. In the evening the patient had some shivering, and was feverish. On the next day he was bled to thirty-two ounces, after which twelve ounces were slowly transfused, but without any effect. On the following day the patient had considerably changed—his face was pale, his eyes glassy, and the dread of water so violent, that he was taken with shivering at the mere sight of it. In the afternoon, after a bleeding of six ounces, five ounces of blood were again injected. Immediately after the operation he drank some water, but died in the course of an hour, in violent convulsions.

The object of the practitioner is to allay, if possible, that state of excessive nervous irritation which constitutes the character of the essence of this dreadful disease; but, unfortunately for him, just in proportion to the morbid excitement of the system the most powerful medicines often seem to lose all their influence. One grain of the aqueous extract of opium injected into the veins of a healthy dog will produce a sleep of some hours' duration. Ten grains have been injected into the veins of the rabid dog without the slightest narcotic effect. It is said that Dr. Babington administered 180 grains of opium to a patient without any benefit, and even without procuring sleep. Even prussic acid has been injected into the veins of the rabid dog without causing any perceptible relaxation of the symptoms. I am ashamed to say, that, among my numerous patients, I never had recourse to the injection of powerful sedative medicines into the veins. I know not why it should have been so, and I now regret it; but I believe that almost every prophylactic that could be administered by the mouth was, at some time or other, put fairly to the test.

The Box seemed not to have any power either in decoction or in extract.

The *Alisma Plantago* had great effect. A coach dog belonging to the Sardinian Ambassador was rabid, and dreadfully ferocious.

With some difficulty, however, I contrived to give him two drachms of the powdered root, suspended in mucilage. He was evidently quieter. Six hours afterwards the dose was repeated. The desire to do mischief altogether ceased, and a torpor amounting to almost paralysis succeeded, and he died in twenty hours after the second dose. About the same time I administered the same drug to another dog, rabid and ferocious. His ferocity was subdued, and the disease was protracted; but he died. At other times the alisma did not produce the slightest effect. This was the case with almost all the sedatives to which I had recourse. The best marked case of the power of the alisma was that of a dog belonging to a lady well known at that period.—Mrs. Hodges, of four-in-hand celebrity.

Feb. 13, 1819.—Her spaniel had for two days refused his food. He had become exceedingly irritable—he lapped his own urine—and uttered the characteristic howl—he was indubitably rabid. He was brought to me early in the morning, and at nine A.M. two drachms of the powdered alisma plantago were given to him, suspended in mucilage. For an hour he continued restless, and howling as before; but about noon the howl had changed to a fainter sound, resembling a whine, and the dog was sitting on his haunches, quiet and depressed. At two P.M. the dose was repeated. He struggled against it, and, from partial suffocation or exhaustion, he fell on his side, stretched himself out, and appeared as if he were dying. He soon recovered, and, half an hour afterwards, I found him quietly seated as before. His breathing was less laborious—his whining had ceased—the wildness of his countenance was gone; he wagged his tail when spoken to, and arose and came slowly towards me. He refused to eat, but lapped, and seemed to swallow a little water; and I saw him void his urine, and turn from it without lapping it. At seven P.M. he was sitting quietly, but his muzzle hanging as low as his elbow. He recognized me, and faintly wagged his tail. Another drachm of the alisma was given: he again struggled, and seemed to be dying, but in ten minutes he resumed his sitting posture. I saw him in this situation at ten o'clock, but at six on the following morning he was dead.

A fair trial was given to the Belladonna, which was said to have been very successfully used on the continent. The most violent symptoms were almost invariably mitigated, but rapid exhaustion followed, and frequently paralysis. The ferocity of the animal generally ceased, but he died about the usual time.

To this followed the *Scutellaria Laterifolia* of Dr. Spalding. I was at some pains to get it genuine, but in no one instance

was it successful. It lulled the irritability of the dog; it not unfrequently sent him to sleep; but he died completely exhausted, and almost paralytic.

Mr. Caesar Hawkins had the kindness to try the effect of the South American Guaco on a dog in my hospital. The lower jaw was dependent, the tongue black, the breathing hoarse and grating, and the true rabid howl was heard almost every minute. About half an ounce of the expressed juice of the Guaco was given in the afternoon, and a second dose in the evening, with some slight relief of the symptoms; but, after the third dose, on the following morning, the change was most extraordinary. The dog recovered to a very considerable degree the use of the jaw, and could not only masticate the food when put into his mouth, but could with tolerable ease pick it from the floor. The howl ceased, and the restlessness disappeared. We were surprised and pleased, and almost began to think that the grand specific was at length discovered. The medicine was continued during the day; but on the next morning the dog was found breathing slowly, with every limb paralyzed, and, presently afterwards, he died.

The Guaco was afterwards given, at St. Thomas's Hospital, to a young man evidently hydrophobic. He experienced some slight and temporary relief after every dose; but he died.

About the same time Mr. Hawkins tried the *Veratrum Sevadilla*, regarded in South America as a specific in the cure of rabies. We administered four times the quantity that was said to cure the disease, but not the slightest effect was produced.

Mr. Morgan having kindly given me some of the *Ticunas* poison, I inoculated a rabid dog with it, in order to see what effect one poison might have in weakening or destroying another. Although not ferocious, the animal had been in a considerable state of excitation. An incision through the integument was made on the inside of the arm, and a pointed bit of wood that had been dipped in the poison was rubbed on the exposed fasciæ. No effect being produced, the same bit of wood was introduced into an incision more deeply made. In less than two minutes the dog was more tranquil, and at the expiration of five minutes he dropped motionless, the only indication of life being a regular and not laborious breathing. In this state he continued eight hours, when I left him for the night. On the following morning I found him dead.

Sir Anthony Carlisle gave me the opportunity of testing another South American cure for hydrophobia, prepared from a plant of the cactus family. It produced a considerable degree of temporary excitement, I might almost say insanity; but this

passed over in the course of a few hours, and the disease pursued its fatal course. It was also, about the same time, given to the human being without effect.

And now, gentlemen, I have told you nearly all that I know respecting rabies. I have seen some palpable cases of spontaneous recovery. I have seemingly gone a great way towards effecting a cure, but I never fully accomplished my purpose. I hope that I may have contributed to prevent the appearance of the disease in a great many patients; but I have never cured one. Others have, or say that they have. Spalding has cured nearly eight hundred with the *Scutellaria*—Brera has succeeded with the *Belladonna*—and Blaise with mercurial inunction. I dare not say, I do not believe, that they are designedly imposing on the public by fabricated or false accounts; but such has not been my good fortune, although I believe that I have experimented on more animals than any other practitioner in any part of the world. I have seen again and again decided good impression made upon the disease, but the cure has not been worked out. The subject should not be dropped. The nature of the disease is fully known—the kind of medicines to be employed are sufficiently indicated—and I have no manner of doubt that the successful treatment of rabies will ere long be discovered. This will, indeed, be one of the noblest triumphs of medicine. It is one that seems especially allotted to the veterinary surgeon. He sees the disease in all its various forms in the animal that is most frequently the subject of it. His daily practice teaches him the way of coercing and managing this animal, a struggle with whom might be fatal to others. I need not tell you how well *he* would deserve of his country, by whom the successful treatment of rabies is discovered, and with what blessings his name would be hailed. Let this be your path—cautiously but confidently trodden. It will conduct you to the deserved estimation and gratitude of your brethren and the public.

CASES OF PRESSURE ON THE MEDULLA SPINALIS, FROM EXCESSIVE HEAT OF THE SUN.

By Mr. JOHN TOMBS, *Pershore*.

July 7th, 1837.—I WAS desired to visit two entire cart colts, belonging to a highly respectable agriculturist in this neighbourhood. When I first saw them, they were grazing in a meadow contiguous to a large wood. They were exposed to a grilling sun; it was a perfect calm, and the ground was like a hot-bed. When I approached them, they began rolling, swerving, and

staggering about, as though their backs were badly rigged. The one, a red roan colt, had been observed to stagger as he walked two or three days before. He could not now stand sufficiently quiet to graze; his hind parts were perpetually rolling right and left. When he was driven along, I thought he would have been precipitated head foremost into the river Avon (forming the western boundary of the meadow in which they were), having lost the proper command of the voluntary muscles of the body. The other, a blue roan colt, was attacked this morning: he swerves a little. They were driven home and housed with some difficulty; being unbroken, wild and awkward. I could not correctly judge of the state of the pulse, they were so intractable and excited. They were both bled, and had a solution of aloes given them.

8th.—They have had halters on all night, and are more tractable and composed. The red roan has amaurosis, and staggers very much, evidently indicating too great a determination of blood to the brain and medulla spinalis, and caused by the intense heat of the sun. The blue roan to-day staggers as badly as the red one: the pulse of the former is sixty, and hard; and of the latter seventy, and wiry. Their bowels are constipated. The treatment was repeated—venesection, and Barb. aloes in solution.

9th.—Respiration disturbed—bowels sluggish—pulse of both sixty, and hard. I bled, and gave laxatives, and applied sheep-skins reeking hot to the loins. Their diet was bran mashes, which they ate voraciously, and in a very peculiar manner, from the imperfect action of the muscles of mastication.

11th.—They stagger exceedingly—the pulse forty, and soft—the breathing tranquil—the perspiration is running off their backs down the legs, from the influence of the sheep-skins. Remove them, and wash their backs with tepid water. The bowels are relaxed. Give digitalis and tartar emetic.

13th.—No visible improvement in either of them. The blue roan's tail is erect, similar to that of an animal with tetanus. Give aloes, digitalis, common turpentine and nitre; and stimulate the loins.

15th.—The red roan is much the same, with the exception of the pupils being contracted. The action of the posterior extremities is quite involuntary, inducing me to fear an effusion between the theca vertebralis and the spinal marrow. I applied a stimulating charge along the back bone, from the superior part of the scapula to the gluteal muscles. The blue roan is considerably improved. Continue the stimulus to his loins—repeat also the nauseating medicine.

18th.—The red roan swerves less. The blue roan's extremities are swollen, but the rolling motion has nearly disappeared. Turn them out in the daytime.

20th.—Both vastly better. Their bowels are regular, and pulse at the natural standard. Apply a charge to the blue roan, and turn them out altogether.

From this time to the 1st of August they progressed surprisingly, and on the 8th I again saw them: they appeared quite recovered, and were as fat as porpoises.

August 16th.—I was summoned in great haste to attend the red roan. I found him much in the same state as he was when originally attacked. He was staggering about like a man beastly intoxicated, and had amaurosis. There was some fever—the pulse sixty, but the appetite was good. Bled to syncope, and give physic.

18th.—Much the same. Give small doses of aloe and digitalis.

23d.—Great improvement,—a trifling staggering gait. Blister the back extensively.

September 1st.—The swerving motion slightly perceptible. Turned out.

20th.—Still too much lateral motion. Repeat the blister.

November 27th.—Can walk perfectly upright.

April 1838.—Both colts are quite well, and at plough.

I had two similar cases at the same time: the one a mare giving suck, and the other a waggon horse. They both recovered in a few days by copious bloodletting and powerful purgatives.

These complaints are not frequent in England; but I have seen a disease very much resembling it, and very frequent in India. It is termed Kumree.

A CASE OF EMPHYSEMA OF THE NECK, BREAST, CHEST, &c., IN A HORSE.

By Mr. W. A. CARTWRIGHT, *Whitchurch*.

MR. COOKSON, sen., of the Woodhouses, lent his grey cart-horse to his son. He was in good condition—I may say fat—and had about twelve months ago been castrated.

June 7th, 1838.—Between five and six in the morning, the horses were fetched up. They galloped about very much, and snorted a great deal. He, amongst others, was then put into the stack-yard for awhile. Soon after he took a sort of bounding deer's leap on all fours; then all of a sudden stopped, humped his back up, and was very stiff. The front and sides of his neck were puffed out instantly to a great size, and the swelling presently extended all over his breast, shoulders, and the lower

parts of the thorax: the whole of this swelling was caused by the effusion of air.

The son, as soon as possible, took six or seven quarts of blood from him, but had some difficulty in finding the vein. He was kept in the stable all day, and at night was sent home, a distance of four miles, to his father's, who, finding his respiration too quick, took five quarts more blood from him.

8th.—Mr. C. came and informed me of the circumstance. I could not then go to him, but I sent three alterative balls, one to be given night and morning.

9th.—I saw him, and found him nearly as Mr. C. had told me; but who now said that he was not so bad as he had been. He had on the front of the neck, about eight inches from the breast, an enlargement of the size of one's fist, though more flattened, with the appearance of there being effusion beneath, but which did not pit under the finger. It was slightly sore and inflamed, and there was but little emphysema mixed with it. It was not attached to the trachea. The breathing of the horse was natural, and I could not detect any opening into the trachea. I ordered the neck and breast to be fomented, and the horse kept up, and fed on mashes and grass.

14th.—I again saw him, and found that the emphysema had been absorbed gradually, and there was scarcely any to be felt, except about the breast, where it still remained in a considerable degree, the skin being loose. The tumour at the front of the neck was less, but very distinct, and altogether unconnected with the trachea. It was certainly more tender, and he shrunk on my pressing the windpipe there. The respiration was natural. He has been worked a little. I now gave him a dose of physic, and ordered him to rest for three days more, when all became right.

I send you this case in consequence of seeing such a dearth of English communications in your late numbers. I think it is really strange—not to use a stronger word. One would imagine that your table would be loaded with communications, from the number of veterinary surgeons now scattered over every part of the country.

THE POISONOUS EFFECTS OF THE FLESH OF DISEASED CATTLE, AND THE GASES PROCEEDING FROM THEIR CARCASSES.

ALTHOUGH we think the danger over-rated, yet the following extract from a Report by M. Frank, provincial councillor of Magdeburgh, and published under the authority of the Prussian government, may convey a salutary caution to those who have much to do with cattle.

A labourer in a neighbouring village died in consequence of the sting of a fly, which he received on the back of his left hand a few days before. The arm of another has mortified so far as the elbow joint, from a similar cause. The physicians of Magdeburgh thought it probable that the flies which stung these individuals took the poison which they communicated from cattle that had died of malignant fever, and whose carcasses had been left unburied.

Several proprietors of cattle were afflicted with dangerous fevers, from having touched the mouths or gangrenous ulcers of cattle labouring under inflammatory fever. Some men died who had bled heifers attacked with this disease. A woman who drank milk from a cow thus diseased, died: and several dogs and pigs died within twenty-four hours after having eaten of the flesh.

We may add to the above, that M. Fœderé (*sur les Epidémies*, p. 117) states, that a febrile dysentery prevailed among the troops at Entreaux, produced by the flesh of cattle that had been overdriven, and were afflicted with red-water: that at a village near Nice, a man died, and all his family became dreadfully ill, after eating the flesh of a cow that died of the epidemic then prevailing: and that so many accidents of the same nature occurred during another epidemic, that the sale of the flesh of cattle that had died of it was forbidden, under a heavy penalty.

About four years ago I was examining a cow that died of inflammatory fever. I was immediately afterwards seized with violent vomiting, and felt considerable lassitude for several days. The butcher who opened her became dangerously ill.

In the year 1823, I was required to ascertain the cause of the death of four cows that had dropped suddenly. A saturated solution of corrosive sublimate had been applied as a cure for mange. The butcher who opened them for me took to his bed the same evening, and had severe inflammatory fever.

A singular case connected with the above, and shewing the rapidity with which the animal fluids may be converted into the direst poisons, is extracted from the *Journal der Practischen Heilkunde*. A person at Munster quarrelled with a soldier who lodged in his house. The latter drew his sabre and attacked his host, whose wife, terrified at the danger of her husband, rushed upon his adversary and wrested the weapon from him. She then, in the highest state of agitation, took her infant from the cradle and applied it to her breast. The child had scarcely begun to suck ere it sighed, and expired. The milk of the mother had been converted into a deadly poison.

Y.

EXPERIMENTS ON THE USE OF CREOSTOTE IN THE DISEASES OF DOMESTICATED ANIMALS.

By HERR MAASSEN, V. S. Wurtemberg.

THE first experiment was on a setter with canker in the ear. The owner of the dog had ordered it to be hanged, as all remedies had failed in producing a cure. Herr Maassen prescribed creostote $\mathfrak{z}\mathfrak{s}\mathfrak{s}$, et spirit. vini rectificat. $\mathfrak{z}\mathfrak{i}\mathfrak{j}$. This mixture was applied once in every day to the diseased part. In a few weeks the dog was completely cured; and has since had no return of the complaint. In a terrier, and also in three other spaniels, the effect of this application was equally satisfactory. In some cases, where the disease shewed itself in a less degree, the creostote was dissolved in water instead of spirits of wine. It is always necessary to take away the collar while the dog is under treatment, in order that the flap of the ear may not be injured by striking against it.

He tried the same medicine with equally favourable results on horses. He had a four-year-old thorough-bred horse brought to him, which was suffering from a cancerous sore in the frog of the right fore foot. The disease had several times appeared to be cured by different remedies; but as soon as the horse was used, it returned again as badly as ever. He first placed the foot in a poultice, in order to soften the horn. He then removed all the diseased parts with a knife, and by that means allowed the pent-up matter to escape. Then, having subdued the inflammation by cold applications, he ordered creostote $\mathfrak{z}\mathfrak{s}\mathfrak{s}$, and spirit. vini rectificat. $\mathfrak{z}\mathfrak{i}\mathfrak{j}$, to be applied to the surface of the frog once a day. The foot was then bound up with dry bandages; and this treatment was continued during several days.

In the course of ten days the frog was so far healed, that the lotion was only required to be used in a diluted state. The treatment of this case commenced on the 14th of April. In the middle of May, the horse was shod, and fit for service; and, notwithstanding that he was worked hard, no trace of the disease was afterwards to be seen.

A cart-horse, that had suffered some time from an old painful sore on the lateral cartilage of the right fore foot, was brought to him. The sore was swelled at the edges, and exhaled a very fetid odour. Several modes of treatment had been resorted to, and some of them had certainly done good; but still unhealthy pus continued to be secreted, and the wound was altogether indisposed to heal. He immediately ordered the creostote to be

used. The wound was very soon completely healed, and the animal has never experienced any relapse.

He intends to try the effect of this drug, which has already been found to be so valuable in human medicine, as a remedy for the foot-rot in sheep; and from what he has already seen of it, he confidently anticipates the most favourable results.

Hippologische Blätter.

CASE OF RUPTURE OF THE LEFT CORONARY ARTERY IN A HORSE.

By HERR BORGMAN, of Hirschberg.

On the 18th of June 1833, a levy of horses was made in Hirschberg, for the remount of the second squadron of the seventh provincial cavalry regiment. In order that these horses might be carefully examined as to their soundness and fitness for service, they were ridden rapidly in the streets. One of them slipped up, and came suddenly and violently on his side. He, however, immediately got up, to all appearance uninjured, and afterwards ate his food with avidity.

When the examination had taken place, the commander of the troop left all the horses together, to be fed and shod; and, this being done, each under officer was quartered on some neighbouring village, and ordered to set out with his men. One of these, who had left his own horse in the suburbs in order to be shod, mounted the one that had fallen down in the morning. The animal appeared quite calm and tractable; but just as the officer gave the word of command to march, it, without any apparent cause, leaped so high that it nearly sprang into the shop-window of a neighbouring house, and then fell back on the ground and died immediately.

He was not opened until the next day. After the most careful examination of the stomach and intestines, I could find nothing abnormal; but on opening the chest, the pericardium appeared to be very much distended, and of a blue colour. On cutting into it, I found between it and the heart a mass of coagulated blood, weighing nearly 4lb.

It is my opinion that, in consequence of this mass of blood, the heart could no longer beat, and the circulation of blood being necessarily stopped, this caused the death of the animal. The cause of this coagulated blood I found to be a rupture of the left coronary artery; and the cause of this rupture was doubtless the fall he had in the morning. It appears to me most pro-

bable that, after the rupture, the blood continued to ooze slowly out of the coronary artery into the pericardic bag, and when this was as full as it could be of blood, the horse died.

Magazin für die gesammte Thierheilkunde.

ON PUS.

THE MEANS OF RECOGNIZING THE PRESENCE OF PUS IN THE DIFFERENT FLUIDS WITH WHICH IT MAY BE MIXED, AND PARTICULARLY IN THE BLOOD.

By M. AL. DONNE.

IN a memoir published by this gentleman in the *Archives générales de Médecine*, he has endeavoured to explain the manner in which pus may be detected in the following fluids: the serum of the blood—the serosity—the mucus of the saliva—an aqueous solution of albumen—bile—urine—semen—pure milk—milk having undergone certain changes—colostrine and blood. He found the characters presented by the pus to be two in number. 1st, its being transformed by ammonia into a transparent and tenacious jelly. This effect is equally appreciable in a small portion of pus diffused in water. 2d. The same re-agent does not change any of the properties of the blood. By the former of these properties, the pus in the first nine of these substances is distinguished; and by the second, the difference of the blood. These characters, however, although generally to be relied on, are not always infallible.

He has also compared the difference between the globules of pus and those of blood, and he has obtained the following results:—

The globules of pus are,	The globules of blood are,
1st. Spherical.	1st. Globular.
2d. Of $\frac{1}{1175}$ th part of a millimetre in diameter.	2d. Their greatest diameter does not exceed $\frac{1}{1175}$ th part of a millimetre.
3d. Wrinkled or furrowed.	3d. Smooth.
4th. Terminated by a fringed border.	4th. Terminated by a plain border.
5th. Traversed by lines, which cross each other, and give an appearance of mesh-work.	5th. Presenting no reticulated appearance.
6th. Without any appreciable nucleus.	6th. Having a central nucleus.
7th. Not changed by ammonia.	7th. Entirely insoluble in ammonia.

These characters are not invariable.

Pus mixed with a certain quantity of healthy blood communicates to it all its physical properties; and the new pus, thus formed, can produce the same phenomena when mingled with more blood.

Jour. de Méd. Vét.

A CASE OF SYMPTOMATIC VERTIGO (STOMACH STAGGERS) IN A POST HORSE.

By M. OLIVIER, M.V., à St. Maximin.

ON the 17th of October, 1837, M. Jussieu, a post-master, left a horse, six years old, at my infirmary, that had refused his food during two days, and had lost all his usual spirit.

It was about five o'clock in the evening when I saw him. He staggered as he walked—fell upon the man who held him—and pushed obstinately forward if any one attempted to stop him. My prognosis was unfavourable. I inserted two setons on the sides of his head, and applied stimulating sinapisms over the setons. An ounce of tartarized antimony was administered in an infusion of chamomile: laxative injections were also thrown up, to one of which I added an ounce of aloes, and a similar quantity of sulphate of soda. We were compelled to cast him, in order to accomplish this.

We kept him in this position until one o'clock in the morning, when his agitation becoming extreme, and the sweat running from him in torrents, we released him, and, placing him in a large stable, took every care that he should injure himself as little as possible. During the moments of calm he bore heavily on the halters to which he was attached, pushing forward—holding his head low, and which he inclined constantly to the left side. He was sadly outrageous during the greater part of the night. The injections which had been thrown up were not returned. The urine was of a red colour, and very forcibly expelled.

From four o'clock in the morning until mid-day, he maintained one fixed position, constantly pushing forward. He was unconscious of every thing around him, and was completely blind and deaf.

To this lethargic state succeeded new and still more violent struggles. We continued the mucilaginous injections, with the addition, twice in the course of the day, of an ounce of emetic tartar, with the same quantity of sulphate of soda and nitrate of potash. The sinapisms were re-applied on the side of the head.

The following night was comparatively calm. The comatose state remained the same, but the access of fury was much more rare. Four setons were now applied, with the administration of mucilaginous injections.

On the morning of the 20th, hanging back on the reins, and in such a state of immobility that it was only by severe blows with a whip that we could induce him to quit this position, diametrically opposite to that which he maintained so obstinately on the preceding days.

An evacuation of semi-fluid faecal matter now took place, which I regarded as a good augury. He also urined in sufficient quantity, and without any violent efforts, which he had not done before. Finally, the development of his pulse—the moisture of the skin—the countenance a little animated, accompanied by some degree of hearing, and a confused perception of objects, and the cessation of the tendency to bore forward were pleasing, and from which I was tempted to draw a favourable prognosis. I presented him with some whitened water, and he drank nearly half a pailful. In the course of the day he drank several times of the same liquid with avidity, even plunging his muzzle into it.

21st.—He continued to improve—his bowels were opened—he searched for something to eat, and perfectly distinguished surrounding objects.

30th.—He was dismissed.

From this isolated fact, to which, however, I could add many others, I do not pretend to draw the conclusion that emetic tartar will always effect a cure in symptomatic vertigo; but it tends to support the opinion of the learned professor *Gilbert*, and the valuable observations of MM. Philippe and Crepin. These distinguished veterinarians have proved, in a memoir full of interest, and containing a great number of well-detailed cases*, they have proved, I say, the immense advantage that may be derived from the employment of large doses of tartarized antimony in sympathetic vertigo. They have well-demonstrated the danger of having recourse to blood-letting in the treatment of this fearful disease, the primary cause of which is bad food, or a bad mode of feeding too long continued, and which must necessarily lead to irritation of the primæ viæ, and, sooner or later, dangerous vertiginous affection. For my part, I am well-assured that, in a practice of twenty years, I have seen more than sufficient of the inefficacy of bleeding in cases of staggers, and par-

* See the *Journal of Veterinary Medicine*, theoretical and practical, for February 1835; and the *Veterinarian* for 1835, p. 257 and 258.

ticularly in post-horses, who often have not time fairly to digest their food, on account of the rapid and unexpected work which they are often called on to perform.

Journal Vét. du Midi, Avril 1838.

[It is difficult to understand the *modus operandi* of the tartarized antimony in stomach staggers, and the forbidding of venesection sounds strangely to the ear of the English veterinary surgeon; but the cases of MM. Phillipe and Crepin, related in the eighth volume of THE VETERINARIAN, deserve serious attention, although we must freely confess that we do not rank among the converts to their opinion.—Y.]

LETTERS TO A STUDENT.

No. IV.

By Professor STEWART, Glasgow.

Beware of DRENCHES.

An article in the last VETERINARIAN reminds me that I owe you a letter upon a subject of considerable importance.

When you are under the necessity of drenching a horse, you are not to put a twitch on him, nor to pull out the tongue, nor to raise the head any higher than is absolutely necessary. You are to keep the neck straight; and the mouth-strap is to be placed close behind the tusks, and not pressing the cheeks into the mouth. Make the assistant who supports the head stand fairly below it, and with the staff perpendicular to the ground. If the draught is pungent, or very disagreeable, give it in small quantities; and if it is so much as a quart, let the head down for a few seconds after you have given one half of it. A strong, wide-mouthed, smooth-necked bottle is better than a horn. Keep it away from the teeth. You are not a good operator if you let it be crushed between them.

Notwithstanding every precaution, and the greatest care, accidents will sometimes happen in drenching horses. By raising the muzzle you depress the larynx and open the glottis. In some short, thick-necked horses much elevation of the muzzle seems to render them *unable* to close the glottis.

The safest way of giving a draught is to administer it when the horse is lying, as he mostly always is in the cases which imperiously demand a draught. Place your hand in the mouth, and take a very firm hold of the cheek at the labial angle; raise the

muzzle, and keep the poll on the ground; let another man hold by an ear and the collar to keep down the head; then pour in the fluid by degrees. The head and the neck should form such an angle as they have when the horse stands at ease. If the angle is very acute, the horse cannot swallow; if very obtuse, he is apt to choke.

When the horse is standing you may sometimes get a draught into him without using a staff to support the head. When you can support the head by placing your hand under the lower jaw, the horse will generally bring it down by force, before he will let the draught run down the trachea. The staff he cannot overcome so easily, and often a draught cannot be given without it.

Now, you are never to give a draught to any horse unless you think he will die without it. There is one disease to which slow working horses, and all others who live on bulky food, are very liable. This disease often destroys life; and, so far as my experience has gone, it can very seldom be cured by any medicine given in a solid form. Fluids must, therefore, be administered, though it is at considerable risk. But draughts are often recommended, and often given, when balls would do nearly as well. It is not generally known that there is any danger in drenches; and I have never met with any objection to their use solely on account of their danger. I am, however, well persuaded that the following brief record of cases will enable many practitioners to recollect others of the same kind.

CASE I.—In November, 1836, a draught-horse got three doses of a mixture, containing turpentine, ammonia, and water; he coughed after the last one, and was then bled to eight quarts. Subsequently he shewed symptoms of bronchitis, and for which he was treated. He recovered, but has been broken-winded ever since.

CASE II.—A draught-mare got spirit, tereb. and oil. She coughed once. Next day she had bronchitis, but received little attention. She died on the ninth or tenth day.

CASE III.—A draught-horse got ammonia, linseed oil, and spirit, tereb., and died without treatment on the fourth day. The breathing was very quick from beginning to end.

CASE IV.—Another horse got the same draught, coughed violently; received no treatment but one bleeding, and died on the sixteenth day.

CASE V.—A horse got a draught, containing sulphur, ginger, and water. Next day he had bronchitis, and was put under treatment. He did not lie down until the nineteenth day, but his recovery was perfect.

CASE VI.—A very handsome draught-horse had been fre-

quently ill, and so often drenched, that at last no fluid medicine could be given, either standing or lying. The twitch was applied, though the surgeon had previously experienced its danger. The horse, however, got every drop of the medicine, and was instantly relieved from pain. He did not cough, but his breathing shewed at once that he was in danger. At night, the respiration was 50, the pulse 75, small and hard. On auscultation at the trachea, there was distinct gurgling. The horse was immediately put under treatment, which I think was not sufficiently active, though it seemed to produce amendment until the fifth day. On the fourteenth day he died, never having lain after he had the draught.

Except a small portion on the upper part of the left side, the pleura was intensely inflamed, and coated with lymph nearly an inch thick. The chest was full of water, in which were large masses of loose lymph. The lungs were completely disorganized; the bronchial tubes in some places filled with solid lymph, in others with pus, in others with bloody froth. There were large collections of matter in the lungs, some fluid, and some semi-solid.

CASE VII.—A horse had the same medicine; but neither coughed nor breathed as if any had entered the trachea. Next day it was sufficiently plain, and he was promptly treated. He lay down on the tenth night, and went to easy work on the seventeenth day. Since that time the horse has been four times ill; but only once so severely as to require the same medicine, which did no harm upon that occasion.

CASE VIII.—A carriage horse got ginger, sp. terebin., ammonia, oil, and water. He took the medicine without the least resistance, but his throat was on the stretch when head was up, and when it was let down some of the medicine ran from the nostrils. He coughed none. In a few minutes his breathing was at 110; he was immediately put under treatment, and recovered. He did not lie down until the twelfth night.

CASE IX.—A horse got ammonia, spirit. tereb., and sulph. zinci, in water: he coughed and became ill, but was neglected, and died on the eleventh day. The chest presented the same effusion of fluid, inflammation of the pleura, and disorganization of the lungs, as found in case VI.

CASE X.—A foal got warm ale, pepper and gruel, and died the same day.

CASE XI.—A draught-horse got spirits of turpentine alone, and died instantly.

CASE XII.—A brown mare got a bottle of linseed oil, and died in about a fortnight. There was much water in the chest, and

disorganization of the lungs; but the pleura was only roughened with lymph.

In my next letter I may say a few words more on this subject, particularly on the treatment of what may be termed accidental bronchitis.

MR. MAYER IN REPLY TO MR. CHARLES CLARK.

Sir,—I have had the honour of laying before a meeting of veterinary surgeons your communication*, inserted in the last month's *VETERINARIAN*, and am authorised by them to address to you the following communication, which they have agreed upon.

I remain, yours, &c.

THOMAS WALTON MAYER, V.S.
Chairman.

It is with considerable reluctance we find ourselves called upon to make observations and statements which, we trusted, the method we had taken of making known our sentiments to you would have totally prevented; and were it not that you have thought proper to make several statements which will, if allowed to remain uncontradicted, materially alter the tone and character of your intended reply, we should have considered it unbecoming in us to offer any farther observations.

We learn, with considerable surprise, from the first part of your communication, that "you find yourself called upon to prove an allegation made in *The Lancet* some ten years ago." It surprises us, because we read the statement which we called upon you to prove in *THE VETERINARIAN* in the month of *May last*. With any statements made by you in journals not directly connected with veterinary science, we beg to inform you we have nothing to do; nor were we aware that you had ever previously made such a statement, until acquainted by yourself; consequently, we could not call upon you to maintain it.

The statement in your reply to Mr. Morgan, and which was inserted in the May number of *THE VETERINARIAN* (and which we requested you to prove), is, we presumed, what you believed to be true, and intended to appear as the truth up to the time of its insertion, to which insertion we beg to remind you the following note was attached* :—"If this singular assertion is doubted, I am ready to undertake the proof of it in a concise and straightforward manner.—C. C." This was the challenge—a challenge not made by us—made by you, and proclaimed before the whole

* See *VETERINARIAN*, May, p. 247.

profession. We took up the gauntlet which you threw down—we quietly, and, we trust, gentlemanly, expressed our doubts upon the subject, by calling upon you, not to prove assertions made “some ten years ago,” but as our resolution, “ambiguous,” as you are kind enough to call it, expresses, to prove, by a reference to well-authenticated facts, the statement made by you and inserted in *THE VETERINARIAN* in *May last*.

Such being the plain facts of the case, we were surprised, knowing with what *nice gentlemanly feelings your breast is actuated*, and how *careful on all occasions you are* lest an inadvertent word should tax others with doing what, perhaps, they had not done, and what they did not intend to do, to find ourselves charged with having “the simplicity to require” you “to defend assertions made ten years ago by a reference to present opinions, changed from what they then were.” Sir, we have perused (as requested by yourself) the resolutions again, and we certainly do not find any such assertion, nor can it be implied; nor do we find that we “distinctly accused the Professor of having veered round and changed the doctrines which he pertinaciously promulgated thirty years before.” God forbid that we should accuse any man of changing his opinions: on the contrary, believing as we do, that every man is a *student* in his walk through life, constantly striving after, yet never arriving at perfection, we should consider that his investigations had led to little profit, did they not either tend to the establishment of new principles, or to the overthrow of old ones, if borne out by facts and experience: and sooner than censure a man for adopting that which he believes to be the truth, to the overthrow of his former opinions, we think that he is entitled to the *highest praise*.

But, Sir, you further state that, by allowing that Professor Coleman's opinions have undergone considerable alteration and modification, we have “*non-suited ourselves*.” What! have we proved that the word *College* no longer commands respect? Have we proved the failure of all Mr. Coleman's patents, and that the peculiar theories which he enforces with most weight and vehemence are almost uniformly true when exactly reversed?

We are obliged to you for your *kind and considerate* remarks on our objects, and on the friendly feelings by which we are actuated towards Professor Coleman. It is not our intention to inform you whence we derive our information on veterinary matters; suffice it to say, we do not obtain it from *The Lancet*: and we will stake our character and reputation for the truth of the statement which our resolution contained.

You say, that it is the body of the profession which has changed; you truly say this in one sense of the word. It has

changed: for it will no longer allow any man to direct his attacks on its members (especially one who cannot correctly be called one of the profession) without taking up the pen in their behalf; nor will it permit yourself to ride rough-shod over that profession, at the expense of individual members of the same, even allowing that *your shoes have the advantage of being put on without nails*.

Finally, we will not debate with you the terms upon which you accept our challenge, as you term it; but we at once declare to you that, if you undertake the proof of the assertion as made in 1838, “in a concise straightforward manner;”—if you wish to act in perfect truth and justice,—you must prove it from facts of the same date as the assertion just mentioned, and not from books, pamphlets, and letters, some twenty or thirty years old.

August 11th, 1838.

THE VETERINARIAN, SEPTEMBER 1, 1838.

Ne quid falsi dicere audeat, ne quid veri non audeat.—CICERO.

THE LAWS RELATING TO WARRANTY IN THE GERMANIC STATES.

By M. J. IMLIN, of Strasbourg.

AUSTRIA.

If an animal becomes ill or dies within twenty-four hours after its delivery to the purchaser, it is presumed that the disease existed at the time of sale.

The same thing is presumed when, within eight days after the sale, MANGE appears in the *hog*, and FOOT-ROT or SCAB in the *sheep*, or, in the last animal, THE ROT occurs within the space of two months: also if CONSUMPTION occurs within thirty days after the sale in *cattle*, or, in *horses*, and especially of much value, STRANGLES, or GLANDERS, or BROKEN-WIND should appear within fifteen days, or FARCY, or RESTIVENESS, or AMAUROISIS, or SPECIFIC OPHTHALMIA within thirty days.

The laws impose on the purchaser the obligation of giving notice to the seller, or, in his absence, to the legal authority of the place, of the existence of the unsoundness the moment he discovers it, and always in sufficient time for the tribunal to proceed to the examination of the living or dead animal, through

the medium of experienced persons, before the expiration of the legal period of warranty. They likewise authorise the purchaser to institute his action after the prescribed time, if he can produce actual proof that the unsoundness existed before the sale; and, on the other hand, the seller is permitted to adduce proof that the first appearance of the unsoundness was subsequent to the sale.

The law makes no distinction between the animal being sold, or given in exchange, or coming in right of inheritance. It likewise permits the action to include a pair of horses, or other animals, when one only of them becomes unsound, and the price was not fixed for each animal separately.

It does not matter whether the seller was cognizant of the unsoundness. If he is condemned to retake the animal, he must pay all the expenses of his keep, and of the legal proceedings.

The buyer loses all right of bringing his action for unsoundness when he fails of plainly and expressly pointing out the nature of the evil of which he complains. The owner of the horse may at the time of sale point out any faults against which he will not give a warranty, but the vague expression, that he does not warrant the horse at all, will not relieve him from future liability.

The simple disposition or apparent liability to a certain disease is not sufficient; the purchaser must prove the actual existence and manifest development of the disease. On the declaration of the veterinary surgeons appointed by the tribunal to examine into the case that there is some doubt as to the disposition to, or development of, a certain disease, the judges have the power of prolonging the usual duration of the warranty. The duration of the warranty commences from the delivery of the animal.

With regard to other faults or vices beside those expressly named in the laws relating to the warranty of any animals, and which may become apparent within twenty-four hours of the sale, the purchaser may institute an action for any sum not exceeding half the price, or he may bring an action generally for fraud; but he must in these cases furnish proof that these faults existed before the sale.

BAVARIA.

In the different states which now compose the kingdom of Bavaria the old laws of warranty are in full force.

A. In the common law of Old Bavaria there are laws relating to GLANDERS, MANGE, and PHTHISIS* in the horse, and ex-

* The French term is *VIEILLE COURBATURE*, a vague and unscientific term. The words consumption, or palmonary phthisis, will best convey its true meaning.

tending to fifteen days after the sale—to CONSUMPTION in cattle, to the SCAB in sheep, and to CUTANEOUS ERUPTIONS in pigs, extending to four weeks. In case of fraud on the part of the vender, the period of the commencement of the action is not limited. Sales effected by order of government, or in consequence of the legal seizure of certain animals, are exempt from the laws of warranty.

B. In the old principality of Wurtzbourg an old law of 1742 is still in full vigour, considering as unsoundness in the horse GLANDERS, MANGE, CHRONIC AFFECTION OF THE LUNGS, BROKEN-WIND, MAD STAGGERS, and IMMOBILITY. With regard to these diseases the warranty extends to thirty days, before the expiration of which the purchaser must commence his action, either by complaining to the authorities of the place, or by laying before the authorities, whether in his own district or that in which the vender may reside, a certificate from qualified veterinary surgeons, stating the existence and nature of the unsoundness. A warranty of three months with regard to CONSUMPTION extends to cattle; and all the expenses of legal proceedings will fall upon the seller, and the money paid must be restored to him, whether the beast may die in the meantime or be destroyed by the butcher. If this malady should be recognized after the three months have expired, but before the end of the sixth month, the loss in the value of the beast shall be equally divided between the purchaser and the seller, but the latter shall not be subject to any other expense. In either case the vender may commence legal proceedings against those from whom he may have purchased the cattle, if within the period of six months. The warranty against the ROT in sheep extends to three months. If the ROT is not discovered until the slaughter of the animal, the seller is not liable to any fine or return of money if the meat is uninjured; but if that is rendered unsaleable, or is deteriorated in value, he must compensate the buyer for the loss sustained. A warranty of four weeks is allowed for TURNSICK, EPILEPSY, CALCULI in the urine or bladder, INVERSION OF THE WOMB, DROPSY, POLYPUS IN THE PHARYNX, or OBSTINATE REFUSAL OF FOOD. FOR THE MEASLES in pigs there can afterwards be no action of warranty when the buyer has not examined the animals at the time of sale. When at the time of sale the buyer recognizes the existence of the *cysticercus**, he should return the animals immediately to the vender, for if he retains them it will be at his

* The *cysticercus* is the supposed hydatid in the cellular membrane of the hog, and connected with or the cause of that peculiar cutaneous or subcutaneous affection recognized in this country by the name of *measles*, from its supposed resemblance to the eruption of measles.

own loss. The vender, however, may be called upon for remuneration, if, on killing the pig within six weeks from the time of purchase, these worms should be found in any other part beside the tongue, although they had been visible in that organ at the time of sale. The foreign cattle merchants are treated according to the laws of their own country whenever the matter can be brought to a speedier termination, or an animal evidently affected with some prohibited disease is not attempted to be forced on the purchaser.

C. The common law of the *ci-devant* principality of Bamberg admits a warranty of seven days only against the sale of *horses* with CHRONIC AFFECTION OF THE LUNGS, GLANDERS, MANGE, and IMMOBILITY. With regard to the ROT in *sheep* the warranty is only conventional.

D. In the district of Lauffen, formerly a part of the archbishopric of Salsbourg, an ordinance of so old a date as 1716 remains in force; the prohibited diseases, with a warranty of fifteen days, are GLANDERS, IMMOBILITY or VERTIGO, PULMONARY PHTHISIS, and BROKEN WIND.

E. At Aschaffenburg, and the neighbouring country, formerly dependent on the Electorate of Mayence, there exists a law which says that a warranty of four weeks and a day is granted for *cattle* and *sheep* with regard to the ROT, EPILEPSY, TURNSICK, PULMONARY CATARRH, and the RED-WATER in *cows*; and a year and a day with regard to PULMONARY PHTHISIS. As to *horses*, there is a warranty of four weeks and a day against GLANDERS, BROKEN WIND, and IMMOBILITY, or VERTIGO. Conventional guarantees, in order to be available, must be transcribed into the market-book; and when the duration of any warranty is not expressly stated, it is supposed to be for four weeks and a day. This warranty can extend only to serious diseases and defects, on account of the numerous difficulties which would otherwise continually occur. When a beast, with regard to which there is any dispute, dies, the buyer must immediately send to the vender, inviting him to be present at the post-mortem examination of him. If, at the expiration of thirty-six hours, he does not make his appearance, the examination is conducted by persons legally appointed for this purpose; a *procès verbal* is then sent to the vender, together with the skin of the animal.

When an animal, the subject of dispute, is destroyed, it must be effected in the presence of two proper officers, whose certificates shall be produced before the tribunal. The purchaser loses his claim on the vender if, without previous reference to the tribunal, he sends the animal while living to the butcher.

When the tribunal pronounces against the vender, all the ex-

pense of the keep of the animal and of the legal proceedings shall be defrayed by him. When one of a couple of oxen proves to be unsound, the seller can be only compelled to take back the defective one, or to return half of the money paid; and the same rule will hold good when more than a pair of oxen have been sold. When a cow is declared by the vender to be in calf, and the calving does not take place until a month after the time he has indicated, the purchaser has a right to some indemnity; but if it is found, still later, that she was not in calf at all, the seller must return to the buyer half the estimated value of a calf dropped at the time which he had indicated. The purchaser has also a claim to indemnity for the time during which he has not been able to use the beast. This last arrangement, however, will not take place in case of abortion; but it will be required of the seller to prove that abortion.

F. In the principalities of Anspach and Bayreuth, formerly belonging to Prussia, the custom of that country, and which will be presently described, is followed.

G. In that part of Bavaria bordering on the Rhine the code of Napoleon is observed, with some few additions. A cow may be returned as vicious when she milks herself, when she kicks during the milking, or when she labours under dysentery. If a beast is sold without warranty, it may be returned on account of any unsoundness or vice of which it can be proved that the owner had cognizance. It is supposed to pass into the hands of the purchaser without unsoundness or vice.

In the communes formerly a part of the Palatinate, the warranty extends, in the *horse*, to GLANDERS, EPILEPSY, IMMOBILITY, RESTIVENESS, and latent FISTULA of any part. The warranty comprises four weeks and a day. In *cattle* and *sheep* it extends to PHTHISIS, EPILEPSY, THE ROT, TURNSICK, and CRIES OR CANCER OF THE JAW. The duration of the warranty is four weeks and a day, with the exception of PHTHISIS, in which it extends to two months; with the proviso, that if the animal dies within the first three months, the seller shall return the whole of the price, and half of it only if the animal should be lost in the second three months. When an animal is sold as sound, and without any defect, this sweeping warranty continues fifteen days against every vice or ailment.

In the communes formerly attached to the bishopric of Spire the warranty continues two months against GLANDERS in the *horse*, and four weeks and a day against MANGE, IMMOBILITY, BROKEN WIND, and PHTHISIS; and in *cattle* and *sheep* six months against CONSUMPTION, THE ROT, TURNSICK, and CRIES OF THE JAW. If any of these diseases appear within

the first three months, the whole of the loss is sustained by the seller, and half of it during the second three months. In *sheep* the warranty extends to MANGE; and in the *pig* to MEASLES with a continuation of four weeks and a day.

In the city of Spire the warranty against MANGE, IMMOBILITY, and BROKEN WIND in the *horse* lasts four weeks and a day; against CONSUMPTION in *cattle* four months; against CHRONIC PLEURISY and TURNSICK four weeks and a day; and against CHARBON four-and-twenty hours.

At Landau and Bergzabern the period of warranty extends to three months in the *horse* with regard to MANGE, BROKEN WIND, IMMOBILITY, and PHTHISIS. There is also a general warranty against all diseases for fifteen days. In *horned cattle* the warranty extends to a twelvemonth and a day with regard to PHTHISIS, six months for EPILEPSY, and three months against TURNSICK; and also fifteen days with regard to every other disease.

WURTEMBERG.

The ordinance of the 17th of February, 1767, is still in force. GLANDERS, IMMOBILITY, every description of VERTIGO, MANGE, FISTULA, FARCY, STRANGLES, and almost every kind of malady, AS CANCER OF THE EAR, CARIES OF THE JAW, CANCER OF THE PREPUCE OR VAGINA, OF MAMMÆ, and also BROKEN WIND, and PHTHISIS, and SPECIFIC OPHTHALMIA, are considered as unsoundness. The warranty extends to eight weeks for OPHTHALMIA, and four weeks and three days for all the other maladies.

In *cattle*, PHTHISIS and TURNSICK constitute unsoundness, with a warranty of two months with respect to the former, and four weeks and three days with regard to the latter. In *sheep* the HUMID and the DRY MANGE, with a warranty of a fortnight and a day. In *pigs*, CONSUMPTION and the MEASLES, with a warranty of four weeks and three days. No particular malady is designated with regard to goats; but the law says that if they are unsound, the loss must fall on the vender. There is no warranty with regard to sows that are suckling their young ones. No action on the warranty can be brought except for these diseases; and the time of the warranty having once expired, the purchaser loses all claim for compensation, even although he may be able to prove that the cause of unsoundness did exist before the sale. No declaration that he will not warrant the animal against certain diseases or causes of unsoundness will protect the seller. The warranty extends to every circumstance mentioned in the law. The sale may be annulled when the value of the animal is diminished one-third by the disease complained of.

BADEN.

With some variations not devoid of interest, the laws of warranty are the same in Baden as in Wurtemberg. In case of diseases recognized by these laws, or in consequence of a mutual understanding between the parties, the purchaser has the right of demanding at his pleasure either the annulment of the bargain or the return of a certain portion of the price. When in this case of conventional warranty no precise time is fixed, it is supposed to extend to four weeks and three days. When the contract for sale is not precise and clear, either an abrogation of the bargain or a diminution of price may be determined on, according to the option of the parties.

In case of abrogation of the bargain, the vender must take back the animal if it is still living, and not only restore the money paid, but defray every expense that may have been incurred.

When it can be proved that the seller was aware of the unsoundness at the time of sale, he shall not only restore the money paid, and indemnify the purchaser for any inconvenience or loss which he may have sustained, but he shall also be fined a sum equal to one-tenth of the price of the animal.

When the flesh of an animal destroyed on account of unsoundness can still be used, the sum awarded to the purchaser will be regulated by the diminished value of the meat, and the expenses actually incurred.

The expiration of the warranty is calculated from the day and the hour of the contract; and the time having passed, the buyer is deprived of all remedy, except in cases of fraud or conventional prolongation of the warranty.

Three persons appointed for the purpose, either veterinary surgeons, or, in default of them, farriers or butchers, must prove the existence of the breach of warranty, by visiting and examining the animal in the presence of one of the presiding officers of the court, and of the parties or their representatives. The *procès verbal* must be written immediately, and transmitted to the parties within twenty-four hours after such examination. The description of the state of the animal must be drawn up immediately after the visit, under pain of vitiating the whole proceedings. In the absence of the vender, whether accidental or designed, his representative shall be named by the court.

There is no regular appeal against the sentence of the judge; but in certain circumstances the *procès verbal* may be submitted to the examination of the board of health of that district.

When the examination of a living animal has been conducted according to law, it needs not to be repeated; but the opening of

an animal that has died of disease, or been destroyed by the authorities, may take place under the following conditions:—Other veterinary surgeons are named, whose opinion will not prejudice the conclusions of those by whom the animal has been condemned, except it is manifest, from the lesions found in the course of the dissection, that they had mistaken the disease. It is not indispensable that this examination should take place during the continuance of the warranty; it must be evident from the post-mortem appearances that the disease was actually existing at the time of sale.

[We make no apology for the introduction of this long extract, and which will be continued in a subsequent number. Some of the regulations are of a singular character, and not a little inconsistent with our habits of thinking on this subject; and the confinement of unsoundness to particular diseases, instead of assuming one grand principle applicable to all cases, is a great defect in continental veterinary jurisprudence. But we must permit M. Imlin to complete his sketch ere we, or, what we should far better like, some of our correspondents, take up the subject in good earnest.]

The cause of veterinary science, as connected with the agriculture of our country, progresses—slowly, indeed, but surely. Some who have great weight now, and whose influence by and by will be yet more powerful, have honourably declared their approbation of the improvement which the Agricultural Society is endeavouring to work out. If there are others who hesitate or oppose—a course so inconsistent and so injurious cannot be long, and never triumphantly, pursued. Good sense and good feeling will interpose; and that wish, so natural and so dear to the human heart, to live in the kind remembrance of those with whom we have been associated, will ultimately unite us all in the accomplishment of the noblest improvement our art has yet received.

IMMOBILITY IN THE HORSE.

This disease, which has been so often referred to in the list of *redhibitory unsoundness*, consists of a paralytic or cataleptic affection of the cerebral or spinal nerves, or both. The power of voluntary motion, and of sensation too, is, to a certain degree, impaired, or altogether suspended. Hurtrel D'Arboval, following Chabert, thus describes it: "There are two principal pathognomonic symptoms, which can scarcely be overlooked, and which are always decisive: first, the absolute or nearly absolute im-

possibility of backing, and especially if, in the usual way, the muzzle is bent against the chest in the attempt to back him, and also the impossibility of uncrossing the fore legs, if they are accidentally or designedly placed across each other. The four extremities remain fixed, or they are convulsively agitated, and the horse either falls on his nose or his side, or abandons himself to the most irregular and spasmodic efforts."

It rarely comes on suddenly, or if it does, if the horse is at work, he all at once stops, seems half unconscious, staggers, and throws his feet apart, in order to keep himself from falling.

When its approach is gradual, the animal appears unusually stupid—inattentive to the voice of his driver, and lost to all surrounding objects. Sometimes he suddenly rouses from this state of unconsciousness, but he presently yields to it again. His motions become more irregular—a general stiffness steals over him—occasionally he falls, but at other times he is fixed as a statue. By and by, urged by the whip, or the blood resuming its natural course, he rouses, and slowly and somewhat stupidly resumes his work.

This tendency to somnolency, or to catalepsy, however, gradually increases,—the fits are more frequent and more obstinate, and the disease terminates in paralysis, either partial or general, and the horse wastes away, and dies.

On examination after death, the lateral ventricles are generally found distended, the plexus choroides enlarged, and often filled with concretions, and the substance of the brain considerably softened. The spinal canal is usually filled with a serous fluid, the meninges frequently injected, and the substance of the cord softened.

There is a gentleman to whom we have lately been indebted for much valuable information respecting the state of the veterinary art, and the different character assumed by various diseases on the neighbouring continent. A graphic description of *Immobilité*, *Charbon*, and some other maladies, would be most welcome from him.

Y.

VOL. XI. 3 T

Review.

Quid sit pulchrum, quid turpe, quid utile, quid non.—Hæc.

A TREATISE ON THE ARTICULATIONS OF THE HORSE. By J. F. RIGOT, Chief of the Anatomical Works at the Royal Veterinary School of Alfort. Paris, Bechet, Jun. 1827.

[Concluded from page 452.]

In the last number we alluded to the opinions held by M. Rigot, relative to the little excavations so frequently met with between the condyles of the astragalus, and on the middle ridge of the inferior articular surface of the tibia. We also declined attempting to decide whether or not they were correct; fearing that we might possibly involve ourselves in a disputation which has lately taken place between two well-known veterinarians, one a public teacher, the other a private practitioner. The subject, however, being one of no little pathological importance, we hope the reader will pardon us for altering our determination, and giving the result of our own observations, which, on this particular point, have not been very limited.

In every case of decided hock lameness, where we have had an opportunity of anatomically examining the articulation, and this has happened some hundreds of times, we have invariably met with some alteration of structure sufficiently great, independent of the excavations in question, satisfactorily to account for the phenomena that presented themselves during the lifetime of the animal.

It is true, that, in a few instances, these excavations, or, as some are pleased to call them, sulci, were in a decidedly diseased state: but, be it remembered, that whenever this was the case, there was invariably present inflammation of the synovial membrane and its appendices. We regard the inflammation of the membrane as the primitive disease, and the altered state of the sulci as a consequence. Lameness in the horse, arising from inflammation of the synovial membrane, although some go so far as to deny even its existence, is not of such unfrequent occurrence as the generality of practitioners may imagine. We have met with some cases which, though few, put the matter beyond doubt.

Where an absorption of the diarthrodial articular cartilage is the effect of inflammatory action, the lesion has quite a different appearance from the little sulci to which we so often allude. How unlike is their aspect to any change resulting from morbid

action, when we compare them with the inferior surface of a diseased navicular bone. In the one we have an alteration of structure, and sometimes a loss of substance, which is never to be met with, except in cases where it has been preceded by such a considerable degree of inflammation that lameness of the most painful nature is the almost invariable consequence; while in the other, we have simply a removal of matter, evidently for the purpose of forming a reservoir, by the existence of which a large supply of lubricating fluid is continually kept in the vicinity of those parts of the articular surfaces most liable to be affected by friction. They are useful in another way,—they allow a free passage of the synovia from the anterior to the posterior parts of the articular cavity: this is of great consequence when there is an accumulation of this fluid.

The synovial cavity formed by the articulation of the tibia with the astragalus, may be divided into two parts; one anterior to the tibia, the other posterior to it. Their capacities are not at all times the same. They are altered by the flexion and extension of the limb. When the cavity in front is rendered smaller by a motion of the joint, the one posterior is at the same moment rendered larger, and by exactly the same movement. Therefore, such a communication between the two parts of the articular cavity as will admit of a ready passage of the fluid contained therein, is, in cases of hydrops articularis, a desideratum of no little importance. Even in the hock-joint not affected with this disease, were it not for the little space between the middle articular ridge of the tibia and the groove between the condyles of the astragalus, along with some other communications between the anterior and posterior parts of the articular cavity, the pressure of the fluid contained in one of them, when its capacity is being diminished by either the flexion or extension of the joint, would distend or even rupture the synovial membrane, and thus produce bog-spavin, or thorough-pin, or, perhaps, both.

From the fact of these excavations being more frequently found, and to a greater extent, in the old than in the young animal, it would appear that they are constructed for the purpose of acting as synovial reservoirs, in order to assist in counteracting the bad effects likely to result from long-continued and inordinate exertion.

Let us, but for a moment, reflect on the great degree of pressure, and the intensity of the friction resulting therefrom, which must exist between the tibia and astragalus of the hunter carrying a welter weight across a close and heavy country, or the gigantic dray-horse drawing a load of a couple of tons over the rough stones, and up the steep hills of our capital. As a matter

of course, Nature, ever wise for the protection and well-being of what she has constructed with such consummate skill, will, in such cases, make some extra provisions against the injuries likely to accrue to one of her most perfect pieces of animal mechanism, namely, the tarsal joint.

We have heard many surgeons, both human and veterinary, on examining these sulci, say that, from the situation which they occupy, and from the irregularity of their appearance, such a state of the cartilage must be preceded by inflammation. Indeed, many imagine that every alteration of structure, and, in fact, all abnormal phenomena met with in the different tissues which form the living animal, are necessarily brought about by inflammatory action. This is one of the many absurdities in which the fire-side pathologist is most apt to indulge.

Observation teaches us, that we ought not to regard every increase and diminution of the action of the different vessels, whether nutrient, absorbent, or otherwise, as inflammatory: there are many diseases in which inflammation takes no part; yet some of them are, nevertheless, fatal in their termination.

There is, however, a fact, which goes much further than any thing we have as yet adduced towards proving that the synovial fossæ are not the results of inflammatory action, and that they should not be considered as morbid productions.

It must be admitted, that there are a great number of our domestic animals which are rarely, if ever, affected with lameness, excepting that resulting from accident. The cat is one. Our feathered bipeds may also be included in the list. Now, in the cat we have frequently met with excavations, to a greater or less extent, in the articulatory cartilages; but more particularly in those of the elbow and tarsal joints. While dissecting the articulations of common poultry, and those of many of the wild species of birds, especially the larger of the raptures, we have often seen the same appearance. Some time ago, through the kindness of our friend Mr. Youatt, we had an opportunity of examining some of the articulations of a very large deer. Here we found excavations both extensive and deep in some parts of the diarthrodial cartilages, yet the animal shewed no lameness previous to death.

We wish it to be particularly understood, that we do not assert that there is *never* an ulceration of the cartilages of the tibia and astragalus, unaccompanied by any other lesion, and which is, at the same time, the sole cause of lameness. We only say, that we have never met with it uncombined with disease elsewhere.

The following are the conclusions to which we have been led by our investigations:—

1. That the synovial fossæ, which by some have been mistaken for ulcerations causing lameness, are frequently to be found in the articulations of all our domestic animals; but more particularly in the solipedes.

2. That they are developed as the animal advances in life, and as his labour is great.

3. That their appearance is quite different from that presented by cartilage when suffering under the effects of inflammation; for instance, the inferior surface of the diseased navicular and those of the cuneiform bones of the hock affected with osseous spavin.

4. That, when found in a diseased condition, it is always in conjunction with an altered state of the synovial membrane.

From these facts we are induced to believe, in common with professors Dick and Rigot, that, so far from these fossæ being the result of disease, and one of the causes of hock lameness, they are neither more nor less than reservoirs for supplying with a lubricating fluid those parts of the articulation which, by their mechanical relations to each other, are most liable to be affected by friction.

Frequent examinations of diseased articulations in the horse settled the dispute so long on the tapis between human pathologists, as to whether or not the diarthrodial cartilages are possessed of a sufficiently high degree of organization to be acted upon by their own vessels.

How often is ulceration seen to commence on the free surface of cartilage, when there is not even the slightest trace of any process or elongation of the synovial membrane having communication with the ulcerating surface, as described by Mr. Key, in the "Medico Chirurgical Transactions." This gentleman seems to think, that absorption of the cartilage cannot, with propriety, be attributed to any change originating in the cartilage itself; but to the vessels of the synovial membrane. It is a curious theory, of one part of the body being absorbed by the mere contact of another, especially when there is no continuity of substance between them. We must confess that we are doubtful as to its possibility, there being no means of proving the veracity of such a statement. This, however, is not going so far as Cruveiller, who says, that the diarthrodial cartilages are totally devoid of vitality. We have only to examine the changes they undergo in herbivorous animals, to be thoroughly convinced of the fallacy of such an assertion. We have succeeded more than once in tracing vessels running from the articular edge, under the cartilage, to its centre, where they passed through its thickness, and were eventually lost on the surface of a very superficial ulceration.

These cartilages are, however, frequently the seat of little miliary concretions, and many other alterations of structure, which fully prove that, although it is impossible to demonstrate their vessels, they are possessed of a considerable degree of vitality.

The cartilage of the glenoid cavity of the scapula sometimes presents an appearance totally different from any to which I have just alluded. By the loss of the uniting cellular membrane, the fibres of the cartilage lose their lateral connexions with each other and are attached only at their base to the bone, the other extremity being free, and in some instances projecting some length above the surface of the surrounding cartilage. This degeneration is never the cause of lameness; at least we have never either known or heard of its being so. We mention it, because we have not met with an account of it in any veterinary work. The changes to which cartilage is subject are so very numerous, that, to do any degree of justice to the subject, we should be obliged to occupy several pages: and in the end the production would be more curious than useful. We fear we have sadly transgressed in so far outstepping the limits of a review as to give a detail of our own opinion. But we thought that the subject was one that had been too much neglected by the generality of English practitioners: this, and this alone, must plead our excuse.

M. Rigot is at present engaged in finishing another work. We have seen some parts of it. It treats on the comparative anatomy and physiology of the articulations of almost all our domestic quadrupeds, with a minuteness of detail hitherto unequalled by any veterinary, and certainly never surpassed by any other anatomical author. It is illustrated by many accurate and highly-finished drawings, the productions of the author's own pencil, shewing the different parts of each articulation.

As a veterinary anatomist, we know of none more able than professor Rigot. He is a man of great talent, and, when he has any object in view, one of unremitting industry. These, combined with a good education, have raised him to the distinguished rank which he now holds in his profession. We expect much from him, and must not be disappointed.

H. F.

RABIES.

[In a letter with which I have been lately favoured by Dr. Forster, of Bruxelles, are the following observations, to which I certainly cannot give my assent, but which deserve to be recorded. I acknowledge that fits, and particularly the fits of distemper, are occasionally mistaken for rabies, and I have seen some cases of enteritis which a person not thoroughly drilled into a knowledge of the nature and symptoms of rabies might mistake for it; but, generally speaking, the characters of the disease can scarcely be mistaken; and there can be no doubt with regard to its too frequent occurrence. Who the English veterinarian to whom he alludes is, I know not.—Y.]

"I CANNOT quit the subject of dogs without adding a note on canine madness, drawn from the *Observations on Hydrophobia, by an English Veterinarian*. 'The custom of muzzling dogs during the dog-days has long been discontinued in England, where, after the most careful examination, it has been found that true rabies in the dog is a malady exceedingly rare; and that the greater number of cases reported as hydrophobous, are those of dogs attacked by a malady of a very different nature, and which are perfectly harmless. The reasons for abolishing the use of the muzzle in dogs are,

'1st, That it prevents them from drinking, and therefore may produce fever.

'2d, The very precaution may generate fear in the public mind, and consequently be favourable to the production of the disease, for fear is a powerful predisposing cause. For this reason the newspapers in certain countries are not permitted to report cases of rabies.

'3d, In Turkey, Italy, Portugal, and other hot countries, where the dogs run the streets in troops, hydrophobia is not heard of even in the summer; whence it must be concluded, that the apprehension of this malady in the north of Europe is much exaggerated by the fear and timidity natural to the human mind. If all the dogs suspected to be hydrophobous were chained up instead of being killed, we should probably find that there were very few that were really mad.'

As a confirmation of this opinion, Dr. Forster quotes an observation made by the late Mr. Abernethy. It is characterized by his peculiarity of style and peremptoriness of decision—it is a perfect gem in this respect; but it is altogether erroneous.

"Some persons have asserted that there is no such thing as canine madness. This is not true. Good cases exist, but they are rare indeed. A hundred other disorders are mistaken for it. A dog worried, or in pain, or out of health, or merely wanting

water, runs wildly, or merely runs wildly in search of a lost master. A pack of fools imagine that he is mad, and run after him, just as they do after what are called mad bulls. Public alarm does the rest, and the incautious editors of newspapers, who, by the by, ought never to report such cases; for I know it for a fact, that the mind may be so affected by fear, and the parts about the throat so disordered in consequence by sympathy, that an imaginary disorder, sufficient to kill the patient, may be brought on. A dog that has bitten a person should be tied up, and carefully fed and watered and observed, that it may be known, as in most cases it will be, that he is not mad, and the patient's mind set at rest. Witchcraft was formerly believed in, and, when that was the case, persons actually swore to enchantments in courts of law. The witches were burned, and the bewitched died of imaginary disorders."

PIG-SPAYING EXTRAORDINARY.

Sir,—If you think the following account of the performance of an operation in a very important branch of veterinary practice, viz. spaying, worthy of insertion in your valuable periodical, it is at your service, and I will vouch for the authenticity of it.

I remain, Sir, your obedient servant,

JOSEPH GAMGEE, V.S.

Mr. John Bowman, of Howden, a practitioner there, and a member of the Veterinary College, undertook, for a wager, to spay one hundred sow pigs in two hundred minutes. The number being procured, and umpires chosen from amongst the neighbouring practitioners, Mr. B. commenced at ten o'clock in the morning, at the Wellington Inn. The first eleven were disposed of in twelve minutes; the next forty-five in fifty-nine minutes and thirty seconds, and the remaining forty-four in seventy-nine minutes and fifty seconds; thus completing the task in two hours, thirty-one minutes, and twenty seconds,—being forty minutes and forty seconds within the given time, and averaging only one and a half minute for operating on each.

The ninety-third occupied only fifty-six seconds. The task would have been accomplished sooner, but it took Mr. B. six minutes to operate on the forty-fifth pig, from a malformation of the ovaria, and the consequent difficulty in extracting it.

[We esteem this adroitness, even in so humble a branch of our art as the spaying of pigs, highly honourable to Mr. Bowman; and we thank our friend from Florence for the opportunity of inserting an account of this unique performance in our Journal.—Y.]

RESULTS OF EXPERIMENTS ON THE SPAYING OF COWS.

By M. LEVRAT, Veterinary Surgeon, Lausanne, Switzerland.

[In THE VETERINARIAN of 1835, two very valuable papers on this subject, communicated by this gentleman, were introduced. We fear that they did not make a due impression. To enable a cow, for an indefinite period, and, probably, until old age, to yield her greatest quantity of milk, must be of immense advantage to the dairyman. A more important discovery could scarcely be made than the perpetuation of the milking powers of the cow. After three additional years' experience, M. Levrat resumes the subject. He commences by giving a fair and honourable account of the introduction of this operation. The castration of the cow, like that of many other of our female domesticated animals, is of very ancient date. Most authors that have written on the castration of domesticated animals have said that it was practised on mares, cows, sheep, and swine, in order to increase their disposition to accumulate fat; but it is astonishing that not one of them has described this effect of the operation on the cow.—Y.]

M. WINN, a land-proprietor at Natchez, in Louisiana, in America, was the first person who practised this operation with the view of preserving for a longer time in the cow the quantity of milk which she yielded during the month after parturition. The following is the report of his early experiments taken from *Le Journal des Connaissances utiles*, Feb., 1833.

"M. Winn was induced, by a course of experiments which he had instituted, to perform the operation of spaying on a cow a little while after she had calved, and at the time when she yielded most milk. She continued to yield milk for many years after this without interruption, and without any diminution beyond that which would arise from change of food. The operation was performed a month after she had produced her third calf. It was far from being a very painful one, and the fever which followed was neither considerable nor of long continuance. The wound was healed in a few days, and she soon began to give as much milk as before. She died in consequence of an accident some years afterwards.

"He then spayed a second cow, and with the same success; but she also perished, being staked in leaping over a fence.

"Being, however, fully assured of the advantages to be derived

from the operation, M. Winn castrated two other valuable cows, and, determined to avoid the casualties by which the others had been destroyed, he resolved to keep them in the cow-house, or in some inclosed place in which no harm could come to them; and he kept them there, and fed them well during a whole year.

"This experiment completely confirmed the value of the operation when it is desired to reserve the cows as milkers; and an American traveller, who saw these cows, three years after they had been spayed, was assured by M. Winn, that they had constantly given the same quantity of milk during the whole of the time, and he was convinced that they were in as good condition as could possibly be desired."

M. Winn has been dead some years. He was a well-informed man, and much respected at Natchez. He had refused to make the discovery public at first, because he feared that there was no novelty in it; and afterwards he thought that the very nature of it would render it ridiculous in the estimation of those who had not had actual experience of its value.

The operation ought to be performed between the twentieth and thirty-sixth day after calving, on a sound cow and in good health, and after her second or third calving, for that was the time when she yielded the greatest quantity of milk, and her œstrum had not then returned—a circumstance that was to be avoided with the greatest care. No preparation was necessary before the operation, except not to give the usual quantity of food on the preceding night, and to effect the operation before she ate on the following morning.

The spaying might be performed on cows of all ages and at all seasons, but it would answer better on young cattle; and, if it were convenient, should take place on some fine morning in the spring or autumn.

The method in which M. Levrat was accustomed to perform the operation of spaying has been already related in the eighth volume of *THE VETERINARIAN*, p. 459, but he has since somewhat altered his mode of proceeding; we adopt his own language:—

The cow being fixed as before described, I no longer use the bar of wood which was placed in front of her hind legs, but I confine them by means of a cord passed round them in the form of a figure of 8, and secured with a running knot.

Having made the proper incision, and found the ovaries, as formerly mentioned, I seize one of them, and with the thumb on one side and the middle finger on the other break through the layers of the ligaments of the ovary immediately above that organ, and by a motion of the thumb from the right to the left, or from before backwards, as it regards the ovary, I detach the

greater part of it in such a manner, that it is held only by two peduncles, the one anteriorly and the other posteriorly. I now seize the anterior peduncle between the thumb and the index finger, and, by means of the nail of the thumb, I cut or scrape through the ligamentous portion, which is the hardest. When this portion is cut through, I give the ovary a half-turn, and, continuing to work away with my nail, and pulling gently as I proceed, I break through the peduncle. I proceed in the same manner with the posterior peduncle, with the precaution of not breaking it by twisting it too tightly. As soon as the ligament is sufficiently cut through, the rest easily yields. The operator, whose thumb-nail is too short or too weak to scrape through the ligamentous portion of these organs, may effect his purpose by means of a blunt-pointed bistoury.

The ovaries being thus removed, I bring together the lips of the wound, and confine them by means of the interrupted suture.

The incision into the abdomen must not be kept too long open, especially in cold weather. If any small arterial vessel should continue to bleed after the ovaries are separated, the hemorrhage must be stopped by means of the torsion forceps. The blood should be absorbed by means of sponge or lint, and the wound closed as quickly as possible.

I have also practised the spaying of the cow in another way, which seems to deserve the attention of the veterinary surgeon, namely, destroying the action of the ovaries by the application of torsion to their vessels.

After having penetrated through the layers of the suspensory ligaments of the ovary, immediately above that organ, as already described (having taken care not to make the opening larger than is necessary for the passage of the ovary), I twist it twice upon itself from left to right, with relation to the body of the animal. After that I press each of the peduncles of the ovary between my thumb and index finger, in order to diminish as much as possible the elasticity of the parts. That being done, I take the ovary thus twisted in my hand; I raise it in order to reach about three fingers' breadth above the layers of the ligament of the ovary: I seize them with the points of my fingers; then I pierce them anew with my middle finger; I introduce the ovary through them by a movement of the thumb, and one of my fingers seizes it on the opposite side, care being taken to draw it in a horizontal direction, in order not to enlarge the aperture too much. When one ovary is sufficiently entangled to be retained in its situation by its own proper weight, I operate in the same manner upon the other, and then close the opening into the flank in the way already described.

In order to operate in this way, it will be necessary to practise often on cows that are destroyed at the slaughter-house, and so to obtain sufficient dexterity in the performance of these manipulations. It is also necessary that the nails of the thumb and middle finger of the right hand should be long, and rounded at the extremity.

The operation being performed either in the one way or the other, the cow should be led back to her stall, and the person that is most accustomed to her should be with her and caress her; and a little salt should be given to her with the hand, or on some moistened bran. An hour afterwards a little dry meat may be given to her, and she may have warm water with some barley or other meal in it to drink. Her food should be given to her in small quantities, and her meal and water or gruel should be warm until the sixth or eighth day, when she may return to her usual provender, or be turned to grass if the season will permit. During this time it will be necessary to avoid direct exposure to cold, and especially all currents of air.

When the cow has been taken back to her stall, the wound should be bathed with cold water several times in the day. On the following morning she should be bled while fasting. This bleeding will be particularly useful if any inflammatory symptoms begin to develop themselves, as will sometimes be the case in cows of irritable habits.

Sometimes immediately after the operation the cows will grind their teeth, and the lower jaw will be partially fixed, in which case the administration of two spoonfuls of sulphuric ether in a bottle of cold water will be useful.

If the cow does not ruminate, or constipation ensues, or the dung is hard, a pound of Glauber's salt should be administered, dissolved in warm water. If at the expiration of twenty-four hours she has not dunged, the dose should be repeated; frequent mucilaginous drinks, and particularly barley-water, administered, and oily mucilaginous injections thrown up.

After the spaying, the milk diminishes all at once, but it insensibly returns. Some almost immediately yield the same quantity of milk that they were accustomed to do; with others six, eight, fifteen, or even twenty days pass before the secretion is perfectly re-established. This much depends on the degree of fever that follows the operation, and the irritability of the patient. The state of the wound may also have considerable influence.

I cannot, perhaps, better conclude than by relating a few cases of the performance of this operation, and with reference to the general health of the cow, the quantity of milk yielded, and the tendency to fatten.

CASE I.—*May 1832.* I operated on a cow that was in a state of rapid emaciation. She did not yield any milk, but she quickly regained her ordinary habits and good condition.

CASE II.—*June 28th, 1833.* I operated on a cow of M. Francillon-Michaud, a distinguished and zealous agriculturist, residing near Lausanne. This cow, six years old, gave, in the preceding years, eight pots of milk soon after calving, and, during part of the summer, six pots*. After the operation she yielded, when at grass, nine pots; and, when fed on hay, seven pots. M. Manchillon has averaged her produce of milk during twenty-eight months at six pots and a quart per day. After that time she began to get very fat, still however yielding four pots of milk every day. She was sent to the butcher in October 1835, being then in excellent condition.

CASE III.—Encouraged by this success, M. Francolin begged me to operate on a second cow, of the Schwytz breed, thirteen or fourteen years old, and that had had eight calves. She yielded, soon after calving, twelve pots of milk per day. The average quantity of milk during the preceding years had been about eight pots per day. This cow was submitted to the operation on the 18th of November, 1833, thirty-three days after she had produced two calves. She had suffered very much in the calving, and there was a muco-purulent discharge from the vulva when I operated. She seemed to suffer very little from the operation. She produced while she was at grass eleven pots of milk per day, and gave on an average seven pots and three-quarters per day. After this time she began to get fat on her ordinary food; still, however, continuing to give six pots of milk daily. In March 1836, she was sent to the butcher in the best condition, and sold for one-third more than her owner had calculated upon.

CASE IV.—A cow, eleven years old, and that had had eight calves, was spayed on the 24th of December, 1833. She yielded immediately after her calving ten pots of milk. During the winter she yielded seven pots; and during the following summer, and while she was supplied with green food, from seven and a half pots to eight daily. She has given on the average during fifteen months six pots of milk per day. She then began to get fat, after which she yielded four pots a-day; and on the 30th of April, 1835, she was delivered to the butcher in the highest state condition.

CASE V.—On the 6th of January, 1834, I operated on a young cow, three years and a half old, thirty-three days after she had

* A pot is equal to 45 oz., or rather more than 2½ pints.

calved. She gave about eight and a half pints daily of clear serous milk. She did not lose a single meal in consequence of the operation. As early as the eleventh day her milk returned, but it had become much more creamy than before the spaying. She continued to give the same quantity of milk, but of a better character, for eight months, when she ate some poisonous mushrooms. The swelling came on so rapidly, and to so great an extent, that she was destroyed.

CASE VI.—On the 8th of March, 1834, a cow, belonging to M. Francillon, eight years old, and having had five calves, was spayed thirty days after her calving. She yielded before the operation nine pints of milk daily. During many months she continued to yield eight or nine pints, according to the goodness of her keep, and then the milk began to diminish gradually in proportion as she got fat.

M. Francillon calculated that during ten months she yielded on an average at least seven pints of milk per day; then she began rapidly to grow fat, and the milk diminished to one pint. It is, however, to be observed that this cow, which had been vicious, and which had seemed to have forgotten her vicious habits after she had been spayed, began again to exhibit all her former tricks as she got into condition, and it at length became necessary to trammel her, in order to milk her without danger. She fought stoutly against this, and this constant struggle might have materially contributed to the diminution of her milk. In spite of all these things she accumulated flesh and fat, and was killed in good condition, for the supply of the household of M. Francillon, in March 1835.

CASE VII.—On the 10th of March, in the same year, M. Miliquet requested me to spay a cow that had had a calf four months before. Having understood that she was at heat, I refused to operate, explaining to M. Miliquet the danger that would ensue from spaying a cow in that condition. I told him that, although I had not yet acquired any experience on this point, I deemed it prudent to defer the operation for some days. He replied that the cow would not remain at heat more than twenty-four hours, and entreated me not to delay the operation, since he was, on the following week, compelled to go into a neighbouring canton to purchase cows. The operation was performed without any hemorrhage, a point which the proprietor himself remarked. During that day she did not appear to have suffered materially from the operation, but towards the evening of the following day she was dull, and refused to eat. Instead of sending for me, M. Miliquet went to consult the cowherd of M. Francillon, who told him that he had nothing to fear. On the following morn-

ing, however, she was found dead. The post-mortem examination discovered a slow internal hemorrhage from the vessels of the ovaries, and yet there was not found more than four or five pounds of blood, coagulated and very black. I may add, that M. Miliquet has done me the justice to acknowledge that I took all possible pains to make him aware of the danger which would attend the performance of the operation during the œstrum of the animal.

CASE VIII.—On the 15th of May I spayed a cow that had had a calf three months before, whose milk was completely dry, and who was incessantly bulling. On the fourth or fifth day after the operation she appeared to have some degree of fever, but on the eleventh day she was quite well again. Three months afterwards she was sent to the butcher, and her beautiful condition was the remark of many amateurs.

CASE IX.—On the 16th of July, in despite of the oppressive heat which prevailed, I operated on a cow belonging to M. Francillon. She was about six years and a half old, and had had three calves. Soon after calving she had yielded twelve pints of milk, but at the time of operation she gave only ten. The wound healed by the first intention, but the cow had some slight degree of fever. Soon after the operation the scarcity of fodder compelled them to allow her only one-third of grass and two-thirds of dry food, but, in despite of this diminution of succulent food, she continued to give from nine to ten pints of milk during the summer and the autumn, until the time when all cows are usually put on dry food. Her milk diminished until the following spring, when it again began to increase. She yielded on the average seven pints and a half daily of milk during sixteen months; but about the close of that time she was in good condition, still yielding six pints of milk every day. She was sold to the butcher on the 25th of March, 1836.

Rec. de Méd. Vet., Juillet 1838.

[To be continued.]

Miscellanea.

THE HUMAN BEING AND THE DOG.

[Extract of a Letter from Dr. Forster, of Bruxelles.]

IN taking a retrospect of the history of man from the remotest times, the collateral history of the dog casts thereon a melancholy shadow, which deadens its lustre, and shews us the ingratitude and vicious selfishness of our species in their worst and truest

point of view. The dog, constant to his master, exhibits from first to last a gratifying example of unshaken fidelity. The master, too generally faithless to the dog from the moment when his utility ceases, presents a disgusting picture of base cupidity and ingratitude. Like the black slave, whom the white Christian scourges at his work while he insults him with the title of a brother, the dog, bearing the false name of a friend, is treated by his master as if he were his worst enemy; he is hunted, beaten, harnessed to carts, and made to do every sort of work; and undergoes a larger share of oppression than falls to the lot of other animals: and when his services are over, he is either left, like the old servants of tender-hearted gentility, to shift for himself, and gain a miserable subsistence on a muckheap in the street, or unceremoniously flung into a river and drowned, like the superfluous children of the Chinese—and this, too, after the most eminently useful and acknowledged services. Man, as general oppressor, seems to taint every thing which he touches. Depraved in his own appetites, and suffering from a thousand moral and physical ills which his perverted passions have entailed on him, he seems capable of transferring, by domestication, many of his disorders to the brute creation; and thus the tame dog and the horse have engendered various distempers unknown to the wild specimens. This is a curious reflection, and it leads to another,—namely, whether the punishment that follows deviations from nature, both moral and physical, does not attach itself to other animals as well as to man. The constant warfare of all animated beings is an awful spectacle of life supported by death throughout all the creation.

ENGLISH SHORT HORNS.

THE following is extracted from the "*Journal des Haras*," August, 1838. The Editor of that Journal is speaking:—"At last the truth has conquered, and by dint of repetition—that a valuable breed of cattle not only cannot flourish, but cannot even exist, without a very considerable outlay, we have at length been heard, and our demand is about to be listened to. The Ministry of Commerce and Agriculture has decided that a herd of English short-horned Durham cattle, and other valuable breeds, shall be purchased and placed in the stud at Pin. Messrs. Yvart, inspector of the veterinary schools, and Sainte-Marie, the inspector of 'Haras,' at Pin, are charged with the selection and purchase of these cattle."

SECOND SIGHT.

[Extract of a Letter from Dr. Forster, of Bruxelles.]

I MUST mention an odd circumstance which occurred about two months before the death of my favourite dog. He was sleeping at dusk on the sofa, which he usually occupied, when I saw with great distinctness, as I thought, a white dog, having precisely his form and size, cross the room at a rapid rate. I presently ascertained this to be merely one of those phantoms of the imagination which often occur when one has long nursed a sick friend, or been intent on any one particular object. I could relate a hundred such cases: they are not rare, and constitute what, in Scotland, is called *second sight*; they occur before death, as it seems, because it is by the forms of dying persons long cherished or nursed that the imagination is most strongly affected, particularly in individuals who have the requisite cerebral organization. I mention the circumstance here only to shew how complete is the parallel between the death of men and other animals, even in the most minute particulars of those supernatural appearances which are said to hover round them, and which are capable, by their coincidence with coming events, of exciting very marvellous ideas.

THE SYMPTOMS ACCOMPANYING A CASE OF ENLARGED AND RUPTURED LIVER.

By Mr. J. P. CHEETHAM, London.

[In a box which has for many a long month been unexamined, we found the following sketch. We add it to the few but valuable notes of the symptoms of Hepatirrhœa which the third and fourth volumes of our Journal contain. The liver in this case was exceedingly enlarged and softened; the fissure in the peritoneal coat was not very extensive, but the abdomen was deluged with blood.—Y.]

FROM the desire which you expressed to have the symptoms of my poor patient recorded, I am induced to send you the following sketch:—

A black dray-horse, nearly seventeen years old, and in high condition, came from a journey of twenty miles. Nothing appeared to ail him until he entered the stable, and then he began to evince what seemed to be colicky pains, attended, however, with an appearance of exhaustion that was very striking. The

pulse was 48, languid; and weak. The surface of his body was soon bedewed with cold perspiration, and the uneasiness rapidly increased—he pawed considerably, yet his breathing was very little disturbed.

An ounce each of laudanum and spirit of nitrous ether was administered, properly diluted, but with very trivial mitigation of the symptoms.

I then set six men to work to rub him until he became dry and warm, and then turned him into a loose box. He immediately lay down, and turned his muzzle towards his side. The pulse was still 48, and very little altered from its former languid character. I had him raised, and I attempted to extract a little blood; but I met with considerable difficulty in finding the vein. Before eight ounces of blood had flowed, symptoms of fainting occurred, and I hastily closed the orifice. He began again to perspire profusely. His cloth was removed, and he was again rubbed dry. By the use of ventilation and internal stimulants the faintness disappeared; but his pulse now suddenly and fearfully changed. It rose in a very few minutes to 120; but it was fluttering, and almost imperceptible. Partial sweats broke out about him—the mucous membranes of the mouth and nose assumed a deathly paleness—the pupils became greatly dilated, and the eyes were insensible to light: the respiration, however, was but slightly affected, except that there was an occasional sobbing inspiration. By the use of stimulants and restoratives he lived twenty-three hours after his first attack, and then died, almost without a struggle.

CAVALRY STABLES IN FRANCE.

The following is the appeal of a French cavalry officer to his brethren:—

“A commission having been appointed to-day, to decide on the measures which it will be useful to adopt, in order to ameliorate the state of the barracks, with reference to the comfort and health of the horse, it is the duty of every cavalry officer to give his opinion of the improvements in contemplation, the urgency of which cannot for a moment be denied.

“A few days ago the ninth regiment of Chasseurs arrived at Auch, and found the stables so miserably constructed that the greater part of the officers refused to let their horses go into them.

“By allowing a *metre* (39.37100 English inches) to each animal, the quartermasters have been able to cram twenty-nine

horses into each stable; but the lowness of the ceiling, and the deficiency of apertures for the supply of air were so great, compared with such a number of horses, that, in order to avoid the most infectious diseases, it has been necessary to leave the doors and windows open day and night,—an expedient which, while it may preserve the greater number of them, will inevitably destroy some.

“The floor is formed of pebbles, and many of them of the smallest size. The urine is easily absorbed through the spaces between them, and so perfect is this infiltration, that not a drop of fluid runs or is swept out of the stable. Many pails of this fluid are consequently imbibed by the soil, or, evaporating, mix with the atmosphere of the stables.

“The horses nearly touching each other, the floor is always wet, and the emanations from this focus of infection are such as no animal can possibly bear, unless the windows are always open.

“Applications have been made to the competent authorities to lessen the number of horses in each stable, and to let four only occupy the room now taken up by five, an alteration acknowledged to be necessary, and which has already been granted in many of the cavalry barracks. The authorities refused us, saying, that a man of talent who had constructed the stables, had assigned one metre as the proper space to be occupied by one horse, and that the old stalls must be occupied.

“It may, perhaps, be thought that these stables have been many years constructed—alas, no! they have just been built—some of them are not yet finished.

“Since, then, a commission has been at last appointed, and attention has been directed to the subject, let us hope that no delay will take place, for these ill-constructed buildings are costing France more than a thousand francs a day. Especially let it not be forgotten, that reform on two points is absolutely indispensable.

“One, and which draws after it no expense, is the diminution of the number of horses in each stable in proportion to the height of the roof, the quality of the soil, and the apertures for ventilation; and this, which would vary considerably in different stables, to be entrusted, not to these men of talent—not to the usual inspectors, but to veterinary surgeons, and the council of administration in the various corps, who alone are competent to the task. The other improvement, and which cannot be too much insisted upon, is to repair the pavement of the stables, and to fill up with cement the spaces between the pebbles, and particularly at the places where the urine falls, and thus to render it

easy for the stable to be thoroughly cleansed from time to time. Then alone, brother officers, shall we have ground to hope that we shall no longer see our best horses perishing before they have rendered the state service, and that the Government will no longer purchase them only to lose them.

"A. DES BOURDELIERS,
"Captain of the Ninth Chasseurs."

We cordially agree with the author of the above document, and which we most readily publish. We unite with him in demanding the adoption of measures like those which he has described, in order that the frightful mortality which, beyond all comparison with what is observed in the armies of other countries, prevails in the French cavalry may be staid. It is calculated that the Prussian and the Wurtemberg cavalry are remounted once in fourteen years; while a seventh part of our horses are destroyed by glanders every year. If this improvement is scientifically and honestly carried on, the expenses which may attend it, let them be as great as they will, will speedily appear to be the exercise of the truest economy.

Let us hope that the commission alluded to will be fully conscious of the importance of the task which it is appointed to execute, and a new era will commence for the poor cavalry-horse, hitherto so shamefully maltreated in France.—A. de M.

Journal des Haras, Juin 1838.

OUR FOREFATHERS' OPINIONS.—FARCY.

Is an inflammatory state of the blood, gradually increasing to the greatest pitch of acrimony, and affecting the system by degrees, till the whole mass is corrupted. Although the attack may be local, the course being inflammatory, it must soon be universal, from the very nature of the circulation.

From a MS. of the last Century.

SPLENT.

A SPLENT is an enlargement of the membrane covering the bone. By a rupture of the small vessels the fluid collected becomes ossified, and constitutes a bony substance.

Ibid.

Mr. Ferguson.—From the appearance of the bladder he did not think that there was any acute inflammation of it at the time of death; but it was very evident that inflammation to a very considerable extent had, at some period or other, existed. False membranes had been formed on the mucous lining, which was in some places amazingly thickened, and covered with what are commonly called calcareous incrustations. The greater number of pathologists considered that this calcareous matter was deposited by the arteries ramifying in the mucous membrane where it was found. This, however, was not the case. The mucous lining of the bladder having been irritated by the great number of small calculi which had escaped from the ureters into that viscus, became inflamed; lymph was poured out, and the smallest calculi, resembling the finest sand, when they came in contact with this lymph became attached to it. The lymph eventually became organized; and thus were the calcareous incrustations formed, without the earthy matter having been secreted by the arteries in the vicinity. If any person wished to test the truth of this opinion, they might do so by cutting down on the urethra of a dog, and, having first created inflammation of the bladder by injections, then introduce some extremely fine sand. The result of this experiment would be, if the animal did not immediately die, that there would be found calcareous incrustations on the mucous membrane.

The President.—There might have been, and which occurs more frequently than is suspected, some minute perforation in the bladder, whence this fluid might have slowly found its way into the abdominal cavity.

Mr. Carter.—A considerable quantity of small calculi were found in the ureters. Could this have occasioned rupture of the pelvis of the kidney?

The President had seen obstruction of the *ureter* by calculi without rupture of the pelvis of the kidney. When the *urethra* is closed there is considerably more danger. Rupture of the bladder has occurred when the calculus was no larger than a nut.

Mr. Spooner.—This animal would then have suffered very great pain, but he heard nothing of exhaustion or the evident approach of dissolution. The bullock was destroyed for pecuniary reasons; he would not have been saleable had he been suffered to linger on until he died. The progress of these and similar bodies through contracted passages must be attended by very acute suffering. The passage of gall-stones through the gall-ducts is an illustration of this. It is astonishing what pain is sometimes felt in these cases. If this had existed here, it would have been so stated. Nothing, however, was said but of the water in the abdomen, which, in his opinion, was the consequence of peritoneal inflammation.

The President.—It is altogether a very obscure case.

Mr. Spooner.—The presence of the fluid in the abdomen is not difficult to explain. If there had been much inflammation of the mucous surface of the bladder, it would probably extend to the peritoneum, and effusion would be the result. He had seen many cases of ascites. During acute inflammation of the peritoneum, or any abdominal viscus, there is extreme pain; but when effusion has commenced, that pain is considerably mitigated, or has disappeared. The secretion of the fluid is an effort of nature to abate the inflammation, and produce restoration to health. He had taken away seven or eight gallons of fluid from a mare with ascites. It was some weeks in accumulating, and during the whole of the period there was no spasm or inflammation, or apparent pain. She is now in apparent health.

A vote of thanks was then directed to be forwarded to Mr. Carter for his interesting communication.

The following Essay on Grease, by Mr. Rushton, was afterwards read:—

The subcutaneous glands of the heels of the horse secrete a sebaceous fluid for the purpose of suppleing parts which are subject to so great an extent and variety of motion during the act of progression. The disease that is now come under our consideration is dependent on certain alterations of this secretion, either in quantity or in quality. It is a morbid alteration in the vascular system of the skin covering the heels.

CAUSES.—The causes may be divided into predisposing and exciting.

The predisposing causes are those which produce local and general debility. Local debility may be engendered by washing the legs with cold water and leaving them wet. There is nothing immediately detrimental in the act of washing a horse's legs, but the evil arises from the legs being permitted to dry without friction; and thus, in the slow process of evaporation, much cold is generated, the local determination of which tends to weaken the part.

General debility may arise from the want of proper food and exercise—from long exposure to wet and cold in the autumn and winter—and from large and repeated doses of diuretic and purgative medicines. Over-feeding becomes a predisposing cause, by inducing plethora. An undue quantity of blood is formed, and there is, consequently, vascular distention. If to this plethoric state the want of exercise is added, the legs will be the parts most likely to suffer. An inflammatory action will follow distention, and the sebaceous glands will be stimulated to both an increased and unhealthy deposit, which is the very essence of the disease. Cold also operates as a local cause by weakening the energy of the parts, and thus suspending the circulation; so that on the return of warmth the circulation is preternaturally increased, and the distended and previously weakened vessels are rendered incapable of contracting on their contents.

The acclivity in the floor of many stables has been considered as a cause of grease, by throwing considerable weight on the hinder extremities, and by the unnatural position of the heels, putting the parts on the stretch, and hence weakening them.

Grease rarely attacks the fore legs, for which there are several reasons. The hind legs are farther removed from the centre of circulation, and the vessels have farther to press onward their accumulated contents. They are also more exposed to the exhalations from dung and wet litter, and they too frequently miss a great deal of the attention that is paid to the fore legs.

The exciting causes are, increased temperature in badly-venti-

lated stables, strong and repeated blisters to the legs, and various other circumstances.

When I was with a practitioner in the country, we had the management of a large establishment of coach horses. The grooms were in the constant habit of washing them as soon as they came from work, and the consequence was that we had frequent cases of malignant grease. Directions were given to discontinue the practice of washing, and the result has been that we have never had a case since.

I believe hand-rubbing the legs to be very beneficial in preventing this disorder, because it causes the blood to circulate more freely. Large-sized horses are most disposed to grease, and also those that have fleshy or white legs.

Grease may have a local origin, but in its latter stages it assumes a constitutional form.

Some imagine that it is capable of being propagated by inoculation. I do not know any recorded case of it; but certainly I know, that if a severe blister is applied to the legs, and the discharge allowed to trickle down the heels, and lodge and accumulate there, it will engender, if not grease, yet an affection that simulates it.

SYMPTOMS.—The earliest are redness, tenderness, and heat of the parts, attended by a swelling of the legs generally, and more particularly of the hinder ones. In a more advanced stage there is considerable inflammation, with or without ulceration. Most frequently there is sloughing of small portions of the skin, called *cracks*, and which cause considerable pain and lameness. The hairs in the flexure of the heels stand erect and bristly, and the horse goes *stiffly* when first taken out of the stable; this, however, wears off with a little exercise. In some cases the skin of the hind legs, from the heels half way up to the hocks, becomes covered with insensible horny excrescences, termed *grapes*, from their resemblance to a bunch of that fruit.

In both this and the former stages of the disease the fluids that are secreted have an unpleasant smell, and they infect every part with which they come in contact, as the skin of the coronet, frog and sole, and often give rise to thrush and canker.

TREATMENT.—In an early stage of the disease, warm bran poultices are useful, mixed, after a few days, with some mild astringents. A solution of alum is, perhaps, the best that can be employed. If the patient is allowed rest, a dose of physic should be administered, with moderate walking exercise, and occasional mild diuretics, such as resin and nitre combined. If my patient was in good condition, I would also bleed. If, however, he was in a weak and debilitated state, I would omit the

purgative and the venesection, and confine myself to alterative medicine.

Should the disease be in any degree virulent, I would employ the bran poultice with a solution of the chloride of lime; but, as soon as the discharge ceased to be foetid, and assumed a healthy character, substituting an alum ointment for the chloride of lime. The poultices should be removed every morning, and the horse allowed to take walking exercise; a thing indispensable in almost every stage of this complaint. The patient should also, if possible, be placed in a loose box.

If the grapes should become of a cartilaginous or horny nature, they must be removed by the actual cautery. A little astringent powder should then be sprinkled on the part, and a bandage applied until the bleeding has ceased. After this the treatment already described should be resorted to.

If the patient is much emaciated, some vegetable tonic is admissible; the best is gentian, in combination with ginger.

Green food will constitute the best provender, when it can be procured. Beans should never be allowed in an early stage of the complaint; but in cases of considerable debility they may be given with advantage. A seton in the thigh will always be serviceable.

Sometimes a thickening of the legs remains after the subsidence of every inflammatory symptom. Blistering, and even firing, may then be had recourse to. I have seen mild blisters beneficial when the horse is at grass; but by good management, gentle exercise, and frequent hand-rubbing, we may, in most instances, effect a cure without them.

Cleanliness is most requisite during every stage of this disease: it is absolutely essential to the effecting of a cure.

There is one symptom that I have omitted, namely, a peculiar straddling gait in the hind legs; and if one of them is lifted from the ground the animal exhibits very considerable pain. This gait is characteristic of the disease, and particularly that stage in which a sloughing of the integument has taken place.

The President produced a very fine specimen of the fettered or *grapy* heel. Every one of the excrescences, he said, had a separate peduncle, and the collected mass resembled an enormous cauliflower. Each of the grapes might be cut off separately, as enops from a bunch, or the branches from a cauliflower. He was accustomed to cut them off singly with a probe-pointed bistoury, which he was able to introduce among them, and he stopped the bleeding with the budding-iron. This was far less painful than the excision of them by means of a heated iron. He would beg leave to ask the author of the essay, whether he considered the disease to be of an infectious nature—whether it could be communicated to other horses by contact or the atmosphere of the stable? The French veterinarians considered it to be infectious.

Mr. Ferguson begged to state, that this notion had been exploded among the most enlightened French professors for these thirty or forty years.

The President.—It was taught by our first professor, who was a Frenchman.

Mr. Ferguson.—No French veterinarian of any note now believes anything of the kind. The matter of grease will produce other maladies by inoculation. It will produce farcy, but not grease. He had put this to the test of experiment.

The President had done the same; but it used to be a common opinion. The sloughing heel, however, is distinct from grease. Cracked and ulcerated heels have been confounded with grease, but they are very different.

Mr. Ferguson.—Perhaps the author of the essay will explain, why horses with white legs are more subject to grease than others.

Mr. Rushworth.—We are taught that the circulation in such parts is weaker.

Mr. Ferguson.—Certain it is that they are more subject to grease than horses with black or bay legs, but not at all from weakness in the circulatory vessels. He was more inclined to adopt the opinion of a friend and pupil of Mr. Morton's. This gentleman believed that a white leg parts much less readily with its heat than one of a darker colour. This may be accounted for by the phenomena that present themselves during experiments made for ascertaining how far the degree of heat radiated from bodies is influenced by the colour of their surfaces. The results of such experiments have proved that black substances radiate a much greater quantity of caloric in a given time than those of a lighter colour. This being the case, a white leg affected with some degree of inflammation must, as a matter of course, retain a greater quantity of the heat caused by the inflammatory action than were it of any other colour. The heat, by its accumulation, becomes a cause of increasing the degree of inflammation; and, if proper treatment is not had recourse to, grease makes its appearance. When the affected leg is black, the result is quite different: here the power of radiation is sufficiently great to carry off the superabundant caloric; and the slight inflammation which, in a white leg, would terminate in grease, ends in resolution. We observe that animals inhabiting warm climates are much darker than those inhabiting cold ones. This may be well illustrated by a comparison between the complexion of the African negro, and that of those persons who are of northern extraction.

Mr. Morton stated, that the profession was indebted to Mr. M'Taggart, of Liverpool, for the first suggestion of the opinion, that the disposition to grease was much influenced by the difference in power of radiating caloric in the black and the white leg. The idea, he believed, suddenly struck Mr. M'Taggart as he was listening to one of his lectures on the radiation of heat. It was an ingenious and happy application of a philosophical principle, and he confessed himself a convert to his friend's views.

The President was anxious to come at the fact, whether white-legged horses were more disposed to grease than others. His father, he perfectly remembered, would have nothing but grey horses on his farm. He has as many as twelve or sixteen, and he took great pride in them. He does not recollect a single case of grease among them. Many coachmasters and many noblemen were partial to grey horses. Prince Leopold would drive nothing else. It would be easy to ascertain whether these horses were more subject to grease than others. One of our regiments consists entirely of grey horses. Are they more subject to grease than other divisions of our cavalry? There are some large establishments which are almost entirely composed of white and grey horses—as Newman's in Regent

Street, and Nelson's, at the Bull Inn, Aldgate. Is grease more prevalent there than elsewhere?

The Secretary had kept an account of the number of horses with grease that had been received at the Veterinary College, and their colours. He found that the white prevailed over the others in the proportion of three to two.

Mr. Ferguson.—The matter might be easily settled in another way. Put a blister of the same strength on a white leg and a black one: in his opinion there would be far more inflammation and swelling in the white leg than in the other.

Mr. Rushton was inclined to agree with those who considered white legs to be most commonly affected. In the establishment which he had managed, the white legs were certainly the first attacked.

Mr. Spooner was of opinion that there was a want of power in that portion of the integument that was covered with white hairs to withstand the inflammatory action, which a darker portion would readily throw off. This was not confined to the horse; he had often observed it in swine. When they were turned on the clover stubble in the harvest, he had often remarked that the white pigs, and the white portions of the parti-coloured ones, were covered with scabs and cracks, while the black pigs, or the black portions, were unaffected. The same agents were acting on both, but the black coat could withstand or repel the morbid influence under which the other succumbed. He also referred to the different radiating power of the one and the other. He would ask whether other persons had made the same observation. In the same litter of pigs, some were more subject to scurfy eruptions than others: they were those that were white, or had white shoulders.

Mr. Ferguson.—Hurtrel d'Arboval was of the same opinion as to the prevalence of these cutaneous affections in animals with different coloured hair; but he did not account for it in precisely the same manner.

During Mr. F.'s stay at the Royal Veterinary School of Alfort, he had frequent occasions of remarking the truth of Mr. Spooner's statement. A number of the British cattle which he had purchased for the French government were attacked with mange on their arrival at the school. Those that were red and dun were cured in a very short time; but it was many months before they could eradicate it from two light cream-coloured cows.

Mr. Simonds would beg to add his testimony to the truth of this observation. White pigs feeding on the barley stubbles will frequently be covered with cracks, while the black ones comparatively or altogether escape. It is so in pigs of the same litter. He had noticed it a thousand times. With regard to white and grey horses, he had no doubt on the subject; at least in those situations in which grease was most apt to abound. He did not look for it in the stables of the gentleman, or of the postmaster, or coach-proprietor on a large extent, for there was generally fair and good management. The stables of the country farmer were the places in which this disease most abounded. The white-legged Suffolk cart-horse was a too frequent victim.

It was the change of food which produces the eruptive disease in the pigs. It was only to turn them into a different pasture, to give them lighter food, and to have recourse to some simple unguent, and they presently got well: but if the farmer would not take warning—if he would still keep them on the barley stubble—the disease would become more virulent, and many, perhaps, would die.

Mr. Carter perfectly agreed in what had been stated; but he would beg to observe, that the number of bay and black horses with white legs far exceeded that of entire greys or whites; and it was to the legs of such horses

that principal reference should be made, and which would best illustrate the influence of colour on this disease.

Mr. Simonds.—A great number of grey horses were kept in his neighbourhood, and he acknowledged that their legs were not so subject to grease as the white legs of horses of other colours.

TUESDAY, MAY 22, 1888.

Mr. Spooner, Treasurer, in the chair.

The consideration of Mr. Rushton's thesis on Grease was resumed.

Mr. Spooner, the author of the Essay having in all stages of this disease advocated exercise, would ask, what good effects are derivable from it, and the ill consequences likely to follow a neglect of it?

Mr. Rushton.—Exercise accelerates the circulation of the blood through the legs, and thus causes a perceptible diminution of the swelling which exists in this affection of the skin of the heels; and it also assists the healing process. On the other hand, when this is withheld, and the animal allowed to stand in the stable, a greater deposition takes place into the cellular tissue, causing an oedematous state of the limb, and the cure is much protracted. At any rate, his experience had taught him that such is the almost invariable consequence.

Mr. Carter.—It is a practice adopted by some, and sanctioned by this establishment, to remove the shoes and lower the heels: would Mr. Rushton advise it?

Mr. Rushton not only would not advise it, but he deprecated the act, conceiving that it would be attended with evil, since it would place in a state of tension an inflamed and ulcerated surface.

Mr. Robinson believed this to have been designated a constitutional disease. Under what circumstances is it such? Is it hereditary?

Mr. Rushton.—In the first stages it may be a purely local affection; but if allowed to exist long, the constitution sympathises, and calls for the employment of constitutional remedies, in order to effect its eradication; of which class are alteratives, combined occasionally with a laxative, and at other times with a diuretic agent. He could not say that he considered the disease hereditary, although colts may be disposed, from peculiarity of conformation derived from the sire or dam, to take it on.

Mr. Spooner.—In the aggravated stage of this disease, when the discharge from the flexion of the heel has become ichorous and offensive, and the hairs stand erect, it is no uncommon thing to see canker supervene. Is this the effect of an extension of the inflammatory action from the integument to the sensitive parts of the foot, or does it arise from the acidity of the discharge first decomposing the horny covering, and then setting up inflammation in the vascular sole and frog? He would also ask Mr. Rushton what would be his after-treatment, when the semicorneous excrescences, commonly called grapes, had been extirpated?

Mr. Rushton.—My subsequent treatment would not be very dissimilar to that recommended in a case of ordinary grease. The actual canter, with which they are removed, will induce a new and perhaps favourable action in the vessels of the skin, and which may be promoted by the use of poultices.

As soon as this is obtained, astringent remedies may be had recourse to, and a revulsion of the inflammation attempted, by the employment of setons in the thighs; at the same time the system should be supported by a generous diet. This more particularly should be the case if the animal is low and emaciated.

As it respects canker following this disease—this probably arises, as stated by Mr. Spooner, in the extension of the inflammatory action arising from continuity of tissue.

The Secretary.—In what form have you employed the chlorinated lime?

Mr. Rushton.—When grease is confirmed, and the discharge highly fetid, he had first washed the parts with it in solution, and then applied a poultice in which some of the solution was introduced. Occasionally the ointment had been used by him, in the proportions of one part of chlorinated lime to eight parts of lard; but although this had effectually destroyed the fetor by the known affinity existing between chlorine and the hydrogen of the disengaged compound gases, he had been necessitated to have recourse to some more active astringent before he could effect a cure.

Mr. Kingsley.—In what way does the effluvia existing in ill-ventilated stables produce this malady? Does the system become debilitated by it, or has it merely a local influence? He dissented from the opinion entertained, that this is a constitutional disease, in the general acceptance of the term.

Mr. Rushton did not think that the gases given off from accumulated dung and urine, noxious as they are known to be, would of themselves generate this affection; therefore he should be inclined to look to some one of them as an excitant to parts already predisposed by the debilitating effects of cold and moisture; and this one would be ammonia, which we know to be freely generated during the decomposition of animal excretions. As he had before said, it is only when the disease has existed for some time that it can be said to assume a constitutional form.

Mr. Ferguson.—It seems that Mr. Rushton has neglected to mention in his essay, that there are some diseases to which the horse is subject which, by improper treatment, are very likely to terminate in grease. It should not be forgotten by the veterinarian, that in a great number, indeed he might say the majority of cases, the affection called grease is preceded by oedema. This latter malady is, in general, one of the effects produced by long functional derangement in the system, especially of the secretory organs. In this case grease is the termination of another disease, and may with great propriety be called constitutional.

We, however, have it frequently produced as an affection purely local. Any inflammatory action, no matter what may be its cause, set up in the cutaneous tissue of the heels, may, in time, resolve itself into the disease in question.

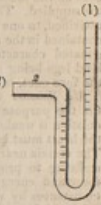
The author of the essay has said but little on the terminations of grease. Sometimes a small quantity of the matter discharged finds entrance into one or more of the adjacent lymphatics. When this happens, the vessel and its surrounding cellular tissue become inflamed and tumefied; thus forming what is commonly denominated a farcy cord. This cord continues to extend; buttons make their appearance in its track; and the animal becomes affected with confirmed farcy, sometimes in its most acute form. There is another termination of grease, of which Mr. Rushton has not taken the least notice, namely, gangrene; and this, if the disease is allowed to run an uninterrupted course, is not of such unusual occurrence as the inexperienced may imagine.

The author of the essay has given it as his decided opinion, that the reason why the hind legs are more subject to this disease than the fore ones, is, that the arterial circulation is much more feeble in the former than in the latter.

He also states, that the force and rate of the blood's motion in the arteries diminishes in proportion as their distance from the heart increases. The experiments of M. Poiseuille, and those of my own,

prove, in a most satisfactory way, that there is no truth in this statement. Take a bent glass tube, that has its arms parallel to each other (1); turn the extremity of one of the arms, so as to have a horizontal position (2). Pour some mercury into this tube, and you will find that its level is exactly the same in both branches. Having put in the mercury, fill up the side that has got the horizontal portion with a solution of carbonate of potash (this prevents the blood from coagulating). The apparatus being thus prepared, if blood is made to flow from an artery into the horizontal portion of the tube, it will exert a pressure on the mercury equal to the force with which it was propelled while in the artery. The depth to which the mercury descends in one side of the instrument, added to the height to which it ascends in the other, gives the dimensions of the mercurial column necessary to counteract the force with which the blood is propelled. In calculating this, however, there must be subtracted from the column the weight of the blood which occupies the space of the mercury it has displaced. With this instrument and a knowledge of hydrostatics you may calculate, with mathematical precision, the force with which the blood is moved in the different arteries of the body. The results of such experiments prove that the force of the blood's motion is exactly the same in all the arteries, no matter what be their distance from the heart. If the diameter of the mercurial column corresponds with the diameter of the artery on which we experiment, we shall find that the metacarpal and metatarsal arteries raise their respective columns of blood to exactly the same height. This fact is sufficient to refute the generally received opinion, that the force which propels the blood through the arterial system diminishes in proportion as the distance increases from what some are pleased to call the "central pump of the circulation."

I wish it to be perfectly understood that, although I deny that the force which moves the blood in the arterial system of the hind extremities is less than that in the fore legs, I do not mean to deny that there is any difference whatsoever in the general circulation of these parts. Quite the contrary. There is a difference, and a very marked one; but it depends not in the degree of force with which the vital fluid is propelled through the arteries towards the capillaries. It rests in the venous system. The blood in its return to the heart from the posterior parts of the animal meets with much more impediment than it does when coming from the fore extremities. This arises from the situation of the posterior vena in the abdomen, where it is subject to the pressure of the viscera there contained; and also from its traversing the diaphragm, to which it is firmly attached, and by the movements of which it is continually displaced, and altered in calibre. This latter phenomenon, coupled with the fact, that horses that respire with difficulty increase the muscular exertion of the fore extremities in endeavouring to dilate the thorax to the fullest possible extent by standing with their fore legs far apart, and thus bringing into action the pectoral muscles, explains, in a very satisfactory way, why the fore legs of these animals, when they are labouring under the effects of acute pulmonary inflammation, are of so much higher a temperature than the hind ones. Where there is increased muscular exertion there is also an increased degree of heat, there being a greater quantity of blood sent to the part. I do not, however, consider that the dif-



ference of the predisposition between the hind and the fore legs to become affected with grease depends so much on the force of the circulation, as it does on the difference in the character of the blood with which these parts are supplied. The distance this fluid has to travel from the lungs, where it is revived, to one part, is much greater than to the other; therefore, the blood contained in the arteries of the posterior members is not of so highly an oxygenated character as that in the vessels of the fore extremities. But the hind legs being more exposed to the exciting causes of this disease than the fore ones, is sufficient to account why they are the oftener affected.

Mr. Spooner thought there was no need of mathematical calculations for the purpose of ascertaining the truth of the position, that the circulation is weaker in the hind legs than in the fore. In the vascular system, the heart must be viewed as the primum mobile of the circulatory apparatus. The vessels near to this pump are comparatively inert tubes, the *vis à tergo* sufficing to propel forwards their contents; but at a distance, where the force and energy of the current have been checked by many opposing circumstances by which it has become enfeebled, Nature has wisely endowed the conduits with a vital power in their parietes, thus giving a compensating principle, by which the circulation becomes synchronous throughout the body. If we look to some of the lower tribes of animals, we shall find corroboratives; thus, in the eel the length of its body seemed to call for some means by which the blood might be returned to the heart in a uniform stream, that there might be no interruptions to the transmission of the vital fluid throughout the frame; and here we find a second heart—a caudal heart, situated near the tail. Take, if you please, as still more confirmative, different parts of the same animal; for instance, the difference in the tone of the pulse at the submaxillary and the metatarsal or metacarpal arteries. Again, in the brain, placed, it must be allowed, near the centre of the circulatory system, here, lest the impetus should be too great, and interfere with the functions of this important organ, we find the vessels thrown into a tortuous form, and the same construction we see in the vessels of the testes; but there it is so for a different purpose. He begged to be understood as not speaking in condemnation of the application of mathematical principles; he allowed their weight, importance, and value, and rejoiced when they could be brought to bear in the elucidation of any vital phenomena; but, while he was content to love science for its own sake, yet there were, in his opinion, some things so palpable, that only the exertion of our common sense was called for to impress the mind with their truth. Reverting to the treatment of confirmed or virulent grease, and particularly after the removal of the excrescences, he said he had been informed that the late Mr. Moneyment, of Norwich, applied a blister with a beneficial effect. Had Mr. Rushton ever adopted this mode of treatment?

Mr. Rushton had not, nor could he for a moment think it good practice, since it is well known that blisters are often a cause of the malady, by inducing debility in the vessels of the skin; so that on the application of an excitant, inflammation is set up, which terminates in suppuration, the progress of which is characteristic, owing to the peculiar structure of the part.

Mr. Goodwyn approved of the use of blisters, conceiving that they would create a new action in the vessels, and thus hasten the cure.

Mr. Spooner believed that there was much force in this remark, for certainly a desirable object with us is to set up a new action overcoming the morbid action which already exists. A trifling circumstance appeared to him to have been passed unnoticed, and yet he could not in strict propriety designate it trifling,—it was the convulsive way in which the animal snatches up his leg when cracks in the skin exist. This is the case often, while as yet there is

but little to be seen externally, and the inexperienced may be deceived by it; but on a careful inspection small fissures in the integument will be found.

Certificates of membership were awarded to Mr. Barth and Mr. Rushton, and of fellowship to the last named. The meeting then adjourned.

MAY 29, 1838.

Mr. Halliday, V.S. of Hitchin, and Mr. Musgrave, a Student, were elected members of the Association.

The President produced a beautiful specimen of enlarged plexus choroides in one of the ventricles of the brain of a horse. It had been sent to him by Mr. N. Leigh, of Bristol. It was fully the size of a pullet's egg, and completely occupied the ventricle in which it was found. It had a somewhat flattened shape, and had adapted itself to the form of the ventricle. Not the slightest cerebral affection had been observed. The horse had been employed in slow draught work.

He recollected another horse that, after having been under treatment a considerable time, was destroyed on account of vertigo. The plexus choroides was found enlarged on one side. There were no violent symptoms about this horse, but he was subject to attacks of stupor when overworked or overfed, or in very hot weather. This species of vertigo is not at all uncommon among horses. Water in the ventricles would produce a similar effect.

Mr. Youatt referred to the opinion commonly received among medical men, that when the enlargement was greater in one ventricle than the other, there was usually paralysis of the opposite side of the frame. This was contrary to his experience in the horse. He had rarely seen much difference of size in the two plexus, and when one had attained a greater size than the other, there had not been any nervous affection, or increased or diminished power on one side more than on the other.

Mr. Ferguson.—The disease in question is decidedly one of the most insidious with which the veterinarian has to combat. An enlarged choroid plexus may exist for an indefinite length of time, without causing any apparent derangement in the cerebral system; and even when an animal does become affected with the disease, of which an enlarged plexus is sometimes, though not often, the cause, namely, phrenitis, it is impossible for the practitioner to discover any peculiarity of symptoms that would enable him to diagnose, with any degree of certainty, whether there existed only simple meningitis, or congestion and pressure arising from an enlarged and encysted state of the viscera contained in the cerebral ventricles, or both. This, however, is of but trifling consequence, both diseases requiring precisely the same mode of treatment. It is not by any means infrequent to meet with the plexus choroides of animals that have died of other diseases,

and that did not, at any period of their lives, evince the least cerebral indisposition, in the same, or even in a worse state than the specimen which the President had just shewn. He had frequently met with this alteration of structure in the brains of horses that had been sacrificed either for experiment or anatomical research; yet these animals did not appear to have been in the least incommoded by it previous to their death. In some instances the plexus of one side was as large as an hen's egg; and had, from its great pressure, caused the absorption of the corpus striatum and hippocampus of the ventricle to which it belonged. Sometimes the enlarged plexus, having forced itself through the septum lucidum, occupied a portion of the ventricle on the other side.

All enlarged plexus choroides do not present the same anatomical, physical, or chemical characters. Sometimes they are of a semi-cartilaginous nature, and extremely hard; sometimes they are even bony. This latter state is, however, very rare, the cyst being, in most instances, an assemblage of small, yellowish, semitransparent, pisiform bodies, each about the size of a pin's head, and enclosed in a perfect capsule; the whole being invested by a membrane, which appears to be nothing more than the pia mater distended, and in some degree thickened. These cysts yield, on analysis, albuminous matter in abundance; a small portion of fatty substance of a whitish yellow colour, and a considerable quantity of phosphates of lime and magnesia. These enlargements vary much in size in different subjects. They may be met with from the size of a small pea to that of a large hen's egg. They also vary in the same animal, it being very seldom that the plexus of both lateral ventricles, when they are abnormal, are of the same dimensions; on the contrary, the cyst of one side may frequently be found ten times larger than that of the other. There are none of our domestic animals so subject to this disease as the monodactyles, the horse in particular. From the fact of his having so frequently observed an encysted state of the plexus choroides in the brains of horses that had never evinced during their lifetime any symptoms that indicated such a change of structure, he was inclined to regard it more as a predisposing than an exciting cause of phrenitis. The following case would shew the extreme difficulty, if not the impossibility, of distinguishing this disease from meningitis.

On the 14th of June, 1837, he was called to attend a brown horse, eight years old, the property of an English nobleman then residing at the Hotel Meurice, Paris. This animal was about half bred, and very remarkable for the symmetry of its proportions and the brilliancy of its action. Its owner assured him that, until about a week previous to his consulting him, it was with great difficulty he could prevent his favourite from running away whenever he was driving him. In fact, once launched, there was no controlling the pace. But that having been driven to and back from Versailles in the same day, on the following morning, when called upon to perform his usual work, he was found to be extremely sluggish, and to pay little or no attention to the blows inflicted on him with the whip. His owner, thinking that this sluggishness was merely the effect of fatigue, resulting from the animal having been overdriven on the previous day, did not press him, but ordered him to be put back into the stable, and to have a couple of days' rest, at the expiration of which time he was again tried in the cabriolet, but was found to be even more sluggish than ever. The groom finding the horse off his feed, asked the advice of a farrier, who bled the animal, and gave him some tartar emetic in his water. He continued under this man's treatment until the morning of the 14th, when, on the groom's entering the stable, he found that the patient had broken his halter in the night, and was at that moment in a corner, sweating from every pore, his hind legs extended, and his head thrust against the wall,

just as if he were trying to force a passage through it. With difficulty they removed him from this situation, and replaced him in his stall. The owner seeing his favourite in such a state, sent for him. He arrived at twelve o'clock.

He found his patient in a cold sweat—his fore feet in the manger—his head thrust against the wall, for he had forced it through the rack—his breathing very quick, and his eyes perfectly amaurotic. He could detect no pulse on placing his finger on the maxillary or any other artery; but the heart could be felt beating with violence—a violence that precluded the possibility of his counting the number of beats per minute; they must have been considerably more than a hundred. He did not shew any symptoms of vice, his only object appearing to be to force a passage through the wall which resisted his progress forwards. He bled him from the left jugular vein, and considered that, from the dimensions of the stream and the time he allowed it to run, for the situation was too dangerous to hold a vessel, he must have lost at least half an ordinary sized stable-pailful of blood. This bleeding did not in the least abate the urgency of the symptoms. In about ten minutes after, he became so violent that no person could approach him except at the imminent risk of their life. Seeing the deplorable state in which he was, he determined to give him another chance by opening the temporal artery, which he did after some difficulty; but the horse did not bleed as freely as he had expected. At this period the proprietor determined to have him destroyed if he thought there was no chance of his recovery. He at once told him that there was little or no chance of his recovery, but at the same time requested that he might be permitted to live until the disease should carry him off. He had scarcely uttered these words when the animal threw himself back, struggled for a few moments, and died; thus saving us the trouble of becoming his executioners.

Sectio cadaveris.—The viscera of the thorax did not present any abnormal appearance, excepting that the lungs seemed to be very slightly inflamed; which was not to be wondered at, when the extent of the increased arterial action, which must have existed some hours previous to death, was taken into consideration.

All the viscera of the abdomen were in a perfectly healthy state. On opening the cranium and spinal canal, nothing was seen that could in any way account for loss of life accompanied with such violent symptoms, excepting an extremely slight injection of the vascular ramifications which are observed on the pia mater even in the most healthy subjects. Not finding any thing particular in the appearance of the meninges, he proceeded to expose the lateral ventricles. His scalpel had scarcely entered the substance of the cerebrum, when its progress was impeded by something of considerable hardness. He withdrew the knife, and commenced dissecting with care the cortical and medullary substance from around this body, which he imagined occupied the right ventricle. His dissection finished, he was not a little surprised to find, that the substance which had so firmly resisted his scalpel, and which was fully as large as an ordinary sized egg, was neither more nor less than the plexus choroides, which had become a cyst of extremely hard semitransparent yellow tubercles, having a great quantity of lymph interposed between them. This cyst was covered with a serous membrane, under which might be seen several minute vessels that eventually terminated in the vena Galeni. This latter vessel was very turgid. So great was the pressure resulting from the size of this cyst, that the septum lucidum was ruptured, allowing a portion of this abnormal growth to extend itself into the left lateral ventricle. The right corpus striatum was entirely absorbed, as was also a proportion of the hippocampus of the same side. The left

plexus choroides was also enlarged; but its size did not exceed that of a sparrow's egg. The lining membrane of the right ventricle was slightly inflamed. The olfactory sinuses were filled with a fluid of a pale straw colour.

An encysted state of the plexus choroides is an alteration of structure very rarely met with in the human subject. He had a case of it during his residence at Charenton. It was a quarry man, named Deveaux, aged 26, and of a very muscular temperament. This man had been subject to severe headaches ever since the age of eighteen, especially during the summer months. One morning, after having passed the preceding night in dancing and drinking, he awoke with a violent headache, and sense of pressure on the eyeballs. This continued to increase until the next day, when he became comatose. He remained in this state two days. On the fifth from the commencement of the attack he was seized with convulsions. He was then sent for; but in despite of all the treatment that either himself or his two colleagues, whom he met in consultation, could suggest, the unhappy sufferer expired that night. They opened the body twelve hours after death. All the thoracic and abdominal viscera were in a healthy state. The vascularity of the pia mater was greater than usual. The lateral ventricles contained a pale straw-coloured fluid. Both plexus choroides were enlarged, and presented the appearance of a cyst of small yellow semi-transparent tubercles, such as those sometimes met with in the choroid plexus of horses. The left plexus was much larger than the right, it being about the size of a gage-plum. The parietes of the ventricles were softened. The hippocampi and corpora striata partially absorbed. The spinal marrow was normal.

Here was a case where an encysted plexus choroides seemed to have been the cause of frequent headache. Are we justified in saying that the same indisposition does not occur in the horse, and from the same cause? No, certainly not. His being dumb, and our want of perception sufficiently acute to detect the symptoms of so slight a derangement in the cerebral system, are no reasons why he should be exempt from such a disease. Here, too, is one of the many proofs that the science of veterinary medicine is enveloped in much greater difficulties than the unreflecting human practitioner may imagine. Diagnosis is one of the foundation-stones of medical science; and who will be bold enough to contend that it is not a thousand-fold more difficult in veterinary than in human practice?

The President recollects the case of a hard-working horse in whose brain the plexus choroides were found very much enlarged. There was nothing about him that could possibly have indicated the existence of these lesions. Some chronic affection of the meninges, or rather some predisposition to inflammation, might be set up, and then, the horse being overfed, or overworked, or unusually excited, phrenitis would appear. With regard to the affection of one side of the body only, he thought that there might be something in this. He had seen tetanus confined to one side, and connected with injury of the spinal cord and the cerebellum. Another horse that had been injured on shipboard had a tetanic rigidity of one side of the head and neck. He had been kept on the deck during a long passage, exposed to the wind and the rain. There was no invariable analogy between the indications and associations of disease in the human being and the brute. A frequent and seldom deceitful indication of diseased liver in the human being was pain in the shoulder; but in horses with liver in the highest state of disease there has never been lameness, or alteration of gait indicative of this pain. The disease proceeds until the liver is ultimately found in the highest state of congestion; but there has been no premonitory lameness.

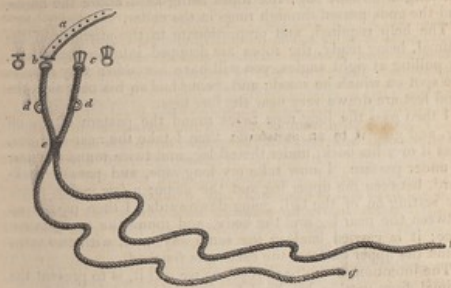
Mr. Youatt asked whether these enlargements of the plexus proceeded

from a distended and varicose state of the vessels, or from the deposition of calcareous or other matter on the meninges?

The President was not aware of any altered or varicose state of the vessels. There seemed to be a deposition of phosphate of lime on the membranes. The tumour consisted of an aggregation of minute depositions of this kind. He had seen similar tumours that required the aid of a fine saw in order to divide them.

Mr. Ferguson had seen the plexus of the fourth ventricle affected in the same way, but to a very slight degree. In such cases the plexus of the lateral ventricles were very much enlarged.

The conversation on this subject then terminated, and the remainder of the time was occupied in the examination of Mr. Carter's Improved Hobbles.



- a. A strap of stout leather, two feet long, one and a half inch broad, and of double thickness.
 - b. A T ring of iron, one quarter inch thick. The T end being made fast in the double leather through a hole cut in it, the ring receives the rope through it, about four inches. The end untwisted is passed through the same rope again and again, and made perfectly secure. A piece of leather should be sewn over it, to prevent the ends from coming out.
 - c. A buckle, one quarter inch thick, with a ring to receive the other end of the rope, and made fast in the same way.
 - d, d. Two rings of iron, about one and a quarter inch in diameter on the inside; the outer part made hollow to receive the rope. When a ring is intended to be put in, untwist the rope a little; pass the end through; insert the ring, and draw it tight. A piece of leather is then put through the ring, and round the double rope, and made fast.
 - e. The lower part of the collar made by passing the ends through each rope, and a strong piece of leather, two inches broad, made fast round it.
- The rope should be made of the best hemp, about two inches in circumference; from c to f nine and a half yards long; from b to g seven and a half yards. Supposing that the strap is buckled in the first hole, the collar should be about two and a half feet deep. The rings may be inserted about midway between b, c, and e.

Mr. Carter then gave the following account of his manner of proceeding:—

Supposing the colt in hand and ready for casting, I, standing on the off-side, commence by taking hold of the collar, an assistant taking the other part of the rope. If the colt is quiet and will allow me, I pass it between his hind legs, then between his fore ones, and with my right hand place the strap under and round the neck, and buckle it on the off side. Many colts will not allow themselves to be roped in this way: then I am obliged to pass the collar over the head with a pole. When the collar is on his neck, and the ropes between his fore and hind legs, my assistant takes the short or near-side rope, while I have hold of the long or off-side one; the ropes being kept above the hocks, and the ends passed through rings in the collar.

The help required, and proportionate to the strength of the animal, being ready, the ropes are dropped into his heels, and, by pulling at right angles, you will have him down very near to the spot on which he stood; and, being laid on his near side, the hind feet are drawn very near the fore legs.

I then pass the long rope twice round the pastern of the off leg, and give it to an assistant; then I take the near-side one, pass it over his back, under the off leg, and twice round the near or under pastern. I now take my long rope, and pass it backward, between the upper leg and the ileum, a few inches above the setting on of the tail, going downwards; I then pass it up between the near leg and the body, and round the near pastern once: it is carried back in the same way, and, with two turns round the upper pastern, the casting is finished.

The intention of this back rope, as we call it, is to prevent the animal from getting his hind legs straight, and it has a great tendency to keep him quiet during the operation.

In letting him up, standing at the back of his hind legs, I commence by slipping the back ropes off his tail, and taking them off his legs. When they are all off except one in each heel, I order the strap to be undone. If he still lies quiet, I go to the front of the fore legs, and loose the near leg by drawing the rope from under his body. It is always better to take this precaution if he will allow you, as he will rise better than if the rope was left on it.

Mr. Carter now shewed, on the skeleton of the horse, the manner of using the hobbles; and a vote of thanks being awarded to him, the meeting separated.

TUESDAY, JUNE 12, 1838.

The Association met, pursuant to adjournment over the Whitsuntide holidays. Mr. Spooner in the Chair.

Mr. Wardle, of East Sheem, practitioner, and Mr. Hinge and Mr. Briggs, students, were elected members.

Certificates of membership were awarded to Messrs. E. Sewell, J. Green, and T. Richardson.

The following Essay, by Mr. Gibson, on "Phlebotomy—the Modus Operandi—occasional untoward Results from the Operation, and a few remedial Suggestions," was then read.

Mr. President and Gentlemen, I hail with delight the success which has hitherto attended the exertions of those noble-minded individuals, sanguine in the cause of science, who were the founders and are the supporters of this truly useful Association. If by any means the veterinary art can be exalted to the dignity of a science, it will be by the union of the staid and experienced practitioner with the zealous but younger inquirer after truth. There are some who are so illiberal as to deny that any instruction is to be derived from the perusal of our debates; but I have seen, and I have felt, how often the sentiments here expressed have been a stimulus to research, and that research has been the production of the development of some important truth. Hence knowledge has been imperceptibly accumulating, and the Association is becoming the storehouse of the whole body—the deposit of all useful truth; although, at present, I am persuaded, presenting merely the germ of its future greatness.

There has hitherto been a lamentable disunion among veterinary practitioners—a mutual distrust and jealousy—an empirical secrecy—direful clogs to the advancement of our art, but which our Association is well calculated to remove; for there can be few so selfish and so sordid who, when they benefit by the experience of other practitioners, here generously divulged for the public good, can remain silent if it is in their power to promote the common cause. I trust that I shall not be charged with presumption in making these introductory remarks. The profession is dear to me, and every thing tending to its advancement is a subject of deep interest.

The operation of VENESECTION is the one which the practitioner is most frequently called on to perform. Although it has been often unnecessarily resorted to, and injudiciously practised, it forms the basis of our therapeutic means, on account of the inflammatory disposition of the greater part of our patients.

Simple as the operation is, it is of great importance that it should be performed with tact, precision, and neatness. The young practitioner, while executing it, will often feel himself under the scrutinizing eye of scarcely-formed and wavering confidence; and nothing is more calculated to make a favourable impression on the mind of his employer than the methodical and scientific abstraction of blood.

Many of the absurd, inefficient, and even destructive intended-to-be remedies of our ancestors are now abandoned. Several of the instruments of the olden times have fallen into disuse; yet we continue to see in the modern pharmacy, although "curtailed of their fair proportions," some of the instruments of a barbarous age. The farrier occasionally shoulders his soul-destroying buttress; and even the veterinary surgeon often cudgels the fleam into the neck of the horse. Many instruments have been abandoned, because others more appropriate and more consistent with science and humanity have been invented. Why should the fleam still maintain its ground, when we now have an instrument not only more comely in its appearance, but decidedly more useful—the lancet?

Several authors who have written on veterinary subjects have made mention of the lancet; yet they appear to treat it with a kind of indifference, as if not familiarized to its use. They acknowledge that they employ it, but they do not in plain language recommend it. Not one of them is bold enough to assert that it supersedes the fleam. Mr. Blaine says, "The lancet has the most surgical appearance, but it requires much more skill and dexterity to become master of it." That very difficulty should excite our ambition to become master of it. Stablemen and butchers use a fleam as well as we can, but few of them have yet dared to venture on the lancet. As professional men, we ought to take a higher stand than they dare venture to do.

"I always myself," says Mr. Blaine, in a note, "bleed with an abscess lancet." The kind of lancet, to which I beg to draw your attention, is no broader than one employed for the human being; but it is considerably stronger, and it has a *file* at its base. This is a very important addition. It is occasionally difficult to retain a smooth polished body, like this, within the grasp. I have, more than once, seen a ludicrous accident, with the smooth-based lancet. A young man has been attempting to bleed a horse, and the lancet has slipped from his grasp: he is confounded, and directs his eyes to the ground in search of it, when he is brought to his recollection by the groom, who tells him that it is in the horse's neck. The simple human lancet will not answer when the thick skin of the horse is concerned.

I have tried the abscess lancet of Mr. Blaine, but I do not like it so well as the straight one; it is an awkward instrument to use, and apt to form notches in the skin.

The lancet employed at the Veterinary College, and by veterinary surgeons in general, is an uncouth, clumsy-looking thing, and unnecessarily large. The object of its broad shoulders is to ensure a sufficiently large orifice; it being forgotten how easily and beautifully this may be done when the instrument is retracted. It is often too dull, and its great strength increases the tendency to this. The sharpness of the point and the edge is the most important of all considerations in the use of a lancet. The point of every lancet is apt to be turned by the hair, particularly when it is coarse and abundant: the point should, therefore, be carefully insinuated into the coat, and brought in contact with the skin before the thrust is made; or, if the patient is irritable, the hair should be wetted and divided with the finger.

The size of the orifice is not dependent on the breadth of the lancet. A skilful surgeon will make as large an orifice with a small as with a large one: then the smaller one will be most useful to the veterinary surgeon, whose patients are of such varied size. My father has used the small lancet during a practice of thirty years, and was often complimented on the neatness and dexterity with which he performed the operation of bleeding.

"The principal view in the withdrawal of blood is to lessen its quantity," says Boardman. We have now, however, been taught that the sanguiferous is under the controul of the nervous system; and, therefore, our object is not only or mainly to diminish the quantity of blood, but to make a peculiar impression on the sensorium; and this is sooner and more effectually done by withdrawing the blood rapidly through a large orifice, than slowly through a smaller one. A variety of circumstances will demand that the punctures shall be variously proportioned, and that instrument which can be made most subservient to the wishes of the practitioner and the demands of the case, will be the most valuable. The grand disadvantage of a fleam is, that we can only make an orifice of a definite size, and that we must always have with us instruments of various widths in order to accomplish different purposes. In proportion, however, to the width of the star or lancet part of the fleam, the depth of it is also increased, which necessarily makes it a somewhat dangerous weapon. It is plainly so when we have to make a large orifice in a thin-skinned or small animal. A case once occurred to me which proved the decided superiority of the lancet over the fleam. I was called some distance into the country to a valuable cart-stallion, immensely fat, and labouring under pneumonia. Before

my arrival the groom had been endeavouring to bleed, but although he had plentifully studded the animal's neck with incisions, he had not obtained more than a few drops of blood. I was aware of the necessity of free bloodletting; but I found that the vein was so deeply imbedded in fat, that neither his fleam nor my lancet would touch it: I therefore dilated one of the orifices that had been made, and then cut through the superficial adipose matter, and thus effected a free opening into the vessel. The horse recovered; but he would have been inevitably lost had there been no other instrument but the fleam to have had recourse to.

Mr. Blaine, in his very able work, recommends the lancet for opening the small veins, and for bleeding from the palate or from the saphena vein, which he says may be punctured with the fleam, but with much greater safety and propriety with a lancet. The superficial brachial vein should be opened with a lancet. Arteriotomy should be performed with this instrument. He even recommends the lancet in preference to the scarificator for dividing the vessels of the conjunctiva. In fact, the lancet is advocated wherever tact and skill are required: but as soon as he comes to the jugular vein, the easiest to operate upon in the whole body, the fleam is introduced. I do not mean to say that the use of the lancet is deprecated, but the author seems to be unwilling to discard the fleam. I am disposed to think that a fleam should be now as seldom seen in the hands of a veterinary surgeon as a buttress in those of a modern farrier.

Mr. Blaine is of opinion that it is prudent to avoid puncturing directly over a former bleeding-place. My father, however, has been accustomed to select those places, unless they have been recently wounded, or are materially thickened by former mismanagement or inflammation. The vein is here generally varicose to a greater or less degree; it is more intimately connected with the integument, and the interposed interstitial substance is of a denser character, consequently there is less danger of extravasation. The dilated state of the vein admits of the lancet being introduced more freely for the purpose of making a large orifice with less danger of wounding the opposite side of the vein. We may also conclude that there is no valve there, from the operation having previously succeeded, or, if there should be one, a division of it will be of less consequence in an expanded than in a more contracted part.

The requisite quantity of blood being withdrawn, the securing of the vessel is of equal importance with its section, and carelessness with regard to this is sometimes productive of very serious consequences.

While the patient is bleeding, the operator should prepare his tow and pin. The tow should not be twisted too tightly. When somewhat loose, it best adapts itself to the surface to which it is applied. It should not be more than four inches in length; for if it is too long the application of it will be tedious, and the parts are pulled unnecessarily about. The pin should be short, small, and sharp.

Blood having been withdrawn to the requisite amount, the blood-can should be gradually removed, and the wound gently compressed with the fingers; this will often stop the hemorrhage and render the pinning-up less troublesome. Then by means of the thumb and fore-finger of the left hand the lips of the wound are gradually approximated, and, their edges being accurately brought together, and any hairs that may have found their way into the incision being withdrawn, the pin is inserted with the right hand. It is usual to bite the extremity of the pin, and there is much propriety in this—it passes more easily through the integuments, and makes a larger wound, and consequently the body of the pin presses less on the parts immediately in contact with it, and there is less pain and inflammation.

The tow is then wrapped round the pin, crossed in the figure of 8, and the superfluous part of the pin cut off.

Perhaps this termination of the bleeding might be performed with a little more elegance and despatch than are sometimes displayed. Some practitioners are very properly furnished with a short pair of scissors with a notch in one of the blades; others use any scissors they can get hold of; while some waste much time, and often do much mischief, by sawing off the extremities of the pin; and a few carelessly leave both extremities untouched. The best instrument with which to remove the projections is a small pair of nippers with round or blunted points.

It is a common practice to tie a noose in the tow, and then to pass it over the pin and draw it tight. This is a bad plan; for it is almost impossible to tie it without some degree of puckering, and the edges of the incision being pressed outwards, and considerable pain and inflammation occasionally ensuing.

Some practitioners endeavour to dispense with this pinning-up; but dangerous and fatal consequences have then ensued from a return of the hemorrhage. Many withdraw the pin on the day following the operation. This I hold to be bad practice; for the lips of the wound are not always perfectly united, and hemorrhage, or inflammation, and great swelling, have ensued. I never saw any objection to leaving the pin in until the third day.

Among the unwelcome sequelæ of venesection, tumefaction of

the integument and cellular tissue in the immediate neighbourhood of the puncture most commonly occurs.

Fomentations with warm water, or bathing with cold salt and water, will often be serviceable in such cases. The head should be tied up to the rack, and the horse should not be put to draught work until every unpleasant symptom has disappeared.

In cases of extravasation, however extensive, I would not remove the effused blood until sufficient time has been allowed for the opening in the vein to close. I would then extract the pin, and enlarge the orifice in proportion to the size of the thrombus, and carefully dislodge the coagulum. I should not wash or foment the wound for the purpose of cleansing it; for the remaining blood will form a protection from the action of the air on the vein and the incised surface generally. If, after the clot has been removed, fresh blood is observed to accumulate, it may be considered that the incision in the vein is not perfectly closed. A little pad of fine tow must, in such case, be inserted into the wound, to act as a compress on the vessel; and the lips of the external wound should be drawn together by suture, which may be removed on the following day, or the day after that: the tent will then be ejected as the part heals. This is much better than allowing an abscess to form, in which the vein is more liable to be implicated.

Phlebitis, or inflammation of the vein, is not always or often to be attributed to any fault of the bleeder. It seems to depend on a species of idiosyncrasy. Why the inflammation, whether it exists in the lining membrane of the vein, or any one of its coats, or affects the whole substance of the vessel, or is confined to its cellular envelope, should spread upwards toward the head, has never been satisfactorily accounted for. There are many cases in which the vein will be found pervious, yet inflammation extends up the vessel, contrary to the course of the circulation.

Some of the worst lesions consequent on bloodletting have been increased, if not entirely occasioned, by the want of a little care and attention in the first instance, and when the derangement was confined to the tegumentary tissue and cellular substance.

The usual treatment of inflamed vein consists in repeated blisters, and by which I have seen very excellent effects produced: but when it is considered that, possibly, the reputation of the practitioner is very unfairly at stake, and that there will be too much dissatisfaction on the part of our employers if the animal is long kept out of work, or much blemished, we are anxious to discover other and equally efficacious modes of cure.

If the disease is in its incipient state, we have been accus-

tomed to apply fomentations as hot as they can be borne without scalding, and to persevere incessantly in the application of them throughout the night. Intermittent fomentations will rarely be of service. If, however, they are resorted to, the surface to which they have been applied should, as soon as they are withdrawn, be rubbed with a stimulating embrocation.

If we do not succeed by fomentations, we employ strong stimulating liniments, or we blister.

Coagula have been extracted many inches in length from jugular veins, and the vessels have afterwards become efficient; but, of course, the mere taking out of the clot will be of little use unless the original obstruction to the progress of the blood is obliterated. I have thought in such cases, that there has been a partial division of a valve, which has become thickened, and so resistant from inflammation, as to offer a barrier to the passage of the blood. I imagine that a complete section of a valve is attended with less danger than if it is only partly or nearly divided; for, in the first instance, the valve would collapse, and come into contact with the parietes of the vein, and adhesion might ensue: in the other case this could not occur, for the action of the valve would be continued.

When a sinus, or numerous sinuses, appear, and there seems to be no hope of recovery, we should resort to the knife, and cautiously lay open the tumour through the whole extent of the diseased portion. By this operation we exterminate the vein, but we remove none of the ill effects that arise from its suspended function. We should, therefore, be slow in adopting this *dernier* resort, for the *vis medicatrix* of nature often effects wonders.

Mr. Brody, while he would offer his humble meed of praise to the author of the Essay, both on account of its practical nature and the labour evinced in comparing the advantages and disadvantages resulting from the employment of the two instruments for the performance of so common, nevertheless so important, an operation as bloodletting, could not but think that *Mr. Gibson* was not justified in discarding one which possessed much value, and unceremoniously treating, as nothing-worth, a tried and well-approved friend. The arguments adduced by him, however, were worthy of mature consideration.

It appeared that, while he so strongly recommended the use of the lancet in bleeding, he as forcibly deprecated the fleam; and the principal reason assigned by him was, that it was old-fashioned, and employed commonly by farriers and stablemen. Now, he could not but assent to the position, that the lancet was by far the more surgical instrument, and, in the hands of the well-taught and experienced veterinarian, was equally efficacious with the fleam; but did not circumstances now and then occur when the operation of depletion was of necessity entrusted to less skilful hands?

As it respected its being old-fashioned, he could not on that account see any valid reason for deprecating its use. Perhaps in the present day we were

too fond of novelty. This was certainly an age of improvement; but it did not of necessity follow that all that our forefathers did or said merits only our contempt. We should, perhaps, do but little were it not for what they had done. He maintained that these arguments were not sufficiently strong for our rejecting the fleam; although, at the same time, he was ready to confess that he invariably made use of the lancet.

He considered the fleam a good instrument, because it acted with precision, and was safe. The greatest objection that he could see to its employment was the use of its accompaniment—the bloodstick; but this, at the present day, had been in a great degree obviated, by giving a patent back, as it is called, to the fleam.

The lancet was, certainly, the more scientific and portable instrument; and it called for more tact and judgment in its use. It could also be resorted to in many cases when, from the position of the animal, or the nature of the disease under which he was labouring, the fleam could not be used; yet, for all that, he could not consent to its unqualified condemnation. It was old, and had served us well for many years; it had been tried and found highly useful; let us not, then, altogether reject it. Were he to offer a suggestion in the way of improvement of the lancet advocated by Mr. Gibson, he should say, that it should have a broad shoulder. Perhaps the one he now exhibited, and which he constantly used, might alarm many of the members from its size; but the greater portion of his practice was among the larger dray horses in London, and he was an advocate for the withdrawal of blood from a large orifice, by which we more quickly lessen inflammatory action, while, at the same time, the amount of blood withdrawn is not so great, and therefore the act is not attended with so much exhaustion of the physical powers. One thing in bleeding with the lancet should be carefully attended to. Let the integument and the vein be pierced simultaneously; for if the opening in the skin does not correspond with that in the vein, a thrombus will follow, or, perhaps, something worse.

Mr. Gibson explained. It was not merely on account of its being old-fashioned that he deprecated the use of the fleam, nor from its being the common instrument of farriers; but because it was one not generally or at all times available. Again, different sized blades are required, according to the aperture we are desirous of making in the vein, or according to the size of the animal. Now, with a lancet all this is unnecessary, for with that instrument we obtain orifices of any size, and all kinds of horses can be bled with it, from the pony to the dray-horse; while effusion into the cellular tissue is far less likely to supervene after the use of the lancet than the fleam, since we avoid contusing the part.

In bleeding with a lancet, the section of the vein is made by two movements: first, a thrust inwards, passing through the integument and the coats of the vein, which is effected by a motion of the fore-finger and the thumb; and then a cut outwards, caused by the motion of the wrist: sometimes this is unexpectedly, but equally and as effectually done by the animal's suddenly plunging in an opposite direction.

Mr. Beech could not approve of this method of using the lancet: he did not think that sufficient force could be exerted to penetrate the skin. For cart-horses he invariably preferred the fleam to the lancet.

Mr. Robinson.—In bleeding the human subject the operation was carried on as described by Mr. Gibson; nor could he see why the same should not obtain in the horse. For his own part, he had been in the habit of using the same lancet for the human being and the horse, making no distinction between the heavier and the lighter breeds.

ON THE
STUDY OF ETHNOLOGY,

BY
DR. ERNEST DIEFFENBACH.

READ AT A MEETING
PRELIMINARY TO THE FORMATION OF THE ETHNOLOGICAL SOCIETY,
Held at Dr. Hodgkin's, 9 Lower Brook Street, Grosvenor Square,
Jan. 31, 1843.

THE different races of which the Human Family is composed are distinguished from each other by certain anatomical and physiological marks; which, although they do not justify us in dividing mankind into classes, genera, and species, as we do with plants and animals, yet they are to a certain degree very durable and lasting, and, in the opinion of some very distinguished writers, even constant. If we take the civilised nations of Europe, Asia, and America, as an instance, we find everywhere a distinct type of formation and features—of physical, intellectual, and moral character—various religious ideas, and different forms of social life—peculiar sympathies and antipathies, enduring for long ages of national existence— manifold adversities, mixtures, and migrations. And what is the case in Europe, especially where we should imagine civilisation to have thrown down all barriers and extinguished all differences between nations, is still more the case with nations more widely different: and for this reason we are able to divide mankind into large families and subdivisions, and to establish a description of them, in as concise terms as the Botanist, when he examines the plants of a certain region of the globe. The study of this, the Natural History of Man, is what has been termed the Science of Ethnology.

This Country, above all others, seems to be qualified to

open a new era for this important science; and yet, although Societies exist for all possible branches of human knowledge, we have not yet established a Society to investigate the Natural History of Man. The sceptre of England sweeps the easternmost boundaries of Asia, and the westernmost of America. The Anglo-Saxon in the seas of China, approaching from the westward, gives his hand to the fur-merchant at the Western Coast of America, who arrived there from the eastward. England encircles the globe in glorious enterprise: her sons come in contact with the Esquimaux of the Arctic Seas, with the red hunter of Northern America, with the cunning Chinese, with the mild Hindoo, and with the mountaineer of Afghanistan. The same is the case in the Southern Hemisphere. New empires are springing up in New Zealand, New South-Wales, Van Diemen's Land, and at the Cape of Good Hope, where the Englishman has intercourse with man in the most opposite stages of development. No nation, therefore, has such opportunity for investigating the history of races:—for no nation is it so essential to be well acquainted with these races. Much material, it is true, is dispersed in the works of Navigators and Travellers: but a Society is wanting, for collecting, sifting, and extending such information—a focus for Travellers of all nations, where information regarding the human race may be received and given—a Society, which will not reject the minutest detail that may elucidate the history of the human race, and spread an interest in the subject among the different classes of the community.

It must be a main object with the Ethnological Society, to collect facts, from which we may arrive, by philosophical reasoning and induction, at results which ought to excite at least an equal interest with the history of animals and plants, because they bear so intimately upon the highest interests of human nature. We may expect from the Science of Ethnology not only important results as regards the origin and education of man, the causes of the varieties of the physical and mental development of nations, the origin and difference of languages and religious ideas, but we may be able to collect the colours of the prism, each of them rich and beautiful, into the pure ray of light, and confirm

by inductive science the cherished unity of mankind. We do not, however, yet possess sufficient materials to draw such results.

It is necessary first to fix the proper boundaries of Ethnology: which is the more desirable, as it is believed, by some, that the subject rather belongs to the Geographer, the Historian, and the Philologist; the pursuits of all of which are indeed very incomplete without a proper study of the Natural History of Man.

We may examine the races of men under two different points of view. 1st, we may glance at them as they present themselves at the present moment, contemporaneously: and, 2dly, we may not only trace back the nations to their sources, and from actual remains and historical survey give a true picture of the changes which their physical nature has undergone in the vicissitudes of time, but we may bring to light the nations which have disappeared. The latter may be termed the Historical part of Ethnology. The Natural History of Plants and Animals presents the same two points of view. The Naturalist is no longer satisfied with being acquainted with the different plants and animals as they exist at the present moment: many genera and species have existed in times gone by, and have been lost in the succession of ages: from their remains, imbedded in the crust of the globe, he obtains not only an insight into the changes which the latter has undergone, but the structural differences between extinct and existing species enable him to fill up any chasm which may appear in the natural system of the present day. He may restore the chain of that astonishing variety of creatures by means of fossil links, and humbly lift the veil from those great and harmonious laws which we see pervading the universe with every step that we advance in the knowledge of the animal and vegetable world.

It is sufficiently obvious that the inquirer into the physical history of man must combine both methods.

It must be the object of the Ethnological Society, therefore, to obtain an exact knowledge of any nation or tribe, as they exist at the present moment. Ethnology begins with Ethnography—with an authentic description of the physical

condition of each nation: and for this purpose it will be necessary to collect every thing that will throw light on this subject. It is not sufficient that authentic skulls should be collected of all races, or casts of such; but whole skeletons. We have sufficient materials to distinguish races from the form of the skull, as there are large collections in England and on the Continent; but we are almost ignorant of Comparative Human Osteology. We have no accurate data of the relative proportions between the abdomen and the chest, amongst the different races; of the number of vertebrae; of the shape and capacity of the pelvis; of the length and strength of the extremities. The muscles, the internal organs, and the organs of voice, have been still less thoroughly examined. The weight and structure of the brain of the European and Negro, it is true, have been compared by Tiedeman and Soemmering, and have been pronounced to be identical in both; but no notice has been taken of the other races; and the few researches which have been made do not entitle us to form an opinion, either on the identity of the human race, or on the influence of external circumstances on the structure of parts. It cannot be doubted, that marked differences exist in the average weight of the brain, and the development of its different parts; but further investigation is necessary, to shew that they are constant. Still less has been done in regard to numerous interesting points connected with the fecundity of different races; in comparing the muscular strength of races, their durability, longevity, national diseases, and a great many other subjects.—It suffices here merely to indicate the fields of investigation with which the Ethnological Society should be occupied: it will be the especial duty of the Council to promote the publication of interesting matter contained in the books of Travellers of different nations, and the illustration of their publications with accurately-coloured portraits of typical specimens.

It is well known, that many nations are in the habit of marking their faces, and other parts of their bodies, with certain figures or lines. This custom existed amongst the ancient nations of the East, and we find it in all parts of the globe. To trace this singular custom through all its

different degrees will perhaps be the means of identifying nations, though living at great distances from each other. Such an investigation will open an insight into the origin and real meaning of this custom, with which we are now all but unacquainted.

I may mention here a subject on which the labours of modern chemistry have thrown a new and most interesting light; viz. the food of the different races of men, or inhabitants of parts of the globe which are widely different from each other in their geographical and climatological relations. The substances which serve as food, or the quantity which is taken, appear, to the superficial observer, often of a most extraordinary nature, because they are apparently so heterogeneous from what we are accustomed to; so that Travellers relating such facts do not withhold their astonishment or reprobation. But it has been demonstrated, that the general use of certain articles—for instance, tea and coffee, betel, tobacco, and wine—depends upon the presence, in those substances, of elements which are often identical, and which are necessary to the maintenance of the animal economy, more or less, according to the presence or absence of other elements in the food, the different occupation, mode of living, and so on. The fact of the Esquimaux consuming large quantities of train-oil and blubber ceases to be astonishing, when we reflect that these highly-carbonized substances serve to furnish fuel to his increased respiration. In one word, it is necessary, in the present state of chemical and physiological science, to collect an analysis of all the substances which are consumed by a particular race, either as food or drink, or, by an habitual custom, as so called matters of luxury, or as medicines. It is also desirable that the average quantity consumed of all such articles should be ascertained. The Ethnological Society may have the great merit of working here hand in hand with chemists and physiologists, and fill up, in that manner, a most important chapter in the Natural History of Man; as it will be shewn how instinct and necessity have led him to adopt different customs, and to make use of different articles of consumption in different climates.

I need scarcely direct attention to the necessity of

extending the investigation to the domestic animals and cultivated plants of the different races of men—to the materials used for their dresses—to their rude arts—and to all other things which belong to their physical life. The results at which we might arrive, if we were able to compare, in a Museum, models of the houses of all the tribes of the South Seas or of the American Continent, their canoes, their instruments, &c., can scarcely be conceived. There is one more palpable result from such an investigation;—it would increase the stock of materials used by our manufacturers for various purposes; for instance, dyeing and varnishing.

A subject which the inquirer into the history of the human race claims particularly as his own, as the most important and indispensable, is Language. Language distinguishes man from the inferior animals. The communication of his inward feelings, the expression of his thoughts by means of words, is common to man, in all the different stages of physical, mental, and social development. From his language we can perceive the structure and disposition of his mind, his prevailing passions and tendencies: in language, the changes and revolutions which the mind of a nation has undergone have left indelible traces: its structure alone allows us to search into the character of a people, and throw a light into the dark labyrinth of their history, if no written or monumental record, and not even a tradition, has been preserved. Of all differences which distinguish races from one another, language is the most lasting and enduring; and where physical traits and peculiarities have lasted longer, this is only an exception to the rule. To destroy a language of a nation, is to destroy its nationality: it has been the object of all despots and conquerors, but has rarely been attained. By means of language we may trace the relation of one race, one nation, or tribe, to another; by means of language, alone, we can follow their migrations, and retrace them, step by step, to their source: language leads us back to those times when mankind was in its infancy; and we become acquainted with the laws which guided its gradual development, from a more primitive state to its present condition. If any thing can confirm the unity of mankind, and can bring it home to the most sceptic mind, it will be

when, by a thorough study of all the languages of the globe, we can prove them to be merely dialects, and derivations from one and the same source. But we are now far from such a result; and the Ethnological Society must, therefore, particularly devote its attention to collect materials for the study of Comparative Philology*: it must collect in its Library, Dictionaries and Grammars of all existing languages and dialects, and encourage their publication. There are MSS. existing of various languages; but from a want of patronage, these materials (which have perhaps been collected during a life of trouble and hardship amongst barbarous nations, and which are of the utmost interest to the scientific) are scarcely of any use. In the Library of the Ethnological Society such MSS. would find a most appropriate place.

In speaking of Language, I wish to draw attention to those first beginnings of the art of Writing which has been observed even amongst the rudest nations;—amongst the Indians of the Orinoco and Bolivia, the wandering tribes of North America, the natives of Australia, and in some of the Polynesian Islands. It is a writing in pictures, or rather in drawings, of animate and inanimate objects, which are often found on rocks, on the way-side, or on trees, and represent the occurrence of some event. It is desirable to obtain exact copies of all such drawings, and to collect and compare them.

An inquiry from which we may obtain nearly the same results as from Language, although in a far inferior degree, is Music. It is to be expected that the manner in which sensations or thoughts are expressed, by a modulation of the human voice, or by instruments, will be one of the characteristic differences of races. All nations, however barbarous they may be, have been found to be possessed of some sort of music: it will belong to the Ethnological Society to collect the materials for a Comparative Music of the Races of Man; materials which, of course, must be written in music's own characters—in notes. The want of musical knowledge in Travellers has often prevented acquisitions of this kind; but

* In this it does not clash with the Philological Society; on the contrary, it may render important service, by collecting materials for its use.

in the metropolis of England frequent opportunity offers for musicians to write down musical compositions from the oral recitation of Natives. We may merit in this the thanks of the musician, by furnishing him with materials of a decidedly novel and original character.

The great result of such a contemporaneous study of the races of man will be the construction of an Ethnological Map of the World. An ethnological map must represent the geographical limits of people of the same origin and language: it must also bring before the eye the subdivisions of a principal race, its migrations and intermixtures. Such a map will, no doubt, present to the eye, especially in countries which are continental and contiguous, a law of the migrations of man, as dependent upon the physical geography of the different countries, upon their systems of mountains, great rivers, and valleys; or, in islands, upon the directions of atmospherical and marine currents. In this respect, it will shew how indispensable the study of geography is to the ethnologist, and in what manner the two sciences are connected. I am happy to mention here, that two of my friends, Gustaf Kombst at Edinburgh and Wilhelm Obermüller at Paris, have lately published such maps of Europe, and part of Africa and Asia, which must be regarded as a great acquisition to the study of ethnology, and will contribute much towards making the science more popular. Both maps are somewhat different in their construction; but both the methods pursued by these gentlemen have their advantages. Gustaf Kombst's map is more elementary: he has sharply marked where, in his opinion, two races separate; and, from its graphical execution, and the lucid explanation which accompanies it, he gives a very clear view of the principal races of Europe, and their physical and moral character. On the other hand, as nations are nowhere in nature so sharply separated, Wilhelm Obermüller has attempted to express their nearer or more distant relation by more or less vivid colours; and their mixtures and combinations, by mixtures and combinations of colours. An ethnological map of the whole world, constructed like these maps of Europe, does not yet exist; although the labours of the illustrious Klaproth, Ritter, Humboldt, and D'Orbigny have done much towards its completion. It will

be the useful aim of the Ethnological Society to combine the labour of its members to produce such a map, on different modes of construction, that they may serve in Public Schools. Ethnological maps, however, must not only exist of the races of men at the present moment, but of any given time of man's existence; as, like the surface of the globe, and the still less constant political institutions, the dwelling-places of men are ever changing.

Thus far I have pointed out some few of the paths which the Ethnological Society must follow, in pursuing the Natural History of Man, from what I have called a contemporaneous view. But races and nations have lived before us—perhaps many—of which History is silent, and of which no remains are left, or not yet discovered. Of others, we have astonishing pyramids, the ruins of "cloud-capp'd towers, gorgeous palaces, solemn temples," or monuments upon which records are written in an unknown tongue. Mummies are preserved of others, in crevices of rocks or in artificial tombs, through long ages, as those of the Guanches in the Canary Islands, and of the Aymaras on the Lake of Titicaca and the Coast of Peru. The ethnologist must tell us what was the physical condition of the race which left these remains—what were their manners, customs, habits, and general state of society—what progress they had made in the arts of civilised life, isolated as they often were from the rest of mankind. He must shew us their origin and relations. Ethnology must be the ground-work to their history; as the social development of man, his thinking and doing, the succession of events which led to the rise and fall of nations, must, of necessity, in a great measure depend upon the physical condition of the race itself. And here will be perceived at once the close connection of Ethnological and Historical studies. Herder, one of the principal representatives of a number of illustrious contemporaneous writers in Germany, has forcibly pointed out this connection, in his "Ideas towards the Philosophy of the History of Mankind." As I have not the book at hand, I can do nothing better than quote here what the writer of an able Article on the "Modern Art and Science of History," in the "Westminster Review" of October last,

says on this subject:—"History, in Herder's view, is a living organic whole; a growth of Nature, growing, like other natural things, by laws that are simple and changeless, though endlessly varied in their manifestations in place and time; drawing nutriment, like Nature's other products, from all the elements of Nature, and susceptible of light and illustration from all other natural sciences. Thus he examines the influence of climate in determining the physical and moral character of races; in deciding, by its connection with diet and its consequent action on organization—by the different stimuli which different climates administer to the senses and imagination—by the different modes of living (as in doors or out of doors) which different climates necessitate or favour—and by the different ways and directions in which practical intellect and industrial skill are elicited under different circumstances of soil, weather, and production—the particular form which civilisation shall assume in any given time and place; for instance, the rapid and early physical maturity of woman in the East, its determining or modifying cause, whose operation reaches all through the social culture, the domestic habits, and the political institutions of the Oriental World. Even so slight-seeming a circumstance as the characteristic nature and habits of domesticated animals goes some way in determining the social life and manners, the mental development, and consequently the entire public history of a people. The horse, the dog, the camel, the sheep, the Peruvian lama, are influencing agents in the histories of the countries that respectively produce them. In the same spirit of research, at once minute and comprehensive, Herder traces the connection of Physical Geography with the History of Society; shewing how Nature has marked out, in the rough, the ground-plan of civil and political history, the courses of migration and settlement, and the boundaries of nations and empires, in the directions she has given to the great mountain-chains, and to the streams that flow down their sides and communicate with the Ocean. He suggests, too, a philosophical comparison of languages, with reference to their elementary sounds—their etymological inflections, and syntactical constructions—their richness or poverty of metaphor—their relative

fitness for philosophy, eloquence, or song—as one mode of interpreting diversities of popular genius and character. He would read the moral physiognomies of nations in what may be called the physiognomies of their dialects."

I trust I have said sufficient, in recommendation, and on the general purposes of Ethnological Studies. This Country excels all others in producing great results in Science by a combination of labour: let us hope that the Science of Man equally deserves such a combination;—that it will not only be supported by those who pursue sciences which will be mostly benefitted by a more extensive study of the physical history of man—by the politician, the physician, the historian, and the artist,—but also by those to whom a knowledge of man must be of great interest and vital importance, from their connections with the Colonial possessions of this great Empire. If we have examined, step by step, the physical history of the human race—if we have entered the wigwam of the Red Indian, and followed the hunter in obtaining the scanty means of his precarious existence—if we have endured an Arctic winter in the snow hut of the Esquimaux, and have ceased to sneer at him, when we find that no other life was possible under the circumstances in which he is placed—in one word, if we have traced Humanity through all the forms, simple and complicated, rude and civilised, of social existence, and have found that in each state there is something recommendable, then, and not till then, shall we treat with consideration those who differ from us, instead of warring against individualities and forms which are not the same as our own.

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- Natural History of Newera Ellia.
- Description of Ceylon Mammalia.
- Catalogue of Ceylon Birds.
- Description of New Species of Ceylon Birds.
- Description of Ceylon Reptiles.

APPENDIX.

Dr. Gardner on the Flora of Ceylon.

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United Service Mag., November, 1853.

PRODROMUS
FAUNÆ ZEYLANICÆ;

BEING

CONTRIBUTIONS TO THE ZOOLOGY

OF

CEYLON.

BY

E. F. KELAART, M.D., F. L. S., ETC., ETC.

STAFF SURGEON TO THE FORCES.

"Nihil est aliud magnum quam multa minuta."

VOL. II.—PART I.

COLOMBO:

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M.DCCC.LIII.

[From the Journal of the Asiatic Society of Ceylon, 1853.]

NOTICE.

IN presenting to Naturalists a more complete Synopsis of Ceylon Reptiles, which the continued kindness of friends in various parts of the Island has enabled me to form, I have much pleasure in expressing my best thanks to the Members of the Asiatic Society of Ceylon, and to Sir George Anderson, Dr. Fergusson, P.M.O., and the Hon'ble C. J. MacCarthy, Esq., Colonial Secretary, for the facilities they have afforded me in my further researches in the Natural productions of the Island.

The joint labours of Mr. Edgar Layard and myself have nearly completed the examination of the Mammals, Birds, Shells, Reptiles and Insects of this Island. The fresh water Fishes are now engaging my attention, and any specimens of this Class will be thankfully received. Dr. Schmarda, Professor of Zoology of the University of Prague, is now on a visit to the Island for the chief purpose of examining the Infusoria, and Professor Harvey of Trinity College, Dublin, is also, I believe, at present engaged in examining into the Echinodermata and Crustacea of the Island, so that it may be hoped, it will not be very long before the neglected Fauna of Ceylon is completely worked out.

SYNOPSIS
OF
CEYLON REPTILES.

ORDER. SAURA. *Lizards.*

SUB-ORDER. I. LEPTOGLOSSÆ.

Slender-tongued Lizards.

FAM: MONITORIDÆ. *Monitors.*

- 1 Monitor *Dracæna, Gray.* Guana. Talla Goya, *Sing.*
2 *Hydrosaurus Salvator, Wagler.* Water Guana. Cabara Goya, *Sing.*

FAM: SCINCIDÆ. *The Scincs.*

- 3 *Riopa punctata, Gray.* Puchee Bramin, *Port.*
4 *Riopa Hardwicki, Gray.* do.
5 *Mabouia elegans. (?) Gray.* do.
6 *Taliqua rufescens, Gray.* Large do.

FAM: ACONTIADÆ.

- 7 *Nessia Bartoni. (?) Gray.* Ground Bramin.
8 *Acontias (?) Layardi, n. s., nobis.* Layard's do.

FAM: TYPHLOPSIDÆ. *Typhlops.*

- 9 *Argyrophis Bramicus, Daud.* Silver snake.
——— 2 varieties.

FAM: UROPELTIDÆ. *Rough tails, or False snakes.**Dapatnaya, Sing.*

- 10 *Uropeltis grandis*, *n. s., nobis.*
 11 *Uropeltis Pardalis*, *n. s., nobis.*
 12 *Uropeltis Saffragamus*, *n. s., nobis.*
 13 *Rhinophis Blythii*, *n. s., nobis.*
 14 *Dapatnaya Lankadivana*, *n. s., nobis.*
 15 *Dapatnaya Trevelyana*, *n. s., nobis.*
 16 *Siluboura Ceylonicus*, *Gray.*

SUB-ORDER II. PACHYGLOSSÆ.

*Thick-tongued Lizards.*FAM: GECKOTIDÆ. *The Geckoes.**Ceechas, Port. Hooma, Sing.*

- 17 *Hemidactylus trihedrus*, *Lesson.* The triangular tubercled Gecko.
 18 *Hemidactylus maculatus*, *Dum.* The spotted do.
et Bib.
 19 *Hemidactylus Pieresii*, *n. s., nobis.* Pieres' Gecko.
 20 *Hemidactylus Coctoi*, *Dum. et Bib.* Cocto's do.
 21 *Hemidactylus frenatus*, *Schlegel.* The streaked Gecko.
 22 *Hemidactylus Leschenaultii*, (?) Leschenault's do.
Dum. et Bib.
 23 *Boltalia sublevis*, *Gray.* The Boltalia.
 24 *Peripia Peronii*, *Dum. et Bib.* Peron's Gecko.
 25 *Gymnodactylus* (?) *Kandianus*, The diurnal Gecko.
n. s., nobis.

FAM: AGAMIDÆ. *The Agamas.**Blood-suckers, Vulg. KattooSah, Sing.*

- 26 *Sitana Ponticercana*, *Cuvier.* The Sitana.
 27 *Lyriocephalus scutatus*, *Wagler.* Lyre-headed Lizard.
 28 *Ceratophora Stoddartii*, *Gray.* The needle-nosed Lizard
 29 *Salea Jerdonii*, *Gray.* Dr. Jerdon's Salea.
 30 *Calotes Ophiomachus*, *Gray.* Red headed Green Lizard.
 31 *Calotes Rouxi*, *Gray. Blyth.* } Green Lizard.
vel C. Viridis, Gray. apud nos. }
 32 *Calotes mystaceus*, *Dum. et Bib.* Red spotted Green Lizard.
 33 *Calotes versicolor*, *Dum. et Bib.* The common Blood-sucker.

FAM: CHAMELEONIDÆ. *Chameleons.*

- 34 *Chameleo vulgaris*, *Daud.* The true Chameleon.

ORDER. OPHIDIA. *Serpents.**Cobras, Port. Saroopeya, Satta, Sing.**Innocuous Serpents.*

FAM: BOIDÆ.

BURROWING.

- 35 *Cylindrophis maculata*, *Wagler.* The red and black netted Snake.

TERRESTRIAL.

- 36 *Python molarus*, *Gray.* The Rock Snake.

FAM: COLUBERIDÆ. *Bonaparte.*

TERRESTRIAL.

- 37 *Calamaria Scytale* ?
 38 *Lycodon*, 2 or more species.
 39 *Xenodon purpurascens*, *Schlegel.* var.
 40 *Coluber Korros*, *Reinwardt.*

ARBORIAL.

- 41 *Dipsas multimaculata* (?) *Schlegel*.
 42 *Dryinus Prasinus*, *Reinhardt*. The Whip Snake.
D. nasutus 2 or more varieties.
 43 *Leptophis pictus*, *Gmelin*.
 44 *Leptophis ornatus*, *Shaw*.
 45 *Leptophis* sp.

AQUATIC. Deye Naya, *Sing*.

- 46 *Tropidonotus umbratus*, *Daudin*. (var.) Large blackish water Snake
 47 *Tropidonotus stollatus*, *Linn*. The speckled do.
 48 *Tropidonotus schistosus*, *Daud*. The olive brown do.
 49 *Cerberus cinereus*, *Cuvier*. The black do.

Venomous Serpents.

FAM: VIPERIDÆ. *Vipers*.

TERRESTRIAL.

- 50 *Bungarus candidus*, *Linn*.
 51 *Naya lutescens*, *Daudin*. var. Cobra de Capello.
 —var. *Nigra*.

ARBORIAL.

- 52 *Trigonocephalus Hypnale*, *Wagler*. Caravilla.
 53 *Trimesurus gramineus*, *Shaw*. The green Caravilla.
 54 *Trimesurus Ceylonensis* (?) *Gray*. *apud Gray*.
 55 *Megaera trigonocephala*, *Wagler*. Palagolla.
 56 *Daboia elegans*, *Gray*.
 57 *Daboia Russellii*, *Gray*. } Polanga.

N.B.—There are about six or eight more Ceylon Snakes, which have not yet been identified.

ORDER. CHELONIA. *Tortoises*.Cowdo, *Port*. Iba, *Sing*.

FAM: TESTUDINIDÆ.

- 58 *Testuda Indica*, *Gmelin*. The large land Tortoise.
 59 *Testuda stellata*, *Schweig*. The yellow starred do.

FAM: EMYDIDÆ.

- 60 *Emys trijuga*, var. *Schweig*. Marsh Tortoise. Gal Iba. *Sing*.
 61 *Emys Seba*, *Gray*, *apud Blyth*. do.
 62 *Emyda punctata*, *Gray*. River Tortoise. Keeree Iba. *Sing*.

FAM: CHELONIDÆ.

- 63 *Caretta imbricata*, *Gray*. Sea Turtle. Kokooolookoosomba, *Sing*.
 64 *Chelonia virgata*, *Schweig*. Edible do. Gal koosomba, *Sing*.

ORDER. EMYDOSAURI. *Crocodiles*.Lagartoo, *Port*. Kimboola, *Sing*.

- 65 *Crocodylus porosus*, *Schweig*. { The Indian river Crocodile.
 Allie Kimboola, *Sing*.
 66 *Crocodylus palustris*, *Lesson*, *male*. { Marsh Crocodile.
 Halle Kimboola. *Sing*.

Crocodylus Bombifrons, *female*. *apud Blyth*.

AMPHIBIA.

ORDER. BATRACHIA, *Frogs*.Madookoo, *Port*. Attykitta, *Sing*.FAM: RANIDÆ, *Water Frogs*.

- 67 *Rana cutipora*, *Dum. et Bib*. Large green bull Frog.
 68 *Rana Malabarica*, *Dum. et Bib*. The green spotted bull Frog.
 69 *Rana Bengalensis*, *Gray*. The small green do.
 70 *Rana Tigrina*, *Daudin*. The golden Frog.
 71 *Rana Newera Elliana*, *nobis*. The blackish Frog.
 72 *Rana Kandiana*, *nobis*. The red bellied Frog.
 73 *Rana Leschenaultii* (?) *Dum. et Bib*. Leschenault's Frog.

Bib. apud Blyth.

FAM: HYLIDÆ. *Tree Frogs.*Ghas Gemba. Attykitta. *Sing.*

- 74 Polypedates leucomystax, }
Gravenhorst. } The common tree Frog.
- 75 Polypedates cruciger, *Blyth.* The cross-backed do.
- 76 Polypedates stellata, *nobis.* The white spotted green tree Frog.
- 77 Limnodytes mutabilis, *nobis.* The changeable tree Frog.
- 78 Limnodytes maculata, *nobis.* The brown spotted do.

FAM: BUFONIDÆ. *Toads.*Gemmadæ, *Sing.*

- 79 Bufo melanostictus, *Schneider.* The common house Toad.
- 80 Engystoma marmorata, *Gray.* The green spotted do.
- 81 Engystoma cinnamomea, *nobis.* The red do.

ORDER. PSEUDOPHIDIA.

FAM: CÆCILIDÆ.

- 82 Ichthyophis glutinosus, *Gray.* The glutinous Slow worm.

N. B.—Mr. Blyth in his last Report, J.A.S.B. No. 4 of 1853, enumerates among the Reptiles I sent him, the following new species, *Limnodytes levidus*, *L. macularis*, *Engystoma rubrum*, Jerdon. *Pyricephalus Fodiens*, Jerdon, and *Rana robusta*.

DESCRIPTION
 OF
 NEW OR LITTLE KNOWN SPECIES
 OF
 REPTILES.

ORDER. SAURA.

TRIBE. GEISSOSAURA.

FAM. ACONTIAS, (ACONTIADÆ.)

Head small, shielded. Muzzle conical. Rostral rather large, cup-shaped, internasal short, frontal large, frontoparietal none, interparietal triangular, moderate. Eyes distinct. Eyelids, lower well developed, upper small or wanting. Nostrils in the middle of the side of the rostral shield, with a slit to its hinder edge. Tongue scaly, imbricate, nicked at the point. Ears very small or hidden. Femoral pores none. Body cylindrical. Limbs 4, very short, or none. Scales smooth.—*Gray.*

GENUS. NESSIA. *Gray.*

Muzzle conical. Ears very small, dotlike. Body cylindrical, elongate, sides rounded. Scales smooth. Legs 4, very short, far apart. Toes 3-3, subequal, clawed.

NESSIA BURTONI? *Gray.*

Dark rufous-brown above, and spotted longitudinally with darker brown spots. Dark grey beneath, clouded and indis-

tinctly spotted. Tail cylindrical, rounded at the end, and coloured and spotted like the body. Limbs 4, very small, each with 3 subequal toes.

Length $5\frac{1}{2}$ inches.

Habitat. Allagalla (3000 feet.)

The only specimen which I have examined is one obtained from vegetable soil in a Coffee Estate under the superintendence of Dr. Marshall.* I am informed that this reptile is common at Ambegammaoa.

If my identification is correct, it would appear probable that the only specimen in Europe, found in the Army Medical Officers' Museum at Fort Pitt, was sent from Ceylon. The specimen was named after Staff Surgeon Burton, who was the Curator of the Museum when Dr. Gray visited that establishment.

GENUS. ACONTIAS. *Cuvier.*

Head conical. Nostrils lateral. Internasal broad, 6 sides, frontal large, 6 sided, frontonasals and frontoparietals none; interparietals small, triangular, parietal moderate. Tongue flat, scaly, nicked at the tip. Teeth conical, blunt. Palate not toothed, with a longitudinal groove. Eyes very small. Upper eyelid wanting, lower short, scaly, opaque. Ears hidden under the skin. Body cylindrical, elongate. Scales smooth. Limbs none, exerted. Tail cylindrical, short rounded at the end.—*Gray.*

ACONTIAS (?) LAYARDI. *n. s., nobis.*

Light olive, and spotted longitudinally with brown spots, paler beneath.

Length of young 4 inches.

Habitat. Soil of the Cinnamon Gardens of Colombo.

* I have since received several smaller specimens from Kadoganava.

The form of this reptile is distinguished from that of *Nessia*, above described, by the absence of limbs; in other respects it is very like the outline characters of *Nessia*. Mr. Layard procured me the specimens (apparently young) of this curious lizard.

The only other species described in Gray's Catalogue, is one from the Cape of Good Hope.

Mr. Blyth writes that he has described an allied Genus, from Rangoon, by the name of OPHISEPS.

FAM. UROPELTIDÆ.

ROUGH TAILS. DAPAT-NAYA, *Sing.*

Head conical, compressed, shelving and acute in front, flat above, behind. Crown covered with regular shields. Rostral produced, moderate, convex, horny, subtriangular, erect, produced, and acute behind. Nostrils roundish, lateral, simple, in the middle of the front of an erect subtriangular band-like nasal. Internasal none, frontonasal subtriangular, truncated below, contiguous above, frontal and frontoparietal distinct. Nape scaly. Labial shields † distinct. Eyes distinct, lateral, in the middle of the front of an erect eye-shield, and covered by the shield without any eyelids. Eyebrow shield none. Tongue elongate, flat, forked at the tip. Body cylindrical. Scales 6 sided, smooth, those of the hinder part and above the tail sometimes 2-keeled. Vent with three scales in front. Tail cylindrical, obliquely truncated above.

This family is intermediate between the Lizards and the Snakes; following Cuvier and others, I was induced to refer it to the latter order, and therefore did not insert it in the Synopsis of the Families: but on re-examination and comparison with the various modifications presented by the genera of Typhlopsidae, I have been induced to place it in the order of Lizards. Cuvier seems to have been in doubt, for though he placed the

genus with the Snakes, he also regarded the species as a section of the Typhlopses. Schlegel names the group Pseudo Typhlops.—(*Gray's Synopsis*.)

This curiously formed family of Reptiles is known in the Island as *Dapat Nayas*, or double headed snakes. The natives consider them poisonous, but my experience of their habits makes me believe that they are perfectly harmless, and that they are timid creatures, seldom making their appearance above ground; living chiefly in ant-hills or dunghills, sometimes also several feet deep in rich loamy soil. They feed on ants, small earth-worms and larvæ of insects.

It appears from Dr. Gray's Catalogue, that in the Museums of Europe there are only three species, and of these three only one is from Ceylon, viz., *Siluboura Ceylonicus*,* many specimens of which I have found in the Kandyan Hills since my work on the Fauna of Ceylon was published. I have also, since the publication of that work, collected four other species, making in all six distinct undescribed or new species of Rough Tails (*Uropeltide*) in the Island of Ceylon, which I shall now describe in a connected form.

Dr. Gray subdivides the family into three groups, and I have added a fourth, to admit of two species, only one of which I have described in the Prodomus.

GENUS. RHINOPHIS. *Hempr.*

Tail obliquely truncated, upper part rather convex, covered with a small oblong shield, lower edge rounded, simple, rather produced. Head acute, tapering in front. Vent shields in one marginal row.

RHINOPHIS BLYTHII. *n. s., nobis.*

Dark yellowish brown above, with darker brown spots on the anterior third of scales. Paler beneath. Rostrum

* *Vide Prodomus Faune Zeylanicæ.*

yellow. Sides of nape and neck waved with angular marks of a yellowish hue; yellow spots on each side of vent. Tail thick, slightly truncated, conical, upper part near termination has a small subtriangular nearly smooth shield, lower surface covered with broad scales. Vent shields 1-2.

Length 16 inches, circumference of the middle of body 1.3-10 inches. Tail and neck rather thicker.

Habitat. Mountains of Ceylon.—Three specimens found 3 or 4 feet below the surface soil of Coffee plantations.

GENUS. UROPELTIS, (part). *Cuvier.*

Tail obliquely truncated, flattish, and covered with a flat roundish radiating granular shield, lower edge rounded, the under side of tail with 6 series of small scales.

UROPELTIS SAFFRAGAMUS. *n. s., nobis.*

Head dark olive brown, the rest of the upper surface of a blackish brown colour, with bluish bronze reflections. Beneath white. A pale white spot on each side of neck near the head. Tail deeply truncated and nearly covered with a large flat circular blackish granular shield, white and rounded beneath, and lower part covered with five series of small scales, the central series broader than the lateral ones. Vent shields 1-2. The neck and forepart of the body much thicker.

Length 9 inches.

Habitat. District of Saffragam, near Adam's Peak.

The only specimen of this species, which I have as yet seen, is one sent by Mr. Barnes De Zilva from Ratnapoora.

UROPELTIS GRANDIS. *n. s., nobis.*

Above dark brown with a bluish metallic lustre, anterior part of each scale with a blackish spot. Beneath of a pale yellow colour, spotted brown on the anterior part of scale.

Head of a light olive brown colour. Tail short, abruptly truncated; the truncated surface entirely covered with a large circular, granular shield. Vent scales 1-2.

Total length superiorly, 1 foot 7 inches. Inferiorly, 1 foot 8 inches. Tail shield nearly the size of a shilling piece. Head 8-10 inch in length.—Greatest circumference $2\frac{3}{4}$ inches, near the neck.

Habitat. Southern Province.

The only specimen I have seen of this very large Rough Tail, is one procured by Mr. Balkhuysen of the Colonial Medical Service, from Kerinday near Matura.

UROPELTIS PARDALES. n. s., nobis.

Head small, dark olive. Upper parts black with beautiful bluish bronze reflections, irregularly spotted white. Beneath yellowish white, marked with large and small black spots, variously shaped; some pale eyed. Tail very short, obliquely truncated, and with a large flat orbicular granular shield. Length, $6\frac{1}{4}$ inches; circumference $\frac{3}{4}$ inch.

Habitat. Matura. I am indebted to the Rev. Mr. Ondaatje for the only specimen I have examined of this species. The black spots on the lower parts occupy more than one scale, generally two or three contiguous scales; and they are placed without any order in various directions. The chin and throat immaculate.

GENUS. DAPATNAYA. *n. g.*

Tail obliquely truncated, upper part and tip covered with a large semiconical granular shield. Vent shields 1-2.

DAPATNAYA LANKADIVANA. *n. s., nobis.*

Above dark brown, beneath paler. Scales with pale margins. Head yellowish in some. Rostrum yellow. Shield

of tail sloping down to the lower surface. Vent scales yellow; in some the yellow spot extends beyond the vent.

Length, from 1 to 2 feet.

Thickness nearly the same throughout, about 1 inch.

Habitat. Common at Trincomalie, and in the Kandyan Province. Found 2 or 3 feet under ground, and in ant-hills. The young is of a dark olive brown colour.

DAPATNAYA TREVELYANA. *n. s., nobis.*

Black above, margin of scales pale. White beneath, with longitudinal series of black spots, formed of central spots on each scale. A line of triangular white spots, with their apices directed upwards, along each side. Vent white. A white line rising from this spot runs over the tail, and another whitish line extends forwards from the vent for about $\frac{3}{4}$ inch. Tail short, and nearly covered with a white semi-conical granular shield, tip ridged, a little produced.

Length from 12 to 18 inches; nearly of the same thickness throughout, about 1 inch.

Habitat. Kandyan Hills, 3 or 4 feet under ground, and in the soil near the roots of Coffee trees and Cane.

Some of the young are of a bluish colour, others are spotted on the back.

TRIBE. NYCTISAURA.

FAM. GECKOTIDÆ.

BOLTALIA.

Toes free, ovate and dilated towards the end, with two series of straight narrow nearly transverse closely adpressed plates beneath, divided by a deep narrow groove, last joint compressed, free, rather exerted, clawed. Thumb dilated, and with a compressed last joint, like the toes, but clawless. Back with granular scales and a few scattered larger granules.

Sides simple. Tail rather depressed, tapering, ringed, with a central series of broad scales beneath.

BOLTALIA SUBLEVIS. Gray.

Above dark rufous brown; beneath sulphureous yellow, sometimes clouded with black. Back granular, with two or more longitudinal lines of larger granules on each side. Scales of chin large. Tail armed with adpressed spines in rings. Femoral pores in males only, from 16 to 20.

Length $5\frac{1}{2}$ inches.

Habitat. The Central and Southern Provinces.

This Gecko I obtained in great abundance in Galle; I have also seen a few at Kaduganava, after the publication of the Prodrômus. It is rarely seen on the walls of houses, generally on trees, and on the roofs of houses. The rufous brown colour changes at times into a dark grey, mottled with black. When immersed in spirits the rufous brown colour is entirely lost.

This Gecko has been mistaken for *Hemidactylus Leschnaultii*, and also, for a large variety of *H. frenatus*, but the clawless compressed last joint of the thumb will distinguish this Lizard from others. From *Peripia Peronii* it is sufficiently removed by its granular back and armed tail. This Lizard cannot be confounded with *Hemidactylus Coctæi*. It has very little resemblance to it, and the thumb of *H. Coctæi* is clawed. (*Vide Prodrômus Faunæ Zeylanicæ*, vol. I.)

ORDER. BATRACHIA.

SUB-ORDER I. SALIENTIA.

FAM. RANIDÆ. *Frogs. ATTY KITTA. Sing.*

GENUS. RANA. *Linn.*

Skin smooth, hinder extremities very long, formed for leaping; toes palmated, teeth in the upper jaw, and in the palate.

RANA KANDIANA. *n. s., nobis.*

Beautiful grass green above, beneath orange red; inside of limbs slightly vermiculated with yellow. Skin of upper parts rather rough, coriaceous; a white granular ridge on each side. Length $3\frac{1}{2}$ inches.

Habitat. Kaduganava, Kandyan Province.

I have only seen two specimens of this rare frog.

FAM. HYLIDÆ. *Tree Frogs. GHAS ATTY KITTA. Sing.*

GENUS. LIMNODYTES. *Dum. et Bib.*

Tongue long, narrowed in front, widened, forked, free behind; the teeth on the vomer forming two groups, between the internal openings of the nostrils; tympanum distinct; Eustachian tubes middling, fore fingers free; toes completely or partly webbed; subdigital disks slightly dilated; process of the first os cuneiforme blunt, very minute; males with vocal sacs; sacral transversal processes not dilated.

LIMNODYTES MUTABILIS. *n. s., nobis.*

Upper parts very changeable; generally, of a bright green above and yellow beneath; a red line on the outer edge of limbs; sometimes of a dark chesnut on the upper parts, and variegated with yellow and green on the sides and limbs.—Of a more slender form than the common tree Frogs *Polypedates cruciger*, and *P. Leucomystax*. About 2 inches long.

Habitat. Cinnamon gardens; Cotta near Colombo. If not mistaken, we have also seen this elegant frog at Nuwera Ellia. In spirits the colours fade into a pale leaden hue; the red streak on the limbs disappears last.

LIMNODYTES MACULATA. *n. s., nobis. **

Brown, spotted and streaked with black or dark brown; beneath pale, seldom spotted. About $1\frac{1}{4}$ inch long.

* I have received from Dr. Webb a Tree frog, $2\frac{1}{2}$ inches long, having the diagonal black marks of *Polypedates cruciger* of Blyth; but, unlike it,

Habitat. Galle, Southern Province.

I believe this species to be generally distributed, but have before mistaken it for the young of *Polypedates Leucomystax*.

FAM. BUFONIDÆ. *Toads.* GAMEDEA, *Sing.*

GENUS. ENGYSTOMA. *Gray.*

No tympanum nor parotid visible externally; an oval body; the head and mouth very small, and feet but slightly palmated.

ENGYSTOMA RUBRUM. *n. s., Jerdon.*

Synon. Engystoma cinnamomea. *M. s. n. b.*

Cinnamon red, spotted black; limbs also spotted; belly whitish. Length (young) $1\frac{1}{2}$ inch.

Habitat. Southern India (?) Ceylon.—

this frog is maculated above and beneath, and the posterior sides of thighs are also spotted. White granular spots around the anus. Sides of body yellowish, spotted. Throat white, dotted with black. If Dr. Webb's specimen is of a distinct species, the two other (young) specimens of a very dark brown colour, spotted, with a pale back, are perhaps of the same species. In another (also young) the space, marked like an hour glass, on the back, is of a jet black colour. I am more inclined to believe that all these are different phases of the new species, named by Mr. Blyth, *Polypedates cruciger*.

ADDENDA.

ORDER. SAURA.

TRIBE. GEISSOSAURA.

FAM: SCINCIDÆ, *Scinc.*

EUMECES TAPROBANENSIS, *novis.*

Above dark brown, with 6 lines of black dots on the back. Sides of neck and body of a darker brown, minutely dotted white; a few white dots also on the limbs. Beneath whitish. Upper surface of tail of the same colour as the body; under surface dark grey, each scale with a large blackish spot. Head short, subtriangular; muzzle narrow, rounded. Nostril pierced on the upper edge of nasal plate. Eyes large, eyelids scaly, edges lightly granular; lower lid with a series of larger scales. Ears small, circular, dentated anteriorly by two or three scales. Body rather short, subcylindrical. Tail elongated, rounded, tapering, pointed. Limbs 4, small, not wide apart. Toes 5-5, short, unequal, tubercular beneath, clawed. Palms and soles granular.

Head and body, 1. 7-10 inches.

Tail, 2. 6-10 inches.

Found at Newera Ellia by Dr. Webb, Staff Assistant Surgeon. This Scinc resembles in colour and in its elongated form the Lizard which I have identified with *Nessia Burtonii*.

ORDER BATRACHIA.

FAM: HYLLIDÆ.

POLYPEDATES (?) SCHMARDA, n. s., nobis.

Above brownish grey; beneath white, posterior half of abdomen marbled with black. Eye-brows armed with spines. Back and sides tuberculated. Limbs studded with tubercular sharp pointed spines. About 1½ inch long.

Hab. Adam's Peak.

This novel form of Tree Frog was first found on Adam's Peak by Dr. Schmarda, Professor of Zoology in the University of Prague, to whom I have dedicated this species. It has no resemblance to any other found in Ceylon or Southern India.

Gray's Genera of Birds adapted to Ceylon Ornithology.

ORDER. I. ACCIPITRES.

This order embraces the Birds of Prey; they have the *bill* of various length and form, more or less compressed, with the *culmen* suddenly hooked at the tip, and acute: the base more or less covered with a *cere*, in which are pierced the variously formed nostrils; the *wings* lengthened and pointed; the *feet* strong, with *tarsi* moderate, generally rounded and covered with scales of different shapes; the *toes* three before and one behind, all armed with strong *claws*, and their soles invariably rough.

The first Sub-Order

ACCIPITRES DIURNI, or Diurnal Birds of Prey,

are distinguished from those that pursue their food in the twilight or at night, by the lateral position of their *eyes*: the base of the upper mandible covered for nearly half its length with a prominent *cere*, in which the nostrils are placed: the *tarsi* moderate, scaled, and rarely covered with plumes.

FAM: FALCONIDÆ.

SUB-FAM: AQUILINE, or EAGLES.

GENUS. AQUILA. *Mahr.*

Bill strong at the base, and with the apical portion of the *culmen* much curved to the tip, which is greatly hooked and acute: the sides much compressed, and the lateral margins festooned; the *nostrils* placed at the *cere*, large, and rather oblique. *Wings* lengthened and acute, with the fourth and fifth quills equal and longest. *Tail* long and wedge-shaped, or rounded at the end. *Tarsi* rather longer than the middle toe, robust, and entirely clothed to the base of the toes with feathers. *Toes* moderate, strong, lateral ones unequal, and all armed with strong, curved, acute claws: the inner the strongest.

GENUS. SPIZAËTUS. Vieill.

Bill moderate, the *culmen* straight at the base, and much arched to the tip, which is acute and compressed, and the lateral margins festooned; the *nostrils* large and rather rounded. *Wings* moderate, reaching to half the length of the tail, with the fourth and fifth quills equal and longest. *Tail* long and slightly rounded at the end. *Tarsi* slender, much longer than the middle toe, and plumed to the base of the toes. *Toes* long, strong, the inner one much longer than the outer, which is united to the middle one by a membrane: all covered above with small scales except at the apex, where there are a few transverse ones, and each toe armed with a long, strong, curved and acute claw.

GENUS. CIRCAËTUS. Vieill.

Bill moderate, *culmen* at the base straight, and much curved to the tip, which is hooked and acute, the lateral margins festooned; the *nostrils* large, oblique, and suboval. *Wings* lengthened and acute, with the third and fourth quills equal and longest. *Tail* long and even. *Tarsi* much longer than the middle toe, feathered below the knee, and entirely reticulated. *Toes* short, strong, and covered with small scales, except at the tips; the outer united to the middle by a membrane; the *claws* long, curved, and acute.

GENUS. HALIAËTUS. Sav.

Bill large, *culmen* straight at the base, and curved to the tip, which is hooked and acute, the sides compressed, the lateral margins slightly festooned, and the lores naked; the *nostrils* moderate, linear, and oblique. *Wings* lengthened and acute, with the third, fourth, and fifth quills nearly equal and longest. *Tail* moderate and rounded, *Tarsi* short, strong,

covered in front with transverse narrow scales, and with small irregular ones posteriorly, and on the sides. *Toes* long, mostly covered above with transverse scales; the *claws* long, curved, and acute.

GENUS. PONTOAËTUS. Kaup.

The characters of *Haliaëtus*, except that the *tarsi* are covered in front with transverse scales, posteriorly with large irregularly placed scales, and on the inner side with small ones. *Toes* almost entirely covered above with transverse scales.

GENUS. HALIASTUR. Selby.

Most of the characters are similar to those of *Haliaëtus*; but the *tarsi* are covered in front with large obliquely placed scales, posteriorly with large irregular scales, and the sides with small ones.

SUB-FAM: FALCONINÆ, TRUE FALCONS.

GENUS. FALCO. Linn.

Bill short, strong, with the *culmen* much arched from the base to the tip, which is acute: the sides compressed, the lateral margins strongly toothed near the tip; the *nostrils* placed in a short cere, naked and rounded, with a central tubercle. *Wings* lengthened and acute, with the second and third quills the longest, and the first and second notched near the tip. *Tail* long and rounded. *Tarsi* short and strong, covered with small irregular scales, and the tibial feathers covering the knee. *Toes* lengthened and strong, the lateral ones unequal; the hind toe long, armed as well as the inner with a strong, hooked, acute claw.

GENUS. HYPOTRIORCHIS. *Boie.*

The characters in common with Falco; but the *tarsi* more or less lengthened, somewhat slender, and covered in front with large hexagonal scales. *Toes* very long and slender.

GENUS. TINNUNCULUS. *Vieill.*

The forms agree with those of Falco. But the *tarsi* are more or less lengthened, strong, and covered in front with large transverse hexagonal scales. *Toes* moderate and strong.

SUB-FAM: MILVINEÆ, or KITES.

GENUS. BAZA. *Hodgson.*

Bill moderate, elevated at the base, which is broad, the sides compressed, the *culmen* much arched, and the lateral margins furnished with two small teeth near the tip, the lower mandible also furnished with small teeth near the tip; *lores* clothed with small feathers; the *nostrils* placed in the *cere*, in the form of an oblique slit. *Wings* lengthened, nearly reaching to the end of the tail, with the third and fourth quills the longest. *Tail* long and broad. *Tarsi* thick and very short, longer than the middle toe; basal half plumed, and the other part covered with small scales. *Toes* thick, free at their base, the inner longer than the outer; the claws small and acute.

GENUS. MILVUS. *Cuv.*

Bill laterally compressed, the *culmen* straight at the base, and then much curved to the tip, which is acute, the lateral margins somewhat straight; the *nostrils* oval, and placed rather obliquely in the *cere*. *Wings* very long, with the third and fourth quills the longest. *Tail* very long, broad, rounded, or more or less forked at the end. *Tarsi* very short, partly

plumed from the base, and the rest covered with scales. *Toes* rather short, with the outer united at its base to the middle one, the *claws* long and curved.

GENUS. ELANUS. *Sav.*

Bill short, very broad at the base, and compressed towards the tip, which is hooked and acute; the *nostrils* large and suboval. *Wings* very long, pointed, and reaching beyond the tail, with the second quill the longest. *Tail* long, and slightly emarginated. *Tarsi* short, nearly the length of the middle toe, with the basal part plumed, and the other covered with reticulated scales. *Toes* moderate, broad, and free at the base, with the outer much shorter than the inner toe; the *claws* strong and curved.

SUB-FAM: ACCIPITRINEÆ, or SPARROW HAWKS.

GENUS. ASTUR. *Lacep.*

Bill short, broad at the base, and with the sides gradually compressed to the tip; the *culmen* elevated, and much arched to the tip, which is acute, and the lateral margins festooned in the middle; the *nostrils* large and suboval, and placed in the basal *cere*. *Wings* long, reaching to the middle of the tail, with the third, fourth, and fifth quills nearly equal, and longest. *Tail* long and broad. *Tarsi* rather longer than the middle toe; the anterior and posterior sides covered with broad transverse scales. *Toes* more or less lengthened, strong, and padded beneath each joint; the lateral toes unequal; the inner and hinder ones equally long and strong, and armed with long, strong, and curved claws.

GENUS. ACCIPITER. *Kriss.*

Bill very short with the *culmen* much arched to the tip, which is acute; the sides much compressed, and the lateral

margins festooned; the *nostrils* placed anteriorly in the cere, large and suboval, partly concealed by the projecting hairs of the lores. *Wings* moderate, with the fourth and fifth quills nearly equal, and longest. *Tail* long, ample, and nearly square at the end. *Tarsi* longer than the middle toe, covered in front with nearly obsolete scales. *Toes* more or less lengthened, and padded beneath the joints; the lateral ones unequal, and the inner and hind toes equal in length, and both with a strong curved claw.

SUB-FAM: CIRCINÆ, or HARRIERS.

GENUS. *CIRCUS. Lacep.*

Bill moderate, elevated at the base of the culmen and arched to the tip, which is hooked; the sides compressed, and the lateral margins festooned; the *nostrils* large, oval, and partly concealed by the curved hairs of the bases. *Wings* long, with the third and fourth quills nearly equal and longest. *Tail* long and rounded on the sides. *Tarsi* long, slender, and compressed, the outer side covered with transverse scales, and the inner with small scales. *Toes* moderate, with the outer one longer than the inner; the *claws* long, slender and acute.

SUB-ORDER. ACCIPITRES NOCTURNI.

Nocturnal Birds of Prey.

FAM: STRIGIDÆ, or OWLS.

SUB-FAM: SURINÆ, or HAWK OWLS.

GENUS. *ATHENE. Boie.*

Bill short, partly concealed by the projecting plumes, the sides compressed, the *culmen* much arched to the tip,

which is hooked and acute; the *nostrils* basal, lateral and hidden by the frontal plumes. *Wings* rather long and rounded, with the third and fourth quills the longest. *Tail* moderate and nearly even. *Tarsi* longer than the middle toe, and covered with plumes. *Toes* short, and covered with scattered hairs; the *claws* long, arched and acute.

SUB-FAM: BUBONINÆ, or HORNED OWLS.

GENUS. *EPHIALTES, Keys, and Bl.*

Bill moderate, the base concealed and broad, the sides compressed, with the *culmen* flattened at the base, and curved to the tip, which is hooked; the lateral margins curved; the *nostrils* rounded, placed in the forepart of the cere, and covered by the basal plumes. *Wings* long, with the second, third and fourth quills nearly equal and longest. *Tail* short, and more or less even. *Tarsi* rather long, and covered with short plumes to the base of the toes. *Toes* long, covered with small scales at the base, and with three or four transverse scales at the tip; the *claws* moderate, strong and curved.

GENUS. *KETUPA. Less.*

Bill large, broad at the base, compressed towards the tip, which is hooked, the lateral margins slightly arched; the *nostrils* lateral, placed in the anterior part of the cere, and hidden by the projecting plumes. *Wings* moderate, with the third and fourth quills equal and longest. *Tail* short, and nearly even at the end. *Tarsi* rather long, covered at the base with down, and towards the tips with minute scales. *Toes* moderate, strong, covered with small scales, except at the tips which are furnished with three or four transverse scales, the lateral toes unequal, the *claws* long, strong and curved.

SUB-FAM: SYRNIIINÆ, or OWLETS.

GENUS. SYRNIUM. Sav.

Bill moderate, strong, with the base broad, and concealed by the frontal plumes; the sides compressed, and the culmen arched to the tip, which is hooked; the *nostrils* basal, lateral, and medial, with the opening large and oval. *Wings* long and rounded, with the fourth and fifth quills equal and longest. *Tail* long, broad, and more or less rounded. *Tarsi* short, strong, thick, and densely clothed with plumes. *Toes* moderate, densely plumed to the end of each toe, the *claws* long, acute, and slightly curved.

SUB-FAM: STRIGINÆ, or OWLS.

GENUS. STRIX. Linn.

Bill long, mostly concealed at the base by the projecting plumes; the sides much compressed, and the culmen arched to the tip, which is acute and hooked; the *nostrils* large, partly covered by a membrane, with the opening oval, and slightly hidden by the hairs. *Wings* very long, with the second quill the longest. *Tail* short and generally even. *Tarsi* much longer than the middle toe, slender and covered with short soft plumes. *Toes* long, and covered with scattered hairs; the lateral ones unequal, the outer much shorter than the inner; the hind toe short and thick; the *claws* long, curved and acute.

DESCRIPTION

OF

BIRDS OF CEYLON.

ORDER. ACCIPITRES.

SUB-ORDER. ACCIPITRES DIURNI.

Diurnal Birds of Prey.

FAM: FALCONIDÆ.

a. AQUILINÆ.

AQUILA BONELLII, Temm. The Genoese Eagle.

Syn. Aquila intermedia, Bonelli.

Aquila fasciata, Vieill.

Nisaetus grandis, Hodgson.

Uniform dark brown above, white beneath with dark brown longitudinal streaks on the chin, throat and breast; upper part of inner web of primaries streaked and spotted with white. Tail slaty blue with darker transversal bands, tip palish; under tail coverts white, irregularly barred with rufous brown. Thighs barred with rufous brown. Bill horn colour, cere yellow, tarsi yellow, claws black.

The only specimen of this Eagle examined is one found by Dr. Templeton, it measured 2 feet 3 inches; closed winged, 1 foot 6 inches. *Habitat* not known.

AQUILA PENNATA, *Gmel.* The Pennated Eagle.

Syn. *Aquila minuta*, *Brehm.*

Spizaëtus milvoides, *Jerdon.*

Head and neck light rufous brown with dark medial streaks. Back, wings, and upper surface of tail dark brown. Under parts white, breast and lower part of neck streaked with rufous brown; shafts of feathers of a darker shade. Under wing coverts white, shafts brown, a few brown spots on the outer ones. Upper tail coverts rufous with whitish tips and margins. Extreme tip of tail pale. Thighs and legs white, indistinctly spotted light rufous. Bill slaty blue; toes yellow; claws black. Length 1 foot 6 inches. Wing 1 foot 3 inches.

Shot at Point Pedro by Mr. Edgar Layard. This is one of our rarest indigenous birds of prey.

AQUILA MALAYENSIS, *Reinv.* The Black Eagle.

Syn. *Aquila pernigra*, *Hodgson.*

Neopus perniger, *Hodgson.*

Nisaëtus? ovivorous, *Jerdon.*

Ictinaëtus Malaiensis, *Blyth.*

Uniform black above, rather dusky below (a few white spots scattered on the body and tail of the specimen examined). Tail barred with broad brownish bands. Bill blue with black tip, cere yellow. Toes yellow, claws black. Irides brown. Occipital crest black. About 2 feet 6 inches long.

This is also a rare bird of prey in the Island. One or two only are occasionally seen on the Kandy Hills. The specimen we have examined was procured by Mr. Charles Kelhart, at Kaduganava.

SPIZAËTUS NIPALENSIS, *Hodgson, Gray.* The Beautiful Crested Eagle.

Syn. *Nisaëtus Nipalensis*, crested var. young, *Hodg.*

Nis. pulcher, adult, *Hodg.*

Falco orientalis?

Falco lanceolatus? } *Temm. et Schlegel.*

Head and neck brown; the cheeks and chin have a triple longitudinal marking of brown. Thighs, tarsi and inferior tails coverts transversely barred with dark fawn. A graceful pendant crest, composed of plumes of a brown colour. Bill blue at the base, black at the tip; the cere greenish yellow, the iris golden; the toes yellow, and the nails black. *Hodgson.**

The above description is evidently of a young bird. An adult specimen which I sent to Mr. Blyth has been identified by that Naturalist with *Sp. Nipalensis*. This beautiful Eagle is rarely seen, and that, too, only on the Alpine parts of the Island. The only specimen I was so fortunate as to secure is now in the Museum of the Asiatic Society of Bengal. It is 30 inches long.

SPIZAËTUS LYMAËTUS, var. *Horsf.* The Common Crested Eagle.

Syn. *Falco cirrhatu*, *Gmel.*

Falco cristatellus, *Temm.*

Falco caligatus, var. *Raffles.*

Falco niveus, var. *Temm.*

Nisaëtus Nepalensis, crestless var. *Hodgson.*

This Eagle is seen of several shades of colour and markings. Two most distinct varieties are seen in Ceylon, one much darker than the other and with a more developed crest. The darker variety may be thus described.

* The whole of the under parts, from the head downwards, were beautifully barred rufous in the Ceylon specimen.

Hair brown above, white beneath. Head and neck of a rufous brown with narrow blackish mesial streaks. Chin, throat, and under part of body marked with broad dark rufous brown longitudinal streaks. Tail barred broadly. Under surface of alar and tail quills white and barred with dark brown. Under tail coverts rufous. Under wing coverts white and spotted dark brown. Thighs rufous and barred with pale white. Tarsi white and freckled with brown. Occipital crest black with white tips, 4 inches long. Length 1 foot 10 inches. Wing 13 inches.

The above characters correspond closely with Mr. Blyth's description of a specimen of this Eagle received from Midnapore. (J. A. S. B. vol. 1845, page 175).

The paler and more numerous variety, is not unlike *Falco niveus*, Temm.

White predominating. Head and neck pale white or rufous with narrow mesial streaks of a more or less dark rufous colour. Base of dorsal feathers white and the tips of some of the longer ones also white. The cheeks, chin and throat immaculate. Breast and abdomen with a few rufous spots and mesial streaks. Thighs indistinctly spotted with light rufous. Tarsi white, immaculate. Occipital crest composed of white and black plumes, some of the latter also white tipped. Rather smaller than the dark variety.

There is also a crestless variety of this species, in colour resembling the last described bird.

The crested variety is rather abundant at certain seasons of the year in all the Maritime provinces and lower Kandyan Hills. It is a bold and powerful bird, attacking full grown fowls in the native villages. In the Northern provinces they are frequently seen on the topmost branches of the Banyan tree, where the nests of crows furnish them with dainty food. At Kaduganava this Eagle is often seen soaring above Dawson's monument, sometimes alighting on its summit to devour its prey; it is not however a very common bird on the Kandyan Hills. I procured only a few specimens from

Kandy and Trincomalie, whereas, as many as seven were shot by Mr. Layard on one tree in the Court yard at Point Pedro.

PONTOAËTUS LEUCOGASTER, Gmel. The White Bellied Sea Eagle.

Syn. *Falco leucogaster*, Gmel.
Falco Blagrus, Daudin.
Falco dimidiatus, Raffles.
Falco albicilla, var. Latham.
Ichthyaëtus cultrungis, Blyth.
Haliaëtus sphenurus, Gould.

Head, neck, and abdomen white. Back and wings ashy brown, primaries nearly black; most part of the inner web of secondaries white. Anterior two-thirds of tail dark brown, the rest white. Upper tail coverts brown, slightly tipped with white; lower white throughout; shorter lower wing coverts white, with a few ashy splashes; longer ones ashy brown. Thighs white. Tarsi and toes yellow, claws black. Bill bluish. Length 3 feet 5 inches. Wing 1 foot 9 inches.

This is the largest of our Ceylon Birds of prey, common on the Northern Coasts, rarely seen on the South-Western parts of the Island; very abundant at the Salt lakes of Hambantotte, and the Bay of Calpentyn. Mr Layard writes, "In fact, wherever there is much shallow salt water, particularly if the receding tide leaves bare a large extent of mud, there will this Sea Eagle be found fishing in company with *Haliastur Indicus* and *Milvus Govinda*. Unlike them, however, the Fish Eagle prefers living prey to garbage, and is particularly destructive among the sea snakes, which are very abundant in the sea between Point Pedro and Point Calamanar on the opposite coast. We have frequently seen this bird capturing its prey. It keeps soaring very high, and then descends very rapidly,

with its wings half closed and upraised, its legs pendant, and its body swaying to and fro, like the weight attached to a parachute. When close to the object of its pursuit, the legs are suddenly darted out, the prey seized near the head, the fall arrested, and without touching the mud or water, the noble bird soars upwards with its victim writhing in its claws. If it fails to kill the eel or snake when on high, the booty is dropped, and we have often secured a good specimen of marine snake from this mishap of the bird."

"The Sea Eagle constructs a large nest (to which it adds yearly) among the branches of some large tree, generally an aged Bo-tree (*Ficus religiosa*) is selected, and owes its security to the superstitious fears of the ignorant natives, who, in spite of all our promised reward, have invariably refused to ascend the trees, alleging that the guardian demons of the sacred tree would hurl them headlong down if they attempted to climb the tree for such profane purpose as robbing a bird's nest. We have, therefore, as our own climbing days are over, never been able to procure the eggs of this species." [Mr. Layard in MS.]

PONTOAËTUS ICHTHYAËTUS, *Horsf.* Brown Tank Eagle.

Syn. Falco Ichthyaëtus, *Horsf.*

Plumage in general brownish; quills deep brown; head grey; chin whitish; neck brownish grey; breast and belly pale ferruginous brown; lower part of the latter, the vent and thighs white; tail dusky at the tip. Length 2 feet 4 inches. *Latham.*

The only specimen secured by Mr. Layard is now with Mr. Blyth.

Found in the large tanks in the Northern parts of the Island.

CIRCAËTUS CHEELA, *Latham.* The Undulated Bacha.

Syn. Falco cheela, *Latham* and *Gmelin.*

Hæmatornis undulatus, Vig.

Circaëtus Nepalensis, Hodg.

Falco albidus, Temm.

Falco Bacha, Daudin. (African Race.)

Falco Bido, Horsf. (Malabar Race.)

Head black, occipital crest tipped white or fulvous. Back fuscus brown. Small wing coverts spotted white. Upper tail coverts tipped white. Neck and breast dark rufous brown with palish tips. Abdomen, lower wing and tail coverts rufescent and spotted with small round white spots, (a blackish ring round some of the white spots). Wings and tail barred broadly with black and cream colour. Tip of tail feathers whitish. Thighs rufescent and spotted with white ocelli. Length 1 foot 10 inches. Wing 1 foot 2 inches.

Very common in the Northern parts of the Island. Rarely seen on the Kandyan Hills. There is a lighter coloured allied species, found at Trincomalie, which Mr. Blyth has named

HÆMATORNIS SPILOGASTER, Blyth.

In this species the tips of all the feathers of the head and neck are more broadly tipped with fulvescent white. The breast and abdomen of a lighter rufous colour, and more thickly spotted with white. Mr. Blyth in describing this new species, which I considered only a local variety of *C. Cheela*, observes, that "it is rather smaller than *H. Cheela*, (*Latham, vel H. undulatus, Vigors*) and remarkable for having the under parts as in the adult of that species, while the upper parts, throat and tail retain in the adult the same colouring as that of the young of *H. Cheela*, a phase of plumage which we have never seen among the multitude of Bengal specimens of *H. Cheela* examined, but which is exhibited in the two now received from different parts of Ceylon." Irides yellow.

There is another specimen in my possession which cannot be referred to either *H. Cheela* or *H. Spilogaster*, and which I shall now describe.

CIRCAËTUS, (?) *Sp.*; probably new.

Upper parts like *H. Spilogaster*. Breast with broad dark rufous brown drops, and rufous mesial lines on the outer abdominal feathers. Ventral, lower tail coverts and thigh feathers barred with rufous. Lower wing coverts brown, and ocellated with white. Tail and wing quills as in *H. Cheela*. Bill yellow, with blackish tip. Feet yellow. Irides golden. Length 2 feet 1 inch. Wing 1 foot. Found at Trincomalee in company with *H. Spilogaster*.

HALIASTUR INDUS. *Bodd.* Shiva's Fishing Eagle.

Syn. *Falco Indus*, *Boddaert*.
Falco Ponticerianus, *Gmelin*.
Haliaëtus girrenara, *Veillot*.
Haliaëtus garruda, *Lesson*.
Milvus rotundicaudatus, *Hodg.* (young.)

Head, neck, and breast white with narrow brown mesial streaks. Back, tail, legs, and under tail and wing coverts bright rufous chestnut. Shafts of dorsal feathers, and upper wing coverts black. Wing rufous, with the exception of the outer quills, which are blackish. Bill pale yellow at the tip. Irides pale yellow. Tarsi and toes pale yellow. Claws black. Length 1 foot 5 inches. Wing 1 foot.

This handsome Eagle is found with the common Fishing Kite (*Milvus Govinda*) in all the Maritime provinces, and very abundant at Trincomalee and Jaffna; as many as twenty or thirty are sometimes seen together. Although fish is their favourite food, they are not unfrequently seen to eat carrion, or even to carry away poultry.

SUB-FAM: FALCONIDÆ.

FALCO PEREGRINUS, *Linn.* The Peregrine Falcon.

Syn. *Falco barbarus*, *Linn.*
Falco communis, *Brisson*.
Falco calidus, *Latham*.
Falco lunulatus, *Daudin*.

Upper parts ashy. Head and neck shaded with white; a black patch under the eye. Beneath white with brown or blackish streaks. Wings ashy, inner web of quills barred with white, or rufescent white, upper wing coverts and scapularies, white or rufescent. Under wing coverts white, and barred with brown. Tail and upper tail coverts, ashy brown with white transverse bars; tip white. Bill bluish. Tarsi and toes yellow; claws black. *Female*.

Males are darker throughout and blackish on the head. Dorsal feathers spotted and barred with black.

Length 1 foot 4½ inches. Wing 1 foot.

This widely distributed Falcon is found in the Northern and North-Eastern Provinces. Seldom more than a pair seen at a time. They are generally found on open plains in search of birds and small reptiles.

FALCO PEREGRINATOR, *Sundeval.* The Shaheen Falcon.

Syn. *Falco shaheen*, *Jerdon*.
Falco sultaneus, *Hodgson*.

My indefatigable friend Mr. Edgar Layard obtained a specimen of this elegant Falcon in Saffragam near the foot of Adam's Peak, but which I have not had the pleasure of inspecting.

The young male of this bird is thus described by Dr. Jerdon.

Young male. Plumage above generally of a dark cinereous or dusky blackish hue, darkest on the head, hind neck and cheek striped; most of the feathers are narrowly edged with rufous, those on the lower part of the back and rump more broadly so. There is some rufous on the forehead, and on the back of the head where it forms a sort of crucial mark. Tail paler than the rest of the body, faintly barred with rufous, and tipped the same. Chin and throat pale rufous yellow, unspotted; cheeks of the same tint, with narrow dark stripes. The rest of the plumage beneath bright rufous or chestnut, with longitudinal dark brown markings on the centre of the body, oblong spots on the sides, and arrow-shaped markings on the lower part of the abdomen. Under wing coverts, rufous, barred with brown; quills barred with rufous on their inner webs. Bill bluish, darker on the tip; cere and orbital skin pale yellow; irides dark brown; legs and feet yellow. Length 15 inches. Wing 11 inches. Tail $5\frac{1}{2}$ inches. Tarsus nearly 2 inches. Centre toe and claw $2\frac{1}{2}$ inches. Weight 1lb.

The young female "differed from the male in having the chin, throat and cheeks white, in the rufous edgings to the feathers being very indistinct, and the plumage generally being of a darker hue."

"After the first moult, the markings of the lower surface disappear, except a few on the lower part of the abdomen and leg feathers, the plumage above becomes more of a bluish tinge, and the edgings and barrings disappear; with further moultings, the shade of the plumage above becomes still lighter, and of a slaty blue, the markings beneath vanish entirely, and the rufous tint of the breast becomes paler."

This handsome Falcon "is found throughout the whole of India, from the Himalayas to the extreme south of the Peninsula. It is well known to native Falconers." In Ceylon we presume it is a rare bird, as Mr. Layard only met with one specimen during his eight years' residence in the Island.

TINNUNCULUS ALAUDARIUS, *Brisson*. The Kestrel Falcon.

Syn. Falco alaudarius, *Brisson*.
Falco Tinnunculus, *Linn*.
Falco fasciatus, *Reszius*.
Falco bruneus, *Bechotim*.
Falco rufescens, *Scainson*.
Falco interstinctus, *McClelland*.

Head and neck pale ash with blackish shafts. Cheek paler, a darker streak from the mouth downwards. Back and upper wing coverts cinnamon brown with triangular black spots at the terminal edge of feathers. Wing quills brown, inner web dentilated with white. Under parts of body pale rufous, with black streaks and spots. Under wing coverts white with brown spots. Tail ashy grey with black subterminal band and white tip. Bill bluish, tip black. Irides yellow. Tarsi and toes yellow. Claws black. Length 1 foot 2 inches. Wing $9\frac{1}{2}$ inches. The female is a more elegant bird and differently maculated.

Upper parts rufescent with dark brown streaks on the head, and blackish bars on dorsal feathers and upper wing coverts. Breast and abdomen rufescent white with brown mesial streaks. Wings dark brown or blackish above and whitish beneath; inner web of quills barred, zigzag white or rufous; lower wing coverts white with small brown spots. Tail rufous, with blackish bars, and a broad black subterminal band; tip whitish. Rather smaller than the male.

The Kestrel, a well known bird of Europe and America, is also very common in India and Ceylon. I have found it in all the Maritime provinces, and also at Kandy and Newera Ellia. They are frequently seen in pairs on open plains in search of field rats, toads, and the young of *Monitor Dracana*. I have found the Kestrel's nest on the rocks of Trincomalie, and also on the sea-girt batteries of that old Fort.

HYPOTRIORCHIS CHICQUERA, *Shaw*. The Small Red-Headed Falcon.

Syn. Falco ruficollis, *et* } *Swainson*.
Falco macrodactylus. }
Falco cirrhatus, *var.* } *Latham*.
Fasciated Falcon. }

Head and upper parts of neck dark rufous, under parts of neck white. Back ashy grey. Breast, abdomen, lower tail coverts and thighs white, with dark ashy bars. Wing dark ashy brown; inner web of quills white with ashy bars. Upper wing coverts ashy and minutely barred brown; lower wing coverts white and barred with brown. Tail ashy above with narrow transverse streaks, paler beneath. Tip whitish and a broad subterminal band. Bill yellow, with black tip. Tarsi and toes yellow; claws black.

Length $11\frac{1}{2}$ inches. Wing $7\frac{1}{2}$ inches. A pair of this beautiful Falcon were seen by Mr. Layard at Point Pedro, where even it is a very rare bird.

SUB-FAM: MILVINE.

ELANUS MELANOPTERUS, *Daudin*. Black-Winged White Kite.

Syn. Falco clamorus, *Shaw*.
Falco melanopterus, *Daudin*.
Falco vociferus, *Latham*.
Elanus cærius, *Savigny*.

Above ashy white. Beneath white. Upper wing coverts and ridge black. Tail white, middle feathers greyish. Bill black. Tarsi and toes yellow. Claws black. Irides orange, a black superciliary streak.

Length 1 foot. Wing 10 inches.

This dove-like Falcon is very rare in the Island. I have only one specimen of it from Kandy, and Mr. Layard shot only one near Jaffna.

MILVUS GOVINDA, *Sykes*. The Cheela or Govinda Kite.

Syn. Milvus cheele, *Jerdon*.
Milvus melanotus, *Temm*.
Hiliaëtus lineatus, *Gray*.
Falco ater? *Gmelin*.
Falco niger? *Brisson*.

Dark brown, with a rufescent shade, edge of feathers pale. Beneath with darker mesial streaks. Tail slightly forked, indistinctly barred blackish. Bill black. Tarsi and toes yellow. Claws black.

Length 1 foot 10 inches. Wing 1 foot 4 inches.

This Kite is very common in all the Maritime provinces. I have not seen it on the hills. It feeds chiefly on fish, and the fishermen with difficulty keep these rapacious birds from stealing fish from their nets. As the fishing boats draw near the shore, hundreds of these Kites are sometimes seen to hover around and manage to get a good feed, with very little exertion on their part, in spite of all the resistance the industrious fishermen can offer.

BAZA LOPHOTES, *Temm*. The Cohy Falcon.

Syn. Baza syama, *Hodgson*.
Falco lophotes, *Temm*.
Lophotes Indicus, *Lesson*.

Head black with a crest composed of a few long black feathers. Chin and neck black. Breast rufous, white barred. Abdomen and vent black. The whole of the upper parts black. Wing black with a greenish shade; scapularies rufous and partly white and black. Bill greenish yellow. Tarsi and toes greenish.

Length $12\frac{1}{4}$ inches. Wing 9 inches.

The only specimen I have seen of this small black crested Falcon was a preserved one in Mr. Layard's collection, which he shot, I believe, in the Wanny district.

SUB-FAM: ACCIPITRINÆ.

ASTUR TRIVIRGATUS, *Temm.* The Indian Goshawk.

Syn. *Astur cristatus*, *G. R. Gray.*

Astur Indicus, *Hodgson.*

Astur palumbarius, *Jerdon.*

Spizaetus rufistinctus, *McClell.*

I do not remember seeing a specimen of this bird in Ceylon. Mr. Layard says he saw a live one at Anarnjapooora, and I believe, Mr. Blyth received a preserved specimen of this Hawk from Ceylon.

The following is the original description of McClelland's bird. (*Spizaetus rufistinctus*), which Mr. Gray makes a synonym of *Astur trivirgatus* of *Temm.*

Upper part of the body dark brown, with slight undulations of a deeper tint, breast and throat longitudinally striped with brown; belly and under surface of the wings white, transversely barred with brown; tarsi feathered to the lower third, each feather marked with five transverse bars, the rest shielded; the beak short, much hooked and sharp; claws and toes strong and formidable.

ACCIPITER BADIUS, *Gmel.* Brown's Sparrow Hawk.

Syn. *Falco badius*, *Gmelin.*

Falco Brownii, *Shaw.*

Falco Dussumieri, *Temm.*

Accipiter Dukkenensis, *Sykes.*

Dark rufous brown above, with pale edges to the feathers. Beneath white with brown drops. Wings darker brown on the upper surface; primaries white beneath and barred. Tail ashy and barred with black; lower tail coverts white. Thighs white and spotted slightly. (Young). In the adult, the breast is rufous and barred with white. Bill blue, tip black; irides yellow. Legs yellow. Length 14 inches.

This hawk is known in Ceylon as the "Sparrow Hawk." It is not an uncommon bird in the Kandyan hills and in the Northern Provinces.

ACCIPITER NISUS, *Linn.*

I had once a small live hawk sent from the Kandyan country, but which flew away before I had positively determined the species; it had a close resemblance to the European Sparrow Hawk. Mr. Layard says he has not seen this hawk in the Island. To enable others to identify this bird in Ceylon the following description from Stark's Elements of Natural History is here added:

"Bluish, cinereous above; a white spot on the neck; white beneath, with brown undulations; tail ashy grey, with five bars of blackish cinereous; cere yellowish green; feet and iris yellow. About 12 inches long."

SUB-FAM: CIRCINÆ.

CIRCUS SWAINSONII, *A. Smith.* Swainson's Hen-Harrier.

Syn. *Circus pallidus*, *Sykes.*

Circus albescens, *Lesson.*

Circus Dalmaticus, *Ruppel.*

Falco æquipar, *Cuv., M.S.*

Falco cyaneus, *var. A. Lesson.*

Pale ashy above; white beneath. Inner web of some of the primaries dark brown. Upper tail coverts white, with transverse ashy bars. Tail ashy, indistinctly barred; outer rectrices whitish speckled and barred with ash. Bill blackish. Tarsi and toes yellow; claws black. Length 1 foot 1 inch. Wing 1 foot 2 inches.

Generally found on the plains of the low country, and rarely in the Kandyan provinces. It feeds on small birds, frogs and lizards.

CIRCUS CINERASCENS, *Gould*. The Ashy Brown Harrier.

Syn. Falco cinerascens, *Mont.*
Circus Montagui, *Vieil.*

Ashy brown above. Under part of the neck and breast ashy, with rufous mesial streaks to the feathers of the latter. Abdomen, under surface of tail, lower wing and tail coverts white. Upper surface of tail rufous brown and broadly barred; tip white. Thighs white, streaked. Bill blackish; irides light yellow. Tarsi and toes yellow, claws black.

Length 1 foot 6 inches. Wing 13 inches.

Females are more or less of a rufous brown on the back; they have also a nuchal ring and broader black bars on the tail, and the whole of the abdomen of a light rufous colour. Head rufescent. In young birds the upper tail coverts are white. Probably the latter characters are also present when the bird is moulting. This species in its ashy plumage is not unlike the darker varieties of *Circus Swainsonii*.

The species is common in Kandy and Trincomalee. Reptiles and birds are its chief food.

CIRCUS MELANOLEUCOS, *Pennant*. The White and Black Indian Harrier.

I obtained several specimens of this bird from the open plains in Minery, none of which, however, I now have for description, and therefore give its characters from Latham's work.

Length 16 inches; bill black; irides yellow; head, throat, hind part of neck, and back black; breast, belly, thighs, and rump white; lesser wing coverts white, the middle ones black; the greater and secondary quills silvery ash colour; primary quills black. Tail pale silvery grey; legs rufous.

The female is somewhat bigger; general colour silvery grey; on the wing coverts three round black spots, and three others on the outer webs of the second quills; primaries black; sides of the belly, thighs and vent white, transversely striated with rufous red.

Like the other species of Ceylon Harriers, it is reported to feed on birds, reptiles, and small rats and squirrels.

ORDER. ACCIPITRES.

SUB-ORDER. ACCIPITRES NOCTURNI.

Nocturnal Birds of Prey.

FAM. STRIGIDÆ.

SUB-FAM. SURININÆ.

ATHENE CASTANOTUS, *Blyth*. Ceylon Chestnut-winged Hawk Owl.

Syn. Athene Castanopterus, *apud Blyth*.

Head, neck and breast barred with brown and rufescent white. A white spot under the ears. Back and wings bright chestnut and faintly barred with brown. Abdomen white, maculated with brown spots and streaks. Under wing coverts white, with a few brown spots. Vent and lower tail coverts white. Tail and upper tail coverts dark brown and streaked with narrow whitish bars. Thighs whitish and freckled with brown. Bill yellow. Tarsi yellow. Length 7½ inches. Wing 5 inches.

This Hawk-Owl was added to the Ceylon Fauna by Dr. Templeton. It feeds on small lizards and insects. Not uncommon at Colombo. I have also seen it at Newera Ellis, Galle and Kandy.

ATHENE SCUTELLATA, *Raffles*. The Hairy-legged Owl.

Syn. *Ninox Nepalensis*, *Hodgson*.

Strix scutellata, *Raffles*.

Strix hirsuta, *Temm*.

Strix lugubris, *Titchell*.

Athene Malayensis, *Eyton*.

Upper part of head and neck ashy brown; chin white. Back, scapularies and upper tail coverts rufous brown; dorsal feathers have palish edges and white spots. Primaries have brown above, with pale rufous bars, under surface white barred. Under wing coverts fulvescent and streaked with brown. Under surface of neck and breast rufous brown, with fulvescent white edges to the feathers. Abdomen white with large rufous brown spots. Tail dark ashy brown, with broad blackish bars; tip whitish. Upper tail coverts rufous brown and spotted white; under tail coverts white. Thighs rufous. Bill bluish black, with a yellowish culmen. Irides greenish yellow. Toes yellow, curved, with yellow bristly hairs. Claws black. Length 11 inches. Wing $5\frac{1}{2}$ inches.

Not uncommon in the Cinnamon gardens at Colombo. Feeds on insects and lizards.

SUB-FAM: BUBONINÆ.

EPHIALTES LEMPIJI, *Horsf*. The Lempiji Owl.

Syn. *Strix noctula*, *Reinc*.

Scops Javanicus, *Lesson*.

Scops Lempiji, *Horsf*.

Brown spotted with fulvous and black. Breast and abdomen fulvous with blackish mesial streaks and inconspicuous bars and specks. Vent whitish. Outer web of primaries barred with light fulvous, inner web clouded dusky. Tail

clouded with pale fulvous brown. Tarsi pale, freckled. Bill brown; irides reddish brown. Toes brown, claws brown. Length $7\frac{1}{2}$ inches. Wing $5\frac{3}{4}$ inches.

Some of the specimens are less fulvescent than others, and the blackish spots more numerous, and with a more distinct black patch on the top of the head.

This is the most common of all Ceylon Owls; less common, however, in the higher parts of the Island than in the Maritime provinces. It feeds chiefly on Coleopterous insects.

EPHIALTES SCOPS, *Linn*. The Scops Eared Owl.

Syn. *Scops pennata*, *Hodgson*.

Scops Aldrovandi, *Ray*.

Scops Europeus, *Lesson*.

Rufous brown, minutely speckled and streaked with dusky brown and rufous. Lower parts paler; abdomen, vent and lower tail coverts more white spotted; small white spots on the outer web of primaries; inner web of secondaries white barred. Tail barred and speckled with white and rufous. Bill brown. Irides yellow. Toes brown; claws dark brown. Length $5\frac{1}{2}$ inches. Wing 4 inches.

This elegant little owl is rare in Ceylon. I met with only one specimen (alive) at Trincomalie which Mr. Blyth has seen and identified with *Scops Pennata*, *Hodg*. Mr. Layard has not been so fortunate as to meet with this species, nor has he seen the next.

EPHIALTES SUNIA, *Hodgson*. The Yellowish Red Eared Owl.

Syn. *Ephialtes scops*, *apud Blyth*.

General colour, a bright ferruginous red, with black shafts

to most of the plumage. It is less speckled and barred than *Scops pennata*. An abescent line on the scapularies, with black tips. Abdomen whitish yellow. Rather smaller than the last.

I have not seen this beautiful owl lower than Dimboola (4000 feet). It is not common. Mr. Layard has not met with it in Ceylon. The specimen I sent Mr. Blyth was identified by him with *Scops sunia* of Hodgson, which he still thinks is only a variety of *Ephialtes scops*, Linn.

KETUPA CEYLONENSIS, *Gmel.* The Large Eared Owl.

Syn. *Strix Ceylonensis*, *Gmel.*
Strix Leschenaultii, *Temm.*
Strix Hardwickii, *Gray.*
Strix dumeticola, *Tickell.*
Cultrunguis nigripes, *Hodgson.*

Above light ferruginous brown, with dark brown mesial streaks and pale spots on the dorsal feathers. Under part of neck whitish. Breast and abdomen light rufescent, minutely barred; each feather with a blackish longitudinal streak. Wing quills freckled and barred white and brown. Tail brown, with narrow ferruginous bars; tip rufescent. Bill blackish. Irides bright yellow. Length 1 foot 7 inches.

This large owl is very abundant in the Island, particularly in the Maritime provinces. It feeds on fish, rats, mice, &c.

SUB-FAM. SYRNINÆ.

SYENIUM INDRANI, *Sykes?* The Devil Bird.

I have not seen this bird alive, though it is common enough

in the forests. A specimen of this was many years ago in the Museum at Colombo. Above dark brown, barred. Abdomen whitish, barred. Toes densely covered with plumage. Irides yellow. Claws black. Length about 1 foot.

SUB-FAM. STRIGINÆ.

STRIX JAVANICA. *De Wormb?* The Yellow Barn Owl.

Syn. *Strix flammeus*, *var.*
Strix candida, *Tickell.*

Upper parts ochry yellow, speckled with grey, white and brown spots. Face white, margined with a rufous circle. Under parts yellowish white and spotted with brown. Inner web of primaries indistinctly barred. Tail speckled with dark grey, and slightly streaked. Thighs white. Bill horn colour. Irides pale yellow. Tarsi and toes yellowish brown; claws pale yellow. Length 1 foot 2½ inches. Wing 11 inches.

Found by Mr. Layard in the old Fort at Jaffna. The above description is taken from the only specimen he procured.

APPENDIX.

In order to keep the Ceylon student of Natural History up to other departments of Zoology, in addition to those which the Author is more particularly engaged in, it is his intention to republish in each part of the Prodrômus now in progress of publication, the labours of Naturalists in Europe as they appear in periodical publications of the day. With this object in view, he has much pleasure in adding to this Part characters of new species of Land Shells (collected in Ceylon by Mr. Edgar Layard) from the pen of the celebrated Malacologist W. H. Benson, Esq., and also descriptions of a new species of Mungous by Dr. Gray.

From the Proceedings of the Zoological Society of London.
Annals and Magazine of Natural History, No. 67, July 1853.

Notice of two VIVERRIDÆ from Ceylon, lately living in
the Gardens. By J. E. GRAY, Esq., F.R.S., &c.

The specimens here noticed were brought from Ceylon by
Alexander Grace, Esq., (late Deputy Queen's Advocate, South-
ern Province).

The first is the species which I described some years ago
under the name of *Herpestes Smithii* (Mag. Nat. Hist. 1837,
II.) from a specimen which was living in the Surrey Zoolog-
ical Gardens, now preserved in the collection of the British
Museum: that specimen was said to have been sent from
the Cape of Good Hope, but this must have been a mistake,
as it is quite unknown to Dr. Burchell, Dr. A. Smith, Mr.
Sweet, Dr. Wahlberg, or other Zoologists who have written
on the animals of South Africa. Mr. Grace informs me
that it is an inhabitant of the interior part of Ceylon. It is
by far the most beautiful species of the genus.*

The second is a new species of CYNICTES, which I propose
to call CYNICTIS MACCARTHLE.

Teeth normal. Red brown; hair elongate, flaccid, pale
brown, with a broad, black subterminal band, and a long
whitish brown tip; of hands and feet shorter. Feet blackish
brown, hair white tipped. Claws elongate, slender, com-
pressed, especially of the two middle toes of the fore feet.
Tail redder; hair elongate, one coloured red. Ears rounded,
hairy.

Habitat. Ceylon; Jaffna, North of Ceylon, (A. Grace, Esq.)

* This Mungous I have named in the Prodrômus Faunæ Zeylanicæ,
Herpestes Rubiginosa, and doubtfully cited *H. Smithii* of Gray as a
Synonym. It is now clear that *H. Smithii* is a native of Ceylon, and not
of Africa. I have found it common in the Northern and Southern Pro-
vinces of the Island.—E. F. K.

This species somewhat resembles *Cynictis Melanura* in general colour, but the hairs are much longer, not so adpressed, and when the individual colour of the hair is examined, most distinct.

I have proposed to name this interesting animal after Mrs. MacCarthy, the wife of the Secretary of the Colony, and the daughter of Mr. Hawes, the Assistant Secretary to the Colonies, who is much interested in the study of Natural History, and has kindly sent me several very interesting natural productions of Ceylon.

The skull differs from all the other *Herpestes* that I have examined, in the back of the nape being deeply and sharply notched instead of transversely truncated, the notch in the living animal being filled up with a cartilaginous septum.*

* *Sed non eidi*, E. F. K.

*Characters of new Land Shells, collected by EDGAR L. LAY-
YARD, Esq., in Ceylon.* By W. H. BENSON, Esq.

1. *Streptaxis Layardiana*, nobis.

Testa arcuato-rimata, depresso-ovata, abbreviata, lata, leviter striata, albido-cornea; spira subelevata, apice planulato, excentrico; sutura crenulata: anfractibus 5, convexiusculis, ultimo ad latus deviante, basi convexiuscula, lavi; apertura straminea, subtriangulari uniplicata, marginibus callo lamellam intrantem validam emittente junctis, dextro expanso reflexiusculo, antrorsum arcuato, superne profunde sinuato, columellari et basali reflexis, umbilico intus rugoso-striato.

Diam. major 103, minor 8, alt. 4 mill.

Hab. inter lapides ad verticem rupis Mehintali Insule Ceylon.

In one or two specimens the callus near the upper margin is inclined to form an obtuse tooth at the side of the sinus, which is much deeper than in *H. Perrotetiana*, Petit, from the Nilgherries.

A graphic account of the discovery is contained in Mr. Layard's Journal in page 235, vol. xi. of the 'Annals.' This and the following curious species, of a singular genus, capriciously distributed through the tropical regions of the globe, are the first which have been found in Ceylon. The headquarters of the genus are in South America and Western Africa. The remaining species, of which the locality is known, are solitary in the Nilgherry Hills of South India, at Tavoy on the Tenasserim coast of the Bay of Bengal, in Cochin China, and in the Seychelles and Rodriguez, islands of the Southern Indian Ocean.

M. Petit de la Saussaye objects (Journ. de Conchyl. 1851, p. 369) to the reception of *Streptaxis* as a genus, considering it to be inadmissible in a zoological point of view; and he adduces as an argument for rejection the circumstance of its gradual change into *Helix*, through species which belong to the group, although deficient in the principal character of distortion. The same argument might be used against the reception of most other genera. *Bulinus* and *Achatina* have as little title to separation as this genus and *Helix*; and *Streptaxis* has equal claims to acceptance with Lamarck's genus *Anostoma*, like which the mollusk, to use M. Petit's words, "arriving at a certain stage of growth, abandons in the formation of the shell the regular course followed by the animals of the genus *Helix*."

The separation of such a singular group is, at all events, a great convenience in conchological research.

2. *Streptaxis Cingalensis*, nobis.

Testa arcuata rimato-perforata, depresso-ovata, solida, abbreviata, lata, obsolete costulato-striata, albido-cornea, subtus polita; spira elevatuscula, sutura crenulata, apice convexiusculo; anfractibus 6 convexus, ultimo ad latus devians, basi convexa; apertura subtriangulari, 3-dentata, marginibus callo lamellam intrantem validam emittente junctis, dextro reflexo, antrorsum arcuato, unidentato; superne profunde sinuato, columellari reflexo, ad basin unidentato; umbilico intus striato, linea impressa circumscripto.

Diam. major 11, minor 8, alt. 5 mill.

Hab. in pago Hewagam Corle Insulae Ceylon, inter rimas saxorum.

Nearly allied to the last, but differing in the dentition of the aperture, in the umbilicus, and in the less flattened form. The replication of the sinus at the top of the aperture simulates another tooth at right angles with the laminar plait on the parietes, a character observable also in *S. Perroteti*, and to which *S. Layardiana* also shews a tendency. It is intermediate between those two species in the dentition of the aperture, but wants the additional tooth which occurs at the base, within the aperture, in the first-named species, from which it is moreover much further removed in form than from *S. Layardiana*.

Mr. Layard has communicated the following note on the animal:—"Epiphragm glassy, animal yellow, with a red line on the back, extending up the two superior tentacles, at the tip of which the eyes are situated: all four tentacles clubbed; when crawling the shell is carried nearly level with the back."

The colouring of the animal reminds me of the hues observable in the *Papa* of the Isle of France, as well as in the Indian and Galle species, *P. bicolor* of Hutton. It has already been remarked by others that *Streptaxis* has an obvious affinity with the Mauritian *Papa Pagoda* in the formation of the shell.

3. *Helix ceraria*, nobis.

Testa obtecte perforata, depressa, nitidula, cerea, radiatim rugoso-striata, striis spirallibus remotiusculis decussata, luteo-fulvida, versus apicem rubescente, spira convexiuscula, apice obtusato; anfractibus 4; superne planulatis, ultimo lato, carinato, subtus convexo; apertura magna, late lunata, peristomate simplici, acuto, marginibus callo tenuissimo junctis, columellari arcuato, superne breviter reflexo, perforationem inconspicuam fere tegente.

Diam. major 20, minor 16, alt. 10 mill.

Hab. in agris altis Insulae Ceylon "Horton Plains" dictis.

4. *Helix Pateolus*, nobis.

Testa perspective umbilicata, orbiculato-depressa, lenticulari, supra spurca, albida, subremote radiato-lirata, subtus cornea, radiatim striata; spira planiuscula apice vix prominulo, sutura impressa; an-

fractibus 5½, angustissimis, convexiusculis, arete convolutis, omnibus filoso-carinatis, superne sulco carinaque secunda parallelibus munitis, ultimo subtus planiusculo, ad marginem umbilici mediocri profundè excavati, cyathiformis, valde compresso; apertura angusta, securiformi, peristomate recto, acuto.

Diam. major 5½, minor 5, axis 2½ mill.

Hab. in saxis ad portas "Baladua" dictas, Insulae Ceylon.

This little shell is singular on account of its sculpture, the narrowness of the whorls, and the sudden and deep excavation of the well-like umbilicus. Vide 'Annals,' vol. vii. p. 305.

5. *Helix Mononema*, nobis.

Testa angustissime perforata, trochiformi, radiato-striata, corneo-albida, glabra, non nitida, translucente, spira conoidea, apice obtuso, sutura distincta; anfractibus 6½-7, superne convexiusculis, filo unico, elevato, tenui, mediano cinctis, ultimo acute filoso-carinato, subtus subplanulato; apertura verticali, securiformi, peristomate recto, acuto, margine columellari brevi, verticali, reflexo, perforationem subtegente.

Diam. major 5, minor 4½, axis 4 mill.

Hab. ad Heneratgodde.

Allied to the Himalayan *H. fastigata*, Hutton, and to the Bengal *H. Barrackporensis*, Pir., but well-distinguished by its more depressed form and by the filiform line, which in addition to the keel on the last whorl, runs along the central part of each of the upper whorls. Mr. Layard had not been able to procure a second specimen.

6. *Helix marcida*, nobis.

Testa subobecte perforata, depressa, solida, superne oblique regulariter costulata, subtus leviori, sub epidermide fulvido-cornea albida; spira vix elevata, sutura impressa, apice obtusiusculo; anfractibus 6, lente accrescentibus, ultimo rotundato, subtus planulato, apertura sublate lunata, obliqua, peristomate recto, tenui, margine columellari oblique descendente, crassiusculo, superne breviter reflexo, perforationem subtegente.

Diam. major 14, minor 12, axis 8 mill.

Hab. in Insula Ceylon.

7. *Helix vilipensa*, nobis.

Testa aperte perforata, pervia, depressa, solidiuscula, pellucida, cornea, polita; spira convexiuscula, sutura vix marginata, apice obtuso; anfractibus 5, subplanatis, lente accrescentibus, ultimo rotundato, subtus convexiusculo; apertura late lunata, subobliqua, peristomate recto, acuto, margine columellari oblique descendente, expansiusculo, superne breviter reflexo; umbilico pervio.

Diam. major 8, minor 7, axis 4 mill.

Hab. ad rupem Mehintali.

A little Nanniform *Helix* with no very prominent character, differing from Pfeiffer's *H. Perroteti*, and other small *Helices* of the same group, with the exception of a small un-

named Nilgherry shell which is slightly larger, but otherwise not easily to be distinguished from it. From *H. Perroteti* it differs by its more convex spire, rounder periphery, and more open perforation.

8. *Helix perfucata*, nobis.

Testa vix perforata, depressa, translucente, purpureo-fusca, politissima, spira elevatiuscula, sutura submarginata, apice obtuso; anfractibus 4, convexiusculis, arcte convolutis, ultimo rotundato, subtus convexo; apertura lunata, vix obliqua; peristomate recto, acuto, margine columellari verticali, leviter reflexo, umbilico non pervio.
Diam. major 5, minor 4, axis 3 mill.
Hab. in pago Gallensi, inter saxa.

Remarkable for its deep clear purple-brown colour.

9. *Vitrina Edgariana*, nobis.

Testa valde depressa, tenuissima, subtus membranacea, politissima, pellucida, superne obsolete oblique striatula, lineis nonnullis spiralibus impressis ornata, lutescente-cornea; spira planiuscula, apice prominulo, sutura impressa; anfractibus 3 celeriter accrescentibus, planiusculis, ultimo ad peripheriam valde convexo; apertura obliqua, ovato-lunata.
Diam. major 10, minor 8, alt. 4 mill.
Hab. ad Columbo, Hangwelle, et Ratnapoora, insuper folia arborum.

Mr. E. Layard remarks, that "the mollusk is very long and attenuated, yellow and white. On touching it, it throws itself off the spot where it crawls, and twists and springs about in a most singular manner, often propelling itself several inches from its starting point." I have recorded elsewhere a similar habit in a little freshwater shell, *Planorbis rotula*, nobis.

10. *Vitrina membranacea*, nobis.

Testa valde depressa, submembranacea, obsolete oblique striatula, striis versus apicem confertissimis pellucida, virente-cornea, polita, superne convexiuscula, apice planato, sutura vix marginata; anfractibus 3½, rapide accrescentibus, ultimo antice lato, subtus planulato, ad peripheriam angustam rotundato; apertura obliqua, ovata, margine superiori prorsus arcuato.
Diam. major 11, minor 8, alt. 4 mill.
Hab. ad portas montanas Balcadna dictas, lapidibus adherens.

11. *Achatina Veruina*, nobis.

Testa cylindraceo-turrita, valde elongata, sordide albida, inaequaliter striata, spira superne sensim attenuata, sutura impressa, crenulata, apice obtuso; anfractibus 12½-13 angustis cylindricis, ultimo 2 longitudinis vix aequante; apertura verticali, truncato-ovali, peristomate recto, acuto, margine columellari leviter arcuato.
Long. 25, diam. 5 mill.
Hab. ad Nalande.

12. *Achatina pachycheila*, nobis.

Testa ovato-oblonga, striatula, striis exilissimis confertis, obsolete, spiraler sub lente decussata, nitida fuscocornea, translucente, spira elongata, subconica, apice obtuso, sutura leviter impressa; anfractibus 6, vix convexiusculis, ultimo 2/3 longitudinis vix aequante; apertura verticali, oblongo-ovata, peristomate intus albido-marginata, columella praecurcata, margine basali incrassato.
Axis 11, diam. 5 mill.
Hab. ad Heneratgodde.

This little species of a group, which has so many representatives in Ceylon, the Nilgherries, and the damp woody mountains of North-eastern India, with a more slender form than *A. Oreas*, nobis, is well distinguished from the allied forms by its peculiar sculpture under the lens, and by the internal incrassation of the peristome, a feature which is also observable in the species *A. crassilabris*, nobis, from North-eastern India.

13. *Bulimus Panos*, nobis.

Testa rimata, ovato-oblonga, subrugose striata, cornea, spira elongato-conica, sutura mediocri, apice obtuso; anfractibus 6½ convexiusculis, ultimo 2/5 longitudinis vix aequante, antice subsaccedente, apertura ovata, edentula; peristomate expansiusculo, acuto, marginibus conniventibus, columellari breviter dilatato, albido.
Long. 11, diam. 5 mill.
Hab. ad Nalande.

This shell has a Pupiform aspect.

14. *Pupa Muscerda*, nobis.

Testa rimata, ovato-oblonga, striatula, cornea, spira elongata, apice obtuso; anfractibus 5½-6, convexiusculis, ultimo tertiam partem testae aequante, vix ascendente; apertura ovata, tridentata, peristomate expanso, acuto, marginibus conniventibus, columellari dilatato, albido, plica parietali 1, columellari 1, profundiusculis, dente palatali 1, depresso, obtuso.
Long. 4, diam. 1½ mill.
Hab. ad promontorium "Pedro" dictum.

Found by Mr. E. Layard in old posts, and on Palmyra trees (*Borassus flabelliformis*).

15. *Pupa Mimula*, nobis.

Testa rimata, ovato-oblonga, subcylindracea, oblique tenuiter plicatula, pallide cornea, apice obtuso, sutura profundiuscula; anfractibus 5, convexis, ultimo vix ascendente; apertura ovata, verticali, 5-plicata, peristomate tenui, expanso, marginibus callo tenui expanso junctis, plica 1 intrante, majuscula, parietali, 1 columellari, 1 basali profunda, 2 palatalibus profunda.
Long. 2, diam. 1 mill.
Hab. in vitibus ad Promontorium Pedronis.

A minute species nearly allied to, and somewhat larger than the Himalayan *P. Huttoniana*, nobis. It was taken in abundance by Mr. Layard.

16. *Cataulus Austenianus*, nobis.

Testa subperforata, ovato-pyramidata, solidiuscula, eleganter oblique costulato-striata, sericea, diaphana, saturate rubro-castanea, spira turrita, sutura impressa, apice acutiusculo; anfractibus 8, convexiusculis, ultimo antice subsascedente, carina basali, compressa, prominente, subfuniculata, antice non dilatata; apertura subcirculari, obliqua, sursum spectante, peristomate duplici, incrassato, saturate fusco-aurantiaco, interno continuo, breviter adnato, externo fornicatim patente reflexo, superne et ad columellam subauriculato-dilatato, canali basali aperto, intus vix strictiore, ad sinistram perforato; operculo spiritaliter laminato, anfractus multos appressos reconditos exhibente, facie exteriori anfractus plures angustos mentiente.

Diam. 134, alt. 25 mill.

Hab. ad Honsratgödde in rimis saxorum.

Allied in form and size to *Cataulus pyramidatus*, Pfeiffer, but differs in sculpture, position of the canal, colour of aperture, &c. It was taken by Mr. Edgar L. Layard in the deep fissure of a limestone rock, among the vegetable mould, where it lay concealed under the surface, with the mouth downwards, among the roots of an ancient *Ficus Indica*. In some specimens the last whorl is more swollen than in others.

Mr. Layard has communicated the following description of the animal in a letter written on the spot:—

"The animal is dark brown, mottled, about 9 lines long, 5 broad; tentacles two, brilliant scarlet tipped with brown, 3 lines long, sharp pointed; eyes two, black, situated at the outside of the tentacles. The mollusk is very slow in its motions, crawling with the shell plane over its back, resting on the operculum; this is round and fits into the mouth; it is retractile when the animal is much irritated, but does not close the siphon and canal; when crawling, the flesh of the mollusk does not touch the canal. After placing them in boiling water to clean, I thrust a needle into one and pierced the operculum accidentally; judge of my astonishment, when in attempting to withdraw the instrument, the operculum unfolded revolution after revolution till I counted nine!" Subsequently Mr. Layard counted nineteen whorls in one operculum; and he has aptly likened it to the horn toy, made to resemble a snake by slicing it spirally from end to end, so as form a laminar centreless screw.

At the request of the discoverer, I have much pleasure in dedicating the species to his brother, the enterprising Oriental archaeologist and traveller Dr. Austen Layard.

17. *Cataulus decorus*, nobis.

Testa subperforata, elongato-turrita, solida, confertim costulato-striata, nitidiuscula, rufo-castanea, apice acutiusculo; anfractibus 8, convexiusculis, ultimo ad basin compresso-carinato, carina valde prominente versus marginem funiculata, incrassata, antice vix dilatata; apertura verticali, circulari; peristomate inerassato, reflexo, breviter adnato, hinc aurantiaco, ad basin subproducto, deorsum recedente; canali extus lato perforato; periomphalo magno; operculo ut in precedente.

Diam. vix 10, axis 21 mill.

Hab. ad Ratnapoora.

Mr. Layard wrote regarding this species—"This was found under a rock with two others: animal brownish gray, foot livid. Tentacles pale orange, two; eyes sessile, two, black."

Mr. Layard has, since his return to England, detected in the operculum of *Megalomastoma altum* a structure similar to that which obtains in these *Catauli*, and I have, since the communication of that circumstance, also observed it in the operculum of the Himalayan *Meg. funiculatum*, nobis.

18. *Cyclophorus Parapsis*, nobis.

Testa late et perspective umbilicata orbiculato-depressa, tenui, confertim striatula, olivacea, sub epidermide tenui albida; spira planiuscula, apice vix prominulo, sutura profunda; anfractibus 4 convexiusculis, ultimo cylindrico, leviter descendente, non dilatato; apertura subverticali, subcirculari, intus glaucescente; peristomate continuo, breviter adnato, recto, simplice, acuto; operculo tenui, corneo, arcte spirato, medio concaviusculo, intus obtuse umbonali.

Diam. major 13, minor 10, alt. 5 mill.

Hab. ad Damboul, ad verticem rupis, in rimis saxorum.

It differs from *C. laxostoma*, Pfeiffer (a large and handsomely marked variety of which was found on the same rock), in colouring, epidermis, depth of suture, narrower and deeper umbilicus, and the more circular and vertical aperture, which exhibits nothing of the diagonal departure from the axis observable in Pfeiffer's shell. The peristome also is acute and single, instead of being double, slightly expanded and thickened, as in that species, of which Mr. Layard's specimen is variegated with radiate and undulated chestnut stripes, and with a single band on a fulvous ground.

Annals and Magazine of Natural History, Vol. xii. p. 90.

CATALOGUE
or
CEYLON MAMMALIA.

* * * Doubtful species have been removed from this new Catalogue.
For descriptions see PRODRROMUS FAUNÆ ZEPHANICÆ, VOL. I.

ORDER.
QUADRUMANA.

FAM: SIMIADÆ.

- 1 Presbytis Cephalopterus,
Zimm.
var. P. Albinus, *Kelaart.*
- 2 Presbytis Ursinus, *Blyth.*
- 3 Presbytis Priamus, *Elliot and Blyth.*
- 4 Presbytis Thersites, *Elliot and Blyth.*
- 5 Macacus Sinicus, *Linn.*

FAM: LEMURIDÆ.

- 6 Stenops gracilis, *Geoff.*

ORDER. CARNARIA.

FAM: CHEIROPTERA.

Æ. INSECTIVORA.

- 7 Megaderma Lyra, *Geoff. Desm.*
- 8 Megaderma Spasma, *Geoff. Desm.*
- 9 Taphozous longimanus,
Harde.
- 10 Rhinolophus affinis, (?) *Horsf.*
apud *Blyth.*
3 varieties. R. rubidus, R.
cinerascens, and R. Ram-
manika, *Kelaart.*
- 11 Hipposideros Murinus, *Elliot.*
and 2 var. H. fulvus, *Gray.*
H. atratus, *Kel.*
apud *Blyth.*

- 12 Hipposideros Speoris, *Gray,*
Geoff. and var. H. aureus,
Kelaart.
- 13 Hipposideros vulgaris, *Horsf.*
and 2 var. H. Templetoni,
Kel. H. Blythii, Kel. apud
Blyth.
- 14 Hipposideros nobilis (?) *Horsf.*
vel. *H. Lambadicensis, Kel.*
H. armiger, Hodgson.
- 15 Scotophilus Coromandelicus,
Blyth.
- 16 Vespertilio adversus,
vel. Scotophilus Ceylonicus,
Kelaart.
- 17 Nycticejus Heathii, *Horsf.*
- 18 Nycticejus Belangeri, *Zimm.*
- 19 Nycticejus Tickelli, *Blyth.*
- 20 Kerivoulha picta, *Gray.*

Æ. FREGIVORA.

- 21 Pteropus Edwardsii, *Geoff.*
- 22 Pteropus Leschenaultii, *Dum*
- 23 Cynopterus marginatus, *F*
Cuv. and var. C. Horsfieldii,
Gray.

FAM: INSECTIVORA.

- 24 Sorex Marinus, *Linn.*
- 25 Sorex Kandianus, *Kelaart.*
- 26 Sorex montanus, *Kelaart.*
- 27 Sorex ferrugineus, *Kelaart.*
- 28 Feroculus macropus, *Kelaart.*

FAM: CARNIVORA.

- 29 Prochilus labiatus, *Blaine.*
- 30 Lutra Nairi, *F. Cuv.*
- 31 Canis aureus, *Linn.*
- 32 Viverra Malaccensis, *Gmel.*
- 33 Paradoxurus typus, *F. Cuv.*

- 34 *Paradoxurus Zeylanicus*,
Schreb. and var. *Fuscus*, *Kel.*
35 *Herpestes griseus*, *Sykes*, *Kel.*
36 *Herpestes vitticollis*, *Dennet*.
37 *Herpestes Smithii*, *Gray*,
vel *H. rubiginosus*, *Kel. H.*
Elliottii, *Blyth*.
38 *Herpestes fulvescens*, *Kelaart*.
39 *Cynictis MacCarthiae*, *Gray*.
40 *Felis leopardus*, *Schreb.*
var. *F. melas*, *Peron*.
41 *Felis culidogaster*, *Temm.*
vel *F. viverrinus*, *Bennet*.
42 *Felis rubiginosa*, *Geoff.*
43 *Felis Chaus*, *Guldens*.

ORDER. RODENTIA.

FAM: SCIURIDÆ.

- 44 *Sciurus Macrourus*, *Forster*.
45 *Sciurus Tennanti*, *Layard*.
46 *Sciurus tristriatus*, *Water-*
house, and 3 var. *S. Brodeci*,
Blyth, *S. Layardii*, *Blyth*,
and *S. Kelaartii*, *Layard*.
47 *Sciurus trilineatus*, *Water-*
house.
48 *Pteromys Oral*, *Tickel*.
49 *Sciuropterus Layardii*, *Kel.*

FAM: MURIDÆ.

- 50 *Mus giganteus*, *Hardw.*
51 *Mus Rattus*, *Linn.*
52 *Mus decumanus*, *Pallas*.
53 *Mus Ceylonus*, *Kelaart*.
54 *Mus flavescens*, *Elliott*.
2 var. *Rufoflavescens*, *Kel.*
Kandianus, *Kel.*
55 *Mus memorialis*, *Blyth*.
56 *Mus Asiaticus*, *Gray*.
57 *Mus musculus*, *Linn.*
58 *Mus manicus*, *Gray*.
59 *Mus fulvidiventris*, *Blyth*.
60 *Nesokia Hardwickii*, *Gray*.
61 *Nesokia Kok*, *Gray*.
62 *Golunda Elliottii*, *Gray*.
63 *Golunda Newara*, *Kelaart*.
vel *Golunda melnada*, *Gray*,
apud *Blyth*.
64 *Gerbillus Indicus*, *Desm.*

FAM: HYSTRICIDÆ.

- 65 *Hystrix leucurus*, *Sykes*.

FAM: LEPORIDÆ.

- 66 *Lepus nigricollis*, *F. Cuv.*

ORDER. EDENTATA.

- 67 *Manis crassicaudata*, *Geoff.*

ORDER.
PACHYDERMATA.

- 68 *Elephas Indicus*, *Linn.*
69 *Sus Indicus*, *Gray*.
70 *Sus Zeylonicus*, *Blyth*.

ORDER.

RUMINANTIA.

FAM: MOSCHIDÆ.

- 71 *Meminna Indica*, *Gray*.

FAM: CERVIDÆ.

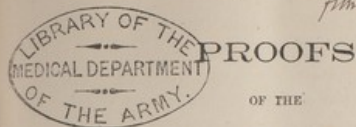
- 72 *Axis maculata*, *H. Smith*.
73 *Axis Oryzus*, *Kelaart*.
vel *Axis porcinus*, var.
74 *Rusa Hippelaphus*, *Cuv.*
75 *Styllocerus Muntjacus*, *H.*
Smith.

FAM: BOVIDÆ.

- 76 *Bos Taurus*, var. *Indicus*, *Linn.*
77 *Bubalus Buffelus*, *Gray*.

ORDER. CETACEA.

- 78 *Helicore Indicus*, *Desm.*
79 *Delphinus velox*, *Dussum.*
80 *Delphinus longirostris*, *Dus-*
ssum.
81 *Delphinus plumbeus*, *Dus-*
ssum.
82 *Phocaena communis*, *Lesson.*

PROOFS
OF THE
NON-EXISTENCE

OF A

SPECIFIC ENTHETIC DISEASE

ADDRESSED

TO THE

SECRETARY OF STATE FOR WAR

BY

DAVID MACLOUGHLIN, M.D.

MEMBER OF THE LEGION OF HONOUR.

&c. &c. &c.

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The object in view for calling the attention of the Secretary of State for War to the following pages, is,

- 1.—That no Medical Practitioner who is acquainted with Medical Science, and who respects himself, can, at the bedside, point out what are the pathognomonic symptoms of a primary syphilitic ulcer, on the genitals, or on any other part of the body.
- 2.—That it follows, that if the best Medical Practitioner, who is acquainted with Medical Science, and who respects himself—cannot, at the bedside, point out what are the pathognomonic symptoms of a primary syphilitic ulcer, on the genitals, or on any other part of the body—how can he point out, at the bedside, the pathognomonic symptoms of secondary syphilis, on any part of the body?
- 3.—In the interest of the army the attention of the Secretary of State for War is called to this subject, as the pathology, the etiology, and the Medical treatment of this so-called syphilitic

disease, has never been scientifically studied by the Army Medical Department, and as their medical treatment is Empirical, and annually commits great ravage in the army, depriving the army of the services of thousands of men, if not destroying the lives of hundreds.

- 4.—And the attention of the Secretary of State for War is called to this subject, as he is in the position to have the pathology, the etiology, and the Medical treatment of this so-called syphilitic disease scientifically studied; and to render, thereby, the greatest service that can be rendered to humanity in general, and to the army in particular.

DAVID MACLOUGHLIN, M.D.
Member of the Legion of Honour.
&c., &c., &c.

London, 14th January, 1863,
 34, Bruton Street, Berkeley Square, W.

To the RIGHT HONOURABLE THE SECRETARY OF STATE FOR WAR,
 EARL DE GREY and RIPON, &c., &c., &c.

MY LORD,

The War Office has, for these some years past, inquired with great attention into the means to improve the hygienic state of the army.

But there is another question of greater importance which is the base of all sanitary measures, and which the War Office has neglected to investigate—I allude to the pathology of the diseases which particularly afflict the army.

The War Office assumes, that all diseases to which the soldiers are liable are well known; and that the Medical officers of the army are well acquainted with the pathology, and with the Medical treatment of such diseases.

Therefore, when an epidemic of any kind breaks out amongst the troops, no commission, composed of Medical Officers, well acquainted with Medical Science, is appointed to go to the bedside—there to study the rise and progress of the disease—to collect facts, and to draw rational conclusions from these facts, and to publish these facts and conclusions for the benefit of the public and of the army.

But, as was seen in India, in 1861, a commission is

appointed to inquire into the outbreak of an epidemic—not composed of Medical officers, but composed, in majority, of gentlemen who have never studied Medical Science, who do not know a disease if they went to the bedside; and of gentlemen who have the name of Medical Gentlemen, but who have not the experience of Medical Practitioners.

Or, an important medical inquiry is referred to one gentleman, not to go, however, himself to the bedside and there to study the rise and progress of the disease, but to receive the reports of other Medical Gentlemen, and without being aware if these reports are correct, to draw up his report—which report is published by the Government, and which, consequently, commands respect; and is accepted, without examination, as correct by the whole world; and after having destroyed millions of the human race, is at last found to be the result of untutored imaginations.

I refer to the report on cholera, published in 1820, by Mr. James Jameson, under the patronage of the Indian Government; and also to that report on cholera, published in 1824, by Mr. William Scot, and also published under the patronage of the Indian Government.

These two gentlemen, in their reports on cholera, completely overlooked the first stage—the most essential stage of the disease—and fixed their attention, and that of the whole Medical world, on the last stage of the disease—that, when, too often, the heart has ceased to contract—when the blood has ceased to circulate—when, too often, the individual is past all human aid; and

then they recommended a medical treatment completely opposed to the pathology of the disease, and which assists the disease to destroy life. And although forty-six years have elapsed since these errors were committed by the above two gentlemen, and that thousands and thousands of men belonging to the army have been destroyed by this disease, assisted by the medical treatment, the War Office has never attempted to have this disease scientifically studied; and if it were the will of Providence to inflict another outbreak of this disease in the army, it would find the army medical department no better prepared to meet such an event than they were in 1817.

But not only has the War Office never attempted to have the pathology and the medical treatment of cholera scientifically studied, but it has, without examination, repudiated the scientific studies relative to the pathology and to the medical treatment, which were undertaken to benefit the public and the army, and which have received the approbation of the scientific Medical world.

It has been stated above that Messrs. James Jameson and William Scot, by their reports on the pathology and medical treatment of cholera, had overlooked the first, the most essential stage of the disease, and had fixed their attention on the last stage of the disease.

In 1853 the first scientific inquiry, in any country, was carried out here in London, under the eyes, and with the valuable assistance of the Registrar-General Major Graham, and the gentlemen of his department,

as to the pathology and medical treatment of cholera ; and the result was the proof that Messrs. James Jameson and William Scot had misled the Medical world as to the pathology and Medical treatment of cholera, and, I repeat, had been the cause of the destruction of millions of the human race.

And the results obtained by the inquiry in 1853 were confirmed by the inquiries carried out by all the Medical Practitioners in charge of Hospitals, Unions, etc., etc., in England and Scotland.

See the Registrar-General's Weekly Report of Births and Deaths for 1853 and 1854.

See the Reports at the General Board of Health from all the Hospitals, Unions, etc., etc., in England and Scotland for 1854.

When the army was in the Crimea, and almost paralysed by cholera, all that an individual could do was done to render the result of these researches useful to the army. But no notice was taken of his representations, and it followed that the Commander-in-chief and thousands of men of that army were hurried into untimely graves.

When, in 1861, cholera broke out in the army in India, attention was called to the necessity to have this disease scientifically studied by the Army Medical Department.

But the answer received was—"that the pathology, and that the Medical treatment of this disease, were well-known to the Army Medical Department, and that no further study of this disease was required."

And it followed that the views of Messrs. James Jameson and William Scot, as to the pathology and Medical treatment of cholera, were adhered to, and that the country had to deplore the loss of above 3,000 valuable lives.

By the Army Medical State's Report for 1861, it is seen that another disease, not less injurious to the army than cholera, rages in the army—I refer to syphilis.

The War Office has assumed that, as syphilis has occupied the attention of the civilised world for nearly four hundred years, consequently that the pathology and the Medical treatment of this disease are also well known, and require no further study.

But, with your lordship's leave, I will place before you the opinions of the first French and the first English Medical Practitioners, on the pathology and the Medical treatment of this so-called syphilitic disease ; and I will analyse their opinions, and I will point out the result of this analysis for your lordship's information.

These gentlemen are answerable for their opinions—I am answerable for the analysis of their opinions.

When this is done, your lordship will see, that in the interest of humanity, that in the interest of the public, and that in the interest of the army, this so-called syphilitic disease ought to be studied scientifically by the Army Medical Department.

But, before bringing under your lordship's notice the opinions of the most distinguished Medical Practitioners with whom I have had opportunities to consult for nearly fifty years—relative to the pathology and Medical

treatment of the so-called syphilitic disease—permit me to place before you what have been the opportunities I have had to see, and to study, this disease at the bedside. Others will tell you what right I have to express an opinion on its pathology, and on its Medical treatment.

I have served in the army, and I have had my share of practice—whether in the army in this country, or on foreign service—or in private practice in Paris, or in London.

When I entered the army, in 1811, I was sent to Portugal. Then all ulcers on the genitals were considered to be syphilitic, and the Medical treatment was mercury, till salivation was induced. The consequence was, that many lives were lost, scores and scores of men suffered the cruellest mutilation that man can suffer, and hundreds and hundreds were rendered unfit for military duty.

At the same time that the Portugese Medical officers treated these ulcers on the genitals as non-syphilitic ulcers; by simple ablution, and without mercury, they cured their patients sooner than us, and without any bad consequences. See *Dr. Ferguson's—Inspector-General of the Portugese Army Medical Department—paper, in the 4th vol. of the Medical and Surgical Transactions, published 1819.*

After the peace in 1814, I was doing duty in Dec. 1814, at Fort Pitt Hospital, Chatham. In January, 1815, two men were admitted into my wards one morning, both having had connection with the same woman the day before—both had ulcers on the genitals.

With these two men I determined to try the experiment, to administer mercury to one, and to do nothing for the other—but to order him to keep the ulcers clean by ablution, night and morning.

This man was discharged, cured, eight days after his admission, and he might have been discharged three days before, but I kept him in hospital to observe what might occur. I accidentally saw this man some months after perfectly healthy.

The unfortunate man, to whom I administered mercury, went from bad to worse; his ulcers increased. I obtained the best Medical advice for him I could lay my hands on. No expense was spared; and had he been the Regent of England, he could not have had more zealous and more devoted care bestowed on him than he received from all the Medical officers of the hospital at Fort Pitt.

But mortification came on, and two months after his admission we had to lament his loss.

After the army entered Paris, in 1815, I was attached to the hospital at St. Denis, where I had charge of the syphilitic patients.

While at St. Denis, I took the opportunity to visit and to attend the civil hospitals in Paris, devoted to the treatment of the primary and secondary stage of this so-called syphilitic disease.

In the beginning of 1816, I was on duty at the General Hospital at Valenciennes. The garrison of Valenciennes was composed entirely of English troops. The municipal law, relative to the fallen

women, was rigidly carried out by the French authorities.

Yet an epidemic outbreak of ulcers on the genitals occurred, that could not be accounted for on the score of connection with infected females. See *Dr. Evans'—then Surgeon of the 57th Regiment—Report*, published in 1819.

I had my share of such cases in the General Hospital. I gave no mercury, and my patients were cured rapidly, by attention to ablation of the ulcers night and morning, and by keeping the patient in bed.

Since 1816, either while I remained in the army, or since I have been in private practice—now forty-five years—I have never prescribed one grain of mercury, for the cure of ulcers on the genitals, and I have the satisfaction to meet, occasionally, friends and former patients of mine—so treated without mercury, for ulcers on the genitals,—themselves, their children, and their grand-children, perfectly healthy.

On the return of the army of occupation from the north of France, in 1818, I was placed on half pay. I settled in Paris as a Medical Practitioner, and for seven-and-twenty years, I had the opportunity to see this so-called syphilitic disease, both in the public hospitals and in private practice, and to consult with the first Medical Practitioners in France; who had, and who have European reputations, on this disease. And with your lordship's leave, I will now place before you, in alphabetical order, the names of these gentlemen, with their

opinion on the pathology and Medical treatment of this so-called syphilitic disease.

ALIBERT, Doctor and chief Physician to the Hospital St. Louis, in Paris, well known in the Medical world as the author of a celebrated work on the diseases of the skin, with plates.

This gentleman was in the habit of stating in his public lectures at his hospital—and I have had opportunities to hear him repeat the same in private consultations—that he considered all ulcers, on the genitals, or any chronic ulcer on any other part of the body, or any cutaneous eruption on the body, which could be cured by mercury, to be syphilitic.

But if he were informed that the patient never had had connection, his reply was, "*he has inherited this disease from his father, or his mother, or from his grand-fathers, or grand-mothers, &c.*"

BIET, Doctor, also one of the Physicians of the Hospital St. Louis, in Paris, and also one of the best authorities on diseases of the skin.

His opinion was that no Medical Practitioner could point out the diagnosis between a primary syphilitic, and a non-syphilitic ulcer on the genitals; or on any other part of the body.

But that every medical practitioner, acquainted with his profession, could point out the diagnosis between a syphilitic and a non-syphilitic eruption on the body—in other words, secondary symptoms of syphilis.

CULLERIER, Doctor, (Nephew) Médecin de l'Hopital du Midi à Paris (Lock Hospital of Paris).

This gentleman stated—both at the bedside, in his hospital, and in private consultations—that he considered all ulcers on the genitals, or chronic ulcers on the lips, the tongue, or in any part of the throat, as syphilitic, and to be treated by mercury.

DUPYTRAIN, Baron, Professor of Surgery, and chief Surgeon to the Hospital (Hotel Dieu, in Paris), with whom I have had frequent opportunities of consulting, relative to this supposed syphilitic disease. He used to say, that the very fact that an ulcer was on the genitals, was for him the proof that it must be a syphilitic ulcer.

But if the patient told him he had not had any connection for a year, then he said that the individual had caught the disease at the water-closet; and every eruption in the skin, which lasted above ten days, was, according to him, syphilitic; as also ulcers in the throat, on the tongue, &c., and to be treated by mercury.

MARJOLIN, Doctor, Professor, and chief Surgeon at l'Hopital Baujon. At an important consultation in 1827, where there were thirteen Medical Practitioners, he admitted that he was not aware that there was any Medical Practitioner that could point out the diagnosis between a primary syphilitic, and a non-syphilitic ulcer on the genitals, or on any other part of the body.

RECORD, Doctor, Surgeon de l'Hopital de l'Urbine—also a Lock Hospital. At a public consultation, two-and-twenty years ago, at which were present some Medical Practitioners, now of London, Dr. Record was brought to admit that neither by the eye, or by the touch, could he establish a diagnosis between a primary syphilitic

and a non-syphilitic, ulcer on the genitals, or any other part of the body. But that he could establish this diagnosis by *inoculation*.

ROUX, Doctor and Professor, Surgeon-in-Chief of Hopital de la Charité. He also considered that all ulcers on the genitals, and all chronic ulcers on any part of the body, and that all cutaneous eruptions, and all ulcers in the throat, that could be cured by mercury, were syphilitic.

It is seen above, that we have here before us the opinion of seven Medical Practitioners, who, in France, were, and are, held up as the best authorities on the pathology, and on the Medical treatment of this so-called syphilitic disease, and whose opinions are looked up to as authorities in the Medical world, in all countries, up to this day.

From what has been stated above, four of these gentlemen, Drs. Alibert, Cullerier, Baron Dupytrain, and Professor Roux, never attempted to study the pathology of this so-called syphilitic disease, so as to be able to establish a diagnosis between a primary so-called syphilitic, and a non-syphilitic, ulcer, on the genitals, or any other part of the body. They assumed that all ulcers, on the genitals, were syphilitic; and that all chronic ulcers, on any other part of the body; and that all cutaneous diseases, that could be cured by mercury, were syphilitic.

These opinions are empirical, not scientific; and deserve no attention from pathologists, and from scientific Medical Practitioners.

It was stated above that Professor Marjolin, at an important consultation in 1827, where there were thirteen Medical Gentlemen consulted, admitted that he was not aware that any Medical Practitioner could point out the diagnosis between a primary syphilitic and a non-syphilitic ulcer on the genitals, or on any other part of the body; and that for his part he did not know of any.

In a not less important case, Dr. Biet gave it as his opinion that no Medical Practitioner could point out the diagnosis between a primary syphilitic and a non-syphilitic ulcer on the genitals, or any other part of the body.

But that every Medical Practitioner, acquainted with his Profession, could point out the diagnosis between a syphilitic and a non-syphilitic eruption on the skin.

The case for which he was consulted was the following:—

A young gentleman, aged 16 years, had connexion with a female of the town. This was the first time he ever had had connexion. The next day he had connexion with another female of the town.

The day after he presented himself to a Medical Practitioner, with an excoriation on his prepuce. He was directed to have a warm bath, to wash the excoriation night and morning with warm water, and to take some cooling medicines.

Three days after this he was quite well.

Three days after, being quite well, he dined at a

public-dinner. He got intoxicated, so much so as to be obliged to be carried to bed. During the night he was taken very ill, with vomiting and purging, and with a severe attack of nettle-rash. He was so ill that a consultation was called. These gentlemen prescribed a warm bath and some medicines, which eased him very much, and he had some hours sleep.

In the morning his whole body was *maculated*, wherever he had rubbed himself strongly, in consequence of the nettle-rash ecchymosis had occurred. The Medical Gentlemen in attendance on him pronounced this ecchymosis to be secondary symptoms of syphilis. Other Medical Practitioners were called into consultation, who expressed a doubt that this was a case of secondary symptoms of syphilis; and, by mutual consent, Dr. Biet, then the highest authority on cutaneous diseases, was called in.

He at once pronounced these ecchymosis to be true eruptions of secondary symptoms of syphilis.

It was certain that this young gentleman never had had connection but with these two females of the town.

These two females were found. They were carefully examined by Dr. Biet and the gentlemen composing the consultation. They were found perfectly healthy.

Their register at the police was referred to, and they never had been reported as being attacked with ulcers on the genitals. The Surgeon-Inspector was requested to examine them, and he reported them as being in perfect health.

Thus, therefore, it is evident that the best authority, then in France, on cutaneous diseases, was not aware what are the pathognomonic symptoms of syphilitic eruptions on the body.

Dr. Ricord, whose name is mentioned above, is a gentleman well-known in the Medical world, as having paid great attention to the pathology of the so-called syphilitic disease.

It is stated above, that two-and-twenty years ago, at a public consultation in Paris, relative to the pathology of this disease, Dr. Ricord admitted that there were no pathognomonic symptoms by which it was possible, either by the examination of the ulcer, by the eyes, or by the touch, to establish the diagnosis between a so-called primary syphilitic and a non-syphilitic ulcer on the genitals, or on any other part of the body; but that he could establish this diagnosis by *inoculation*.

Thus, he said, if the ulcer is syphilitic; the pus taken from this ulcer and introduced under the cuticle, by inoculation, will produce an ulcer similar to that from which the pus has been taken.

If the ulcer is not syphilitic, the pus, taken from this ulcer, will not induce, by inoculation, an ulcer.

But Dr. Ricord forgot to state, that if the pus is taken from a supposed syphilitic ulcer, while in a state of active inflammation, it will induce an ulcer, by inoculation; but that if the pus is taken from this ulcer, when it is in a state of chronic inflammation, it will not induce an ulcer.

Is it not known to every surgeon, that a common

wound, on any part of the body, will, while in a state of active inflammation, secrete pus that, too often, inoculates the surgeons' fingers or hands, and induces suppuration and troublesome ulcers, if not the loss of the hand, or the loss of life?

Therefore, with these facts before us, we must conclude that *inoculation* is no proof that an ulcer on the genitals, or on any other part of the body, is syphilitic; and, therefore, we must conclude, that since the above four Medical Gentlemen, Dr. Alibert, Cullerier, Baron Dupytrain, and Professor Roux, although they had European reputations, as they never had studied the disease scientifically, and as they treated it empirically, we must conclude, I repeat, that they were totally unacquainted with pathology, and with the rational Medical treatment of this so-called syphilitic disease.

We must further conclude that Professor Marjolin, on his own admission, doubted the existence of this so-called syphilitic disease.

Again, we must further conclude that Dr. Biet, by his own showing, knew nothing of what are the pathognomonic symptoms of a primary syphilitic ulcer; and we have seen above, that in an important case, he signally failed to point out what are the pathognomonic symptoms of secondary syphilitic eruptions.

And last, by Dr. Ricord's own admission, he was unable to point out, either by the examination by the eyes, or by the touch, what are the pathognomonic symptoms between a primary supposed syphilitic, and a non-

syphilitic, ulcer on the genitals; or on any other part of the body.

And as to being able by inoculation, to establish this diagnosis, it is evident that Dr. Ricord has brought forward an error, to maintain another error.

And, finally, it follows, that in France, the Medical Profession know nothing of the pathology of this so-called syphilitic disease, and that their Medical treatment is empirical.

With your lordship's leave, I will now place before you, the names and the opinions of the first English Medical Practitioners, who have a right to have, and to give, an opinion on this pathological question.

But, before doing so, I feel it a duty to express my grateful acknowledgments to the gentlemen who have been so kind as to favour me with their opinions on the question I am now endeavouring to call attention to and who have allowed me to make what use I pleased of their opinions.

As I am seeking, only, for truth; if, by careful examination of their opinions—founded on careful recorded facts—I can throw any light on the pathology of syphilis, they will be the first to thank me.

If I fail, they will be the first to be grateful to me for having mooted this question.

The deplorable event which occurred in the wards of the Hospital at Fort Pitt—of which I had charge in the beginning of 1815—as stated above, caused me to pay

more attention to the pathology of this so-called syphilitic disease—than is generally done; and since 1815, I never lost an opportunity to ascertain the opinions of the best informed Medical Practitioner on the pathology of this disease; and your lordship has above the opinions of the best Medical Practitioners in France

And the same motive, which prompted me to study zealously this disease while I resided in France, prompts me now to call your lordship's attention to this so-called syphilitic disease, as you are in a position to render humanity in general—and the army in particular—the greatest service that can be rendered.

But, before I submit to your lordship, how you can benefit humanity, and the army, permit me to place before you the opinions of twelve of the first English Medical Practitioners, who also have European reputations—on this question.

BELL, SIR CHARLES, Professor of Surgery.

I was invited, in 1818, to hear a lecture on syphilis, delivered by the late Sir Charles. He had heard what the Army Medical Officers had observed in the Portuguese Army, that is, to consider ulcers, on the genitals, as simple, and not caused by a syphilitic virus, and so treated these with success—without mercury, and without any bad consequences.

Sir Charles fully expatiated on this, to him, error. He pronouncing the so-called Hunterian chancre as pathognomonic of the existence of a syphilitic virus, and to be cured, only, by mercury.

COOPER, SIR ASTLEY, Bart.

In 1818, I brought a friend to consult Sir Astley, and, in the course of conversation, I put the question to him; what were the symptoms, according to him, pathognomonic, of a primary syphilitic ulcer?

He unhesitatingly informed me—that he knew of no symptom to establish a diagnosis between a syphilitic and a non-syphilitic ulcer.

That he had, long since, made it a rule, in the interest of his patients, not to give a decided opinion on this subject.

That if he were to pronounce, at once, that the ulcers before him were but common excoriations—or common ulcers, and required no medical treatment but ablution and rest, such was the dread of syphilis, that his patients might seek for other advice, and might fall into the hands of empirics, who might destroy their constitution by mercury.

Therefore, he hesitated to give a decided opinion. He was guided in his plan of treatment, according to the impression on his patient's mind. If his patients were under alarm, as to the nature and consequence of their ulcers, he prescribed a very small quantity of mercury, internally, to satisfy his patients that something was done to eradicate the virus; in the meantime ablution, rest, etc., was enjoined, and a cure soon followed.

If his patient was a strong minded man, he enjoined rest, ablution, and some cooling medicines; and success here, also, attended this practice.

COOTE, HOLMES, Esq., Surgeon and Lecturer on Surgery at the St. Bartholomew's Hospital, states,

- 1.—That he believes in the existence of one syphilitic virus, only; and that it is generated in the female, wherever the condition attending prostitution prevails—this is, one woman receiving many men.
- 2.—That this poison produces ulceration; the character of the ulcer being chiefly dependant upon the nature of the tissue on which it is seated.
- 3.—The ulcer, with the indurated base, is, almost without exception, found on the loose tissue, connecting the prepuce and the glans penis.
- 4.—The induration ceases when the ulcer is situated on the firm tissue of the glans.
- 5.—The absence or presence of the hard base constitutes no distinction whatever between syphilitic and non-syphilitic ulcers.
- 6.—The indurated ulcer is rarely seen in women, in whom constitutional symptoms are as common.
- 7.—The "soft chancre" is equally the result of the syphilitic poison, and liable to be followed by constitutional syphilis.
- 8.—He considers that syphilitic ulcers present appearances by which they may, almost always, be recognised.
- 9.—The test of inoculation is a liable source of fallacy.
- 10.—All sores may be successfully treated without mercury; but some require it more than others.

THOMAS CURLING, Esq., F.R.S., Surgeon, London Hospital, says,

That it is often very difficult to ascertain by the eye, if an ulcer on the genitals, or on any other part of the body, be syphilitic or not.

That according to him there are two kinds of syphilitic ulcers.

- 1.—That ulcer having a hard base, and known by the name of *Hunterian chancre*.
- 2.—That ulcer, having no hard base, and known as the soft ulcer.

In the first ulcer, that with the hard base, there will occur buboes; but these seldom ran into suppuration. This ulcer is generally followed by secondary symptoms.

The soft ulcer, on the genitals, is, almost always, followed by buboes, which soon suppurate, and are often difficult to be cured.

He doubts that inoculation can assist, as a true diagnosis, as to what ulcer is, or is not, syphilitic.

He believes that syphilis can remain dormant in the constitution for years; then to break out on the individual, and manifest itself in his offspring.

FERGUSON, WILLIAM, Esq., F.R.S., Professor and Surgeon, King's College Hospital, &c., &c., &c., says—

As to the primary syphilitic ulcer on the genitals,

That he considers the so-called Hunterian chancre as the best proof we have that it is caused by a syphilitic virus.

That yet, he has seen ulcers on the genitals, with a hard base, which were not syphilitic.

Therefore, that it requires great caution, before pronouncing that an ulcer on the genitals is syphilitic, or non-syphilitic.

As to the secondary symptoms of the so-called syphilitic disease, too much caution cannot be exercised by the Medical Practitioner—that such, and such symptoms, are secondary symptoms of syphilis.

And as to establishing a diagnosis between a syphilitic and a non-syphilitic ulcer on the genitals, by the eye, or by the touch, or by inoculation, he does not believe that this can be done.

GASCOYEN, GEORGE GREEN, Esq., Assistant-Surgeon, Lock Hospital.

He has no doubt that the ulcer, on the genital organs, which has a hard base, and which goes by the name of the Hunterian chancre, is a true syphilitic ulcer.

But he adds, that there is another ulcer, which is also syphilitic.

This has no hard base. It is, as it were, punched out of the parts. It is readily inoculable. There are small, unhealthy granulations at the bottom of the ulcer, often accompanied by buboes. The discharge from this ulcer resembles ordinary pus—is of a pale yellow colour, &c., &c.

The first ulcer—that known by the name of the Hunterian chancre—if not promptly and properly cured, is followed by secondary symptoms. These are ulcerated in the throat, or ulcers on the lips, or tongue, eruptions on the skin, of a pale yellow colour, falling off of the hair, &c., &c., &c.

He believes that the second kinds of syphilitic ulcer, if left to itself, will, often, be cured by the efforts of nature, and is not, when promptly cured, usually followed by secondary symptoms.

As to the possibility of establishing a diagnosis between a syphilitic, and a non-syphilitic ulcer on the genitals, by the mere ocular examination, he believes this, in the majority of cases, impossible; and as to inoculation, he believes to be, *per se*, the best test—though not infallible.

He considers that there is no *one* certain means of establishing a diagnosis between a syphilitic, and a non-syphilitic ulcer; but that by making a careful examination, and attentively noticing the several diagnostic points, between the two kinds of ulcers, in the majority of instances, it is possible to distinguish the one from the other; and, finally, he believes that syphilis, once acquired, can lay dormant in the constitution, and be transmitted to the offspring.

LANE, JAMES ROBERT, Esq., Surgeon to the Lock Hospital:—

Question 1.—Two ulcers on the genitals being given, one a self-created ulcer; the other, the result of supposed impure connexion. Can a diagnosis be established between these two ulcers, by the mere inspection by the eyes?

Answer.—An ulcer, originating spontaneously; as, for instance, in Herpes. Or an ulcer produced by mechanical abrasion in sexual intercourse, as from the prolonged contact, with irritating secretions; as, for

instance, gonorrhœal discharge, cannot, in all cases, be distinguished at once by ocular inspection from a true syphilitic ulcer. But the progress of the case will, in most instances, soon clear up the doubts.

Question 2.—Is inoculation a certain test, that the ulcer, from which the matter was taken, is a syphilitic ulcer?

Answer.—Inoculation, the result being *positive*, is a certain test that the ulcer, from which the matter is taken, is truly syphilitic. But the negative result of inoculation does not necessarily prove the non-syphilitic character of the ulcer, since the inoculation may have failed from various causes.

Question 3.—Is it your opinion that syphilis can remain dormant in the constitution, so as to be transmitted to the third or fourth generation, without having manifested itself in the intermediate generations?

Answer.—My opinion is decidedly against the possibility of any such transmission.

LAWRANCE, WILLIAM, Esq., F.R.S., Senior Surgeon to the St. Bartholomew's Hospital. etc., etc., etc.

Question 1.—What are the diagnostic symptoms between a primary syphilitic and a non-syphilitic ulcer on the genitals, or on any other part of the body?

Answer.—This question is too vague to admit of any satisfactory answer. Which of the several primary syphilitic ulcer or ulcers is meant? and what is the affection alluded to as a non-syphilitic ulcer?

Primary syphilitic affections include excoriations, ulcer-

ations—generally superficial—varying in size, form, number, and other details; or superficial ulcer, accompanied by induration, varying in the degree or situation of the hardness, phagedane and sloughing. They differ from other diseases of the same parts, not only in local characters, but also in their history and in their progress.

The nature of syphilis, as I understand it, includes, 1st.—Primary sores, occurring after an interval varying from a few days to six or seven weeks, from connexion with a diseased person who indulges in, more or less, promiscuous intercourse. There are other less frequent modes of infection, such as application of the poison to a raw surface; inoculation; or communication from a pregnant mother, labouring under constitutional symptoms, to her offspring. 2nd.—The possible communication of the disease, so contracted, to healthy persons. 3rd.—The occurrence of secondary symptoms in an uncertain and undefined number of the primary cases.

Question 2.—Can inoculation assist us in establishing a diagnosis between a primary syphilitic and a non-syphilitic ulcer?

Answer.—I have never practised inoculation of syphilis, having originally felt a repugnance to the proposal, having seen or heard of very serious mischiefs from the proceeding; which seems to me, from the published of others, perfectly useless as a means of diagnosis or guidance on treatment.

Question 3.—In all cases of syphilis, is mercury the chief medicine to which you trust?

Answer.—Mercury is not only useless, but hurtful, in the sloughing primary affection, which is easily managed by other means; and has not been followed, in my experience, by secondary symptoms. I think mercury the best general remedy in other forms, without believing it absolutely essential, or resorting to it in all cases, or under all circumstances.

Question 4.—Do you believe that syphilis, once acquired, and not radically cured by mercury, can remain dormant in the constitution, and be transmitted to the offspring?

Answer.—This question includes the subjects of radical cure; the length of time during which the disease may be said to remain dormant, with liability to re-appearance or communication, and the transmission of it to offspring. It is extremely difficult to collect a sufficient quantity of clear evidence on these points to form the base of positive statements. It would be necessary to know, accurately, in each case, the state of health of two or more persons, for periods, not only of months, but of years. Again, the strong motives for concealment and misrepresentation which exists when syphilis occurs in married life, detracts, seriously, from the trustworthiness of accounts received from patients.

To my knowledge there is no collection of such histories; nor do I know of isolated cases in which the necessary conditions of trustworthiness was combined.

When primary syphilis has been cured, whether without the use of mercury, and no secondary symptoms

has occurred, there is, in my opinion, not the slightest grounds for fearing transmission of disease to offspring.

When primary syphilis has come to an end, either under the use of mercury or without its employment, secondary symptoms may or may not ensue. I consider the probability to be greater in the latter, than in the former instance; but the evidence on this point is not sufficiently abundant and strong to have commanded the general assent of the profession.

If a female, having completely recovered from the primary symptoms, should become pregnant, and remain free from disease during the full period of utero-gestation, I should not entertain the slightest apprehension on account of the offspring.

Should a person, so circumstanced, have secondary symptoms after parturition, and should the child continue free from disease, it would show that the so-called dormant state of syphilis is not dangerous to offspring; but, on this point, I have no direct evidence.

If, after the cure of primary syphilis, the person should remain perfectly well for twelve months, there is little chance of secondary symptoms, but such things may occur.

In the successive appearances of secondary symptoms, the appearances of secondary symptoms, the intervals, may be much longer. I have seen particular symptoms of unmistakeable syphilitic character, after the patient had been from seven to ten years free from disease. Such instances are extremely rare, but their occasional

occurrence renders it difficult to speak positively on the subject of radical cure.

All these instances have been in males. I cannot believe that any disease could be communicated to a female, by cohabitation, during such healthy intervals.

I have seen instances, and others are recorded, in which women, who have been affected once, and once only, with primary and secondary syphilis, have produced, in three, or even four, pregnancies, either dead infants, or others, having been affected with syphilis after birth.

Healthy children may be brought into the world after two or three unfortunate occurrences of this kind.

LEE, HENRY, Esq., Senior Surgeon, Lock Hospital; Surgeon, St. George's Hospital, states,

That there are two kinds of primary syphilitic affections—

- 1.—That which he calls the suppurating syphilitic ulcer.
- 2.—And that which he calls the primary syphilitic induration, with, or without, a primary ulceration, or the so-called Hunterian chancre.

In the first kind of ulcer, that which he calls the primary suppurating syphilitic ulcer, this is, he says, a local disease, and he never has known it to be followed by constitutional symptoms.

This kind of ulcer is not benefitted by mercury. It may, in the first instance, be destroyed by caustic.

But, where a variety of applications have been made to this ulcer, it is, too often, tedious to cure it. If left to

nature, it goes on increasing for a time; then remains quiescent some time, and then begins to heal.

The second kind of primary syphilitic induration, with, or without, primary ulceration, or the so-called Hunterian chancre.

This kind of primary syphilitic indurated tubercle, or Hunterian chancre, is that kind of ulcer which is usually followed by secondary symptoms.

This kind of syphilitic infection, at its first appearance, generally, attracts but little attention. It is attended with no inconvenience, and the patient is willing to believe that it is all right. As the disease progresses, it assumes one of these forms, which are all modifications of the adhesive kind of action:—

1.—The cuticle may appear as peeled, from the upper part of the glans penis, or the prepuce, or a circumscribed patch may remain for days together, presenting a livid purple colour. The structures below are not infiltrated, to any extent, and, therefore, there is very limited specific induration.

The secretion consists of epithelial scales, and, and symphatic globules of various sizes, and more or less fully formed, is thrown off from the surface. In women, there is, probably, a corresponding affection of some part of the mucous membrane, not accompanied by induration; but, on account of the difficulties attending the investigation of these complaints in these organs in females, such a condition has not, therefore, been described.

2.—An indurated tubercle, with or without ulceration, may form in the skin, or under the mucous membrane, and will then present all the characteristics of the specific induration, without the loss of substance.

3.—The third ordinary form of syphilitic infection, is that which has been called the indurated Hunterian chancre.

It has been stated, that, according to Mr. Lee, the first kind of syphilitic ulcer, that which is called "*The Suppurating Syphilitic Ulcer*," is a local disease; that it can be cured by the simple application of caustic, or, if left alone, can be cured by the efforts of nature; that it does not injure the constitution, and that it is not followed by constitutional symptoms.

But that the second kind, that of primary syphilitic induration, or Hunterian chancre, the time has often been so long, between the time when the patient was infected, till he presented himself to the surgeon, that it is useless to attempt to eradicate the disease by caustic, or even by excision of the part. Mercury must be had recourse to, for a radical cure.

He says, that the diagnosis, between what he calls, the primary suppurating syphilitic ulcer, and a non-infecting ulcer, depends on the nature of the secretion, or in the existence of induration at the seat of infection, and in the inguinal glands, and upon the inoculability of the secretion, or second time on the same patient.

He further states, that inoculation, is, as a rule,

practicable on the same patient, from the secretion of a suppurating sore—never from an indurated sore.

And his opinion, as to the hereditary nature of syphilis is, that it frequently leaves some effects on the children, even when no distinct symptoms show themselves.

That, in other cases, these symptoms may show themselves up to the period of middle life; and, that in other cases, again the hereditary effects of syphilis may distinctly be cured in the third generation.

HARRIOT, Dr., Late Surgeon of the 6th Dragoon Guards (Carabiniers), says,

That he was in Sicily, with his Regiment, the 61st Foot, in 1808, where he remained about one year. That at that time ulcers on the genitals were very common, and that they were all considered to be syphilitic; and that they were treated by mercury. That he has seen scores of men attacked with what was then called the "Black Lion;" and who suffered the cruellest mutilation that man can suffer; and many men were rendered unfit for military duty, in consequence of the abuse of the mercurial treatment.

That the next year he went to Portugal with his regiment, and he served with the army in the Peninsula till the peace of 1814.

He further states that the ulcers on the genitals were as common in Portugal as he had seen them in Sicily; but as he was always in front, with the army, he had not so good an opportunity to see the result of the Medical treatment in the general Hospitals in Portugal,

as he had seen in Sicily. But from the testimony of others he apprehends that the result of the Medical treatment was as deplorable in the Peninsula, as he had seen it in Sicily.

At the same time he is aware that these ulcers on the genitals were as common in the Portugese regiments, as in the English regiments; and that the Portugese Surgeon treated these as common ulcers, without mercury, and cured their patients without any unfavorable results.

After the peace he was for a couple of years surgeon to the 17th Foot; and, subsequently, he was appointed surgeon to the 6th Dragoon Guards (Carabiniers), in which regiment he remained twenty-five years. His yearly Medical Returns, as to the health of the 17th Foot and 6th Dragoon Guards, are at the Army Medical Department; and it will be seen by these that for seven-and-twenty years he did not administer one grain of mercury in any form, for the cure of ulcers on the genitals; and that he had never had one single case of secondary symptoms, either in the 17th Foot or in the 6th Dragoon Guards.

Having had great opportunities to see and to study these ulcers on the genitals, he is not aware of any means to establish a diagnosis between a syphilitic and a non-syphilitic, ulcer, on the genitals, or on any other part of the body.

He believes that syphilis, if there is such a specific disease, is not transmissible from the parent to the offspring; at least, he has never seen such a case.

PARTRIDGE, Richard, Esq., F.R.S., Professor and Surgeon to King's College Hospital, &c., &c., &c., says,

That he cannot, the first day that an ulcer is remarked on the genitals, say, that that ulcer is syphilitic, or non-syphilitic.

That, in a day or two, after the ulcer has been observed on the genitals, if it has acquired a hard base, then he concludes that the ulcer is syphilitic, and he prescribes small doses of mercury.

But, he is also aware, that ulcers on the genitals, in consequence of the want of proper ablution, etc., etc., may have a hard base, and, therefore, it becomes an impossibility to establish a diagnosis between a primary syphilitic and a non-syphilitic ulcer on the genitals, by the hardness at the base of the ulcer.

He doubts that we can, in all cases, establish a certain diagnostic between a primary syphilitic, and a non-syphilitic ulcer, by inoculation.

His doubts, also, extend to the secondary symptoms, indicated by eruptions on the skin, etc., etc., etc., with regard to which, it is, in some cases, difficult, if not impossible, to distinguish those which have, from those which have not, a syphilitic origin—so also Iritis.

He believes that syphilis can be transmitted from parent to offspring.

SHAW, Alexander, Esq., F.R.S., Surgeon, and Lecturer on Surgery, Middlesex Hospital.

In answer to my question—"What is the diagnosis between a primary syphilitic, and a non-syphilitic, ulcer

on the genitals?" Mr. Shaw has been so kind as to address me the following letter:—

"Dear Sir,—If a young man, a patient, informs me, that five days or a week, or even longer, he had had an impure intercourse, and if I saw on his penis, a sore, circular, indurated, with a slightly moist circuitous surface, sharp edges, and red halo around, I would tell him he had got a syphilitic chancre, and that if he had connection with a female, he would give her syphilis.

"I would treat the patient himself, with mercury, subject to modifications that might arise, as the only known method of giving him any kind of protection from secondary symptoms.

"It is scarcely necessary for me to add, that all sores suspected to be syphilitic, are not truly so, and that the discrimination of them, is not always easy.

"But to enter on the subject of diagnosis, fully, would require a thick volume.

"Excuse me for this delay, and

"Believe me to be, yours truly,

(Signed)

"ALEXANDER SHAW.

"London, 22A, Cavendish Square, W.

"10th December, 1863."

The object in view, for calling attention to the pathology of the so-called syphilitic disease, is to point out that the Medical profession, neither in France nor in England, can, by ocular examination, or by the touch, or by inoculation, ascertain that an ulcer on the genitals, or any other part of the body, is syphilitic.

It has been shown above, that the French Medical Practitioners have not one single symptom by which they can, by ocular examination, or by the touch, or by inoculation, ascertain that an ulcer on the genitals, or any other part of the body, is a syphilitic ulcer.

Above are the names of twelve of the first English Medical Practitioners. Several of these are distinguished syphilidographers, who have devoted their mental energies, and their time, to the study of the pathology, the etiology, and the Medical treatment of this so-called syphilitic disease; and, yet, not one of these twelve English distinguished Medical Practitioners can inform us how to establish a diagnosis between a syphilitic, and a non-syphilitic, ulcer on the genitals, or any other part of the body.

Two of these twelve gentlemen—the late Sir Astley Cooper, Bart.; and Dr. Harriot—both having had great opportunities to see, and to study, this so-called syphilitic disease, have told us, that they knew of no diagnostic symptom between a so-called syphilitic, and a non-syphilitic, ulcer on the genitals, or on any other part of the body.

Ten of the above gentlemen have informed us that there are two kinds of syphilitic ulcers—one with a soft, the other with a hard, base.

Three of these gentlemen have said—1st Mr. Gascoyen, that the ulcer with a soft base, if left to itself, will often be cured by the efforts of nature; and is not, when promptly cured, followed by secondary symptoms. The 2nd, Mr. Lee, has said that the ulcer with a soft

base is a local disease—that it can be destroyed by caustic; and that he never saw this ulcer, when healed, to be followed by secondary symptoms. And 3rd, Mr. Lawrance—an authority which every one respects—tells us that *“mercury is not only useless, but hurtful in the sloughing primary affection, which is easily manageable by other means, and has not been followed by secondary symptoms.”*

Therefore, since, according to the above three gentlemen, this kind of ulcer can be cured by the effort of nature; this is, without the administration of mercury, we have a right to ask them, what proofs have they that this kind of ulcer is syphilitic?

It is evident that they are satisfied that they have before them a syphilitic ulcer, but they have not the means to prove this, by the aspect of the ulcer.

Consequently, we have a right to conclude that this ulcer, with a soft base, is nothing more than a common, non-syphilitic, ulcer.

The above gentlemen, however, insist, that the ulcer with a hard base, or what goes by the name of the Hunterian chancre, is a true syphilitic ulcer.

Yet, Professor Fergusson, one of the above gentlemen, whose opinion on this question is second to none, tells us that he has seen, ulcers on the genitals, with a hard base, which were not syphilitic.

Mr. Partridge, a no less high authority, says, that ulcers on the genitals, in consequence of the want of proper ablution, etc., may have a hard base.

Mr. Holmes Cootes informs us, that the induration

at the base of the ulcer on the genitals, depends on the tissue on which this ulcer is situated; that this induration ceases when the ulcer is situated on the firm tissue of the glans penis, but is found when the ulcer is on the loose tissue of the prepuce.

Therefore, as three out of the above distinguished Medical Practitioners—at the same time that they call attention to the hardness at the base of an ulcer on the genitals, as being pathognomonic—that that hard-based ulcer is caused by a syphilitic virus—yet these three gentlemen warn us, that hardness at the base of the ulcer, depends on the tissue over which this ulcer is situated.

If any doubt remains in the minds of the remaining seven gentlemen, that hardness at the base of an ulcer on the genitals is not pathognomonic or a syphilitic ulcer, let them apply caustic to a healthy prepuce, and they will have an ulcer with an indurated base—a perfect, so-called, Hunterian chancre, which will leave a hard tubercle that may not disappear for months.

Therefore it follows that an induration at the base of an ulcer on the genitals, is no proof that this ulcer is syphilitic.

And, finally, as to the question of inoculation.

It has been seen above, when examining the opinions of the French Medical Practitioners, that inoculation is no proof that an ulcer on the genitals, or on any other part of the body, is syphilitic. Nothing that the English Medical Practitioners have adduced can do away with the fact, that inoculation fails to prove, that

an ulcer on the genitals, or on any other part of the body, is syphilitic; and that, consequently, inoculation is an error put forward to maintain another error.

It follows, therefore, from a careful examination of the above twelve distinguished English Medical Practitioners, that they have no means—either by ocular examination, or by the touch, or by inoculation—to distinguish a primary syphilitic from a non-syphilitic ulcer, on the genitals, or on any other part of the body.

And as it has been also shown above that the no less distinguished seven French Medical Practitioners have no means—either by ocular examination, or by touch, or by inoculation—to distinguish a syphilitic from a non-syphilitic ulcer, on the genitals.

It consequently follows, that since we find by the above inquiry, that in France and in England, where the Medical Professors are second to those of no other nation in the scientific study and knowledge of their profession—since, I say, the Medical Profession in France and in England have not one pathognomonic symptom by which they can point out the distinction between a primary syphilitic and a non-syphilitic ulcer, on the genitals or on any other part of the body, we must conclude either that the so-called syphilitic disease has never been scientifically studied by the Medical Profession in France or in England, or that there is no such disease as syphilis.

The conclusion to which is here arrived at is not now stated for the first time. Two-and-twenty years ago, some Medical Practitioners, now in London, were pre-

sent, in Paris, at a public consultation, where—after having brought Dr. Ricord to admit that he could not point out, by ocular examination, or by the touch, the diagnosis between a primary syphilitic, and a non-syphilitic ulcer on the genitals, but that he could do so by inoculation; I expressed the conviction at the moment, that I would live long enough to see it acknowledged by the profession,—that inoculation, as a test of the existence of syphilis, was an error brought forth to maintain another error.

Although there is such a total want of knowledge of the pathology of this so-called syphilitic disease, both in France and in England, yet, happily, both in France and in England, there is not now that abuse in the use of mercury, in the treatment of this disease, as was formerly.

We no longer hear of, or see, in the hospitals, those distressing results from the abuse of mercury. Many careful Medical Practitioners now act as the late Sir Astley Cooper, Bart., did, fifty years ago. They prescribe fractional doses of mercury, to satisfy the patient that something active is done to cure him; and, thereby, the patient is saved from falling into less scrupulous hands, time is gained, and nature cures him.

We have above the testimony of Mr. Lawrance, who informs us that mercury is injurious in the sloughing primary syphilitic ulcer.

We have the testimony of Mr. Holmes Coote, who tells us that all sores on the genitals may be cured without mercury.

And we have the authority of Dr. Harriot, who witnessed, in 1808, in Sicily, the abuse in the use of mercury in this so-called syphilitic disease, and the dreadful consequences which followed; and who says, that for five-and-twenty years that he was Surgeon to the 6th Carabinier Dragoons, he never prescribed a grain of mercury, in any form, for the cure of ulcers on the genitals; that he had, on an average, thirty patients a year with ulcers on the genitals; that he merely prescribed abluion, rest, and low diet; that he never had, during these five-and-twenty years, one case of secondary symptoms of syphilis.

Therefore we are led to doubt the existence of a so-called syphilitic disease, by the impossibility to point out the diagnosis between a so-called primary syphilitic and a non-syphilitic ulcer; but we are led to doubt the existence of a so-called specific syphilitic disease by the fact that Mr. Lawrance tells us that,—in one kind of syphilitic ulcer the administration of mercury is injurious; by the fact that Mr. Holmes Coote tells us, that all sores on the genitals can be cured without the administration of mercury; and by the fact that Dr. Harriot has cured his patients for the last seven-and-twenty years without the administration of mercury, and that he never had one case of secondary symptoms.

In no disease is the benefit of medical knowledge more valuable than in this so-called syphilitic disease.

How often has not the Medical Practitioner in his hands,—the peace, the happiness, and even the life of

individuals—pure in mind and body—saved or destroyed by his word?

Nearly fifty years ago, two young friends of mine married; they left home on an excursion. About a week after marriage, the wife complained of a bubo in the groin. A Medical Practitioner was called in; he pronounced this to be syphilitic.

Her father, an eminent Medical Practitioner, was sent for. He satisfied himself that her husband was in perfect health; and the proof that this bubo was not syphilitic, was, that in a few days it disappeared.

But the unfortunate wife's mind gave way. She believed herself to have been deceived; and she was carried to her grave a few weeks after—believing herself to have been deceived and injured by her husband.

The unhappy husband prayed for death. He joined his regiment, in the hopes of being killed; and he fell the first time he went before the enemy.

In the case related above—also a newly married couple—where thirteen Medical Practitioners were called in consultation, Professor Marjolin, and one of the gentlemen consulted, declared, that the case before them, was not one of syphilis. The eleven others declared that this was a case of syphilis. The majority of voters were believed; and the result was the death of husband and wife, by their own hands.

I will take the liberty to mention another case, which occurred in 1829; which appeared in the public papers; and which caused a great sensation in the first classes of society.

A young couple belonging to the first rank of society married. A few days after marriage the husband observed something abnormal about his genital organs. He consulted a Surgeon of distinction, who, at once, pronounced this to be syphilis.

The husband requested the Surgeon to examine carefully, stating that he never had approached a woman till he married, a few days ago, his present wife: and that, from *material facts*, he was satisfied that he was the first who had approached her.

The Surgeon maintained his opinion to be correct.

The husband returned home, told his wife what the Surgeon had said; but he did not utter one word offensive to her. He retired to his room, wrote her a heartrending letter, and destroyed himself.

The unhappy wife submitted to every examination. She was found to be in perfect bodily health. Her mind gave way, and she died broken-hearted in a few months.

The report of such cases might be increased, as well as the report of less tragic cases, which every Medical Practitioner has met with.

The conclusion, to which it is wished to draw attention, is this: That, in our want of knowledge of the pathology of this so-called syphilitic disease, no Medical Practitioner, either in France or in England, can pronounce that an ulcer on the genitals, or on any other part of the body, is caused by a syphilitic virus; and that no Medical Practitioner, who respects himself, ought to declare that he has before him a syphilitic ulcer.

The question is put to me by a distinguished Medical Practitioner, who has a right to an answer. He says—What! Has the Medical world been labouring under a delusion for these last three hundred and fifty years, as to the existence of a syphilitic virus?

Is it probable, that so many eminent Medical Practitioners—who have devoted themselves to the study of this so-called syphilitic disease—is it probable, that none of these gentlemen have ever seen any reason to doubt the existence of a syphilitic virus?

I do not intend to enter into the question what induced the Medical Profession, in former years, to believe in the existence of a syphilitic virus.

All that I wish to contend for, is, that now—in the two most distinguished nations in the world, England and France, for their Medical Professors—not one of these gentlemen can point out the diagnosis between a so-called primary syphilitic, and a non-syphilitic, ulcer on the genitals, or on any other part of the body.

However, without entering into the question as to what induced the ancient Medical Practitioners to believe in the existence of a syphilitic virus, I submit the following facts; which are matters of history; and which may assist us to account, why the term, "Syphilis," crept into Medical Science.

In the fifteenth century, a cutaneous pustulous eruption was epidemic in Europe. It was said to be contagious—to be communicated by the breath, by the touch, etc.

As the eruption was pustulous—as the genital organs

were as liable to be the seat of these pustules, as any other part of the body, and as these pustules were said to be contagious—the public, and the Medical profession, concluded that these eruptions were propagated by sexual intercourse—hence, the syphilitic virus.

But, says the syphilidographers, have we not proofs at this moment, that an individual, who has had, what is now called, syphilic ulcers on the genitals, which have not been properly cured, by mercury, and which have left an induration on the prepuce; have we not the proof, they say, that this person, in a few weeks, or in a few months, will have unmistakable secondary syphilitic symptoms—such as ulcers in the throat, cutaneous diseases, postular eruptions on the skin, nodes, microsis, etc., etc., etc.

The first question we must ask the syphilidographers is, are these diseases pathognomonic of a syphilitic virus? or, can they be induced by no other cause than a syphilitic virus?

It is evident that if the above diseases can be induced by any other cause than by a syphilitic virus, the syphilidographers have no right to assume that the above diseases are caused by a syphilitic virus.

Thus every Medical Practitioner knows that males are more subject to ulcers in the throat, from the age of ten to forty years old, than females.

That these ulcers occur in consequence of indigestion, or of costiveness, or of a cold, etc.; and that they are cured by a mild laxation and rest in bed, in a few days.

As to cutaneous diseases.—Is it not known to the

As to cutaneous diseases, is it not known to the Medical Profession that cutaneous diseases were far more prevalent before the so-called syphilis was thought of? Did not the Arabian Physicians teach us to cure cutaneous diseases by the now-called mercurial ointment centuries before syphilis attracted attention? Hence the use of mercury for the so-called syphilitic disease.

And as to postular eruptions, nodes, necroses, etc., etc.

In 1775, the United States of America invaded Canada, under General Montgomery, and laid siege to Quebec for some months. The crops of wheat had failed that year in Canada, and especially at a place called "*La Baie de St. Paul*," on the eastern bank of the St. Lawrence. The presence of the American army increased the scarcity of bread.

Towards the spring of 1776, a postular eruption—attended with nodes, necrosis, etc.—broke out at "*La Baie de St. Paul*," and destroyed a great number of the inhabitants. It spread all over the country, and caused such alarm that the English Government sent Medical Officers from England with food and all kinds of comfort to Canada. This epidemic was supposed to be contagious; and was called by the Medical Profession "*The New Venereal Disease of Canada*." But it had this peculiarity, that although said to be contagious, in general the genital organs were not effected. See *Dr. Swedeor on Syphilis*.

Who does not at once see, in this epidemic outbreak of disease, the want of proper food as the cause.

That part of Portugal through which the French army advanced to the lines of Torres Vedras, in 1810, was laid waste by us, as to food for man and beast, as we retired before the French army. The French army, on its advance to, and on its retreat from, Torres Vedras, consumed and destroyed, the food for man and beast, which had escaped us; and the consequence was, that the inhabitants of that district were in a starving condition; although the English Government spared no expense to relieve them.

After the retreat from Burgos, in 1812, parts of the English army were cantoned in that district. I then was Assistant-Surgeon of the 61st Regiment, one of the regiments forming the 6th division of the army. I was in charge of the hospital of the regiment; and I devoted some hours, every day, to receive any poor inhabitant who wished for medical advice; and I thus saw hundreds of the poor creatures labouring under cutaneous diseases—phagedænic ulcer, nodes, necrosis, etc.

All those persons informed me, that they had been quite well previous to the entry of the French into Portugal; and they dated their illnesses from the want of sufficient food.

In 1816, the most of the crops were destroyed on the continent of Europe, by the deluge of rain that began to fall on the 16th June, 1816, and lasted, almost incessantly, till December.

At the end 1816, I was attached to the Head Quarters of the Cavalry Division of the English army of occupation; and I remained with the Head Quarters of

the Cavalry Division, till the army returned to England at the end of 1818.

While I was with the Head Quarters of the Cavalry Division, they were moved from Cassel to Mul, near St. Omer, to Hordengan, and to Pont de Borgne.

At all these places I made myself useful to the poorer class of inhabitants; and as a famine raged in France, from the end of 1816 to the end of 1817, and as that part of France, where the English Cavalry Division was quartered, was not excepted, I had great opportunities to see the deplorable effects of the want of sufficient food, in all kinds of cutaneous diseases, ulcers, postules, nodes, necrosis, etc.

Therefore, I submit, that the syphilidographers are not justified to insist that the so-called syphilitic diseases alone cause ulcers in the throat, cutaneous diseases, ulcers on the body, postules on the body, nodes, necrosis, etc.

But do we require a specific virus to account for the ravages that an ulcer on the genitals will cause to the human frame, and even to the destruction of life?

Is it not an almost daily occurrence that the slightest wound on the toes, or fingers, will induce buboes in the groin, or axilla; which, if neglected, suppurate, are very tedious to cure; and, too often, injure the constitution, and, too often destroy life?

And if such results attend the slightest wound on the toes or fingers, why should not the same consequences follow a slight wound on the genitals, without requiring the existence of a specific virus to account for any distressing result?

Were not ulcers on the genitals known to be dangerous for thousands of years before syphilis was thought of?

With every respect for the Jewish Faith, it cannot be admitted, at this time of day, that circumcision was a divine ordinance, no more than *ablatio nympharum*, as practised by some savage nations of Africa to this day.

Moses instituted circumcision as a hygienic measure, to uncover the glans penis, and to prevent any foreign substance being detained between the glans and prepuce, and thereby to prevent balanitis and ulceration, and their consequences—so common in warm climates to men not circumcised. And to insure the performance of this mutilation, it was declared, by Moses, to be ordered by the Divinity.

Does not Celsus, in the eighteenth chapter of his sixth book, nearly fourteen centuries before syphilis was thought of, inform us of the danger of ulcers on the genitals?

Therefore, it does not require the presence of a syphilitic virus, to account for the injuries to the constitution, and even for the destruction of life, which may follow ulcers on the genitals.

To resume—

- 1st. Since the two most celebrated nations in the world, England and France, for their pathological Professors.
- 2nd. Since these pathologist Professors, cannot, at the bedside, demonstrate the presence of a syphilitic virus.

- 3rd. Since all the consequences of the presence of this supposed syphilitic virus, may be induced, and are induced, by known and natural causes, irrespective of a syphilitic virus.
- 4th. Since all the consequences of the presence of this syphilitic virus, may be, and are cured, without the administration of its supposed specific remedy, —mercury.
- 5th. We must conclude that there is no such thing as a syphilitic virus.

There is another supposed enthetic disease, which rages in the army—the pathology, the etiology, and the Medical treatment of which, is as little known to the Medical world, as the pathology of syphilis.

I refer to gonorrhœa.

It is the received opinion in the Medical world, that this disease is contracted by the male, only, by connection with a female labouring under gonorrhœa.

But where is the Medical Practitioner, who knows his profession, and who can demonstrate the existence of gonorrhœa in a female?

And where are we with our affirmation, that gonorrhœa can be contracted only by having connection with a female labouring under gonorrhœa? When we meet with individuals, who, previous to an attack of gout, and without having had any connection for months, are first seized with balanitis, and then, suddenly, with a severe gonorrhœa, chordee, etc., which lasts a few days; then ophthalmia, probably, comes on, and then the balanitis

and the gonorrhœa are better, then the joints are better, and the balanitis, the gonorrhœa, and the ophthalmia, disappears; but, if the inflammation suddenly disappears from the joints, the balanitis and the gonorrhœa reappears, and a troublesome gleet remains, which cannot be cured—but by bringing on another fit of gout in the joints; or, where are we with our diagnosis, if we find an individual attacked with gonorrhœa, in consequence of cutting a tooth? See *Hunter's Work on Venereal Diseases*.

But it is not my intention to enter into the inquiry, the etiology, and Medical treatment of gonorrhœa.

Finally, it has been stated above, that your lordship is in a position to render the greatest service that can be rendered to humanity in general, and to the army in particular.

It has been demonstrated above, that the first Medical Professors, in England and in France, are not able to point out, at the bedside, the presence of a syphilitic virus.

As the first Medical Professors in the world cannot point out, at the bedside, the presence of a syphilitic virus, how can it be expected that the Army Medical Officers can be able to point out, at the bedside, the presence of a syphilitic virus?

As the Army Medical Officers have their patients under their charge, and as they can watch over the health of their patients for years, they are in a better position to study, scientifically, the question, as to the existence of a syphilic virus, than any other Medical Practitioners.

The War Office have an army of upwards of 400,000 men, distributed in various parts of the globe, and a staff of about 1500 Medical Officers, also distributed in various parts of the globe.

If the attention of these 1500 Medical Officers were directed to the study of the pathology, the etiology, and Medical treatment of this so-called syphilitic disease, and if the researches of these 1500 Medical Officers were carefully and scientifically recorded; in a few months there would be an amelioration, as to this so-called syphilitic disease, in the army,—the Army Medical Officers would not go on as they are now going on—to consider every ulcer, on the genitals, as syphilitic, and to be treated only by mercury.

And, with submission, if from these 1500 Medical Officers, a commission were formed to visit all the Lock Hospitals in this country, to see cases, and to learn the opinions of the Medical Gentlemen in charge; and then to visit the Lock Hospitals on the Continent—there, also, to see cases, and to learn the opinions of the Medical Gentlemen in charge; then, after their return home, to publish the opinions of the several Medical Gentlemen in charge of Lock Hospitals, whether here, or on the Continent of Europe, with the result of their own researches, and observations.

I am satisfied that the result would be a total revolution, as to the pathology, the etiology, and Medical treatment of this supposed syphilic disease, which, annually, might save thousands of men to the ranks of the army—if not hundreds of lives.

I will not take the liberty to press on your Lordship's attention the result of my experience for these last forty-seven years, in the Medical treatment of this supposed syphilitic disease, without mercury.

But, with your Lordship's leave, I will take the liberty to press on your attention, the testimony of a distinguished Army-Surgeon, who has official documents to support his statement. I refer to Dr. Harriot, late Surgeon of the 6th Dragoon Guards, Carabineers, who informs us, that, for five-and-twenty years, he treated all ulcers on the genitals without mercury; and without having had one single case of secondary symptoms; and, he adds, that your Lordship has, at the Army Medical Department, his yearly medical reports, in support of his present statement.

In conclusion, it is hoped, that your Lordship may see, in the facts that have been brought forward, that in the interest of humanity in general, and of the army in particular, that the pathology, the etiology, and the medical treatment of the so-called syphilitic disease, ought to be scientifically studied.

Before closing this letter, I hope your Lordship will further permit me to call your attention to the necessity that wherever troops are assembled, there means ought to be provided where the men can daily wash their genital organs; and that they ought to be warned, that it is the part between the glans and prepuce that requires greatest attention.

And, further, to carry out these hygienic measures, the men ought to be subjected to a medical examination

once a week. It ought to be pointed out to them that it is an act of humanity to themselves to have this medical examination.

I have the honour to be, my Lord,

Your obedient servant,

DAVID MACLOUGHLIN, M.D.

Member of the Legion of Honour.

Since writing the above, a distinguished friend of mine, who devotes himself to the study of hygienic questions, especially connected with the army, has put the following questions to me:—

- 1st.—Is the examination of the unfortunate females in Paris, conducive to prevent the spread of syphilis?
2nd.—Are men, *ceteris paribus*, less liable to be attacked with syphilis in Paris than in London?

I will alter the word "syphilis" in the above questions to the word "injury."

As to the first question.

The examination of these unfortunate females is an act of humanity towards them; and I have the testimony of one of the Surgeon-Inspectors, that the examination, so far from degrading these unfortunate females in their own estimation, tends to awaken in them a feeling of respect for themselves—gratitude for the care taken of their health; and often recalls them to a better state of existence.

And if there is such a disease as syphilis, the examination must tend to prevent its spread.

And as to the second question.

I doubt that men are less liable to be "injured" in Paris than in London.

Having a doubt as to the existence of a syphilitic virus, when consulted by a patient in Paris, with an ulcer on the genitals, if he were willing to mention the name and address of the female with whom he had had connexion. As a satisfaction to myself as to the nature of the ulcer I had before me, I spared no expense to arrive at the truth.

The Inspector of these unfortunate females, or a Surgeon, was requested to examine and to report if the female was injured or not.

I regret that I have not kept an account of these unfortunate females, who were thus examined; and how few, very few, were reported to be injured.

It has been seen above, that I was at Valenciennes, in the spring of 1816, where the examination of these females was carefully carried out; and the very few that were found injured were immediately sent off to the hospital at Lille. Yet, the so-called syphilitic disease, was an epidemic in the garrison. The number of men injured was out of proportion to the females injured; and it was quite impossible to accuse those few unfortunate females, of having injured the number of men that were found injured.

See Dr. Evans'—the Surgeon of the 57th Regiment—report, published in 1819.

In conclusion—In my opinion, from the number of men that I have found injured, where the females were found not to be injured, it is the man who injures him-

self; not the female who injures him. And, in support of this opinion, I refer the medical reader to *Mr. John Hunter's work on Syphilis*, published in 1835, by Mr. Bell; and, at page 316, it is stated—"A gentleman, in the act of copulation."

Such cases as that, I have repeatedly seen in soldiers, and in the higher grades of society; while on service with the army, and in private practice.

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INTRODUCTORY LECTURE

ON

"THE ORIGIN AND MIGRATIONS OF THE
DIFFERENT FAMILIES OF THE HUMAN RACE
AS ILLUSTRATED BY THEIR LANGUAGES."

BY F. S. B. F. CHAUMONT, ESQ.,
M.D., F.R.C.S.E., STAFF ASSISTANT SURGEON.

DELIVERED ON THE OCCASION OF THE
OPENING OF THE YOUNG MEN'S SOCIETY IN CONNEXION
WITH ST. ANDREW'S CHURCH, SOUTHAMPTON.

PUBLISHED AT THE REQUEST OF THE SOCIETY.

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BY F. S. R. F. CHAMMONT, ESQ.
M.D. PHYSICIAN IN CHIEF TO THE ROYAL HOSPITAL FOR THE BLIND

DELIVERED ON THE OCCASION OF THE
MEETING OF THE LONDON MEDICAL SOCIETY IN CONNECTION
WITH ST. ANDREW'S CHURCH, SOUTHAMPTON

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LECTURE.

"MAN," says Oken, "is the sum of all animals, as well in regard of his form as of his mental powers." In like manner Ethnology, the science which treats of man, may be called the sum of all sciences, and the culminating point of human knowledge. Of its different branches the study of language is perhaps the most interesting, and probably the most important, and to this branch the present lecture will be confined.

The earliest attempts at linguistic science, were founded on the belief that the Hebrew* was the primitive form of speech, and efforts were made to derive all language from that tongue; and there are not wanting men still, who are even now vainly striving in the same direction, allowing their zeal to blind them to the truth. It is not saying too much to affirm that modern linguistic science owes its existence to the establishment of the British power in Hindustan. Previous to that time, Sanskrit was known only by name, and it was not till some time after our power was firmly established, that the Brahmins could be induced to communicate their sacred language to an outcast race, who eat beef and crossed the black water! When at last the knowledge was obtained, it was like the rising of the sun after an Arctic night. A language was found, where none expected it, of such richness and beauty, so elaborate in its grammar, and so copious in vocabulary, as even to surpass the Greek, that vast mine, from which had been extracted the terms of science and philosophy for the whole of the then known world. Here were discovered links which connected the Greek, the Latin, and the Teutonic tongues together: here were roots which explained what appeared in these tongues isolated obscurities; and the origin was found of myths, which had lost their meaning in later times, and lingered apparently as senseless fables. Scarcely less valuable was the later discovery of the Zend, or original sacred language of the ancient Persians. What remained of that remarkable tongue, showed that in its perfection, it must have been at least equal in dignity of expression, and completeness of detail, to the Sanskrit and the Greek, and proved incontestably the right of the Persians to be admitted into the same family, which the eminently Semitic nature of the vocabulary of their recent tongue had rendered doubtful.

*A glance at the tables will show the comparatively recent epoch of that tongue, old as it undoubtedly is.

The next grand step was the deciphering of the Egyptian hieroglyphics, by Young and Champollion. This, like the acquisition of Sanskrit, was due to conquest, as the Rosetta stone was one of the many monuments collected by Napoleon for transport to France, and which were taken by the English fleet and army, under Nelson and Abercromby. The subsequent reading of the cuneiform characters, by Rawlinson, Layard, and others, was a triumph worthy to be ranked with it. And here I need hardly do more than refer to the doubts expressed by the late Sir George C. Lewis, as to the actual interpretation of the Egyptian and Babylonian inscriptions. Cogent and ingenious as were his arguments, and backed as they were by deep and wonderful learning, it is, I think, admitted by the majority of people, that his scepticism was carried too far, and that the meaning of the inscriptions had been in the main arrived at.

The next great step, one of the most important of all, was the discovery of what is called Grimm's law, establishing for the Aryan family and chiefly for the Teutonic group, the laws by which a root is modified in passing from one language to another, and which appears to be nearly as fixed in its operation as any law in the more exact sciences.

The establishing of the Aryan character of the Celtic tongue by Prichard and Pietet, and the labours of Bunsen, Rénaud, Max Müller and others, bring us down to the present time. I ought not to omit the names of Klaproth, Castrén and Wilhelm von Humboldt, not to mention many others, to whom we owe much of what is known concerning the Turanian tongues, and the more recent labours of Stanislas Julien in the vast field of Chinese linguistics. This brief sketch will suffice to show in a general way the progress of the science of which I shall now proceed to speak more particularly.

In examining a language so as to fix its place in any particular group or family, two primary points must be attended to: its vocabulary or phenomenology, and its grammar or nomology. The results of the two studies may be very different, as the vocabulary of a language may change so far as to leave very few of the original words. This is the case with Persian and Turkish, in both of which there are many more Arabic than native words. It is, however, quite easy to see that neither Persian nor Turkish is a Semitic language. The reason is that, although the vocabulary may and does alter, the grammar remains the same. The Persian or the Turk may accept Arabic words to express his thoughts, but neither will express himself as an Arab; the Persian will retain his Aryan, the Turk his Turanian form of thought and expression. In our own tongue we may frame sentences which shall contain nothing but words of Latin origin, and which shall yet infallibly betray their Teutonic form. Thus, to take an example, in the sentence, "John's letter demands attention," every word is of Romance origin, but the possessive case, "John's," shows clearly that the sentence is Teutonic, as no Romance

language is capable of expressing itself in this way. Again it is equally certain, from the termination in *S*, of the verb in the 3rd person singular, that the sentence is of a particular branch of the Teutonic, nearly allied to the Gothic.* It is by such a process of analytic investigation of separate tongues or species that we are led to the arrangement of cognate groups or genera. We then begin to discover some wider relationship between these last, which leads us on to the formation of classes and families, until we arrive at a point when we feel prepared to refer the whole to one or more primary types, beyond which evidence is wanting to carry us further. Leaving, then, the process of induction, we begin that of deduction, and by an operation of synthesis we can both trace the rise and the decadence of the various tongues, and can also refer directly to one or other of our established genera, classes and families, tongues hitherto unexamined.

When we find in different languages a number of words either obviously the same or easily traceable to common roots, we must enquire, 1st, Do these words represent primitive ideas? 2nd, If not, do the words representing primitive ideas belong to the same or to a different class? 3rd, If to a different class, can the words first examined be referred to conquest or to close intercourse with one or more foreign races? If we can ascertain an undoubted connection between the two languages examined, our next point is to settle the degree of relationship. Are they parent and child or children of the same parent? In many cases the vocabulary will suffice of itself to answer this question, but in some it will be necessary to have recourse to the more recondite question of grammar or form. Our first enquiry now will be, Is the language synthetic (*i.e.*, Does it use inflections, such as declensions and numerous tenses of verbs without auxiliaries), or analytic (*i.e.*, Does it use prepositions freely and form its tenses with auxiliary verbs)? As a general principle the synthetic will be the older; at all events, the answer will set at rest the relationship so far as that an analytic can never be the parent of a synthetic tongue, whilst all analytic tongues are the children of previous synthetic ones. If, however, we find the resemblance, although unquestionable, yet so remote as to render it impossible for us to include them in the same genus or perhaps even class, we must carry our enquiry further and see whether the fundamental principles of grammar agree, and how. Here, then, are two main points: 1st, The position of the predicate; 2nd, The nature of the copula. If they agree in these they are of the same family; if they disagree they are not so. Our last step, then, is to enquire if one or the other be truly inflected or merely agglutinated, and the answer here will enable us finally to place each in the grand division to which it belongs.

Returning now to the vocabulary, I would request you to bear in mind that all words are not of the same value for comparison. It

*Fate Max Müller, Lectures on the science of Language. 1st Series.

would, for instance, be less important to find the same words for luxuries, or scientific terms in two languages, than it would be if they were names of primitive ideas, such as "water," "fire," "food," "sun," "moon," "man," &c., or the numerals, personal pronouns, and auxiliary verbs. These are almost always retained, however much the vocabulary may vary otherwise. Thus in English, the Norman element, which has influenced our vocabulary generally to so great an extent, has had no effect whatever upon our numerals, (EXCEPT SECOND), pronouns, or primitive verbs which are still Saxon as before. So, when we find such words as "Captain," "General," "Emperor," and the like, in languages so different as English, French, and Russian, we should not be entitled to say they were of the same group, though we found hundreds of such resemblances. These words not being primitive ideas, are most likely borrowed from a common source; but when we find such words as "water," "fire," "man," &c., the same in English, German, Dutch, Danish, &c., we are entitled, almost certainly, to refer them to the same group. When, in addition to this, we find the numerals and the pronouns alike, the certainty becomes as great as it can be, without taking into consideration the rules of grammar.

If we do not find the words in common use for primitive ideas the same in two languages, it does not necessarily follow that they are different, although divergent. The root which exists in the one may have been lost in the other, but may still be recoverable, if we can only trace the history of the tongue far enough back. Thus "fire" in Italian is "fuoco;" in Russian it is "огонь." Here the difference appears hopeless, till we go back to the parent of the Italian, the Latin, and there we find the same root in "ignis." Again water in Greek is ὕδωρ, in Latin "aqua." But we find in the word "sudar," sweat, the Greek root in a slightly modified form, whilst the Latin one re-appears in the Persian "ab" and the Sanskrit "apa." Again, in the Sanskrit, the word "des," land, (still used commonly in Bengali, but less commonly in Hindustani) re-appears in the "Irish" "des," but seems to be lost in the other languages except as a compound, as in Greek, the termination *νικ* in such words as *ἱστιαοῦν*.*

Words again may become changed in meaning from several causes. Either the original word becomes gradually confined to a part of its meaning only, rendering the use of a cognate word necessary to express the first meaning; or a portion of a compound, or a phrase may be dropped in process of time, leaving the portion that remains inexplicable. Thus, as an example of the first, the Spaniards use the word "hermano" to signify brother, although the original Latin "germanus" meant first cousin, as it does with us at the present time. This arose from the word "frayle," (from *frater*,

*Vide remarks in Pocock's *India in Greece*.

fratellus) having come to signify a brother of a religious order, a friar, in short. It was then necessary to take another cognate word to express the original idea. As an example of the second mode of change, we may take the Italian word "troja," a pig, a word which exists in French as "truie," a sow. Here, etymology is completely at fault, until we discover *historically* that it was originally "porco di Troja," which meant a stuffed pig for roasting, and which got its name from a reference to the famous *stuffed horse of Troy!* This example was cited by the late Sir G. C. Lewis, to show how it would be impossible to decipher an unknown inscription in a lost language. He was so far right, that we have hitherto failed to decipher the Etruscan, to which we had no key, but the example he aimed at was the Egyptian, and there it did not apply, as we had the Coptic to start from.

This loss of words and roots is less marked in the Semitic tongues, but it reaches its acmé in the Turanian. Here, not only words but whole languages disappear in a single generation. This is notably the case among the American Indians, where, since the discovery of the new world, more languages have probably *disappeared* than are now spoken over the whole earth! Of the tongues of the six nations who fought on our side in the American war not ninety years ago, not a trace remains. Humboldt mentions having seen an old parrot in South America that spoke a language which had become entirely extinct. Various causes contribute to this strange state of things, the chief being the nomadic or wandering life of the people, and the utter want of a literature to fix the tongue. Another curious cause is referred to by Max Müller as prevalent in Tahiti, where it is considered etiquette to drop all words which *resemble even* the name of the reigning monarch. Sometimes only a syllable is dropped, sometimes the whole word. "It is" says Max Müller* "as if on the accession of Queen Victoria it had been agreed to drop the word *victory* and use *triumph* instead, or to cease to speak of *Tories* and call them *Liberal Conservatives*." You will easily understand how rapidly a language must change which is made the victim of such strange vagaries. It is this vanishing of roots, words, and tongues that has made the study of the Turanian language so difficult and their classification so imperfect. Of course where a literature exists, as in Turkish, Finnish, Hungarian, &c., the language becomes fixed, although it retains its Turanian form.

These considerations will show the weakness of mere word comparison as the basis of linguistic science, and will prepare us to receive the more readily the aids which can be afforded by the study of grammar.

To examine these in detail would demand more time than could be at present afforded, and it will suffice to take one or two salient points. The fundamental points to which I have already referred

*Lectures on the Science of Language, 2nd series.

are, 1st, The position of the predicate; 2nd, The nature of the copula. Now in the Turanian languages the connection of the different members of a sentence is decided by their proximity without any special reference to declension, case, mood, &c. Thus, as Bunsen has expressed it,* a Turanian would say in reference to the action of striking, "strike" as the first idea; secondly would come the idea of the individual who strikes, "I," so his first phrase would be "strike I." Next, the time would present itself and would naturally be measured by *light*; so the idea would be "light that is," or "this light," hence the phrase "strike I this light" for the present, "strike I that light" for the past. To a people dependent so far upon juxtaposition for expression, such a thing as obliterated or even concealed declension would be intolerable; so that a substantive verb like the Hindustani, which has but one word for the 2nd and 3rd persons singular, and again but one for the 1st and 3rd persons plural, would be unendurable. Let it be observed that this does not touch the subject of *vanishing roots* before referred to, as the roots or words are there conventionally lost; it refers only to the form of expression.

What the Turanian would express by mere agglutination the Aryan-Semitic family would express by inflections and by a more or less elaborate syntax. Here comes the significance of the predicate and the copula. The predicate is that which is said about or which qualifies or limits a subject. The copula is that which binds together the members of a clause or sentence. Thus to take the sentence we have already employed, "John's letter demands attention," "John's" is here the predicate and precedes the noun or subject, as it does in almost all the Aryan tongues not resolved by progressive analysis and the interpolation of prepositions. This, translated literally into Latin, it would be "Johannis literæ attentionem exigunt." Here "Johannis" is the predicate. So in Hindustani (which I prefer to take as being both a good example and a spoken language, familiar to many), it would be "Yahiye-ki chithi lihaj mangti-hai." Here also "Yahiye-ki" is the predicate. This procession of the predicate obtains in nearly all the Aryan tongues; and is indeed one of their chief distinctions. On the other hand, in the Semitic the reverse is the rule. Here, the predicate follows the subject. Thus let us take a name of most common occurrence in Arabic,—Abdallah. This means "servant of God." But had it been an Aryan tongue, it would have been "Allah Abd," "God's servant," "Dei servus." In Greek it becomes "Theodoulos," where the predicate "Theou," God's, precedes the subject "doulos," servant. In Hebrew the same word is "Abdéli." Similarly, if we analyze any compound Hebrew word, we shall find this arrangement: "God's house" becomes "Bethel" or "House God's." Numerous other examples might be cited, which would, I think, be sufficient evidence for the majority of people. Some, however, have opposed this view,

*Outline of Universal History.

and have instanced certain cases in which an Aryan tongue uses a Semitic form. As for instance, in Greek the word ἵπποταμός means literally "horse river" instead of "river horse," Ποταμῖνος as it ought to be. This certainly appeared a curious exception. The deciphering, however, of the Egyptian (which is a pre-Semitic tongue), showed that ἵπποταμός was only a literal translation of the Egyptian word, whose form was of course Semitic. This procession of the predicate obtains however to a certain extent, as I shall have occasion to mention, in the Celtic tongues. With regard to the second point, the nature of the copula, it may perhaps be less easy to render clear to an Aryan audience the peculiarity of the Semitic tongues, as we are so much accustomed to the, apparently to us, necessary presence of the substantive verb in order to complete the sentence. That this is, however, an Aryan peculiarity, I shall proceed to show. When we say "man is mortal," man is here the subject, mortal the predicate, and *is* the copula. If instead of Aryans, we were Semites, we should say "man he mortal," that is we should use "he," the personal pronoun as the copula. Now this may seem very extraordinary to some of you; but I would ask you to take up your Old Testament, a book translated out of the Hebrew and the Chaldee, two Semitic tongues, and you will find that whenever "is" occurs—provided it be not a part of a tense of another verb—it is printed in *italics*, showing that it has no existence in the original tongue. Taking at random any chapter, I find "I *am* this day King of Israel;" original, "I this day &c." "Thy servant *is* come;" original, "Thy servant *he* come." "I *am* this day fourscore years old;" original, "I this day, &c." In every case, unless conjoined with a participle of another verb, the substantive verb has to be supplied. This, however, is not to be construed into a total absence of a substantive verb, because there is one, *kan*, to be, which is used in certain emphatic phrases, but when used it is more a rhetorical than a grammatical form, and quite apart from the nature of a copula. I may illustrate this peculiarity of the Semitic tongues, from a language spoken in our own dominions at the present day, and at no great distance. I mean the Maltese. This language, with its vocabulary enriched from the various languages of Europe, is still entirely a Semitic tongue; so much so, that a Maltese can make himself understood with facility to an Arab of Africa, or vice versa. In this tongue the Semitic forms are well preserved. Suppose we ask "What book is that?" We should say "Xi ctieb hu dac?" Here there is no substantive verb; *hu*, the copula, is the personal pronoun, *he* or *it*. Or again "Where is your book?" "Foin hu il ctieb tighieb?" Where *is he* the book your? or, "This is of gold," "Dina hu tad deheb," "This he of gold." Let us now take another sentence "The master is in his house." This, in Arabic, would be rendered by "Fi id-dar sahib ha." "In house master his." It will thus

be observed that the form of the sentence is as it were reversed. This will be perhaps better seen if we take the sentence we have already used: "John's letter demands attention," in Latin "Johannis littere attentionem exigunt;" or in Hindustani, a language of our own dominions, "Yahiye-ki chithi lihaj mangti hai," "John's letter demands attention." In a Semitic tongue this would become "demands attention letter John's." It will thus be seen that with the Semites the order of thought is 1st, The verb or copula, 2nd, The object, 3rd, The subject, 4th, The predicate. With the Aryans the reverse—1st, The predicate, 2nd, The subject, 3rd, The object, 4th, The verb and copula.

The paramount importance of the verb with the Semites is evident from the fact that every root in their tongues is a verb, the 3rd person singular, of the preterite tense, whilst on the other hand among the Aryans the roots are almost invariably nouns. I have prepared a few examples of sentences in Aryan tongues for comparison,* and there you will see, in the first place, that the Romance languages—French and Italian—are closely related to each other and both to the Latin. It would, of course, require a more profound study of the subject to detect the synthetic form of the latter in contradistinction to the analytic form of the two former. Again it will be evident that the three Celtic are closely related, as are also the seven Slavonic and the ten (including the Scotch and English) Teutonic. We can thus safely say that each group of tongues had, or most probably had, a common origin. To go back further and see whether the parent or typical tongues of these groups can be referred to one more remote, we must examine and see whether we can trace any community of roots. Between the Celtic and the Italic we can do so at once in the name of God which is Deus in Latin, Dia or Duw in the Celtic. Again between the Celtic and the Teutonic there is a connection in the Gothic Dhindos, the Anglo-Saxon Dhioder, and the Irish Daoine. In the Slavonic the word *krav*, blood, is evidently the same root as *crux*, the Latin for gore or clotted blood. Thus we see even in this one sentence clear inter-connections between all the groups; in addition, the numerals are almost identical, and the grammatical and the syntactical forms are nearly the same. With such evidence as this—and it might of course be vastly extended—before us, may I think look back to a period when those tongues were still one before the various members of the family separated, and each took his own road and commenced to develop his own peculiar idiom. This is the tongue which is supposed to have existed, and which has been called the Aryan. In a similar way it could be shown that the Semitic tongues might be referred to a common origin. But we can go even further, for, finding certain primary roots, such as *earth*, *heaven*, &c., present in both Aryan and Semitic tongues, and also certain languages, which, partaking of the grammatical forms of both families, yet differ from them and are evidently older, we are led to

*See page 25.

the consideration of a period when even those venerable tongues were young, and consequently to the conclusion that they must have had a common root from which to spring. This brings us back to the time of the definite settling of the inflected class of tongues. Beyond this it becomes more difficult. It can, however, be shown that even in the Turanian tongues, with their ill settled forms and the vanishing tendency of their roots, traces can be seen of a community of roots not only among themselves, but also with the inflected languages. Some words indeed appear to have been common to all mankind and to have existed in the most remote ages. Curiously enough one of the most remarkable is the word for *goose*, which appears to be present in a more or less modified form in almost every language of the world. In English it is *goose* or *gander*, in German *gans*, in Latin *anser*, in Sanskrit *hansa*, the same in Cingalese and Burmese. In all the tongues of the Turkish stock it is *kas* or *gas*, and so on. We are already accustomed to look upon the goose as an historic bird, from the famous geese who saved the capitol of Rome from Brennus and his Gauls, and we have been taught that on that account they were held sacred ever after. But it is generally forgotten that those very geese that gave the alarm were already sacred, being kept in the Temple and having been spared from destruction, even when the garrison was reduced to great straits. It is therefore certain that the consideration in which the bird was held was of a much older date than the Gallic invasion, and this is, I think, the more clearly proved by the almost universal presence of the name.*

If, now, we are entitled to look back to a time when the Turanian and the inflected languages were one, it must have been a time long ere the two classes divaricated and before the one acquired the power of agglutination or euphrasis and the other that of inflection. At such a time the vocal means of communication between members of the primeval family must have been limited to necessities only, and obviously confined to monosyllabic utterances. Number, beyond the idea of two or perhaps four, could have had no existence, as at the present time among the Bojesmans and some other very primitive savages. Pronouns were as yet unknown, as man had not arrived at the conception of subject and object, and probably spoke of himself as a child does, in the third person and objective case. This is the state of language to which I have given the name of the *Archaic*, from ἀρχή, the beginning.

Where then did the primeval men who spoke this Archaic tongue dwell? All the traditions of the European races, as well as what history we have of their early migrations, point to Asia as their starting point, or at least to the eastward of their present homes. The Greeks certainly used to boast of being *autochthonos* or native-born children of the soil, but their mythology, many of their traditions

*See Sir J. Emerson Tennant's Ceylon.

and the opinions of some of their own greatest philosophers prove such a notion to be the mere offspring of national vanity. All the Semitic and even the Egyptian traditions point to the valleys of the Euphrates and Tigris as their original seat. Among some of the African tribes a tradition, I believe, exists of their coming from the North. The Hindus we know historically to have been trans-Himalayan. The American tribes all, or almost all, believe they came from the North, the high road to Asia. Lastly, even the Chinese have shadowy traditions which place their origin to the West, toward the centre of the continent of Asia. When after a lapse of time the maxims of Confucius were becoming forgotten, and a period of religious anarchy occurred, a tradition still remained that the true faith was to be found in the West, and an emissary was despatched by the then emperor to seek that faith. This messenger arrived in India, where at that time the religion founded by Gotama Buddha—and afterwards supplanted by Brahmanism—was flourishing vigorously. This he conceived to be the true faith, and having made himself master of the wanderings and virtues of Buddha he returned to China, where he published these in Chinese, and where the Buddhist faith reigns to this hour. The full details of this interesting episode have only recently been brought to light by the labours of M. Stanislas Julien, who has devoted more than 20 years to the task of translating. I might almost say *extenuating*, the wanderings of Fo (the Chinese word for Buddha) from the Chinese back into the original Sanskrit. This process was absolutely necessary to the understanding of the matter, as the manner in which foreign names alter in Chinese renders them helplessly obscure, without a systematic restoration. This arises from various causes, such as the absence of the letter *r* and the incapability of the Chinese to pronounce or apprehend except as separate monosyllables. I have already mentioned that Buddha with them became Fo, but this is only one of a thousand examples.

Taking then all the various traditions together, aided by what we have in the way of history, we may conclude that some part in the centre of the continent of Asia was the original seat of primeval man, and it would appear that either the valleys of the Euphrates and Tigris, or perhaps more probably what is now called Independent Tartary, and what was anciently known as Ariana and Bactriana, was the cradle of the human race. There sprang that "Wondrous being, the Lord and king of nature all, whose large and arched brow sublime, of wisdom deep declared the seat."

We must picture to ourselves this primitive Archaic people, few in number, leading their wandering life in the fresh brightness of a virgin world. Their wants were few, so must their tongue have been limited. For numbers of years, probably centuries, perhaps thousands of years, after speech had been given to man, monosyllabic utterances were all that he could have used, juxta-position of words

alone indicating their connection and inter-dependence. The life was probably patriarchal and nomad over the vast plains of Central Asia. At length a branch separating from the Archaic stock departed to the eastward, and arrived at last in a smiling country, well watered with rivers and teeming with verdure. Here they settled themselves and called the land *Sin*, which we have since called China. The beauty of the climate, the fertility and productiveness of the soil, and the absence of the extreme vicissitudes of temperature which characterised their former home, were all highly favorable to fecundity of race, and the rapid development of an early civilisation. But they had carried their Archaic tongue along with them, and civilisation having as it were overtaken them before their language was developed it became fossilised in monosyllables and remains so to this day. They soon acquired the art of writing, first symbolic or hieroglyphic, but in later times the characters have become so modified as to have lost in great measure their pictorial character. This, the earliest civilisation, Bunsen designates the Eastern polarisation of Sinism. The remains of the Archaic stock, which I have designated the Koinic, or that which was common to the rest of the world, must have soon after begun to divide. While one portion continued the nomadic or wandering life of their forefathers, another had probably begun to feel the advisability of working together in a common interest, and in fact took the first step towards political union. The former have been styled the Turanian or Nomadic, and the latter I have called the Demic or Political. The change, it is probable, first took place in the language. The Demic branch felt probably the difficulty caused by the tendency to change in the Turanian roots, the result of the patriarchal life, and united themselves together in a community larger than that caused by mere family ties, and this alone tended to the fixing of their word roots. Soon inflection must have followed, probably dependent originally on mere juxta-position, but rapidly getting beyond it as the Demic mind expanded. The Turanian, on the other hand, stuck to his agglutination, beyond which he has hardly got to this day.

Soon after the division, a branch seems early to have separated itself from the main Demic stock and set out to the west to seek fresh fields and pastures new. A congenial spot they found on the banks of the mighty Nile, a rainless country, but wondrously productive, irrigated as it was by the great river. Here was found the second great civilisation of the old world, and this is called by Bunsen the western polarisation of Khamism.

Here, too, the art of writing was soon acquired and was at first symbolic or hieroglyphic. By and by the pictures became less distinct and a second form called the Hieratic came into use. This corresponds in a great measure to the Chinese writing at the present time. But, whereas the latter stopped at the second stage, the Egyptian went on further and produced a current hand known as

Demotic or Enchorial. All these, however, have passed away with the civilisation, and the Coptic, which is now the representative of the old Egyptian, is printed and written in Greek characters.

In the Egyptian are found grammatical forms partaking of both the Aryan and Semitic, but are themselves of an older, type, indicating that it must have come off from the common stock previous to its division.

That which remained I have called the Euphratic for convenience, and we shall leave it for the present to follow the fortunes of the Turanian family.

Those wandering hunters becoming at length, like a swelling sea, too mighty for its bed, flowed over in successive waves wherever necessity or the spirit of adventure urged them. In search of new hunting grounds some went to the rising, some to the setting sun; some were driven to the ice-bound north, others penetrating at length beyond the hills, whose summits seemed to reach to heaven, spread themselves far and wide over the sunny plains of India. In every part of the world they appear to have been the first to tread the virgin soil, excepting China and perhaps Africa. In Europe we find traces of them to the present day either in history, in antiquities, or in the actual presence of their descendants.

There seem to have been four branches at least. The Iberian we know in historic times occupied the whole of the Spanish peninsula and the south of France; and the Ligurians and Siculi occupied a considerable portion of Italy and the whole of Sicily. Incursions of more powerful nations gradually reduced them to the small patch of the Basque people who call themselves Euscara and their language Eusealdunac, and who occupy the two slopes of the Pyrenees and immediately adjoining land.

The famous Etruscan or Tyrrhenian race appears to have been also Turanian, but of this we cannot be certain as the inscriptions in their language have as yet baffled the efforts of archaeologists.

Another branch is still represented by the Albanians, whose language, like the Basque, was long a puzzle, situated as it was like a boulder of another formation in a stratum of comparatively recent tongues.

The last branch was the Finnic, still represented by the numerous races of northern Europe, and in a later form by a noble and heroic people, the Hungarians. The incursions of this race, known in history as the Huns, was the last successful attempt of the Turanians to penetrate to the west of Europe. Their power was broken and the tide of invasion rolled back at Châlons-sur-Marne by Clovis and Attila. Before leaving the Finnic branch I may mention that it has been recently discovered that the Finns possess in their language, one of the most splendid epics the world has produced, worthy to rank, it would seem, with the *Iliad*, the *Mahabharata*, the *Nibelungen lied*, and the *Roman de la Rose*.

The Turanians who first spread to the East, driven on probably by advancing Tunguses, Samoiedes and Mongols, found their way by Behring's Straits to the American continent, and succeeded in establishing an early civilisation, whose remains we see in the ruined cities of Central America. Upon this the civilisations of Mexico and Peru were a recent superposition.

The great Malay branch, passing to the South, probably urged forward by the Lohitic and Taic races, peopled first the Eastern peninsula, and subsequently spread over the Archipelago, Australasia, and Polynesia.

The present inhabitants of Thibet, the Himalaya ranges, and the Eastern Peninsula, appear to be of a more recent age than the Malays.

The great Dravidian branch which crossed the Hindu-Kush, and the Western Himalayas, established themselves in India proper, and in Ceylon. Though overrun by the Hindus and Mohammedans, they are the most numerous inhabitants of the Madras country under the names of Tamuls, Telingus, &c. In Bengal, however, they linger only as isolated hill tribes, such as those mentioned on the table, the Khasiyas, the Ghonds, the Santhals, with whom we had a war about ten years ago, and some littoral races, as the Uriyas of Orissa, and the people about Chittagong, some of whom go naked, are cannibals and live in trees. They are still represented too, by the degenerate Veldas of Ceylon. A most curious and interesting branch are the Coles, from whom the modern name Cooley is derived. These Coles appear to have been more than a mere branch, and it is a question whether the name did not originally designate a large section of the Turanian race, for traces of them are found in widely separate regions. Doubtless our old friend King Cole was one of them, and his name still remains in some districts, as for instance Coil or Kyle, and Coilsfield, in Ayrshire, as well as in many other parts.

The great Tataric branch spread itself over central Asia, and succeeded as the Mongols and Manchus, in the 17th century, in making themselves masters of the Chinese throne, whilst the Ouigours in the west gradually extended their incursions, until at length, in the fifteenth century, they penetrated to Europe, and under the name of Turks or Osmanlis, overthrew the empire of Constantinople. So great was the terror they inspired that from their name Ouigour comes the word *ogre* in use at the present time, and, as late as the beginning of the last century, hymns were written in which the petition "From Pope and Turk defend us Lord," was frequent. Indeed Europe had no small reason to fear them, and it was only at the end of the 17th century that their attempts on Central and Western Europe were arrested by the brave Sobieski and his gallant Poles, a debt to that noble people which Europe appears to-day to have conveniently forgotten.

*See an interesting paper by Dr. Chevers, in the *Calcutta Review* for 1858.

We must now turn back to the Euphratic race, which was destined in most cases to wrest from the Turanians their fairest possessions. Probably the first change was the separation of the Kushic or Babylonian and Assyrian stock, and their settlement in the Euphratic valley. These tongues present peculiarities of structure which are evidently pre-Semitic but posterior to the Khamitic. It is here the place to speak of the African races. It is generally agreed that they cannot properly be considered as Turanian, as their dialects present appearances of inflection. Their grammatical forms are peculiar as they place the predicate before the subject like the Aryans, but use the personal pronoun as the copula like the Semites. It is therefore probable that they belong to the Euphratic stock, having separated from it before the division into Aryan and Semitic.

Soon after the departure of the Kushites and the Berbers, the Aryo-Semitic branch finally split. Although to both the vanishing roots (what may be called the *marantokhical* condition) of the Turanian stock were intolerable, yet the Aryan found himself too much confined by the rigid retention of roots or *phanerokhical* tendencies of the Semite, and had resort to fusion and concealment of roots or a *cryptokhical* etymology, which was reciprocally offensive to the Semite. So each went his way, the Aryan with his verbal copula and preceding predicate, the Semite with his succeeding predicate and pronominal copula. The history of these two families is the history of the world. Their characteristics were as different as their tongues. In the Aryan, the intellect reigned supreme, giving birth to science, logic and philosophy: in the Semitic, imagination prevailed and gave rise to all the religious movements which have benefited or convulsed the world.

The Semitic family has much fewer branches and fewer members of them than the Aryan. Three main stems can be made out. The Hebrew, the Aramaic and the Himyaritic. The first, the most interesting, includes the Jews, the Phœnicians and the Carthaginians, or Punic race. The second, the Aramaic, included the Syrians and the Chaldees, a branch of this language being the one used in Palestine at the time of our Saviour, as is proved by the sentences recorded in the Evangelists. This branch is considered to be represented at the present day by the Nestorian Christians in Kurdistan. The Himyaritic branch included the Arabic and Amharic or Aethiopic races, the former of which is the dominant Semitic race of the present time, while the latter peopled Abyssinia and the countries bordering on Egypt.

We may view the Assyrians and Babylonians as belonging to the Semites, as they are most nearly allied to them, although properly pre-Semitic. If we do so, then we may consider three great struggles for the mastery of the world, as having taken place between the Semites and the Aryans. In each case the Semitic power blazed up with spasmodic brilliancy, and appeared ready and able to consume every-

thing before it, only to sink in each case as rapidly as it rose, before the steady advance of the Aryan race. The first epoch closed with the extinction of the Babylonian Empire by Cyrus, after which no Semitic power of note came into prominence for some centuries. The Greeks, save some paltry contests with the Phœnicians, never encountered any Semites.

The next great stroke for empire was made by the Carthaginians, and for a time the fate of the Aryan race seemed sealed. But Hannibal's meteoric light faded when Hasdrubal fell at the Metaurus, and Carthage was blotted out from the face of the world.

A lull of well nigh nine centuries followed, till the Saracens having conquered Syria, Egypt, and Northern Africa, and having overrun and subdued Spain, crossed the Pyrenees and ravaged the South of France. The fate of Europe trembled in the balance; but again the Aryan prevailed, and at Tours, Charles Martel slew Abulrahman and destroyed the Saracens, but for which victory, as Gibbon remarks, the Koran might now be taught at Oxford, and the muezzin call the faithful to prayers in the heart of London. Since that time, if we except the Mohammedan impetus given to the Osmanli invasion in the fifteenth, sixteenth, and seventeenth centuries, and to the Persian invasion of Hindustan in the eighteenth, the Semites have been more employed in holding their own than in attempting the conquest of other countries. Their influence, however, in a more peaceful form is extending rapidly in another direction, and Mohammedanism bids fair at no very distant period, to be the universal religion of the African continent.

We have now to consider the great Aryan family—that numerous and mighty race which seems destined to rule the entire world. Soon after it became a distinct family, a branch shot out from the parent stem to the westward. That branch was the Celtic. Three considerations render this early origin probable. First, The Celts evidently preceded in Europe everybody except the Turanians. Secondly, There is evident in the language a tendency to non-Aryan forms, such as a hankering after putting the subject before the predicate. Thirdly, Their are in it certain roots and forms not to be found in a later than a pre-Semitic tongue, requiring us to go back even to the Egyptian itself. Then the course of its wanderings can be traced out by the names it has given to rivers, mountains, and valleys, which have survived centuries of conquest by the most divergent peoples. For a long time the claims of the Celts to be considered Aryans were disputed, but Prichard and Pictet have put the matter beyond doubt. The Celts early divided into two if not three branches. The Ossethic branch is doubtful. Of what the Sarmatian and Alan tongues were like we know little, but the modern Ossethic is said to bear a certain resemblance to the Celtic. I have therefore ventured to put this branch, although with hesitation, as a possible member of the Celtic family. The other two branches

however, are the more interesting to us. The southern or Gadholic Celts, appear to have moved westward first, occupying successively Greece, Italy, and France, as well as Western Germany. In all these countries they drove out, more or less completely, the Turanian races. The vigorous Iberians of the Peninsula, however, proved a match for them, and they only effected a partial lodgment there. From France they passed across to Britain, the greater part of which they occupied, as well as Ireland, in pre-historic times. Here we must leave them for a little, and follow the Kymris, or northern Celts. These, alluded to in Scripture under the name of Gomer, passed through Southern Russia, giving their name to the Crimea, across Eastern Germany to the country between the Elbe and the Vistula. Here they gave their name to the Cimbric Chersonese, and appear to have divided into two streams. One pressed to the westward and occupied all the country to the Seine, driving out their Gaelic kinsman. They then crossed the sea and speedily overran the whole of Southern Britain, driving out or absorbing the old Gaels. They are still represented by the Welsh and the Cornish, the latter extinct as a spoken language only eighty years ago. The second stream went northwards and occupied the Scandinavian peninsula, where they probably mingled with the Finnic race, as their stature undoubtedly deteriorated. From Scandinavia they passed after a time across the seas and overran Scotland as the Picts.

Returning to the Celts in Gaul, we find that the pressure of the Kymris drove many of the Gauls south into the Iberian country, from whence they were expelled, and taking ship, landed in Ireland under the name of Scots. They overran the whole of that country, and afterwards passing into Scotland, not only overcame the Picts, but blotted out both their name and language and imposed their own upon the country.

Three interesting points have been decided of late years, by the examination of the Celtic tongues. The peculiarity presented by them, notably the Gaelic branch, in having no neuter and many of their feminine nouns of the masculine gender, as well as the phenomenon of eclipsis, seemed to remove them far from the Aryan stock. But Zeuss, the great German writer on the subject, discovered that the most ancient Irish manuscripts were to be found, not in Great Britain or Ireland, but in Switzerland and Italy. This will be easily understood when we consider that Ireland, the Island of the Saints, was the great seat of learning in the dark ages, and that from it missionaries proceeded to all parts of Europe, instructing the people and founding monasteries in various places. Among others they founded establishments at St. Gall and Milan, and there were Latin manuscripts discovered, with glosses in native Irish. This proved to be a much older form of the language than any previously known, and in it was found a true neuter gender, whilst the phenomenon of the feminines becoming masculine arose

from their having been originally neuter—as they are in modern German—the neuter gender becoming gradually lost and its place supplied by the masculine as in the modern Romance tongues.

For long the true nature of the ancient Gauls was a moot question, but within the last ten or twelve years, several inscriptions have been discovered in the Gaulish language, which can be interpreted only through the Irish—proving incontestably that it belonged to the Gaelic branch. Lastly the Picts, whose tongue had disappeared, with the exception of one word, have been held by some to be Teutonic; but lately an inscription, which appears to be Pictish, has been deciphered, and proves undoubtedly to be of Kymric character.*

The remainder of the Aryan race, which I have named the Eurasic, probably divided into two, the European and the Asiatic, so designated from their subsequent geographical position. The Asiatic divided again into two main branches, which were probably earlier in their civilisation than any of the European. The one the Iranic, whose chief representative was the Persian, was destined to go through many vicissitudes—first to overthrow the Semitic power in Asia, to succumb afterwards to the Greek, then after a second brief period of independence to fall in its turn before the Semitic power of the Caliphs. The Persian language shows in itself the evidence of these successive changes. As the original sacred language of Zoroaster, the Zend, it is the first we have met with of the great synthetic class, although, unfortunately, little of it has survived. In the form of the Pehlvi, it was the spoken language of the Achemenian period. Since the Alasside conquest it has become loaded with a Semitic vocabulary, and even partially changed with Semitic forms, both of which it carried into India.

The other and more important branch for us, the Indic, is confined entirely to the Peninsula of Hindustan. Starting from the valleys of the Hindu-Kush, the Hindus appear first to have penetrated into Northern India some fifteen or sixteen centuries before our era,—a numerous and vigorous race, speaking Sanskrit, and having already made some advance in arts and sciences. Their religion was Brahmanism, but apparently not the idolatrous faith of later times. They soon drove the Turanians to the south and into the hills, and established themselves over the whole of the north. There appear to have been several kingdoms, either contemporary or successive; Delhi, Allahabad or Prag, Kanauj, Patna, and Rajmahal were the different capitals. The Sanskrit gave way in time, and passed from its synthetic stage through the Prakrit and Pali, until it became broken up into the analytic tongues of modern times. Similarly the religion became corrupted, and more and more overlaid with idolatry until about the sixth century the great reaction of Gotama Buddha took place and Buddhism became the dominant

*Fide Paper by Mr. Skene on the Newton Stone, Antiquarian Society (Scotland).

Indian faith. This was the faith when the Pali-speaking Bengalis, under Wijayo, sallied forth to the conquest of Ceylon. By the time, however, of Alexander the Great and his successor Seleucus, the older Brahman faith had again obtained the mastery, and Porus, who opposed Alexander and the King of Kanauj, who was overthrown by Seleucus, were both Brahman monarchs. The magnificence of the ruins, the vastness of their cities, and the wonderful copiousness and richness of their language and literature, attest the splendour of those early Hindus, and it would be difficult to believe that the mean and sensual Bengali could be the descendant of such a glorious ancestry, had we not their counterpart in the Greek of the present day. The parallelism between the two races is indeed remarkable. Both languages are copious and synthetic in the highest degree. Both races had carried art, as represented in sculpture and architecture, to a pitch of unrivalled perfection. Both made rapid and marvellous strides in the physical sciences, and they are the only two races of the world who have founded independent systems of logic. Finally, as both were mighty in their prime, so are they low in their decadence, the one still under the foot of a conquerer, the other owing to the charity of the stranger what it possesses of a weak and futile independence.

The European stem divided early into two great parts, the Classic or Pelagic and the Barbaric. To these may be added the Scythic, now extinct. It is to linguistic science alone that we owe the settling of the position of this race, for it is from an examination of the few words left in the Greek historians that we are able to say that they were an Aryan race, which has left no direct descendants, but which was probably closely allied to the Lithuanian.

The classic division comprehends the Italic and the Hellenic. Of the latter the Greek alone remains; the other tongues, the Lycian, Carian, and probably also the Phrygian and Lydian, became extinct before our era. In comparing it with the Sanskrit, I have already spoken of the Greek, and it will suffice to remark that it has retained in later times its synthetic form more perfectly than any other language. Probably for this reason it has produced few or no descendants. The Latin, on the other hand—which soon swallowed up the other Italic tongues—had, even before historic times, commenced to pass from the synthetic to the analytic stage, as evinced by the difficulty it had in forming compound words. Probably for this very reason it lent itself the more easily to modern requirements, and became the parent of a numerous and distinguished progeny.

This, of course, is not the place nor have we time to examine the different theories of the formation of the Romance tongues, but I may state generally that no one doubts now that they are direct descendants from the Latin, without any intermediary form. This is proved by their closer inter-resemblance as we approach the parent stem. A monkish writer mentions that an Italian priest of the time

of Charlemagne, meeting a Spanish pilgrim in Germany, understood his language because he was an Italian (*ejus lingue, eo quod esset Italus, notitiam habebat*)^{*} thus proving that the Spanish and Italian in those days were not so different as they are now. In Spain and Italy, where the Celts were few, the language has departed least from the original type, but in France we can still trace their influence. This is peculiarly shown in the numerals, where the Italians say *settante*, the French say 60 and 10, and where the Italians say *ottante*, the French *four score*. This is distinctly a Celtic form, the Gaels having no separate words for their numerals, and counting by scores and tens.

The Wallachian, again, being far separated from the Romance world and hemmed in by Teutons and Slaves, underwent numerous changes, one of the most curious of which is the change of the guttural into the labial consonant—thus, *lingua* becomes *lembe*, *quatuor*, *patro*, &c.

Lastly, the Barbaric division comprehended the Windic and the Teutonic. These races appear to have arrived in Europe from the north-east almost simultaneously, the Teutonic probably slightly preceding the others. The Lettic branch of the Windic is peculiarly interesting, as it appears in the old Prussian to approach more nearly to the Sanskrit than any of the other Aryan tongues. It comprehends the languages spoken in the Polish provinces of Russia, that spoken in little Russia by the peasantry, and the old East Prussian, now extinct. As before mentioned, it is possible that the extinct tongue of the Scythians was closely allied to this branch.

The Slavonic branch may be divided into two, the older or Palaeo-Slavonic, or the newer or Neo-Slavonic. For a long time these races were held in subjection, and the word *slave* in later times has come to mean a servant of servants. The Palaeo-Slaves, including besides those marked on the diagram, the Polabians, Lusitians, Slovacks, Croats, Illyrians, &c., are probably the deposit left in Europe by the Vandals. Their power, after a momentary flash of splendour under Genseric, was extinguished by Belisarius, and they have never since risen to prominence, except in the case of Bohemia.

The Neo-Slaves, however, comprehending the Poles and the Russians, have already made for themselves a name in history, and are doubtless destined hereafter, to exercise a strong influence on the fate of the world. Although in every contest they have as yet succumbed to their brother Aryans, still the victories have cost so dear, that a time may come when we shall exclaim with Pyrrhus, "One more such victory, and we are undone."

The last, and to us the most interesting group, is the Teutonic. What remains we have of its early tongues, prove them to have been highly synthetic. They have, however, all become analytic in

^{*}See Sir G. C. Lewis' Essay on the Romance Languages.

modern times, the German alone retaining its original synthetic power in anything like perfection. An exception, must however, be made in favour of the Islandic, where early colonisation a thousand years ago fossilised the primitive tongue. The three divisions of this group are the Scandinavian, the High Dutch, and the Low Dutch. Of these the High Dutch, represented by the modern German, is probably the most recent. This, however, is a point very difficult to settle. The Scandinavians came into Europe across the north of Russia, and drove out the Picts and other Kymris from the peninsulas and islands. This doubtless caused the Pictish invasion of Scotland. A thousand years ago the Scandinavian tongue was one, now represented by the Islandic, but gradually dialectic differences have become more and more marked, until we have now three spoken languages, besides the dialect of the Farœ Islands, and the extinct tongue of the Orkneys. Those mighty men of the north, the patricians of the world, as Bulwer Lytton ~~is~~ ^{calls} ~~them~~, finding a congenial home on the ocean, spread terror over Europe, conquering and leaving an undying remembrance of themselves in every country they invaded. As men they were perhaps the grandest specimens of nature's handiwork, and they regenerated Europe by high-metling the blood in the veins of its corrupt and decaying peoples. But their day as a conquering nation is over, and although the martial spirit is not dead, as witness the gallant defence of the Danes in the late iniquitous contest, their population is diminishing and their language gradually giving way to younger and more vigorous tongues. They have left a strong impress on our country, particularly in the east and north. In some parts of Scotland they arrived before the Celts, and the western isles were a Scandinavian kingdom for many generations, and to this day most of the names are Norse. There were also many Danish Kings of Ireland; while the brilliant episode of the Danish monarchy in England is known to all.

Of the Low Dutch, the oldest is the Gothic. This people penetrating into Europe from the north, either before or about the same time as the Scandinavians, soon became a terror to the continent, which they sacked and conquered from end to end. They established kingdoms in Italy and Spain, and greatly influenced the languages of both countries.

The other Low Dutch dialects were numerous, but the earliest that came into notice were the Jutes, the Saxons, and the Angles. The Jutes occupied Jutland, were probably driven out by incursions of Danes. It is not improbable that they represent the Cimbri, destroyed by Marius, who were Germans, but whose land was called Cimbric from the former Celtic inhabitants. The Jutes occupied the eastern counties of England. The Saxons of the coast were long a trouble to the Roman provinces, from their determined piracies; and in the lower Empire there was a high functionary, called the *Comes littoris Saxonici*, whose duty it was to protect the shore

from the Saxon inroads. These were the people who occupied the southern provinces of Britain, and whose tongue we now talk.

The inland Saxons, still represented in the dialect of modern Saxony, were a different people, once powerful, but crushed by Charlemagne.

The Frisians still occupy part of modern Holland.

The language of the Netherlands remained one until about three centuries ago, when a part having revolted from Spain, two distinct dialects were established, the Dutch and Flemish.

The last important Low Dutch branch is the Angle, which came from the country about the Elbe. They occupied chiefly Scotland, England as far as the Humber and Mersey, and some of the central parts—probably Warwick, &c. The Angle or Northumbrian tongue, was the parent of the Lowland Scotch, and of the provincial dialects of the North of England, while the Saxon as before mentioned, is represented by modern English. It is a strange instance of how names become misapplied in course of time. The Scotch language is the one that ought to be called English or the tongue of the Angles, whilst the English is the tongue of the Saxon, and by that name it is called in all the Celtic languages. *Scotas* on the other hand, which has ceased to be applied to a Celt, meant, in the days of Bede, an Irishman! Many old Scotch writers call the language in which they write and spoke *English*.

The last and probably latest of the Teutonic group is the High Dutch, the language of Modern Germany. The Germans call themselves *Deutsch*, a word which we have misapplied to the Hollanders. Their language is still very synthetic, and though it has few dialectic varieties of importance, it is gradually pushing the Low Dutch and Scandinavian tongues before it. It was about the time of Luther that the language became definitively fixed in its present form, his translation of the Bible being the first truly German work. Previous to that time they had but one article, but Luther retained it, *der*, as the masculine, and introduced the Low Dutch *die* as the feminine.

It is a matter of some difficulty to assign the proper place to the Franks, who gave their name to France, and considerably modified its language, but I am inclined to think they were a High German people.

And now in this cursory sketch it will be easily understood how much I have been obliged to leave untouched, and how very imperfectly the points dwelt upon have been treated. Nevertheless, from what has been said, it will be evident that as far as language goes, the argument, if insufficient to prove the unity of mankind, is equally imperfect as a proof of its plurality of origin. Viewing mankind then as one race, is it possible that they may ever become of one speech as well as of one blood? Are languages to go on forming, splitting up and dying out, and new ones replacing them to the end of time? Or is it possible that a universal tongue may be established on the

earth? Attempts have been made in this direction and have failed, nor is it possible at present to say how far success is to be expected. At all events it is from the study of the past that we shall find the clue to the reading of the future: and as geology, by discovering the laws which have regulated the changes the earth's surface has undergone, enables us to predict to a certain extent what the future progress of the physical world shall be, so in like manner an enlightened study of ethnology will unquestionably enable us to call up a shadowy outline of the possible future of mankind. It can of course be only as through a glass, darkly, but the view will become clearer as years roll on. For man is regulated by law, as well as the cataclysms of the earth, and the more facts we gather the nearer we shall get to the governing principle, as Tennyson says—

I doubt not through the ages one increasing purpose runs,
And the thoughts of men are widened with the process of the suns.

It is by such a widening of their thoughts that men may be brought to understand that peace is better than war, and that even as material interest it is better to agree with their brothers while they are in the way. But they will never agree until they know each other better, and become more fully convinced that the noblest study of mankind is man. Then may there be some chance of the vision of the poet being fulfilled—

When the war drum throbs no longer, and the battle flags are furled,
In the parliament of man, the federation of the world.



A COMPARATIVE VIEW OF A FEW LANGUAGES OF THE ARYAN
FAMILY.—See page 10.

TEUTONIC.

English—God has made all mankind of one *blood*.

Scottish—God heth made a' mankind o' ane *bluid*.

Dutch—God heeft nit eenen *bloede* het gansche geslacht der menschen *gemaekt*.

Flemish—Godt heeft uyt eenen *bleet* het geheel menschelyk geslachte *voortgebragt*.

Danish—Gud haver gjørt al menneskenes sleegt af eet *blod*.

Swedish—Gud afwer gjört alt mennisko slægtet af ett *blod*.

Anglo-Saxon—God gewörhte of anum *blode* ealle manna dheoda.

Gothic—Godh gaskop allos dhindos af ainamma *Blodha*.

Icelandic—Gud hefur gërt af einu *blode* aller dhioder mannanna.

German—Gott hat von einem *Blute* aller menschen geschlechter *gemacht*.

CELTIC.

Irish—Dia do rine 'a uile ceimel daoine d'aon *fuil* amain.

Scott Gaelic—Dia rinn e dhaon *fhuil* uile chinnich dhaoinne.

Welsh—Duw a wnaeth o un *gwael* bob cenedl o ddyinion.

ITALIC.

Latin—Deus ex uno *sanguine* totas hominum gentes creavit.

French—Dieu a créé d'un *sang* tout le genre humain.

Italian—Dio ha fatto d'uno *sangue* tutte le gente de g-l'uomini.

SLAVONIC.

Bohemian—Buh utschinil jedne *krve* wssecko lidske pokoleni.

Sclavonian—Bog sotworil she esth o edinije *krwee* wes jesikh-tschlwtjetch.

Servian—Opi *krowi* edinoua proesweo Bog saw todh tschelowjetcheskü

Bulgarian—Bog sosdal oo odna *krwe* snschkiat rod tschelowjetcheskü.

Wendish—Boh je tehnil sowot jeneho *krweje* shitkish tchelowkow narod.

Polish—Bog utschynil s iedney *krwi* uschystek rodsay ludski.

Russian—Bog opi odnoi *krowi* proeswel wsjach tscherowjakow.

NOTE.—The Tongues printed in *Italics* are extinct, and without direct descendants.

TABLE I.
ARCHAIC.

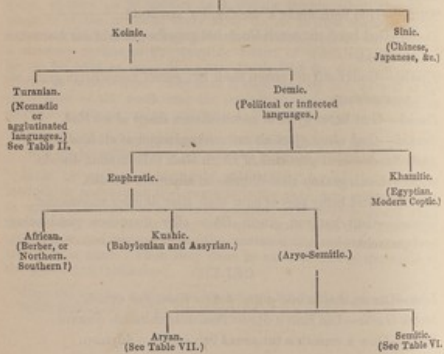


TABLE II.
TURANIAN.

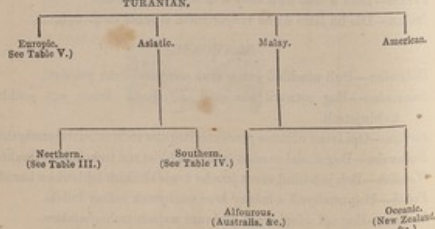


TABLE III.
ASIATIC.
(NORTH TURANIAN.)

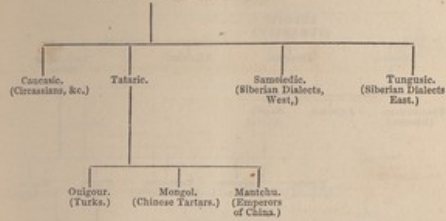


TABLE IV.
ASIATIC.
(SOUTH TURANIAN.)

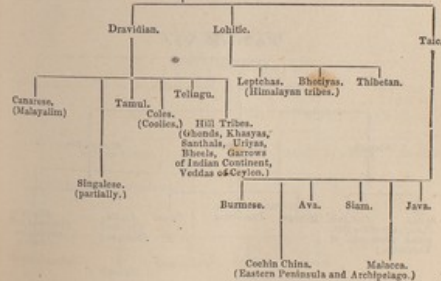


TABLE V.

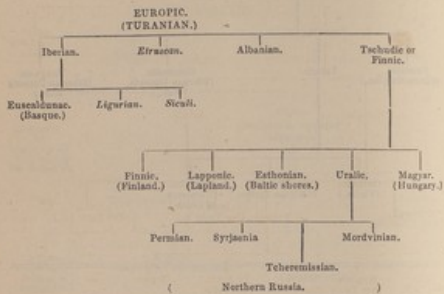


TABLE VI.

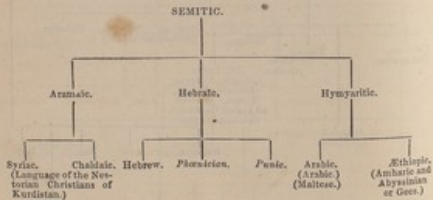


TABLE VII.

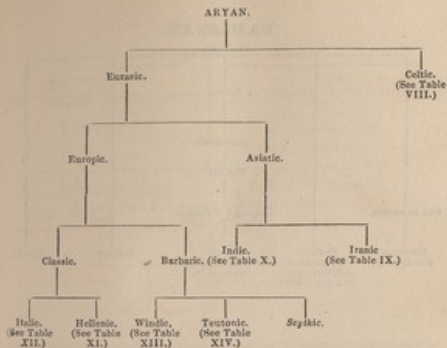


TABLE VIII.



TABLE IX.
IRANIC.

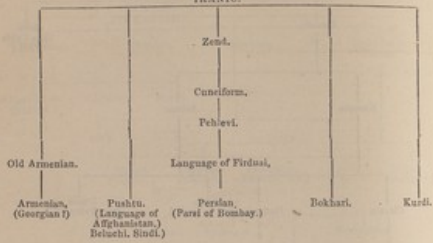


TABLE X.

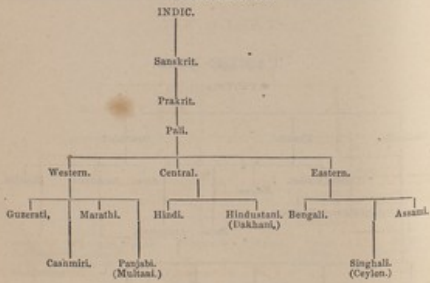


TABLE XI.
HELLENIC.

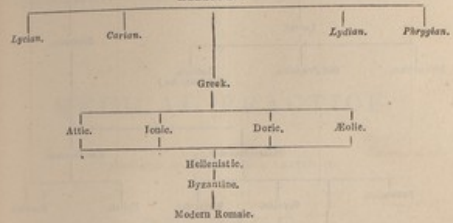


TABLE XII.
ITALIC.

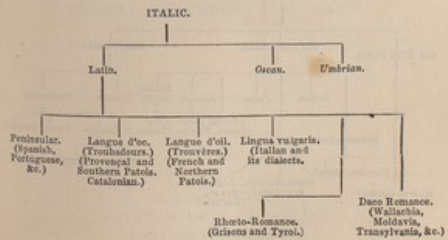


TABLE XIII.

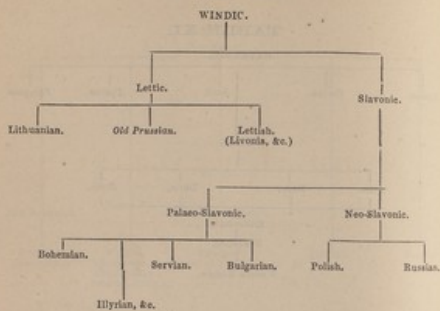
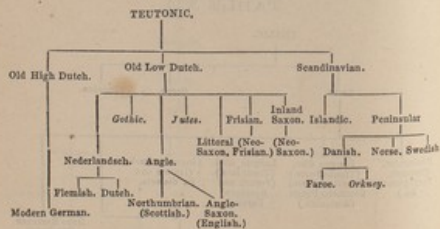


TABLE XIV.



J. J. Bennett, Machine Printing Works, 14, High Street, Southampton.

ON
CERTAIN RECENT ALTERATIONS

IN
MEDICAL PRACTICE,

BEING THE SUBSTANCE OF A LETTER TO

DR. PAGE,

AUTHOR OF THE HARVEIAN ORATION, DELIVERED AT THE
ROYAL COLLEGE OF PHYSICIANS IN 1860.

BY

THOMAS HEBERDEN, M.D., OXON.

LONDON:
PRINTED BY H. M. ARLISS,
GREAT QUEEN STREET, W.C.

1860.

PARK STREET, GROSVENOR SQUARE,
16th August, 1860.

DEAR SIR,

I have been prevented by circumstances from tendering to you, until now, my acknowledgments for the copy of your Harveian Oration, which you were so kind as to send me above a week ago. While thanking you for a composition couched in such elegant Latin, I regret to find it contains some statements with which I cannot concur.

It is quite true, as you say, that at the beginning of this century Depletion of every kind was in great favour with our physicians; and it is equally true that Stimulants, more particularly wine, brandy, and the like, are more freely used now than formerly. True it is, also, as you state, that those who ascribe the introduction of this Method of Cure to the late Dr. Todd are in error. Whether as regards the substitution of wine, brandy, &c., for abstinence, or the exhibition of bark, steel, &c., for Blood-letting and other exhaustive measures, Dr. Todd, if I am not misinformed, was anticipated by many medical men both within and without the walls of our own College.

But in the teeth of facts,—facts patent alike to the majority of the profession and to the better instructed portion of the public,—I cannot agree with you that for this Revolution in Practice we are indebted to no one man in particular. Your assumption that this revolution may, perhaps, have been the gradual result either of some change in the condition of the atmosphere or of some alteration in the human body, you will pardon me for saying, I consider altogether groundless and unphilosophical.

The records of our hospitals, you say, bear you out in this belief. I am glad you qualify that statement with the words "*ni fallor*;" for here assuredly you labour under a misconception. Your impression that the annals of St. George's Hospital shew that this great change of practice began there *thirty years ago* can scarcely be correct. Dr. Chambers, who was senior physician of that hospital at the date you refer to, used depletion very largely up to his retirement from practice;—and his retirement, if my memory serves me, did not take place till about fifteen years afterwards.

You are right, nevertheless, when you say that venesection is every year becoming rarer; and that cupping and leeching, yearly diminished, are now

little employed; while the expenditure in our hospitals for wine, brandy, and malt liquor, has on the contrary increased. You say nothing of the increased expenditure for quinine and other supporting remedies.

This complete REVERSAL of a practice which existed from time immemorial must not only have had a beginning, but it must, moreover, have had some one to begin it. It is not in the nature of things that so sudden and so important a Revolution could originate in many minds at once. If you will turn to the last July number of the *British and Foreign Medical and Chirurgical Review*, you will find that the initiative of this Revolution, "in this country at least, must be assigned to DR. DICKSON, the Apostle of Chrono-thermalism, who has long reviled antiphlogistic proceedings, and lauded a treatment essentially supporting. We do not know (the Reviewer continues) whether our friends will be pleased *stare sub nominis umbra*; but it is certainly true that Dr. Dickson long ago asserted much the same as what the two English Physicians [Dr. Bennett and Dr. Todd] teach."

On comparing dates, it will be found, that though Dr. Todd anticipated Dr. Bennett in his opposition

to the hitherto orthodox practice, Dr. Todd himself only followed in Dr. Dickson's footsteps.

Dr. Dickson began his crusade against blood-letting in 1836. Dr. Todd waited till the year 1850; in which year, while assuming that all diseases originate in "poisoned blood," he adopted the identical practice introduced fourteen years before by Dr. Dickson;—bark, steel, and stimulants, according to Dr. Todd, being the "antidotes" to this hypothetical "blood poison."

In 1836 Dr. Dickson took a surer ground. Appealing to his great discovery, the Unity, Remittency, and Periodicity of all Disease, whereof he considered Ague, or Intermittent Fever to be the type, Dr. Dickson advocated the early employment of bark, steel, wine, &c., in almost every form of disorder—pneumonia and apoplexy included. Holding Remission to be the most unfailling clue to all good treatment, Dr. Dickson addressed himself to the prevention of paroxysmal return, and adopted for his motto "*Principiis obsta.*"

*"Principiis obsta—on the outset act,
And crush disease at once by skill and tact;
Crush it ere from neglect the frequent fit
Becomes a habit difficult to hit.
However fierce the storm, expect a calm;
Now is the time for the preventive balm!*

Whether, as in the Ague's simpler forms,
Remission lasts a day between the storms
Or gives ten minutes respite to the frame,
The principle of cure remains the same.
What is that principle? As in the Ague,
To stop recurrence of the fits that plague you.
Steady the head, and leave the parts alone;
You know the means to give the mainspring tone?
Relying on the lull, however brief,
Push bark and steel to permanent relief;
And failing these,—for both may disagree,—
Change to whatever else makes Fever flee.
If the Remission, treated thus, endure
For an indefinite period, That is Cure!"

For the last twenty-four years, in various publications, all powerfully and perspicuously written, Dr. Dickson has never ceased to urge the total and entire abolition of Blood-letting in all its forms,—giving quinine, steel, and stimulants, in cases where for thirty centuries the great majority of the profession advocated exhaustive measures. For this innovation he has had to pay the penalty of all truly great Reformers, persecution and injustice!

The illustrious HARVEY, to the perpetuation of whose fame your Oration is dedicated, and JENNER, scarcely less dear to Medicine, were both, as we all know, decried and oppressed in their life-time by their professional brethren. With these examples before me, I cannot doubt that so great a Benefactor to man-

kind as the Author of *The Fallacies of the Faculty*, will ere long, like these discoverers, be appreciated as he deserves by the distinguished Body of which yourself and I have the honor to be Fellows.

In the interest of Science, and on public grounds, I propose to print this letter,—for the length of which allow me to apologize. Your Oration being now published, I do not anticipate any objection on your part to the course I mean to pursue.

Believe me to remain,

Yours, my dear Sir, very truly,
THOS. HEBERDEN.

DR. PAGE, F.R.C.P.

With Dr. Smith's Compliments

ON THE
TREATMENT
OF
PNEUMONIA.

BY
ALEXANDER SMITH, M.D. EDIN.,
SURGEON ROYAL ARTILLERY.

EDINBURGH: PRINTED BY OLIVER AND BOYD.

TREATMENT

PNEUMONIA

REPRINTED FROM THE EDINBURGH MEDICAL JOURNAL FOR JULY 1866.

ON THE TREATMENT OF PNEUMONIA.

THE following facts respecting the cases of pneumonia, treated in the hospital of the 47th Regiment, at various stations in Canada, between March 1862 and September 1865 (the period of my charge of that corps), are placed on record as a contribution to the data still required to enable the profession to arrive at definite conclusions, not only on the question of the treatment of pneumonia, but also as regards some points in the nature of that disease. The cases which came under observation occurred chiefly among soldiers of the 47th Regiment, but those of a few men of other corps, who were treated in the same hospital, are also included in what follows. The subjects of the attacks were all males, and the total number treated amounted to 108 cases, of whom 3 died, giving a mortality of 1 in 36.

The cases were distributed over the period in question as under:—

TABLE, No. I.

Years.	Periods.	Cases.	Deaths.	Remarks.	Stations.
1862	Mar. to Dec.	33	1	Priv., Ar. Hos. Cor.	Montreal.
1863	Jan. to Dec.	42	1	Corporal, 47th Regt.	Montreal, 5 m.
					Kingston, 7 "
1864	Jan. to Dec.	24	1	Private, 47th Regt.	Kingston, 5 "
					London, 3 "
					Hamilton, 4 "
1865	Jan. to Sept.	9	Hamilton, 4 "
					Toronto, 5 "
Totals.....		108	3		

The 47th Regiment arrived in Canada in July 1861; and between that time and September 1865, the deaths above recorded were the only casualties which it suffered by pneumonia; that disease having, in the period named, caused the loss of only two men to the corps,

the third man who died having been at the time only temporarily attached to it. The ages of the men attacked ranged from fifteen to forty-four, as under:—

TABLE, No. II.

Years of Age.	No. of Cases treated.	No. of Deaths.
15 to 20	3	0
20 to 25	31	2
25 to 30	47	0
30 to 35	21	1
35 to 40	5	0
44	1	0
Totals,	108	3

Seventy per cent. of the whole attacks occurred in the periods of the different years extending from December to March inclusive; but the months of March, April, and May, were those which gave the highest numbers, having supplied respectively 21, 14, and 16 cases of the whole. The fewest seizures happened in the periods from July to October inclusive; whilst the highest number, 21, and the lowest, 1, recorded in any single month, fell respectively in March and September.

In attempting to analyze these cases, in order to estimate the amount of influence exerted on their duration and mortality by the nature of the treatment adopted, it will be desirable to examine those of each year separately, as well as to make a general comparison of the whole, adding such details of the characteristic cases as may give a general idea of the forms in which the disease occurred. In order, also, the better to show the effects of treatment, the cases will be classed into three divisions, according to the nature of the remedies employed, which may be stated, in a general way, as under:—

1st, Those cases in which general bloodletting was employed, at the outset of the disease, in conjunction with the application of turpentine fomentations to the chest, and the administration of tartar emetic in $\frac{1}{4}$ -gr. doses. The latter remedy was given at first every hour or every second hour, according to the urgency of the symptoms, until vomiting or some decided impression on the force of the circulation was induced. Afterwards, when these results had been produced, the intervals between the doses were extended to three or four hours whilst the symptoms continued with any degree of urgency. Finally, it was given only three times a-day. In cases where symptoms of nervous excitement existed, or the bowels became much relaxed, a few drops of laudanum were given with each dose of the antimony. In some of the early cases, calomel and opium, or hydrargyrum cum creta was administered; but the use of mercury in any form, with a view to the induction of its

constitutional action, was soon entirely abandoned, it having become evident that any apparent good effects which resulted from the administration of that drug were more than counterbalanced by the deterioration of health which, for some time, followed its employment.

When the force of the disease had been broken, and convalescence approached, bicarbonate of potash was given three times a-day, either in infusion of senega or water,—at first in the intervals between the doses of tartar emetic, but alone, after convalescence had been established, and the administration of the latter remedy was no longer considered necessary. Large blisters also were applied in those cases where, after other treatment had been employed, persistent pain indicated that pleuritic affection existed. Wine was likewise prescribed when, after the force of the disease had been broken, there existed signs of nervous disturbance indicated by a degree of increased frequency of pulse to which the rate of the respiration and the temperature of the body did not bear a due proportion.

It was also given, at any time in the course of the disease, when the pulse became weak. It may also be added that, as a general rule, a purgative of a drachm of compound jalap powder, with two grains of calomel, was administered on admission, provided the bowels were not already relaxed; but purging, beyond what might be necessary to obviate constipation, was not afterwards had recourse to as part of the treatment.

2d, The few cases in which cupping, followed by the other treatment detailed above, was employed instead of general bloodletting.

3d, Those attacks in which neither general nor local bleeding was had recourse to, but where reliance was placed on the use of turpentine fomentations, and the employment, according to the circumstances of the case, of the other remedies already mentioned, but with, in general, an earlier use of wine, where support was indicated.

In estimating the duration of the disease, the outset of the attack is reckoned from the occurrence of the rigor, and recovery is counted from the day on which the urgent symptoms had disappeared, and the patient was, as a general rule, placed on a better diet. The full periods of residence in hospital are also given; but it will be necessary to bear in mind that in this respect the results obtained in civil and military hospitals cannot fairly be compared with each other, because, in the majority of cases occurring among soldiers, the period of residence is, from the requirements of military duty, much longer than would be necessary in the case of a patient under treatment in a civil hospital, for an attack of pneumonia of equal severity.

Experience in the management of the sick of corps also has led me to discontinue the practice of allowing men to be convalescent

in barracks, unless under peculiar circumstances of rare occurrence, and to adopt the system of detaining every soldier in hospital, who may have been under treatment there, until fit to undertake at once any duty he might be liable to be called upon to perform. This consideration likewise will, in respect to the period of total residence in hospital, exert an influence on it to the disadvantage of a military hospital, when any attempt is made to compare the results of treatment in civil and military practice.

But in addition to the increased period of residence in hospital, which followed from the causes above named, the fact that not less than 70 per cent. of all the cases occurred during the most trying part of a Canadian winter, had likewise a very important influence on the duration of the period in question, as any increased severity of the weather often rendered it prudent to subject a soldier recently passed through a severe pneumonia, although in all respects well, to a still further period of detention in hospital, in the hope that a favourable change of weather might enable him to return to an every-day mode of life, requiring at all times during the winter months much exposure, not only in the course of duty, but even from the arrangements peculiar to a soldier's residence in a Canadian barrack.

Of the 33 cases treated between March and December 1862, 14 were bled from the arm at the outset of the disease, 3 were cupped, and 16 were neither cupped nor bled. The average duration, counting from the date of the rigor to the beginning of convalescence, in the 14 cases in which bloodletting was employed was $9\frac{1}{2}$ days, the shortest period 5 days, the longest 15; and the average total residence $23\frac{1}{4}$ days; the shortest period 9 days, the longest 58. Three cases, however, whose periods of total residence were 58, 47, and 25 days, had suffered relapses, after apparent recovery, on the twenty-sixth, nineteenth, and ninth days respectively. Of these 14 cases, 3 were bled more than once in the course of the disease. The first, a case of single pneumonia, was bled to 16 oz. on the third, and to 10 oz. on the fifth day of the disease; duration, 12; total residence, 28 days. The second, a case of double pneumonia, was bled to 14, 8, and 7 oz., between the first and fifth days; period of recovery, 8, and of total residence 17 days. The third, a case of single pneumonia, was bled twice, on the first day of the disease, to 7 and 6 oz. This case recovered on the sixth, and was discharged, to duty, on the fourteenth day of the disease. In the remainder of the cases, blood was drawn once in the course of the attack, and the quantity varied from 8 to 16 oz.,—average $12\frac{1}{2}$ oz. Of these 14 cases which were bled, 3 were double pneumonias, the remainder single, and all recovered.

Three cases lost blood by cupping only, but were otherwise treated much as above described, and with the following results:—The first case, one of double pneumonia, was cupped to 4 oz. on the

fourth, and died on the twelfth day of the disease. The second, a case of single pneumonia, was cupped to 8 oz. on the fourth, and recovered on the ninth day of the disease,—total residence, 23 days. The third, also a case of single pneumonia, was cupped to 6 oz. on the third, and recovered on the eighth day of the disease,—total residence, 27 days.

Of the 16 cases treated without bloodletting in any form, the average period of recovery was $8\frac{1}{2}$ days,—the shortest period 2, the longest 13 days. The average total residence was 15 $\frac{1}{2}$ days,—the shortest period 5 days, the longest 42. All these latter cases were, however, examples of the disease in a mild form, and 11 out of the 16 occurred during warm weather, between June and October. The existence of a milder temperature, whilst it caused attacks of a less severe character, led also to the total residence in hospital being shorter than usual in proportion to the periods of recovery. These last were all cases in which only one lung was attacked.

Of the 42 cases admitted in 1863, 5 were bled from the arm, and all recovered. The remainder were neither cupped nor bled, and of these one died. Of the cases not bled, one was received over from another hospital convalescent, and was discharged after 36 days' residence in hospital. Three more of the same number were treated in the Forty-Seventh hospital, but during my absence. The details of these 4 cases are therefore not included with the following. Of the 5 cases which were bled, 4 were simple, and one complicated. The average period of recovery of the 4 simple cases was 9 days, the shortest period 7 days, the longest 11. Average period of total residence, $30\frac{1}{2}$ days,—shortest period 24 days, longest 35. Of these cases, one was a double pneumonia, in which bloodletting was employed to the extent of 12 oz. in the fourth day of the disease, and to 15 oz. on the sixth. Recovery took place on the eleventh, and the man was discharged to duty on the thirty-second day. In the three remaining cases only one lung was affected, and the average quantity of blood drawn was $12\frac{1}{2}$ oz. The fifth case was one of double pneumonia grafted on bronchitis, to which the man was liable. The pneumonic symptoms disappeared in 25 days, but the patient was under treatment for 71 days before the complicating bronchitis abated. Bloodletting, to 12 oz., was employed in this case at the outset of the disease.

Of the remaining 33 cases treated without bleeding, one died on the twenty-fifth day of the disease, and the average period of recovery of the remainder was $8\frac{1}{2}$ days,—the shortest period 2 days, the longest 18. The average period of total residence was $33\frac{1}{2}$ days,—the shortest period 5 days, the longest (in a case where the greater portion of one lung became, for a time, consolidated) 128. These 33 cases were scattered over the whole year, but the larger proportion of them occurred in the winter months.

Of the 24 cases admitted in 1864, 8 were treated in my absence.

In 7, of the remaining 16, bloodletting was employed, and, of those so treated, one case died on the seventh day of the disease. The average period of recovery of the other six was $6\frac{1}{2}$ days,—the shortest period 4 days, the longest 8. The average period of total residence was $24\frac{1}{2}$ days,—the shortest period 20 days, the longest 29. Of the remaining 9 cases treated without loss of blood, the average period of recovery was $6\frac{1}{2}$ days,—the shortest period 4, the longest 10 days; and the average period of total residence was $23\frac{1}{2}$ days,—the shortest period 9, the longest 44 days. Of the 7 cases which were bled, one was a double pneumonia, the remainder were single. The whole of the 9 cases treated without bloodletting were examples of single pneumonia.

Of the 9 cases which were admitted between January and September 1865, 4 were bled from the arm, at the outset of the disease, in quantities varying from 10 to 14 oz. The average period of recovery of these cases was $7\frac{1}{2}$ days,—the shortest period 6, the longest 8 days; and that of total residence, $23\frac{1}{2}$ days,—shortest 20 days, longest 26. Of the remaining 5 which were not bled, the average duration was $6\frac{1}{2}$ days,—the shortest 4, the longest 9 days; and the average total residence, $22\frac{1}{2}$ days,—the shortest period 15, the longest 30 days. These were all cases of single pneumonia.

As regards the relative frequency of the side of the body attacked, the right lung alone was the seat of disease in 58 per cent. of all the cases, the left lung in 24, and both lungs together in 17 per cent.

The following tables will show the relative proportions borne by the periods of recovery and total residence in each year, to those of the other years under observation, in the two classes of cases according as they were treated (1st) with and (2d) without general bloodletting:—

1st, Average duration of uncomplicated cases (one complicated being omitted), treated by general bloodletting:—

TABLE, No. III.

Years.	Period of Recovery.			Period of Total Residence.			Cases.
	Average.	Shortest.	Longest.	Average.	Shortest.	Longest.	
1862	$9\frac{1}{2}$	5	15	$23\frac{1}{2}$	9	58	14
1863	9	7	11	$30\frac{1}{2}$	24	35	4
1864	$6\frac{1}{2}$	4	8	$24\frac{1}{2}$	20	29	6
1865	$7\frac{1}{2}$	6	8	$23\frac{1}{2}$	20	26	4
						Total...	28

2d, Average duration of uncomplicated cases treated without general bloodletting:—

TABLE, No. IV.

Years.	Period of Recovery.			Period of Total Residence.			Cases.
	Average.	Shortest.	Longest.	Average.	Shortest.	Longest.	
1862	$8\frac{1}{2}$	2	13	$15\frac{1}{2}$	5	42	16
1863	$8\frac{1}{2}$	2	18	$33\frac{1}{2}$	5	128	32
1864	$6\frac{1}{2}$	4	10	$23\frac{1}{2}$	9	44	9
1865	$6\frac{1}{2}$	4	9	$22\frac{1}{2}$	15	30	5
						Total...	62

1st, These tables show very distinctly, by numbers, what my own observation of the cases in detail had led me to conclude, viz., that the results noticed in cases treated by general bloodletting were, unquestionably, of a more uniform character than those which were observed in cases which recovered without the use of that remedy. This will best be understood from a statement of the average range in days, between the average lowest and highest periods of recovery and residence in hospital, which were as 5 and 19, to 9 and 53 respectively, for 28 cases bled, as compared with 62 not so treated.

2d, They demonstrate, also, that the relative periods for which the cases were under treatment in 1863 were considerably greater than for any of the other years under consideration; and, to account for this difference, two sets of conditions may be stated as having probably more or less influenced its production. One of these had reference to the circumstances in which the men themselves were actually placed; the other, and probably not the least important of the two, depended on a temporary change which my own views underwent as to the mode of treatment to be adopted.

The first consideration referred to was the removal of the regiment from a barrack placed in a comparatively open, elevated, and airy position, in the town of Montreal, and from the advantages of an hospital where the convalescent sick could have the benefit of open-air exercise in almost any weather, to a set of buildings temporarily occupied as a barrack, and situated in a crowded, low-lying, and unhealthy part of the same town, with the disadvantage, moreover, of an hospital which, from its position and construction, was not only indifferently lighted and ventilated, but was likewise unprovided with means of open-air exercise for the convalescent sick during winter weather. That the circumstances of the soldiers' accommodation had an influence on the type of their diseases, I have very little doubt; and, accordingly, the attacks of pneumonia from which they suffered whilst those conditions lasted were of a more asthenic type than those which came under observation in the course of the previous and subsequent years, when their barrack accommodation was better. I have also a strong suspicion

that the circumstances of the men whilst under treatment in hospital, more especially in respect to open-air exercise in the course of their convalescence, materially influenced the duration of their attacks.

To the circumstance, however, of a temporary change having taken place as regarded my own views of the best plan of medical treatment to be adopted, I am inclined to attach the most importance of all; and I will now briefly state what that change was, and how it originated. Having observed that, under the influence of the warm weather of the summer and autumn of 1862, the cases of pneumonia which occurred in the course of those seasons were much milder in character than those which had occurred during the winter and spring months, I was led to discontinue the use of bleeding; and, having remarked the apparent success which attended that less active plan of treatment, at a time when my mind had been rendered undecided on the question of bloodletting by the strong feeling of opposition to its employment which then existed among the members of the medical profession, I was led to consider, whether, in resorting to bloodletting as the most essential part of the treatment of a disease asserted to be the same under every variety of circumstances, and at all times better treated without that remedy, I had not, after all, adopted a course which was unnecessarily severe.

I was, therefore, induced to inquire whether equally satisfactory results might not, on the whole year, have been obtained without loss of blood at all. Bloodletting was, accordingly, in 1863, employed only in such of the cases as, at the outset, threatened to be unusually severe, and of a nature to deter me from submitting them to the risk of what was virtually an experiment. A review, however, of the results of this less active plan of treatment forced upon me the conclusion that in no respect were they equal to those obtained under a more general practice of bloodletting, the good effects of which were in no way rendered more apparent than by the fact, that, whereas in 1862, when the disease had been actively treated, the lung affection very rarely overstepped the stage of congestion or engorgement, in 1863, on the contrary, that of hepatization rendering a lengthened convalescence inevitable, was frequently reached in cases subjected to a less effective method of treatment.

I therefore resolved to resume the treatment by bloodletting, so soon as the attacks of 1864 should have changed from the mild type of summer to the more severe form of the winter months. This was accordingly done, and with the success anticipated, excepting in the first serious attack of the season, which was, unfortunately, not bled at the outset of the disease: its early indications having been believed to be favourable to recovery without bloodletting. In that case, however, bleeding was subsequently employed, but at a stage of the complaint when there was very little certainty of its

making any impression for good on its progress. The following table of the ratio per cent. of cases bled, and of the average periods of recovery and total residence, for the whole of the cases of each year, will show at a glance—so far as that can be taught by numbers—the influence exerted by bloodletting on the duration of the disease:—

TABLE, No. V.

Years.	Per-centage of Cases Bled from the Arm.	Average Periods of Recovery of Total Cases in each Year.	Average Periods of Total Residence of all Cases in each Year.
1862	42½	8½	20½
1863	13½	8½	32½
1864	37½	6½	23½
1865	4½	7	22½

The subjoined details of a few of the cases will give the reader a good general idea of the whole. The summaries, also, of the three fatal cases with which these extracts close, will, I think, prove instructive:—

CASE I.—Private Henry Veasey, 47th Regiment, age 28. —November 28th, 1864.—A stoutly made man, of bad character, who reported himself sick yesterday morning, and stated that, although feeling slightly ill from a cold for a few days previously, he had been fit for duty until that morning, when he was attacked with rigor, followed by cough and pain in the right breast. No evidence of pneumonia was then detected on examination of the chest. A purge was administered, and he was ordered to bed.

This morning there are undoubted signs of pneumonia affecting the greater part of the right lung; the expectoration is rust-coloured and tenacious, and the cough causes great pain of the right breast. Pulse 80; respiration 28. Was this morning bled to ʒxvj., which he bore without syncope. To have ant. tart. gr. ʒ, every third hour, unless much nauseated. Turpentine fomentations to the affected side three times a-day. Diet, spoon (tea, bread, and arrowroot), with two pints lemonade for drink.

29th.—Pulse 84; resp. 24; crepitation audible, but more air entering the lung than on yesterday. Expectoration copious, very fluid, and plum-juice-coloured; not so much pain. Continue the treatment by antimony and fomentation as above. Diet and drink as on yesterday.

30th.—Pulse 60; resp. 20; air entering the lung freely, with coarse crepitation; expectoration fluid, copious, and rust-coloured. Treatment and diet as on yesterday.

December 1st.—Pulse 68; respiration natural; steadily improving; no pain. Air entering the lung freely, with large crepitation; expectoration copious, fluid, and still rust-coloured, but less so than on yesterday. Antimony to be given three times a-day, and fomentations to be used twice a-day. Beef-tea diet, lemonade.

2d.—Pulse 72; resp. 22; air entering the lung freely; very little crepitation; expectoration scarcely at all tinged, but slightly purulent. To have bicarb. potassæ, gr. xv., in aquæ ʒiv., three times a-day. Omit the antimony and fomentations. Beef-tea and lemonade.

5th.—Pulse 68; resp. 18; no expectoration; scarcely any cough; air entering the lung freely; coarse crepitation at the base, with the expiratory murmur only. Bicarbonate of potash as above. Diet low.

7th.—Pulse 60; resp. 20; air entering the whole lung; prolonged sound of expiration at the base, with harshness more than crepitation; no cough or expectoration. Treatment as above. Diet low, with two eggs and one pint of milk.

10th.—Respiration natural; no cough or expectoration. Treatment and diet as above.

20th.—No relapse; now quite strong; discharged to duty. Diet, roast chop from the 11th, with one pint beer daily from the 14th.

CASE II.—Private Michael Tierney, 47th Regiment, age 28.—November 29th, 1864.—Admitted yesterday from the guard-room, in which he was confined for drunkenness on the 26th inst. On the night of the 27th was attacked with rigor, which was followed by cough. On admission there were distinct signs of pneumonia affecting the greater part of the right lung, and attended with pain and rust-coloured tenacious expectoration. He was bled to ξ xij, without syncope, in the course of the evening.

Passed a restless night. Pulse now 120; resp. 39; crepitation audible over the greater part of the right lung; expectoration plum-juice-coloured, but not of a very dark tint. The blood drawn did not show the "buffy coat." To take ant. tart. gr. $\frac{1}{2}$, with tinct. opii. m. x., every fourth hour; half an ounce of wine every second hour. Turpentine fomentations to the affected side of the chest three times a-day. Diet, spoon, with lemonade.

30th.—By last evening the pulse had fallen to 116; to-day it is 94; resp. 28. Feels considerably better. Air entering the lung freely; coarse crepitation over the greater part of it; expectoration copious, fluid, and tinged rust colour. Remedies, diet and wine as above.

December 1st.—Feels better; pulse 84; air entering the whole of the lung, but crepitation very well marked with the sound of expiration; expectoration fluid and less tinged; no pus in it; slight pain at the lower part of the affected side. Had some sleep in the course of the night, and is less nervous in appearance. Skin moist; tongue loaded with a white fur, but also moist. Continue the antimony with opium, as above, every fourth hour; also the fomentations as before. Diet and wine as above.

3d.—Pulse 80; resp. 28. Has steadily improved since last report. Skin moist; no pain; much less cough; expectoration copious, fluid, and moderately tinged. To have antimony with opium, as above, three times a-day. The fomentations to be omitted. Beef-tea diet from the 2d, wine and lemonade as before.

5th.—Pulse 76; resp. 25. Has steadily improved since the 3d. Air entering the lung freely; coarse crepitation at the base; expectoration fluid, mucous, and untinged; slight pain at the lower part of the right side of the chest on full inspiration. The antimony was omitted yesterday, when potass bicarb. gr. xv., in infus. senegae ξ ij., was ordered three times a-day. The latter to be continued; a blister to be applied to the seat of pain, and pulv. Doveri gr. xij. given at bedtime. Beef-tea diet, wine and lemonade as before.

6th.—Continues steadily improving; very little cough, and a mere trace of expectoration. The blister acted well, and has quite removed the pain complained of in the region of the lower false ribs of the right side, and which was probably pleuritic in its character. Diet and treatment as above.

10th.—Pulse 78; resp. 24; air freely entering the lung, with the exception of a small portion at the base where the respiratory murmur is obscured, but without crepitation; slight increase of dullness on percussion at the spot in question; scarcely any cough; slight, untinged, mucous expectoration. Diet low from the 7th, with wine, two eggs, and one pint milk on this date. The alkali continued as above.

13th.—Steadily improving; neither cough nor expectoration. Omit the medicines. Diet as above.

22d.—Has been steadily improving in strength since last report, and there has been no return of pulmonary symptoms; but being a prisoner he is detained

longer than usual under observation. Diet, roast chop from the 14th, with one pint of beer daily from the same date.

25th.—Quite well; discharged to duty. Diet and beer as above.

CASE III.—Private John Walsh, 47th Regiment, age 20.—November 29th, 1864.—Attacked yesterday morning with rigor, which was followed by cough. Admitted into hospital in the evening, suffering from severe cough, attended with pain in the right side of the chest, and with obscurity of the respiratory murmur at the base of the corresponding lung. There was no expectoration. Pulse 108, full and bounding. He was bled to ξ xvj., with marked relief to his symptoms. This morning the pulse is 96; respiration 28, and attended with less pain; air entering the lung with tolerable freedom; crepitation at the lower part, and the sound of expiration prolonged. Blood drawn last evening presents the "buffy coat." To have ant. tart. gr. $\frac{1}{2}$, every third hour; turpentine fomentations to the chest three times a-day. Spoon diet, with lemonade.

30th.—Now free from pain. Pulse 60; resp. 20; air entering the lung; much less cough; coarse crepitation, mixed with bronchitic rales; expectoration mucous-purulent, only one streak of blood. Diet and remedies as above.

December 1st.—Not quite so well as on yesterday. Cough more troublesome; expectoration slightly tinged with blood; air entering the lung, but crepitation very marked with the expiratory sound,—that of inspiration very little audible. Pulse 92, and slightly weak. Continue the antimony, as above, every third hour, also the fomentations three times a-day. To have two ounces of wine. Diet, beef-tea; drink as above.

3d.—Yesterday morning had much improved, the pulse having fallen to 84, and the respiration to 22, whilst air entered the lung much more freely. To-day the pulse is 68; resp. 20; skin moist; expectoration copious, and fluid. Yesterday, three doses of antimony, with tinct. opii. m. x., in each, were administered in the course of the day. The fomentations also were continued. The antimony and opium to be continued as on yesterday, the fomentations to be omitted. Beef-tea diet, with three ounces of wine.

5th.—Has steadily improved since last report. Was yesterday ordered bicarb. potassae, gr. xv., in infus. senegae, ξ ij., three times a-day. Antimony and fomentations were omitted. To-day there is very little cough, and no expectoration. Pulse 60; resp. 22; air entering the whole of the lung; slight coarse crepitation at the base. Continue the alkali. Low diet.

7th.—Pulse 56; resp. 20; air entering the whole of the lung; occasional large crepitation at the base, chiefly with expiration; no cough or expectoration. Continue the alkali and diet as above, two eggs, one pint milk.

10th.—Pulse 60; resp. quite natural; neither cough nor expectoration. Continue the alkali. Low diet, eggs and milk.

11th.—No sign of disease, but slightly debilitated. Omit the medicines. Low diet, eggs and milk.

14th.—Continues free from disease, and regains strength. Diet, roast chop from the 13th, with one pint beer.

21st.—Respiration natural. Feels strong and able to return to duty, to which he is now discharged. Diet with beer as above.

CASE IV.—Private Thomas Williams, 47th Regiment, age 28.—December 3d, 1864.—Was attacked with rigor on the 30th November, when on guard, and began to suffer from cough on the next day, when he was admitted into hospital with obscure signs of pneumonia. He was ordered a purge, with tartar emetic every third hour, and the usual turpentine fomentations. Yesterday the pulse was 100; respiration 24; and there was evidence of well-marked pneumonia at the base of the right lung; but, as the pulse was rather deficient in strength, it was hoped that antimony and fomentations would be sufficient to control the disease. To-day, however, there is great heat of skin, troublesome cough, and rust-coloured and rather tenacious sputa. Pulse 108, and rather sharp; respiration 32; well-marked pneumonia, in the first stage, at the

base of the right lung. To be bled to $\frac{3}{4}$ viij. Ant. tart. gr. $\frac{1}{2}$, every fourth hour; turpentine fomentations three times a-day. Diet, spoon, with lemonade.

4th.—Became faint when $\frac{3}{4}$ viij. of blood had flowed, but is considerably better. To have ant. tart. gr. $\frac{1}{2}$, with tinct. opii, m. x., three times a-day. Diet as above.

5th.—Pulse 96; resp. 22; expectoration very copious and fluid, but less tinged than on yesterday; air entering the whole of the lung, with coarse crepitation at the base. Continue the antimony and opium as on yesterday. Omit the fomentations. Beef-tea diet, with lemonade.

6th.—Pulse 84; resp. 24; air entering the lung freely; crepitation less marked than on yesterday; expectoration fluid and frothy, very little tinged with blood. To have bicarbonate of potash in infusion of senega, three times a-day. The chest to be twice fomented. Diet, beef-tea and lemonade.

7th.—Pulse 76; resp. 18; very slight cough; expectoration fluid and untinged; large crepitation at the base of the lung. Continue the bicarbonate of potash. Omit the stapes. Diet, beef-tea with lemonade.

8th.—Pulse 72; resp. 22; very little cough; expectoration fluid, mucous, and untinged; air freely entering the whole of the lung; still slight crepitation at the base. Continue the potash. Low diet with lemonade.

10th.—Pulse 76; resp. 22; expectoration copious, and untinged; large crepitation still audible at the base of the lung. Potash continued. Low diet, with two eggs, and one pint of milk.

12th.—Pulse 68; resp. 20; air entering the lung freely; still slight crepitation at the base; expectoration fluid and untinged. Continue the potash. Diet as above.

16th.—Left his bed yesterday for the first time. Free from cough; no expectoration; resp. natural. Omit the potash. Diet, roast chop with a pint of beer.

22d.—Rapidly regaining strength; no cough or expectoration; states that he is quite well. Diet and beer as above.

26th.—States that he feels quite strong, but looks rather delicate; no cough; respiration natural. Diet, etc., as above.

27th.—Continues well, and is anxious to return to duty. Discharged.

CASE V.—Private George Arnold, 47th Regiment, age 23.—December 8th, 1864.—This man, who is employed as an officer's servant, had an attack of pneumonia of the right lung in August 1863. He was then treated by blood-letting, and tartar emetic, and was under treatment for 27 days. On the night of the 6th inst., during severe weather, he was sent a message for his master. In the course of the same night he had a rigor, and began to suffer from cough almost immediately afterwards. He was admitted into hospital on the following morning, with signs of incipient pneumonia at the base of the right lung. Was ordered a purge, and to take ant. tart. gr. $\frac{1}{2}$, every third hour. Turpentine fomentations were also applied three times a-day. In the evening a vein was opened, but as syncope threatened, no blood was drawn. This morning the pulse is 112, respiration 40. Did not begin to expectorate until this morning, and what he brings up is tenacious, and very much tinged. There is crepitation over the right lung, as high as the level of the third ribs, both before and behind. Has been bled to $\frac{3}{4}$ xij, with much relief to the breathing. To have ant. tart. gr. $\frac{1}{2}$, with tinct. opii, m. x., every third hour. Turpentine fomentations three times a-day. Diet, spoon, with lemonade.

9th.—Pulse 100; resp. 40; air entering the whole of the lung; small crepitation, mixed with sonorous rales; expectoration copious, very much tinged, and rather tenacious. Very little uneasiness in the chest; bowels rather relaxed; less thirst than yesterday. Continue antimony every third hour, with tinct. opii, m. x., in the two first doses. Continue the fomentations. Spoon diet and lemonade.

10th.—Pulse 92; resp. 32; air freely entering the whole lung; coarse crepitation only at the base; expectoration more fluid, and very little tinged.

Continue the antimony as above, but without opium. Beef-tea diet, with lemonade.

11th.—Pulse 76; resp. 28; air entering the whole lung freely; prolonged expiration, with crepitation at the base; expectoration copious, fluid, and slightly tinged. Continue the antimony and fomentations. Beef-tea diet and lemonade.

12th.—Pulse 76; resp. 28; air entering the lung freely; very slight crepitation; expectoration very little tinged. Treatment and diet continued as above.

13th.—Pulse 72; resp. 24; air entering the lung freely, accompanied by loud sonorous rales; no crepitation; expectoration copious and untinged. To have bicarbonate of potash in infusion of senega three times a-day. Low diet, two eggs, one pint milk.

14th.—Pulse 68; resp. 20; expectoration small in quantity and untinged; very little cough; respiration almost natural, but sound of expiration still prolonged, and a little rough in tone. Potash and diet as above.

16th.—No cough; progressing favourably. Treatment as above. Diet, roast chop.

19th.—Air freely entering the lung; occasional sonorous rales; no crepitation; neither cough nor expectoration. Diet and remedies as above.

21st.—Air entering the lung freely; no crepitation; neither cough nor expectoration. Omit remedies. Diet as above, with one pint beer daily from 20th.

22d.—Rapidly regaining strength; no cough or expectoration; respiration natural. Diet and beer as above.

24th.—Respiration natural; feels strong and able to return to duty, to which he is discharged.

CASE VI.—Private John Ryan, 47th Regiment, age 21.—December 12th, 1864.—Was discharged from hospital on the 30th of last month, after a very mild attack of scarlatina. He had in the interval fully regained his previous strength. At 9 A.M. yesterday he was attacked with rigor and felt cold during the whole day, and until late in the evening, when he became hot and feverish. About 3 A.M. of to-day he began to suffer from pain in the right side of the chest, attended with slight cough. The pulse is now 100; resp. 40; temperature in the axilla 105°. There is slight crepitation audible with the sound of expiration at the base of the lung; prolonged and roughened sound of expiration at the same point; no expectoration; ant. tart. gr. $\frac{1}{2}$, every third hour; turpentine fomentations three times a-day. Diet, spoon, with lemonade.

13th.—Pulse 108; resp. 28; temperature barely 104°; air freely entering the whole of the lung, no crepitation, but slight roughness with the sound of expiration at the base. Treatment and diet as above.

14th.—Pulse 100, rather sharp; resp. 34; air entering the lung, with tolerable freedom down to the base; crepitation audible about half-way up the lung, but not well marked; cough became more troublesome in the course of the night; considerable distress of breathing, with pain below the right axilla; expectoration not very copious, rust-coloured, and tenacious; considerable thirst; has been, this morning, bled to $\frac{3}{4}$ x. To have ant. tart. gr. $\frac{1}{2}$, with tr. opii, m. x., every third hour; turpentine fomentations as above. Diet, spoon, with lemonade.

15th.—Pulse 112; resp. 28; expectoration tenacious, rather scanty, and considerably tinged; air entering the lung with tolerable freedom; crepitation well marked towards the base; tongue furred and slightly dry at the tip; much thirst, and cough very distressing. The antimony to be continued as on yesterday, but without opium; turpentine fomentations as before. Diet, beef-tea; lemonade, with $\frac{3}{4}$ ss. wine every second hour.

16th.—Pulse 96; resp. 28; cough much less troublesome; expectoration more copious and fluid, but still rust-coloured; air entering the whole of the lung with tolerable freedom, and attended with large crepitation towards the base; tongue moist; less thirst. Treatment and diet as on yesterday.

17th.—Pulse 92; resp. 36; tongue rather furred, but moist; complains of sharp pain at the lower part of the right side; air, however, entering the lung freely with large crepitation; expectoration more copious, fluid, and less tinged than on yesterday; to have a blister, six by four inches, applied to the right side. To take potasse bicarb., gr. xv., three times a-day; the antimony to be omitted, but the fomentations continued until the blister is applied. Three ounces of wine in half-ounce doses, every two hours. Diet as above.

18th.—Pulse 88; resp. 24; the blister has acted well, and he is now quite free from pain; expectoration not very copious, but fluid and untinged with blood. Treatment and diet as above.

19th.—Pulse 80; resp. 24; much less cough; expectoration fluid and untinged; tongue less furred and quite moist. Treatment as above. Diet, beef-tea, with two eggs, and one pint of milk.

20th.—Pulse 76; resp. 24. Treatment as above. Diet, low, with eggs and milk.

21st.—Pulse 84; resp. 24; air entering the lung freely; slight roughness with the sound of expiration, but no crepitation; very little cough; slight mucous expectoration; no tinge of blood. Continue the potash. Diet as above.

24th.—Pulse and respiration natural; no cough or expectoration for the last forty-eight hours. Roast-chop diet; one pint of beer.

30th.—Quite well for more than a week past. Discharged to duty.

CASE VII.—Lance-sergeant Robinson, 47th Regiment, age 37.—*January 5th, 1865.*—Was attacked with rigor in the forenoon of the 3d instant, and cough soon followed. He had suffered from cold for a few days previously; admitted into hospital yesterday morning, but did not then suffer from well-marked signs of pneumonia; to-day there is crepitation over the right lung, as high as the angle of the scapula, which is most marked with the sound of expiration; he states that he had a similar attack last year when at Gananoque, drilling volunteers, and was there treated by a civil practitioner, who bled him to about one pound; states also, that he quite regained his previous strength, and never felt better than a short time previous to his present attack; the expectoration is now rust-coloured, but fluid, and brought up with tolerable freedom; pulse 84; resp. 30; temperature 103°. Since the pulse, etc., was recorded this morning, he has been bled to $\frac{3}{4}$ ij.; to take ant. tart., gr. $\frac{1}{2}$, every third hour; turpentine fomentations three times a-day. Diet, spoon, with lemonade.

6th.—Pulse 76; resp. 20; temperature 98°; air freely entering the lung, down to the base; crepitation still distinct; expectoration less tinged, but rather tenacious; blood drawn yesterday shows the "buffy coat"; bowels constipated. Continue the antimony every third hour, with $\frac{1}{2}$ l of sulph. magnesic in each dose; the fomentations to be continued; diet and drink as above.

7th.—Pulse 76; resp. 20; temperature 100°; air entering the lung freely, with coarse crepitation; signs of bronchitis heard under the axilla; cough short and troublesome; expectoration untinged and fluid, but less in quantity. Antimony, gr. $\frac{1}{2}$, every second hour, until vomiting is produced,—afterwards every third hour; to have tinct. camp. co., $\frac{1}{2}$ l with the first dose of antimony; fomentations to be continued. Diet, beef-tea; drink as above.

8th.—Pulse 96; resp. 32; temperature 103°; air entering the lung freely down to the base; coarse crepitation mixed with sonorous rales; sharp pain in the side a little below the breast; expectoration rather scanty, very tenacious, and of a slightly yellow tinge; tongue furred, but moist. To have a blister applied to the right side; antimony, gr. $\frac{1}{2}$, and tr. opii, m. v., every third hour. Diet, spoon, with lemonade; wine $\frac{3}{4}$ ss., every second hour.

9th.—Pulse 88; resp. 22; temperature 102°; air entering the lung, with coarse crepitation, and sonorous rales, audible chiefly on forced respiration, and then, most distinctly with the sound of expiration; the blister has acted well, and there is less pain; cough less troublesome; expectoration more

copious and brought up with less difficulty, but still very tenacious, and tinged of a yellow colour as if mixed with bile. Antimony to be continued every third hour, and potasse bicarb., gr. xv., in water, $\frac{3}{4}$ ij., also every third hour, but between the doses of antimony. Diet, wine and lemonade as above.

10th.—Pulse 76; resp. 20; temperature 98°; was attacked last evening with a "stitch" a little below the nipple of the right side; a small blister was applied, and the pain is now much less severe; there is still some complaint of pain under the spot over which the first blister was applied; the cough is much less troublesome; the expectoration also is brought up with much more freedom, it is still tinged of a yellow colour and rather tenacious; quantity rather smaller than on yesterday; air entering freely down to the base of the lung; large crepitation. Antimony and bicarbonate of potash three times a-day. Diet, wine, and lemonade as above.

11th.—Pulse 72; resp. 20; temperature natural; no pain; much less cough; expectoration more fluid and much less tinged. Omit the antimony; continue the potash. Diet and wine as above.

12th.—Pulse and respiration natural; much less cough; expectoration fluid and untinged; bowels constipated; to have pulv. jalapa comp. $\frac{1}{2}$ l; continue the potash. Diet, low; wine continued.

18th.—No cough or pain; air entering the lung freely, but still slight crepitations at the fore part of the base, audible chiefly with expiration. Diet, roast chop, with one pint beer, and two eggs, from the 17th.

23d.—Respiration natural; feels well and able to return to duty, to which he is now discharged.

CASE VIII.—Private Patrick Brady, 47th Regiment, age 22.—*January 23d, 1865.*—Went to bed well on the 21st inst., but, about 5 o'clock on the following morning, he was attacked with rigor; about two hours afterwards cough commenced, and about 1 P.M. he began to expectorate mucus tinged with blood; about 2 P.M. of the same day he was admitted into hospital; his pulse was then 96; respiration 24; at 5 P.M. he was bled to $\frac{3}{4}$ xiv., and ordered antimony every third hour, with turpentine fomentations to the chest. This morning the pulse is 104; respiration 24; temperature 100°; the pulse is soft, the skin moist, and the cough less severe; air enters the whole of the lung, but the sounds of respiration are somewhat obscured; there is small crepitation at the base behind; coarser opposite the angle of the scapula; expectoration copious, frothy, and tinged a bright scarlet; to have ant. tart., gr. $\frac{1}{2}$, with sulph. magnesic, $\frac{1}{2}$ l, every third hour; turpentine fomentations three times a-day. Diet, spoon, with lemonade.

24th.—Pulse 72; resp. 20; temperature 101°; air entering the lung freely down to the base; small crepitation audible there chiefly with the sound of expiration, which is in duration as two to one compared with that of inspiration; large crepitation audible behind, on a level with the nipple; expectoration fluid, in considerable quantity, and thoroughly tinged with blood of a bright red colour; cough less troublesome; tongue loaded, but moist; no thirst; slight pain near the right nipple. Ant. tart., gr. $\frac{1}{2}$, three times a-day; omit the fomentations. Diet, spoon, with lemonade.

25th.—Pulse 64; resp. 32; temperature 100°; air entering the lung freely; still slight crepitation; expectoration more fluid and very much less tinged; cough very troublesome last evening, but now much less so since the application of a mustard-plaster. Continue the antimony as above. Diet, beef-tea, with lemonade.

27th.—Air entering the lung freely; large crepitation near the base; respiration natural as to frequency; expectoration fluid, frothy, and very little tinged. Omit the antimony; to take bicarb. potasse, gr. xv., three times a-day. Diet, beef-tea, with wine $\frac{3}{4}$ ij.

29th.—Pulse and respiration natural; air entering the lung; very little crepitation; expectoration fluid, frothy, and not at all tinged. Continue potasse bicarb. as above. Diet, low, with two ounces wine.

February 13th.—By the 3d instant, all the symptoms had abated; since that date he has been gradually regaining strength; now well in every respect; discharged to duty. Diet, roast chop, with one pint of beer daily from the 3d to the present date.

CASE IX.—Private Francis Nash, 47th Regiment, aged 30.—February 5th, 1865.—A weakly man of rather intemperate habits; was under treatment in hospital for 44 days, in July and August 1864, at London, Canada West, for an asthenic attack of pneumonia of the right lung, complicated with ague and diarrhoea, which he contracted when on outpost duty. He was confined in the guard-room for drunkenness about 2 P.M. of the 2d instant; at 4 o'clock on the following morning he had a rigor, and felt sick and ill; admitted into hospital the same morning, suffering, apparently, from the effects of drink; in the course of the same night he began to cough and to suffer from pain in the right side of the chest, and on the 4th he began to expectorate, bringing up mucus slightly tinged with blood. To-day there is moderate cough, and the expectoration is copious and tolerably fluid, but slightly tinged with blood; pulse 96; resp. 28; temperature 98°. To have ant. tart., gr. ʒ, three times a-day; turpentine fomentations every fourth hour. Diet, beef-tea, with lemonade.

8th.—Pulse 104, soft, but rather weak; resp. 30; expectoration a good deal tinged, and in considerable quantity, but very tenacious; tongue furred and rather inclined to be dry; a blister was applied to the side yesterday; it has acted well; he had bicarb. potasse, gr. xv., three times, and no antimony, also two ounces of wine. The potash to be continued to-day as on yesterday, but gr. ʒ ant. tart. to be given with each dose. Diet, beef-tea, with two ounces wine.

9th.—Pulse 100; resp. 32; temperature 103°; tongue furred and slightly dry; face flushed; air entering the lung freely, but small crepitation very distinctly marked towards the base; expectoration moderate in quantity, very tenacious, and brought up with difficulty, tinged throughout with blood; cough troublesome, no pain. Antimony and potash continued as above. Beef-tea diet: two ounces wine, one pint milk.

10th.—Pulse 76; resp. 32; temperature 99°; expectoration more fluid, brought up with less difficulty, and not so much tinged; tongue less furred, but still rather dry at the tip; the cough continues troublesome; less thirst. Continue the remedies; diet, wine, etc., as on yesterday.

11th.—Pulse 60; resp. 28; temperature natural; much less cough; expectoration fluid and easily brought up, but still a little tinged; tongue much less furred and quite moist. Antimony, gr. ʒ, twice a-day; potash three times a-day. Diet, low, with three ounces wine, and one pint milk.

12th.—Pulse 60; resp. 25; tongue clean and moist; expectoration copious, fluid, and slightly tinged; air entering the lung freely down to the base, where there is large crepitation behind; relative length of the sounds of inspiration and expiration natural. Omit the antimony; continue the potash as above. Diet and extras as on yesterday.

15th.—Pulse 72; resp. 24; coarse crepitation still audible at the base of the lung; very little cough; muco-purulent expectoration in small quantity, slightly tinged with blood. Potash continued; also diet and extras as above.

21st.—No cough or expectoration; rapidly regaining strength; slight coarse crepitation still audible at the base of the lung, but air entering freely. Continue the potash as above. Diet, roast chop, with one pint beer.

March 3d.—With the exception of slight debility, there has been no sign of disease during the past week; now strong and able to return to duty, to which he is discharged. Diet, roast chop, with one pint beer daily since last report.

CASE X.—Private Philip Judge, 47th Regiment, age 21.—April 7th, 1865.—Was on guard on the 2d instant; continued in good health until the evening of the 5th, when he had a rigor, and suffered from sickness of stomach followed by pain in the right side of the chest, and slight cough; on the morning of

the 6th, he began to expectorate rust-coloured mucus, and was then admitted with symptoms of incipient pneumonia in the right lung; he had a purge and was given two doses of tartar emetic which caused vomiting; turpentine fomentations were applied twice in the day. This morning the pulse is 88 and full; resp. 20; temperature 102°; very little air entering the base of the right lung; small crepitation audible as high as, and on a level with, the nipple, both in front and behind; expectoration very much tinged, small in quantity and very tenacious. To be bled to $\frac{3}{4}$ x., and to take ant. tart., gr. ʒ, every third hour. Turpentine fomentations three times a-day. Diet, spoon, with lemonade.

8th.—Pulse 92; resp. 20; temperature 102°; pulse soft; air entering down to the base of the lung, where there is large crepitation behind; expectoration more copious, less tenacious, moderately tinged, and muco-purulent in character. Ant. tart., three times a-day. Turpentine fomentations as on yesterday. Diet, beef-tea, with lemonade.

9th.—At 10 A.M., pulse 88; expectoration tolerably fluid, and very little tinged; cough less troublesome; still slight pain at the lower part of the chest; air entering the lung freely down to the base where there is large crepitation with the sound of respiration. Antimony three times a-day; potasse bicarb., gr. xv., three times a-day. Turpentine fomentations as on yesterday. Diet, beef-tea; omit the lemonade. At 5 P.M., pulse 84; resp. 24; temperature 101°.

10th.—Pulse 80; resp. 24; temperature 98°; less cough; expectoration very little tinged; large crepitation still audible at the base of the lung; duration of the sounds of expiration and inspiration nearly equal; skin moist; tongue very little furred; no thirst. Continue ant. tart. and potasse bicarb. as on yesterday. Omit the fomentations. Diet, beef-tea.

11th.—Pulse 80; resp. 22; temperature 100°; expectoration considerably more tinged, and less copious; air entering the lung, but not quite so freely as on yesterday; sonorous rales mixed up with large crepitation; considerable pain of the side on coughing or full inspiration. A large blister to be applied to the side; antimony and potash as on yesterday. Diet, beef-tea.

12th.—Pulse 76; resp. 28; temperature 100°; the blister acted well, and the pain has quite ceased; very little cough; expectoration fluid and much less tinged; skin moist; tongue slightly furred, but moist. Continue the remedies and diet as on yesterday.

13th.—Continues improving. Omit the antimony, but continue the potash. To have low diet to-morrow.

15th.—Free from pain; no cough; a little untinged expectoration yesterday; none to-day. Continue the potash. Diet, low.

19th.—Rapidly convalescing; no pain or cough; respiration at the base of the lung nearly natural; sound of expiration still slightly prolonged. Omit remedies. Roast-chop diet to-morrow.

30th.—Has progressed favourably since last report; now quite strong and able to return to duty, to which he is discharged.

The details of the ten cases above recorded will, I trust, enable the reader to form a very fair estimate of the total series; but my original intention was to have made such a selection from the whole as would have included characteristic examples of the cases of each year of the period embraced in this record. Loss of health in my family, however, made it suddenly necessary that I should at once return to England, and left no time for making the necessary extracts from the public records. Fortunately, however, I had, towards the end of 1864, begun a system of making duplicate notes of the records of cases of pneumonia which came under my care, and, in that way, I have now been enabled to give in detail

information which I hope may be found sufficient for the end in view.

The general deductions given at the beginning of this paper, are based on facts which were tabulated at the time each case occurred.

The following are the records of the three cases which terminated fatally; the two first in detail; the last in a condensed form.

FATAL CASE, No. 1.—Private Francis M'Arille, Army Hospital Corps, age 25; service, 7 years; station, Montreal, Canada East; time on the station, 8 months.—*March 12th, 1862.*—An Irishman; by trade a labourer; a stoutly-made man; was formerly in the 47th Regiment; and has done duty in the hospital of the corps for the last 3 years; was attacked, on the 8th instant, with rigor, which was followed, on the 10th, by pain in the right side of the chest, accompanied by cough and expectoration of rust-coloured sputa. When admitted into hospital, on the latter date, there was obscure crepitation at the base of the right lung. The pulse was 100; the breathing much hurried; and there was great thirst; he was cupped to about $\frac{1}{2}$ in., and was given ant. tart., gr. $\frac{1}{2}$, with hydrarg. cum creta, gr. $\frac{1}{2}$, every third hour. Yesterday he was a good deal better, but the pulse was still high and the breathing hurried; the expectoration was copious, and of the same character as on the previous day; a blister was applied at bedtime, and the antimony continued as above. To-day he is free from pain; the breathing is nearly natural, and the pulse 96; the blister has acted well; the bowels are constipated. To have pulv. jalapae co. \mathfrak{z} i., calomel, gr. \mathfrak{ij} ; the antimony to be continued as before, after the purge has acted. Diet, spoon, with two pints lemonade.

14th.—There was an increase of fever yesterday morning, and the tongue became dry, whilst expectoration was more difficult, and the pulse rose to 108. He had ant. tart., gr. $\frac{1}{2}$, every hour up to about 3 P.M., when he became much under the influence of the drug, and had an attack of syncope after having been to the close-stool, his bowels having been freely acted upon by a purgative enema. Towards evening, he seemed a good deal better, his pulse having fallen in frequency, whilst, however, it lost strength from the action of the antimony: his tongue also had become moist. The antimony was discontinued, and \mathfrak{z} ij. of brandy in \mathfrak{ss} . doses were given in water, in the course of the evening. He passed a restless night, and now complains of slight pain in the left side, where there is evidence of incipient pneumonia at the base of the lung. Pulse 108; resp. 28; tongue dry at the tip; sputa rusty and rather scanty; great thirst. A blister to be applied to the left side of the chest. To take pulv. Doveri, gr. viij., hyd. cum creta, gr. i., ant. tart. gr. $\frac{1}{2}$, three times a-day. Diet, spoon, with lemonade, and \mathfrak{z} ij. of brandy.

15th.—The blister has acted well, and he is quite free from pain. He suffers less from cough, and his breathing is much more quiet. He is now perspiring profusely, and seems on the whole considerably better, although his pulse is feeble, whilst it is at the same time less frequent than it was yesterday. The tongue is less furred, and is now moist. The strangury has ceased. To take the following three times a-day:— \mathfrak{R} Ammoniac sesquicarb., gr. v.; tinct. camph. co. \mathfrak{z} i.; spt. aeth. nit., m. xx.; aquae, \mathfrak{z} i.— \mathfrak{M} . Omit the powders. Diet, beef-tea; two pints lemonade; two ounces brandy.

16th.—There is this morning some distress of breathing, with pain across the chest. The pulse is 104, and inclined to be weak; and the tongue is slightly dry at the tip. There is some increase of dulness on percussion at the lower part of the left side of the chest, as well as an absence of the sound of respiration at the base of the lung. There is large crepitation, mixed with sonorous rales, at the lower part of the right side. The bowels are constipated. A purgative enema to be administered, and a mustard-plaster applied to the front of the chest. The mixture as above ordered, but omitting the tinct.

camph. co., to be given every third hour. The diet, lemonade and brandy, as above. Towards evening, distress of breathing came on attended with suppression of the expectoration. Pulse 120; face flushed. The brandy was omitted and the mixture discontinued.

17th.—After the stimulating treatment had been discontinued, last evening his condition improved, and he passed on the whole a tolerable night. There is less difficulty of breathing, and the pulse has fallen to 100. The tongue is moist, and the face less flushed. There is evidence of induration at the base of the left lung. The right is doing well. The following to be given every second hour:— \mathfrak{R} Vin. ipecac., \mathfrak{ss} .; potassae bicarb., gr. xv.; infus. senegae, \mathfrak{z} i.— \mathfrak{M} . Diet, beef-tea, with two pints lemonade.

18th.—Is considerably better this morning. The pulse has fallen to 80, and is of moderate strength. There is no distress of breathing, and the cough is less troublesome, whilst expectoration is more free and less tinged. The tongue is moist, and but little furred. Percussion gives a clear sound over the base of the left lung, and large crepitation is audible there. Continue the mixture. The following to be given three times a-day:— \mathfrak{R} Pulv. Doveri, gr. v.; hydrarg. cum creta, gr. \mathfrak{ij} .— \mathfrak{M} . Diet, beef-tea, with lemonade.

19th.—Has dozed a good deal in the course of the day. States that his gums are now tender. The tongue is slightly dry at the tip. Omit the powders. To have two ounces of brandy, at four times, in water, in the course of the evening.

20th.—About 2 A.M. his breathing became much distressed, and since then insensibility has gradually come on. His breathing is now very much hurried, and accompanied by occasional long convulsive inspirations. His countenance is dusky and his skin congested. His pulse is extremely feeble, and there has been an involuntary discharge of urine. The hair to be removed, a blister to be applied to the nape of the neck, and a mustard plaster to each calf. To have occasionally a little brandy and water, if he can swallow.

20th.—Remains in much the same condition as that reported yesterday. His countenance is dusky, and he can be only partially roused. His pulse is very frequent, extremely feeble, and somewhat intermittent. His teeth are covered with sordes. The blister has acted well, but he has not become more sensible. The discharges are passed in bed. A turpentine enema to be administered, and to take what stimulants and nourishment he can swallow.

21st.—From yesterday morning the powers of life gradually failed, and he expired in the way of coma at half-past eight the same evening.

Post-mortem examination fourteen hours after death.—There were tolerably strong pleuritic adhesions over the greater part of both lungs, but in a more marked degree on the right side; and in freeing the base and posterior part of the right lung, the tissue of it gave way under the hand. There was intense congestion of the base and lower and posterior half of the right lung, together with the condition of red hepatization at different spots in the same positions, but most marked in degree towards the base. There was intense congestion, but in a less degree at the base of the left lung, which was, however, free from hepatization. There was no tubercular deposit or other disease of either lung. There was slight adhesion of the pericardium to the heart, near its apex, and the disease seemed of recent date. The quantity or character of the pericardial fluid could not be ascertained, as it escaped through a cut accidentally made in removing the thoracic contents. The liver was much congested, and somewhat softened at the posterior part of the right lobe. The kidneys were also congested, and there was a slight trace of the incipient deposit of Bright's disease. There was no other apparent disease of the abdominal contents.

This case was the first which came under my care during my service in Canada, and I fear that I cannot look back with satisfaction to the course I adopted in treating it. The lesson, however, which its progress and result taught me was a valuable one, and I

believe that the details I have given respecting it will prove to be not without interest to others.

FATAL CASE, No. II.—Lance-corporal William Dickson, 47th Regiment; age 21; service 2 $\frac{1}{4}$ years; station, Kingston, Canada West; time in Canada, 2 $\frac{1}{2}$ years. *August 13th, 1863.*—This man was under treatment, at Montreal, for remittent fever, from the 24th September to the 20th October 1862. He is now admitted into hospital on account of general debility and pains in the back and limbs. His tongue is loaded, and his pulse slightly accelerated. There is no headache, but he feels heavy, and is much inclined to sleep. His appetite is bad, and he suffers from thirst. His bowels are tolerably regular. He had to-day a slight rigor followed by sweating. To have a common purgative dose with 5 grains of quinine in it. Diet, spoon, with two pints lemonade.

14th.—Passed a restless night, coughing much, and expectorates tenacious mucus, slightly tinged with blood; pulse 108; much thirst; tongue loaded. There are now undoubted signs of the first stage of pneumonia at the base of the right lung. Was well purged yesterday. To have turpentine fomentations to the side, and to take ant. tart., gr. $\frac{1}{2}$, every second hour. Diet and lemonade as on yesterday.

19th.—Has had well-marked pneumonia, which is now waning; crepitation still distinct; sputa less bloody and tenacious; pulse 95, rather weak. To have the following three times a-day:— \mathcal{R} Ant. tart., gr. $\frac{1}{2}$; tinct. camph. co., 3 ss. Liq. Ammoniac. ac. ʒss. Diet, beef-tea; two pints lemonade; two pints milk.

[Here my charge of the case ceased, and the following entries were made by the medical officer who relieved me.]

21st.—Respiration easy; pulse quiet and regular; slight cough. To have cough-mixture. Diet continued.

22d.—Much better; dislikes the lemonade. Diet continued; omit lemonade.

24th.—Not so well to-day. Breathing more difficult; dullness over the lower lobe of the right lung; general aspect unfavourable. To have ant. tart., gr. $\frac{1}{2}$; calomel, gr. $\frac{1}{2}$; every second hour. Diet, low; two pints milk, one egg.

25th.—Seems a little better, but his respiration is still hurried. Continue treatment. Diet, beef-tea; one pint beer, four ounces wine, one egg, two pints milk.

26th.—Was very weak last night, but is rather better to-day. Continue powders every third hour. Diet, beef-tea; two ounces brandy, four ounces wine, two pints milk, one egg, one tin essence of beef.

27th.—Much better, but still very weak. Diet and extras as on yesterday.

28th.—Same as yesterday. Mouth not affected by the mercury. Continue powders every third hour. Diet and extras continued as above.

29th.—Not so well as on last night. Had some delirium during the night. Very thirsty. Continue powders. Diet, etc., as above.

31st.—Improving slowly. Much difficulty of respiration. Mouth not yet touched by the mercury. Diet, beef-tea; one egg, two pints milk, eight ounces wine, one tin essence of beef.

September 2d.—Not so well. Has had since diarrhoea. Checked. Omit the powders. Diet, etc., continued as above.

3d.—Looks very ill still, and has much dyspnoea. Very weak. Cough-mixture. Diet, etc., continued as above.

4th.—Rather better than on yesterday. Diet, etc., continued as above.

6th.—Worse. Dyspnoea increased. Omit the mixture. \mathcal{R} Spt. ammoniac. aromat.; spt. ætheris comp., ʒi. m. x. Liniment—crotonis tiglii for chest. Diet, etc., continued as above.

7th.—Appears to be dying. Nil. Add two ounces brandy to the extras.

8th.—Died at 2 A.M. to-day.
Post-mortem appearances.—Chest: Right lung condensed, and nearly a quart of serum in the pleural cavity; a few slight recent adhesions, with soft

lymph deposited on the surface. The whole lung in an advanced state of grey hepatization. At the apex softening had commenced, and the tissues had broken down, forming several small cavities. This was also the case at one or two points at the base of the lung. In other parts it was solidified throughout its structure. Left lung crepitant and healthy, without any tubercular deposit. Heart rather large, structure firm, valves healthy. Abdomen: Liver somewhat enlarged, structure healthy. Stomach, intestines, spleen, and kidneys normal.

FATAL CASE, No. III.—Private John Allen, 47th Regiment; age 33; service, 15 years; station, Hamilton, Canada West; time in Canada, 3 $\frac{1}{4}$ years. Had been for several years past employed about the officers' mess; and, when attacked by his last illness, was acting as scullery-man to that establishment. His employment exposed him to considerable alternations of temperature, and he has been generally considered to be a man who regularly consumed a large quantity of drink. Some ten years ago, when serving at Corfu, he had an attack of pneumonia, for which he was bled at the arm; and again, at Cork, in 1860, he had a similar attack, for which he was under treatment for twenty-three days. On the night of the 1st instant (November 1864), he had a rigor, and soon afterwards began to suffer from cough, and pain in the right side of the chest.

He was admitted into hospital on the morning of the 2d, and then his breathing was short, and he was unable to make a full inspiration without suffering from acute pain in the right side. Crepitation could not be detected, but there was obscurity of the respiratory murmur over the lower part of the right lung, with some prolongation of the expiratory sound. Towards evening the expectoration became rust-coloured. He was ordered a purge; turpentine fomentations were applied to the side, and tartar emetic afterwards administered in gr. $\frac{1}{2}$ doses every second hour, until nausea supervened, and then less frequently.

On the morning of the 3d, the pulse was 108 and rather full; respiration 28. The breathing was more free, sputa copious but not coloured, and of considerable tenacity. Crepitation could not be distinctly made out, although air could be heard to enter the lung.

On the morning of the 4th, the pulse had fallen to 96 in frequency, and was soft and of good strength; respiration 28; expectoration not so much tinged, but rather more copious and of less tenacity. No sound of air entering the back part of the base of the right lung, but small crepitation near the lower part in front. Large crepitation near the spine, and on a rather lower level than that of the angle of the scapula. The antimony to be continued every third hour unless it caused much nausea, and also the turpentine fomentations.

On the 5th, the pulse had fallen to 80, and was soft and of good strength; respiration 24; expectoration copious, less deeply tinged, and more fluid; crepitation very distinct over the base of the lung. The antimony was given three times a-day, and the turpentine fomentations were applied the same number of times. In the course of the night of the 5th, he suffered a relapse without any assignable cause; and, on the morning of the 6th, his breathing was found to be considerably distressed; respiration 40, whilst the pulse was 104; the sputa were more tinged, and much more tenacious. The antimony was given more frequently, and the fomentations were continued. In the evening it was found that he had been much nauseated, and the strength of the pulse reduced, but without abatement of the violence of the symptoms. The pulse was 108, and the respiration 44. There was great distress of breathing; and, in order to relieve that, he was bled from the arm to eight ounces, with slight relief. There was bronchial respiration over parts of the lung on a level with the angle of the scapula, whilst no air entered the lung below that position. The blood drawn became to all appearance a solid mass of the "buffy coat." There was no sign of disease in the left lung.

He passed a restless night, and on the morning of the 7th he was still much

distressed. Respiration 44; pulse 120, but of good strength; slight lividity of the lips. The disease still seemed to be confined to the right lung, into which rather more air entered, with sound of coarse crepitation. The sputa were much tinged, less copious, and very tenacious. As it was evident that the greater part of the right lung had become incapable of action, and the left was embarrassed by the amount of blood thrown upon it, twelve ounces of blood were taken from the arm, and with the effect of affording very considerable relief. The countenance became clearer, there was much less distress of breathing, and in the course of the afternoon he had some sleep. In the evening he appeared considerably better. About 9 P.M. a blister was applied to the right side; he was then cheerful, and after that appeared to fall asleep, lying on the left side. About 10 P.M. it was observed that his breathing had become embarrassed; and when I saw him at 11 he was comatose, and could be only partially roused when stimulants were steadily administered, but without any permanent good effect; and he expired in the way of coma at 3 A.M. of the 8th.

Post-mortem examination fourteen hours after death.—Body muscular, and not emaciated. Marks of cupping and blistering of old standing on the right breast. The blister applied on the evening of the 7th had acted partially. Both lungs were firmly adherent to the walls of the chest, but especially the right, which could only be removed with great difficulty, owing to some recent and many old adhesions of great strength. The fore-part of the surface of the right lung was covered with a tough membrane of a yellow colour. With the exception of a small portion of the apex, and a part at the front of the base, the right lung was in the condition of red hepatization. It sank in water, and the larger divisions of the bronchi were filled with fibrinous casts, which retained their form on removal. The left lung was very much gorged with blood, its tubes loaded with mucus tinged with blood, but its tissue crepitated, and seemed throughout to be otherwise healthy. There was slight excentric hypertrophy of the left ventricle of the heart, but no valvular disease. The liver was considerably enlarged, and presented well-marked cirrhosis. The kidneys were of large size, and lobulated in form: one, which was laid open, presented incipient granular deposit of Bright's disease, and was much congested; the other was preserved whole.

The grand error committed in the management of this, the first serious case of the season, was the omission to bleed moderately at the outset of the disease. Had that course been adopted, there is every reason for believing that the favourable result which followed the employment of the remedy in question in so many cases of a similar nature would not have been wanting in this instance. Bloodletting was, however, subsequently made use of in this case, but at a stage of the disease when, hepatization having already taken place, there was no prospect that it could limit the advance of the complaint on the right side of the chest, although it might still, by relieving the engorged state of the left lung, assist in obviating the tendency to death by coma from mal-arterialization of the blood, which the condition of the lips and countenance showed to be imminent. I regret that the administration of wine was not commenced at this stage of the disease; but I must confess that I was not prepared for so early a fatal termination, although I became fully aware of the gravity of the attack from the time that the exacerbation of the symptoms and the sudden advance to hepatization took place. At the same time, however, whilst fully admitting the dangerous condition into which the case

had fallen, I cannot altogether divest myself of the suspicion that the sudden supervention of coma was to a considerable extent determined by the accident of the man having been allowed to fall asleep whilst resting on his left side,—a position which could scarcely fail, in the existing state of the right lung, so to impede the action of the left as to cause a degree of embarrassment of the respiration which would be likely soon to lead to an early termination, such as actually happened.

From a careful study of the facts observed in connexion with the whole series of cases of pneumonia which came under my notice, I have been led to draw the following general conclusions:—

1st, As regards the exciting causes to which the disease could be clearly traced, one of the most frequent was exposure when on guard, and more especially when on sentry at night during severe weather,—conditions which were also, in all probability, materially aggravated by the overcrowding and want of ventilation which, as a general rule, existed in the guard-rooms. Another cause, of frequent occurrence, was confinement in the guard-room when under the influence of drink; as was also a want of sufficient care on the part of the men to avoid getting chilled, on their return to their barrack-rooms, when overheated and fatigued by a march into the country during the winter, or by a trying field-day at other seasons. Other cases, which could not so clearly be traced to their exciting cause, probably owed their origin to a certain amount of exposure during the depth of winter, which was inseparable from the daily routine of their barrack-life. In one instance, also, the disease came on after a severe fall causing slight concussion of the brain.

The time which elapsed between the date of exposure, where that could be clearly determined, and the occurrence of the rigor, was found to vary from a few hours to three or four days. In some of the cases recorded above will be found examples of the shorter periods, whilst the longer interval was well exemplified in the cases of four young and previously healthy men, who took part in a rather fatiguing field-day during warm weather in the month of June, and who were all attacked with pneumonia, which showed symptoms, mild in degree, but very characteristic of the disease. In one of the four, the complaint appeared on the third day, but in the other three it did not begin until the fourth. The man first attacked was under treatment for thirteen, two of the others each for eleven, and the fourth for ten days, the mildness of the weather having, in these, as in other instances, determined the degree of severity of the attack—it having been almost invariably found, that the rapidity with which an attack came on, bore a close ratio to the intensity of the exciting cause, which latter also determined the degree of violence of the attack.

2d, The earliest observed stethoscopic indication of the existence of the disease was a decided obscurity of the sounds of respiration,

accompanied by some degree of alteration of the relative proportions of the inspiratory and expiratory murmurs—the latter having become prolonged, whilst the former was shortened—less so, however, with the first indications of a change from health, but increasing, as the period of the setting in of crepitation approached, until but little of the inspiratory sound could be distinguished, and that of expiration became not only prolonged as to duration, but exaggerated in tone, so as to bear a close resemblance to bronchial breathing, of which it was probably the first degree. This did not, however, depend on actual consolidation, but was owing in all probability to that amount of increased density of structure which was the result of great engorgement of the vascular tissues, and, perhaps, in some measure also to fluid effusion into air-cells as well. In the earlier stage of the obscured respiratory sound, its tone was somewhat altered, by having become less soft in character; but it was not until the stage of crepitation in its first degree approached that the expiratory part of it became exaggerated, to be afterwards, in many cases, for a few hours lost altogether during ordinary respiration, although evidence that air entered the diseased portion of the lung could still be heard during forced respiration. This obscurity of respiration was first noticed by me in 1862, during my examinations of the earlier cases of pneumonia which came under my care; and, in the whole course of my subsequent experience, although often not recorded, this condition was never found wanting when carefully searched for. The alteration of the relative proportions of the respiratory sounds, which attended this state of the breathing, showed that it depended on commencing engorgement of the lung tissues affected in pneumonia; that condition progressing in intensity, as indicated by the change of character of the expiratory sound until the acme of engorgement—without, however, actual consolidation—was reached, as shown by the fact, that but little air then entered the diseased portion of lung during ordinary respiration. To this highest degree of engorgement, in acute cases treated at the outset by bloodletting, and in mild or asthenic ones where the progress of the disease was favourable to recovery without hepatization, relief was, however, soon afforded, by the setting in of secretion as manifested by crepitation, small at first, but gradually becoming of a less fine character, as the lessening engorgement allowed the smaller ramifications of the bronchial tubes to regain more of their natural capacity.

The stage of engorgement in its various degrees generally lasted to near the end of the first or beginning of the second day of the disease. Then, with or without a short interval of absence of respiratory sound, small crepitation set in; and, in asthenic cases early bled, and where everything was favourable, lasted probably for twenty-four hours more, when large crepitation could be heard over part of the lung. This generally happened towards the end of the third, or in the course of the fourth day of the disease. Judging

by the rate of the pulse, taken in conjunction with recorded observations of the temperature of the body, the period at which the accompanying fever attained its greatest height, corresponded with the acme of engorgement and the duration of small crepitation. It gradually abated, however, on the occurrence of large crepitation in such sthenic cases as had been efficiently treated, and the disease then steadily abated without hepatization having occurred at all. For a few days, however, after the cough and expectoration had entirely ceased, large crepitation could be heard at the base of the lung, chiefly during expiration. The evidence of disease, which could be last distinguished, immediately before the return of natural respiration, was some prolongation and roughening of the sound of expiration.

3d, Facts which have come under my own observation, and which are to my mind conclusive, have forced upon me the conviction, that the assertion which has been so confidently maintained to the effect,—1st, That a pneumonia cannot exist and recover without having passed through all its stages, short of gangrene, but up to that of suppuration or gray hepatization; and, 2d, That it implies ignorance of the true pathology of the disease, either to attempt to interfere with the progress of that course, or to believe that bloodletting can act in any other way than to impede what is looked upon as the natural process of recovery;—is true only as regards certain classes of cases, and is not borne out by facts in respect of the disease in general. All my experience of pneumonia leads me to conclude, that the natural course of a sthenic attack of that disease, induced by an exciting cause acting with a high degree of intensity, and treated by bloodletting at the outset of the disease, is in strict accordance with the succession of changes which I have already described in treating of the stethoscopic signs and progress of the complaint.

A perusal of the cases of recovery given above will show the grounds on which the observations I have just made are based, and will explain the characters which I believe to be usual in that acute and sthenic form of attack which should, in my opinion, be considered typical of pneumonia. When, however, an attack originally such as that above referred to is allowed to run its course, without adequate treatment, its tendency is to a fatal termination, which will in all probability happen before more than perhaps a small portion of only one lung has reached the stage of red hepatization. Fatal case No. 1, recorded above, is an example of the disease terminating in this way. In cases, however, which are asthenic from the beginning, or which have been induced by exciting causes acting with moderate intensity, as is generally the case in the summer and autumn months in Canada, and during most seasons of the year in the more temperate climate of Great Britain, as well as in those attacks in the treatment of which it may not have been considered necessary to employ bloodletting, recovery may in a

large proportion of instances be satisfactory, provided they have been early brought under intelligent medical treatment, and that, too, without hepatization ever having occurred. Case No. 9, given above, is a fair example of an asthenic pneumonia occurring in a weakly man, and running its course in the manner which has just been indicated. A proportion of such cases, however, will, without assignable cause, or the existence of any very apparent difference in character from other attacks to all appearance identical with them, run on to hepatization—that stage coming on either gradually or by a sudden exacerbation of symptoms after apparent temporary amendment—and require a lengthened period of treatment for their cure. Cases of this description may also terminate fatally, but then, in comparison with what is observed in sthenic attacks having a similar result, there will be a very marked difference in the rapidity with which they will have run their course, which may not close in death until after the lapse of several weeks. In cases of this description, examination of the body after death will disclose a large portion of lung in the condition of grey hepatization, which may even have gone on to the formation of abscess, as happened in the second fatal case recorded above.

A considerable number of cases of a nature similar to those last described, with the exception that they all recovered, came under my notice in the season of 1863, when from a local cause the complaint was less sthenic in form, and when also, for a reason already given, bloodletting was less frequently had recourse to in its treatment. Of this nature, also, I believe that the majority of the cases of pneumonia treated in civil hospitals in this country partake; and from all I can learn on that subject, I am induced to suspect that a very considerable proportion of those cases are not brought under medical treatment until the stage of hepatization has probably set in.

4th. As will have been inferred from what has already been mentioned in the course of this paper, I have arrived at the conclusion, founded on facts, which I do not think can well be misinterpreted, that contrary to what has been asserted on this point also, variations in the nature and intensity of the exciting cause, as well as the influence of changes in climate, in atmospheric conditions, and in locality, affect, in a marked degree, the form or type in which pneumonia may occur. This was in no way more clearly made apparent than by the modifications of the disease, caused in the course of a single year in Canada, by the variations of a temperature ranging from -35° in the open air to 100° in the shade: the extreme cold of winter producing attacks of pneumonia which were severe in form and rapid in accession in direct proportion to the severity of the exciting cause; whilst, on the other hand, the milder temperature of summer and autumn led to attacks which were slow in accession and correspondingly slight in degree.

5th. My experience of the effects of bloodletting convinced me

that its employment at the outset of pneumonia in its sthenic form was attended with most beneficial results, not only in shortening the duration of the disease, and rendering convalescence satisfactory, but also in giving an amount of certainty and uniformity to the results of treatment which could not be attained by the employment of any other combination of remedies. As to its power in "cutting short" the disease—if by this term is meant to be expressed the probability of its at once arresting, and as it were stamping it out—my own experience would go to show that its employment is not attended with any such result. In proof of this I may mention that so soon as I became aware of the import of the condition of the respiration, which is first observed at the outset of pneumonia, I attempted, by early bleeding before the disease had advanced beyond the stage indicated by obscurity of the respiratory sounds, to arrest it in that of engorgement. In no case, however, was this practice attended with the result desired; but, on the contrary, in every attack so treated, instead of being altogether prevented, small crepitation seemed to undergo an earlier development,—an occurrence which may perhaps be held as in some measure bearing out the accuracy of the views I have expressed as to the nature of the early stage of the disease in sthenic attacks. The subsequent progress of all such cases early bled was otherwise invariably satisfactory.

I would still, however, feel inclined to consider this question in the light of an open one, and to believe, until distinct proof to the contrary shall have been produced, that bloodletting practised soon after the occurrence of the rigor may possibly at once arrest the disease. I am the more inclined to this view of the matter, because Dr Jameson, my colleague in the 47th Regiment, informs me, that in one case which he bled freely immediately on the man's admission into hospital, and within a very short time of the occurrence of an attack of rigor which, from all the attending circumstances, and happening as it did at a time when pneumonia was prevalent among the men of the corps, appeared to be the initial symptom of an attack of that disease, no further indisposition followed. This may or may not have been a case which, if it had not been so treated, would have proved one of pneumonia; but still I believe the fact is worth recording.

A perusal of the records of cases which I have given above will show I think, upon the whole, with considerable clearness, that it was by limiting the stage to which the diseased action advanced, rather than by affecting the extent of lung to be attacked, that bloodletting manifested its power to shorten the duration of the disease. That it also influenced the amount of lung attacked, however, appears evident, from what was found to have happened in some of the fatal cases, neither of which were bled at the outset of the disease. It may be here stated, with regard to the extent of lung affected in cases early bled, that it amounted, as a general

rule, to from one-half to three-fourths; and that in respect of the part first attacked, in no instance did the disease begin at the apex.

After having most carefully watched the whole course of the disease in attacks where bloodletting was employed at the outset, I feel satisfied that in no case so treated did red hepatization take place; both the exaggerated respiratory sound heard near the acme of engorgement, as well as the absence of evidence of the entrance of air, excepting during forced respiration, which frequently for a few hours preceded the setting in of small crepitation, having been, as already so often stated, unconnected with any degree of actual consolidation. Neither were the bronchitic sonorous râles occasionally audible along with large and small crepitation near the middle of the lung, in the course of some of the cases, confounded with the blowing sound of bronchial respiration heard when true hepatization was present. The facts of greatest importance, however, noticed with reference to the employment of bloodletting, were the rapidity with which such cases recovered in proportion to the severity of the attacks, and the uniformity of the results observed on a review of the whole cases so treated, as compared with that obtained in the milder and more asthenic attacks in which bloodletting was not made use of. This has been shown by the tabular statements given at an earlier part of the paper.

A further consideration, possessing also considerable practical importance, is the fact, that in cases not bled it was found that there existed, throughout the greater part of the attack, a danger that a fresh accession of fever, and a rapid advance to hepatization, might not only suddenly occur, but do so at a period of the disease when good results from bloodletting, if it should then be employed, were but little likely to be obtained. In conclusion, I would, however, beg that it may be distinctly understood, that whilst advocating the employment of bloodletting at the outset of sthenic cases of pneumonia, such as are seen in young and previously healthy soldiers, and whilst maintaining also from actual observation that the good results which follow such a mode of treatment surpass in a marked degree those obtained from any other combination of remedies, I do not in any way call in question the value of that mode of treatment termed "restorative," as applied to a particular class of cases, and which has been employed with so much success in the management of the pneumonia seen in civil hospitals in Britain.

It would appear, however, from such limited details as have been given of the cases, on the results obtained, in which this plan of treatment has been based, not only that the attacks were of an asthenic character, but it may also be inferred that, in a large proportion of instances, the disease had advanced to the stage of actual consolidation before it was brought under medical treatment at all. On this supposition, therefore, these were cases in which bloodletting would

in all probability have been inadmissible, but they were exactly such as would derive benefit from the description of treatment in question. My own experience of pneumonia would, accordingly, lead me to conclude that it is only in such asthenic cases as those above referred to, modified as they must be by the minor degree of intensity of their exciting cause, as it prevails in a climate equable on the whole as that of Britain is, and influenced also as they cannot fail to be by conditions of food, clothing, locality, and occupation, that this plan of treatment can be advantageously employed.

In sthenic cases, such as came under my own observation in Canada, and of which it is possible that examples may occasionally be met with at home, facts have convinced me that a restorative plan of treatment could not be exclusively employed without the risk of at least a considerable mortality, or at all events the almost certainty of a recovery protracted beyond what it would have been had bloodletting been made use of. I would further add, that what I have learned in the course of this inquiry induces me to believe that much of the confusion and diversity of opinion which have of late years arisen on the subject of the pathology and treatment of pneumonia has been the result of a somewhat restricted view of the extent of the field of inquiry embraced by the subject under investigation, and the too resolute belief, not only that the asthenic pneumonia, which has of late years supplied the larger proportion of the cases met with in this country, is the sole form in which inflammation of the lungs prevails now, but is even the only type in which that disease has existed at any previous period. I must at the same time beg to be pardoned if I venture also to hint my suspicion that some portion of this state of opinion may likewise be due to the condition of the lungs which exists during the highest state of engorgement having been confounded with that state of actual hepatization the occurrence of which renders it absolutely essential for the cure of the disease that the next highest stage—that of suppuration or grey hepatization—should also follow.

I would now close this paper with the expression of a sincere hope that the facts, with the conclusions deduced from them, which I have endeavoured to record, may prove suggestive to other inquirers; and that they may also, perhaps, be the means of inducing my fellow-labourers in the public service to enter upon an inquiry for the prosecution of which they enjoy advantages that do not often fall within the reach of their brethren in civil life.



NOTES ON DISEASES OF THE LIVER
AND SPLEEN,

By C. A. GORDON, M. D. AND C. B.,

BY INSPECTOR GENERAL OF HOSPITALS.

Written in 1856, when Surgeon of the 10th Regt.

HEPATITIS ACUTA.

The class of diseases on the consideration of which we are now entering, is one of the most important that come under the observation of the Army Medical Officer, whether he look upon them with respect to their mortality, or to the number of persons whose future life is rendered miserable by the sequences of one attack.

It will be well to consider here the acute and chronic forms of inflammation separately, in order to assimilate our observations to the nomenclature adopted in Military Hospitals.—and first, of the acute form.

The cases in India, the statistics of which are at my disposal, are as follow, viz:—

599	among men	or 4.22	per cent.	mean strength.
14	“ Officers	“ 3.42	“	“
14	“ Women	“ 1.17	“	“
1	“ Children	“ 0.06	“	“

The mortality was among these classes

36	among men	or 7.07	per cent.	treated.	
2	“ Officers	“ 14.28	“	“	and
1	“ Women	“ 7.14	“	“	“

With regard to the seasons of the year at which the affection prevailed, my notes give the following results of four years' observations on this point, viz., from

April till June inclusive,	there were among men	68	cases.
July	“ September	“	“ 71
Oct.	“ December	“	“ 50
Jan.	“ March	“	“ 36

making a total number of cases* so observed of 225, and from which we learn that the tendency to Hepatic diseases among Soldiers in India increases with the occurrence of the hot dry season—is at its height during the rains, begins to decrease with the occurrence of cold weather, and attains its minimum at the end of that season. This would likewise teach us another point of considerable importance in the history of inflammation of the liver in India, namely, that the tendency to it is increased in proportion, as the range of the thermometer from our high standard is diminished; for in the rainy season, during which we have found the disease to be at its maximum degree of prevalence, there is but a comparatively slight difference between the maximum and minimum range of the thermometer, while at this time, the atmosphere is at its greatest point of saturation with vapor, and consequently does not aid the elimination from the surface by ordinary perspiration.

We might naturally expect to find the mortality from Hepatitis high in a Regiment during the early portion of its service in an Indian climate, and we should moreover, look for a greater prevalence of the disease itself during the first portion of time a Regiment is in a hot country, than subsequently. The ratios of occurrence and mortality of the chronic form will be given in their proper place, but the following abstract from Hospital Returns will exhibit these in reference to Acute Hepatitis, as connected with length of time a Regiment has been in India. Thus then there were in the different years,

1st—	3 cases,	1 death,	or 33.33 per 100 treated.
2nd—	16	0	0.00
3rd—	29	2	6.89
4th—	6	0	0.00
5th—	28	6	21.42
6th—	91	14	14.44
7th—	49	1	2.04
8th—	24	2	8.33
9th—	38	1	2.47
10th—	89	5	5.61
11th—	30	2	6.66
12th—	49	2	4.08
13th—	57	0	0.00

* This includes both Hepatitis Acuta and Hepatitis Chronica.

These relative numbers shewing that if we take several years together, the liability of a Soldier to the disease is least during the early portion of his tropical service, but the chances of a fatal termination greatest, should he happen to be seized with it; while on the other hand, the liability to the disease increases according to the time he is in such climate, but the chances of recovery from an attack become at the same time greater.

A curious and note-worthy coincidence exists between the degree of liability which we have seen Soldiers to manifest to Remittent Fever, and to acute inflammation of the liver; it being in the former disease, as already shown, 4.66 per 100 mean strength, and in the latter 4.22. We also find that the period of maximum prevalence of the two diseases is nearly alike, it being in Remittent Fever the months of August and September, and in Hepatitis, these two, together with July.

That numerous cases of Fever are complicated with disease of the liver is well known, and its frequent connection with Dysentery will be more particularly discussed when considering that disease. It frequently occurs, however, alone and unaccompanied by any other set of symptoms that can be said to constitute another disease. This then is the form to which the present observations are intended principally to refer.

We have seen acute Hepatitis to be most common among Soldiers, and next among Officers, while women are not by any means exempt from it. It is known that some of the principal causes of this disease are, exposure to atmospheric influences, and the use of large quantities of intoxicating liquors, but especially spirits. Now, the three classes above noted, are subject to these causes in the order in which they are placed.

The common Soldier, at all times careless regarding the way in which he exposes himself to sun, rain, or wind, seeks in a potation of hot burning spirits, a temporary relief to whatever inconvenience he may suffer from his recklessness. The balance of the circulating powers is thus upset—the liver from the increased demand on its functions occasioned by a hot climate, acquires a proportional liability to inflammation, and either becomes chronically congested or actually inflamed.

It was formerly, and may be still the custom in some

Regiments in India to have their Canteens open after mid-day, and when this was the case, a Soldier walked from his barrack through the glare and heat of a summer sun elevating the temperature of the air to upwards of 140 F. He hastily swallowed his dram of rum, and again exposed himself as before, when returning to his quarters. Perhaps he had dined before setting out, perhaps he does so now, but in either case, the mid-day meal consists of such a mixture of hot high seasoned stews and curries, that none but the initiated could bear to taste. A sleep now follows, then the dose of rum is repeated in the afternoon, and thus, he who long escapes Apoplexy, or Fever, or Hepatitis, must indeed be more fortunate than he deserves.

It is to be hoped that the system subsequently introduced of substituting beer for spirits to the Soldier as much as possible, will prove beneficial in reducing the numerical prevalence of Hepatitis as well as of the other diseases directly induced by spirits; for although malt liquor taken in large quantities becomes also a fertile source of the disease, this cannot well occur among the private Soldiers, on account of its high price.

Officers, as a rule, drink less spirits, but more beer than men; their meals are also of a superior description, and of course more carefully and well cooked than those of the Soldier. They are also generally not quite so reckless in the way in which they expose themselves to atmospheric influences, and these various circumstances may, in some degree, account for their less degree of liability to the disease than the others. Women again, are still less liable to Hepatitis, no doubt, from their greater exemption from exposure and more perfect abstinence than characterises either the common Soldier or the Officer.

It must not be supposed, however, that all cases of acute inflammation met with in India are the direct result of "drink", exposure, and highly seasoned meals. Many persons of all grades, of the most abstemious and careful habits become the subject of the disease, and are carried off by it. All that is intended to be meant is, that habits such as have been alluded to, give rise to a predisposition to the disease in those who otherwise had none, and become exciting causes in the instances of persons by constitution or other circumstances predisposed to the affection.

In many cases it is difficult, nay, impossible to say, who is or who is not by constitution liable to Hepatic inflammation; there is, however, a certain combination of characteristics which lead a person experienced in the phenomena of the disease in India to suspect the greater liability of their possessor to this affection; as for instance, the dark muddy complexion, the habitual constipation, the dull morose disposition of an individual of which is called the bilious or melancholic temperament.

When, as often happens, a Soldier on being first attacked with Hepatitis, endeavours to kill the stitch in his side which he experiences, by an extra allowance of spirits, the symptoms when he is under such circumstances brought to Hospital, are sometimes so masked as to render an immediate diagnosis difficult. We find the man most likely bathed in perspiration, and if the case occur in the hot or rainy season, he is most probably covered in limbs and body with Lichen tropicus. He is flushed, the eyes suffused, mouth dry, its corners adherent from tenacious mucus like a film of mucilage, his articulation is impeded and thick. He complains bitterly of pain in the region, breathes rapidly, and as if by spasms. The pulse is rapid and hard, he ardently solicits some drink to quench his thirst, and thus may well puzzle even a Medical Officer of experience to say at a glance, whether he is drunk or suffering from Fever, or affected with Hepatitis, or all three together.

These however, it must for the credit of the Soldier be mentioned, are not the usual circumstances under which Hepatitis is met with. We generally find at the morning visit, that a Soldier is presented, saying he was suddenly attacked during the preceding night with severe pain in the region of the liver, to which he then applies his hand, or that he had for some days suffered from slight pain in the side which suddenly came on with increased severity, so as to awaken him from his sleep.

The accompanying pyrexia and disturbance of the circulation are not at all in proportion to the apparent severity of the local symptoms, the stethoscope does not indicate any abnormal sounds in that side of the chest. On pressing upwards with the fingers behind the cartilages of the false ribs, the patient being placed in a flexed posture, great

increase of pain is immediately complained of, and he usually discovers soon that he is unable to lie otherwise than in one position, sometimes on one side, sometimes the other, and sometimes only on the back, depending upon the precise portion of the organ affected.

There is still another class of cases in which even the local pain is inconsiderable; and from the apparently trivial nature of the symptoms present, the Medical Officer, unacquainted with the nature of the disease to be combated, might be thrown off his guard, until suddenly the patient is seized with a shivering fit. All pain for a time ceases, Hectic Fever sets in, and all the symptoms of extensive suppuration become developed.

The peculiar tendency of Hepatitis to run into suppuration is well known; pus being apparently more readily formed in the liver than in any other organ. It is not in those cases where there is most acute pain or pyrexia, however, that we most dread this unfavorable termination; but it is in those where there is a dull, heavy pain, deeply seated in the side with comparatively little disturbance of the system that we must fear the occurrence of abscess; these being the symptoms that distinguish inflammation of the parenchyma of the liver, from that of the surface and peritoneal coverings.

The Medical Officer cannot be too careful in examining into the condition of a patient complaining of pain in the region of the liver, for although, as will afterwards more particularly appear, all the ordinary symptoms of Hepatitis may be complained of, and yet no disease of the organ actually exist, we find in others, that suppuration occurs when the preliminary symptoms were so slight as not to appear any thing like adequate to produce such a result—and therefore, much better treat as serious what at the time may only appear a slight disease, than neglect one with so dangerous a tendency as we have seen acute Hepatitis to possess.

The extent of an abscess in the liver that may happen even in so short a time as forty-eight hours from the first admission of a Soldier into Hospital, cannot do otherwise than strike us with wonder; for in some cases of death within such early period, the whole surface of the organ is occasionally found to be nothing more than the thin wall of one huge suppurating cavity; almost the entire Hepatic tissue having apparently been transformed into pus.

It is generally believed, although statistics have not so far as I am aware, been instituted to determine the point, that in Bengal, the suppuration when it does occur, is in the majority of such instances situated towards the centre of the liver; but in Madras, towards the surface, and hence the greater success attending the operation of puncturing the organ in the latter than in the former Presidency. This may, or may not be the case, but at all events, the question deserves the attention of Medical Officers in these portions of India.

Of the diseases liable to be confounded with Hepatitis, Pneumonia is one which may, under certain circumstances, be somewhat difficult to distinguish from it, notwithstanding the aid derived from auscultation and percussion. These adjuvants may certainly render the diagnosis easy in the very early stages of the disease, but in its more advanced form, the liver, by enlarging upwards, presses upon the lower lobe of the lung, thus giving rise to symptoms that may altogether mask the primary disease; and in extreme instances of this nature, morbid action may be set up in the substance of the lung itself, giving rise ultimately to hepatisation of the pulmonary tissue or to abscess in the lung.

In Army practice, we are not much accustomed to those fine distinctions that in works and published articles are drawn between "congestion" and "inflammation" of the liver, and perhaps it is as well for the Soldiers under our charge that we are not. It is in reality impossible to say where mere congestion of any organ terminates, and where inflammation, as that term is usually understood, commences. It is therefore better for the success of our treatment, if we consider the first as merely a less severe degree of the latter, for in both, there is superabundance of blood in the organ impeding its functions; and therefore less harm can arise from treating a case actively from the outset than by trifling with remedies, which at best, relieve symptoms for a time without subduing the disease.

In treating a case of idiopathic and uncomplicated acute Hepatitis in an otherwise healthy person, the first grand step to be taken is, to abstract as much blood as the system will bear. The effect of this is of course to diminish the amount of circulating fluid in the body, and thus cut off as it were the supply of fuel to the inflammation going on;

a free dose of Calomel ought then to be given, and in the course of an hour, followed up with a brisk dose of a saline purge, or if we are situated in a locality where such cannot safely be employed on account of the predisposition to Cholera likely to be produced, nothing is better than the Compound Jalap Powder. Calomel, either alone, or in combination with tartrate of Antimony or Opium, or both must then be given freely at short intervals, until a distinct effect is produced, and then as the state of the case may demand.

The instances in which it is advisable to combine the Calomel with Tartarized Antimony are those where there are more than usually urgent pyrexial symptoms, and Opium is advisable where there is a tendency manifested in the bowels to throw off the Calomel, or where there is much constitutional irritation present.

Some Authors doubt the superior efficacy of mercury in Hepatitis, and among others, a late writer on the Diseases of India* remarks—"What, it may be asked, does Calomel possess, which renders it superior to Croton Oil, Opium and Tartar Emetic, the three representatives of these classes of medicines?" Now the simple reply to this is, that it possesses a remarkable power of preventing suppuration, and is in consequence peculiarly applicable to the treatment of Hepatitis, the tendency of that disease being, as has been already seen, to run on rapidly to the formation of abscess.

Why a particular medicine does possess a particular effect on disease is a question to which no satisfactory reply could possibly be given. The knowledge of the fact is sufficient for all practical purposes, and with it, the Army Surgeon may well be satisfied, leaving it to pure theorists to investigate the "why and the wherefore" of such action, which they will probably do with the same amount of success that attends inquiries into the ultimate cause of adhesion, co-adhesion, and vital action.

Some remarks of Dr. W. B. Carpenter, are, however, so apposite to the modus operandi of bleeding and Calomel in acute Hepatitis, that they cannot be passed without some further notice.

In para. 531, of his Manual of Physiology, he states that

* Dr. W. L. McGregor of the E. I. C. Service.

"the quantity of Fibrine exhibits a remarkable increase in inflammation, the amount then found in the blood being from 5 to 6 parts in 1000, to 9, 10, or even 104; according to the extent and intensity of the disease." In para. 638, he observes—"there is every degree of gradation between the plastic or organisable deposit of well elaborated Fibrine, the *caco-plastic* or *imperfectly organisable* matter of Tubercle, and aplastic or non-organisable matter of Pus." Let us again revert to the paragraph originally quoted, which concludes with this remark—"An excess of Fibrine is not much affected by copious bleeding, even if this be frequently repeated; but there is reason to think that the administration of mercury has a tendency to *restrain* its production."

Such then is the opinion of this eminent Physiologist of the manner in which an important class of remedies exert their effect on inflammatory action. And, although venesection does not appear to have that beneficial effect in his opinion which practical results prove it to have; nevertheless, it becomes easy to perceive even from the above quotations how it should have such beneficial influence.

Let us suppose that the proportion of Fibrine in the blood of a patient laboring under acute Hepatitis, is at its maximum (although such seems to be the case in inflammation of Fibrinous and serous tissues). By abstracting a certain quantity of this fluid, we must thereby diminish the amount of Fibrine circulating through the system, and ready to be eliminated in more or less highly organisable condition, according to the nature and seat of the disease.

But we command another result from general bleeding. By taking away a certain amount of the natural vital stimulus, we directly diminish the energies of the system, and thus reduce the force with which the remaining blood is sent to the diseased part—nor must the very important fact be forgotten that the absorption of medicines into the system is materially increased by vascular depletion, and thus the rapidity and power of their action enhanced.

In many cases of the acute disease that present themselves, the secretion of bile is greatly increased, and much suffering to the patient created by its discharge by the mouth, and by the bowels, causing in the latter case severe bilious Diarrhoea. In fact, we not unfrequently find the first effect of Calomel to be to give rise to one or other of these conditions, and

when this happens, a corresponding modification in our treatment must be made.

The bilious Diarrhoea with its attendant heat and scalding around the anus is best moderated by a mild dose of Castor Oil, provided that medicine be of good quality,* and otherwise by occasional doses of Carbonate of Soda, or of Magnesia together with Rhubarb in laxative doses.

Frequent vomiting of bilious matter can only be palliated, either by the administration of alkaline preparations, or Opium, Hyoscyamus, or Aromatic Spirits of Ammonia to increase the tolerance of the stomach. When retching becomes very constant and urgent, a mild emetic may be given, but it must be borne in mind, that the effect of this class of remedies must always be to increase for the time being the sufferings of the patient, and therefore it must become a question for deliberation, whether more benefit will accrue from a free evacuation of the irritating matters contained in the stomach, with the chance of temporarily increasing the pain in the Hepatic region, or from using measures of a milder but less rapidly efficient nature.

In all cases, our main object must be to bring the system under the influence of Mercury as speedily as possible, and therefore other remedies given in the early stages of Hepatitis must be administered as adjuvants, to attain this particular end.

It must be recollected that what is meant by bringing the system under the influence of Mercury does not imply the mere induction of salivation. This may, or may not occur, without the specific effect of the Mercury in this particular disease being manifested, but if we can procure a diminution in the intensity of the inflammatory symptoms without, at the same time, subjecting the patient to all the inconvenience and injury of salivation, our success in the treatment will be thereby doubly increased.

Local bleeding by leeches, is another measure of great importance, either superadded to venesection, or in weakly men, or those who have been for a considerable time in a hot

* This is very necessary to be considered, as in India the Castor Oil supplied to Regiments from the Government Stores, would seem from its action to contain some other oil of an acrid and irritating nature.

climate employed alone: and next in importance comes counter-irritation by the application of a blister to the region over the liver, and afterwards maintaining the discharge by resin or lyttæ dressings. As with other remedies, so with this, a certain amount of caution and discrimination becomes necessary, for if a blister be applied too soon, other remedies, that by an aggravation of the symptoms, may become necessary are thus rendered inapplicable, and in such a case more harm than good accrues.

The lowering effect of a large blister upon a constitution, it may be already much debilitated by disease or climate, is well known, and therefore the size and power of one must be modified according to circumstances of this nature.

Much benefit, and invariably great relief to the patient's feelings is produced by the application of fomentations, and therefore these may, in many instances, be beneficially employed alone or after the application of leeches; and indeed, it is more than probable that as in the case of superficial inflammation, so in Hepatitis, they aid the termination of the disease in what is called "resolution."

On the first decrease of the symptoms, we may diminish the amount and frequency of our doses, keeping the system moderately under the influence of the remedies employed, and then our further treatment must be directed to keep down the tendency to relapse, and to regulate the different functions of the body, giving as convalescence advances, tonics containing Taraxacum, or extract of Gentian, with diluted Nitric or Hydrochloric Acid.

When death occurs from acute Hepatitis, in almost every instance, is from the occurrence of abscess of so extensive a nature, and containing so much fluid, as to be incompatible with the continuance of the functions of nutrition, or perhaps from the matter evacuating itself into the lungs, producing instantaneous suffocation, or into the cavity of the peritoneum, inducing inflammation of that membrane. While the disease is in the acute form, the occurrence of a shivering fit, either alone or followed by a hot and sweating stage, must always be looked upon as indicating, it may almost be said with unerring certainty, the occurrence of suppuration, but the consideration of abscess properly belongs to "Chronic Hepatitis," and must be considered along with the other phenomena constituting that form of disease.

HEPATITIS CHRONICA.

Although this form of Hepatic disease is here brought in for the sake of convenience as a distinct affection, because it is noted as such in the Medical Returns, it must nevertheless be understood to be nothing more than the result of an acute attack of inflammation in the first instance, or the setting up of a less distinct form of morbid action, induced in some instances by protracted indulgence in excess of food or drink.

The various conditions of the liver upon which it directly depends, may be said to be acute inflammation, terminating in one or more abscesses of various sizes, or in the effusion of organizable tissue, causing enlargement of the organ. Hardening and contraction of the Hepatic tissue from slow, morbid action, diminishing the dimensions of the cells formed by prolongations of the Capsule of Glisson induced by indulgence in spirituous liquors, and commonly called the Drunkard's liver—Fatty liver, Hobnail liver, and the various other modifications of altered nutrition of the organ of a degree of intensity inferior to what by common consent constitutes "acute" disease.

There are few Medical Officers who have seen much of the treatment of disease in hot countries, who have not had occasion to treat Soldiers affected with all the ordinary symptoms of Chronic "liver disease," and yet in whose bodies, after death, not a tangible trace of such affection remained. To those who have not actually witnessed such cases, the question might naturally arise—Was there not an error in diagnosis? There need be no such an error, for in old Soldiers, debilitated and exhausted by Foreign Service, yet suffering from no distinctly organic disease, a train of symptoms may arise from impaired action of the digestive apparatus, which, while they include the liver, may nevertheless be unattended by any active disease in it. Nutrition is affected, and finally, the powers of the patient, already brought by mere climate to the lowest ebb compatible with the due performance of their functions, give way, and death actually takes place from exhaustion. On the other hand, actual disease of the liver may exist, and yet the symptoms be so obscure and equivocal, such as mere loss of appetite, irregularity of the bowels, slight and transient febrile flushes, slight uneasiness in the epigastrium, borborygmi, &c., that the case may be referred to "dyspepsia,"

until on the occurrence of death, the real cause of the fatal event is found to be Hepatic abscess.

This form is of most common occurrence in India, during the month of March and early half of April, when the heat of the day is becoming very great, but the nights still remain cool, if not actually cold. At such time, Soldiers not unfrequently are admitted into hospital with symptoms of so trivial and equivocal a nature that, were it not for the peculiar and almost characteristic look of the countenance, their illness might be taken for one of a slight and evanescent description, yet in them suppuration, if it has not already taken place, may occur so insidiously as not to attract immediate attention either of the patient or Medical Officer, until the occurrence of Hectic and Diarrhea indicate too surely the nature of the evil.

The periods of the year in which the disease most prevails, have been incorporated in the summary given of Acute Hepatitis, there being in the Hospital Records no distinction made with reference to this point. There is, however, this amount of difference between Acute and Chronic Hepatitis, that while the former does not often occur in its original intensity, except as a relapse, and that during the hot portions of the year, the Chronic form is liable to come on during the cold season,—after a patient has been sent to a Hill Station for change of climate on account of the acute disease—or on board ship during the alternatives of heat and cold, wet and dryness, to which they are necessarily exposed on the homeward voyage, and even in the United Kingdom.

The statistics on which these remarks are in a considerable measure based, show the prevalence of Chronic Hepatitis in India to have been—

184	among Men	or 1.56	per cent. strength.
11	" Officers "	2.68	"
1	" Women "	0.08	" and
1	" Children "	.06	"

The mortality which was confined to the three first classes were among

Men	13	or 7.06	per cent. cases,
Officers	1	" 9.09	"
Women,	1	" 1.00	"

So far as this series of observations go, they tend to show that the ratio of occurrence of Chronic Hepatitis is much less

than that of the acute form among Men, Officers, and Women, but the rate of mortality among the Soldiers so nearly approximates in both classes, it being 7.07 in the acute and 7.06 in the Chronic, that the coincidence is remarkable.

That very great variety exists both in the rate of prevalence of Chronic Hepatitis and also in its mortality, must be apparent from the following Abstract which gives the number of cases and their proportion of deaths, according to the year of Foreign Service of a Regiment.

Year	Cases	Deaths	Per 100 treated
1st year	28	0	0
2nd "	9	1	11.11
3rd "	9	0	0
4th "	2	0	0
5th "	1	1	100
6th "	8	2	25
7th "	10	2	20
8th "	9	1	11.11
9th "	9	2	22.22
10th "	14	1	7.14
11th "	23	2	8.69
12th "	29	1	4.82
13th "	33	0	0

From this we learn that, while the proportion of cases increases *ceteris paribus* according to length of time in India, the ratio of mortality in like manner diminishes.

But another circumstance well deserving the serious attention of the Medical Officer, is taught by the preceding Table; namely, that the liability of the Soldier to attacks of chronic liver disease, is in India very much influenced by the leniency or severity with which he is treated, according to parades and duties, by his Military Commanders.

A Soldier who has once been the subject of severe inflammation of the liver, will take a long time ere he can again bear the constraint and tightness of belts and accoutrements; neither will he be able to drill with the same rapidity and for the same length of time as his comrades who have not been similarly attacked.

It is clear, that to keep a Soldier in Hospital under such circumstances, would be to destroy his only chance of regaining strength and health, yet it is equally clear that he runs a very considerable risk of suffering from excessive fatigue.

Much of the health, therefore, as well as the happiness of the Soldier rests with the Commanding Officer, for in such cases as those that do not come immediately under the control of the Surgeon, the considerate and humane Commander, by lightening duty to such persons, will tend to their recovery and future efficiency, while, he of an opposite disposition, will, with equal certainty wear out the already debilitated energies of the Soldier, accelerate his progress to the grave, or render him a confirmed invalid for his few remaining years.

There are probably few Soldiers or Officers, who after some years abroad in a hot climate, are altogether free from chronic disease of the liver in some shape or other, but it would seem that the liability to such disease is not alike in all parts of the tropics. For instance, while in India, it is of very common occurrence, it is comparatively rare on the Coast of Guinea, in tropical Africa, not more than 5° N. of the Equator.

The symptoms that characterize chronic Hepatitis may be divided into two classes; namely, those that are manifest to the ordinary beholder, and those experienced in the person of the patient. Of the former is, first, the peculiar pale, shrivelled, sharp, and waxy countenance, so distinctive in persons affected with this disease, and indeed so commonly met with among residents of tropical countries and India especially, as to be looked upon as distinctive of old residents in that country. Then we have the peculiar, sharp, irritable manner, and uncertain temper induced by the impaired functions of the liver; eliminating imperfectly the pernicious products which it is the peculiar province of that viscus to separate from the blood, as well as from the irritation occasioned by continual uneasiness in one region of the body, and the general discomfort occasioned thereby.

The patient himself complains of occasional attacks of pain of variable severity in the region of the liver, and when free from this, of a weight and sensation of a cold or hot mass under the false ribs on the right side. He suffers from pains stretching along that side of the chest to the shoulder, and sometimes down the arm to a considerable extent, a symptom, which although pathologically inexplicable, is nevertheless, of almost, if not actual, invariable occurrence in confirmed cases of chronic liver.

As has been already remarked, these symptoms are aggravated by alternations of weather; and although the acute

form of the disease may, as a general rule, be said to prevail most where the range of the thermometer from a high degree is small, the chronic form is liable to occur on exposure to cold, or when a patient is removed for change of air to a Hill Sanitarium in India.

The state of the digestive organs is usually uncertain and capricious, appetite is usually bad, a full meal followed by greater or less uneasiness, or even vomiting. The bowels usually constipated, the evacuations containing but a scanty supply of bile, at other times an attack of bilious diarrhoea for a time relieves these symptoms and makes the patient forget his sufferings.

There are always more or less severe feverish attacks at regular intervals, depending much upon whether or not any small abscesses exist, and when the disorganization threatens life, colliquative perspirations and Diarrhoea occur, as in other instances of puriform effusion.

Cold feet and extremities is a symptom of constant occurrence in this affection, and one productive of much inconvenience and annoyance to the patient; it being difficult in even tolerably mild weather to maintain the natural warmth of the feet.

It may be that a degree of bulging exists in the side, or in cases where the Hepatic tissues are contracted, the organ may not be tangible on making pressure under the ribs. Pressure, however, and especially percussion always increase the uneasiness, and the patient finds pain on lying in particular positions.

Such then is a general description of the symptoms present in cases of Chronic Hepatitis; their intensity varying with the degree of organic disease present, but in even the slightest cases, so distinct as to leave little doubt of the nature of the affection.

The symptoms that precede the formation of Hepatic abscess are often so trival or equivocal, that some fatal cases of the latter are not unfrequently recorded under the head of dyspepsia and this, as has been already noted, is most commonly the case at the commencement of the hot season. In such, it is clear that a correct diagnosis can only be arrived at, when great care and discrimination are employed in the examination of the case.

It cannot do otherwise than strike us with wonder how

great an extent of disorganization may exist, and yet the functions of life be carried on with tolerable efficiency for many years; yet, on the other hand, in this as in all other diseases, it is often equally surprising how small an amount of morbid degeneration is sufficient to arrest the continuance of life.

Treatment, it must be confessed, can only be of much use where the extent of organic degeneration is slight. In extreme cases, palliative measures may soothe the last days of the sufferer's life but nothing more, nor can remedies in any degree of the disease do more than merely assist the vis-medicatrix nature in bringing about a reparation of the injury occasioned by the morbid action.

The principal indications must be to relieve local attacks of congestion or acute inflammation which from time to time occur, to remove the determination from the liver into some other channel, as by counter-irritation, diaphoretics, or purgatives, to regulate the digestive functions and secretions by the cautious employment of inappropriate food, by regular habits both as regards eating and drinking—the employment of medicines calculated to maintain the regular action of the intestines: and in addition to these, the patient ought to be removed from the local influences under which the attack supervened, while a class of remedies are employed, whose peculiar action is immediately directed to the liver itself.

Of these, the following are some of the principal, *Mercurials*, including Calomel, Blue Pill, and Hydrargyrum cum creta, form a class of remedies of the greatest value in Chronic Hepatitis; first, by inducing the secretion of bile that has been impaired; and secondly, by checking the tendency to deposition of lymph in its organizable or inorganizable state, an effect which we have seen it to produce.

It may not be necessary to produce salivation, nor may it always be necessary to continue the use of a Mercurial for a long period. When however, the patient suffers from what he calls "a fit of liver"—that is, is out of sorts—has a feeling of pain or weight in the side, loss of appetite, constipated bowels, coated tongue and foul breath, a good Mercurial purge will sooner than anything else relieve him; while, if these symptoms in a less severe form persist, it will

be advisable to give repeated doses of Calomel or Blue Pill together with James' Powder, and a tonic, such as Gentian or Taraxacum employing at the same time the Nitric or Nitro Muriatic Acid.

Next to Mercurials, there certainly is no class of medicines that seem to exert so decided an effect upon the liver, as do these acids. The diluted Nitric, or better still, the Nitro Muriatic, either administered internally or applied locally over the region of the liver, would seem to have an almost specific effect upon the sequences of inflammation affecting that organ. When employed judiciously, they restore the secretive functions of the organ, seem to aid the re-absorption of tissues already deposited, improve the weakened digestion, and in fact, do as much towards the recovery of the patient as can be expected from medicine.

In certain states of what is called "torpid liver," frictions over that organ either alone or with a stimulating application such as Camphorated Mercurial Ointment, Iodide of Potassium, or Soap Liniment frequently restore the action of the viscous, and in old standing cases ought never to be neglected.

Counter irritation, either by setons or by the frequent repetition of blisters, is likewise a valuable adjunct to other remedies, and the relief produced by one or other of these in these cases is very striking.

The frequent administration of saline purges is a measure much dwelt upon by some authors, and in such chronic cases as occur in the United Kingdom; but in India, where saline medicines are almost constantly contra-indicated, on account of the tendency to Cholera they produce, it follows as a matter of course that they cannot be much employed.

The next measure to be considered involves exploration and puncture.

Some years ago, the practice of exploring the liver for abscesses, and evacuating them when they could be found by means of a trocar and canula, was a measure of frequent adoption in India, but latterly it has, with much propriety, been allowed to fall into disuse.

I have myself examined the bodies of men who had during life been "explored" for abscesses by Army Medical Officers of high professional reputation, and yet the livers thus penetrated were found to present no trace of disease.

My own personal experience of puncturing the liver for

deep-seated abscess is unfavourable, yet in India, a sufficient number of cases are met with where, on superficial suppuration occurring, an opening was made into the projecting part, the morbid contents evacuated, and an effectual cure effected to justify the adoption of the measure in such cases.

Should an artificial escape be obtained in this way for the contents of an abscess, or should it flow by a spontaneous opening into the lungs, stomach, or intestines, as it may do, the strength must be supported by mild and nourishing articles of food, with a moderate allowance of malt liquor or wine.

Beef tea, would seem in the greater number of cases where there is much prostration and debility of the vital powers, to be the most applicable remedy, and in India, is certainly a far better and more permanent stimulus than either fermented or spirituous drinks. It directly supplies nourishment, while it does not unnaturally excite the circulatory functions, and as in the treatment of such condition as is here alluded to, our endeavors are to be to support the vital energies, and thereby enable them of themselves to repair the injury that has been effected by morbid action, we shall find that the less likely our measures are to occasion disturbance of any of the great functions of the body, so much greater will be the success attending them.

In a great many cases, even those the most unpromising at first sight, an effectual cure may be effected, and the subject of the disease rendered capable of again performing his duties with efficiency; when, however, the attacks remain obstinate, are subject to frequent aggravations, and the general health of the patient is gradually declining, with every prospect of continuing to do so, unless removed to another climate, he ought to be sent to a Hill Sanitarium, to England, if abroad, or discharged the service if at home.

The following contains a numerical summary of the Deaths from Hepatitis, according to complexions of the patients, viz:—

No. of Deaths.	Color of Hair.	Color of Eyes.	Complexion.
3	Light ...	Grey ...	Fresh.
2	Hasel
1	Fair
1	Grey
1	Red ...	Blue
1	Sandy ...	Grey
3	Light Brown ...	Blue
6	Grey
3	Blue ...	Fair
2	Grey
8	Brown	Fresh.
1	Blue
1	Light Blue ...	Fair.
1	Hasel ...	Fresh.
2	Fair.
1	Grey ...	Sallow.
2	Fair.
1	Blue ...	Dark.
4	Dark Brown ...	Grey ...	Fresh.
4	Hasel
1	Grey ...	Fair.
1	Blue ...	Fresh.
1	Sallow.
1	Dark	Fresh.
1	Grey
1	Black	Sallow.
1	Brown
1	Blue ...	Dark.
3	Hasel ...	Dark.
1	Not noted

60 Total cases recorded.

The above shows very clearly, so far as it goes, that the greatest mortality from this class of diseases does not occur among the men of bilious temperament, as might have been expected.

A Table showing the rate of Deaths treated, of cases of Acute and Chronic Hepatitis, in the 10th Foot, during 13 years of service in India.

Year of Service in India.	Year.	ACUTE HEPATITIS.			CHRONIC HEPATITIS.	
		Cases.	Number of Deaths.	Deaths per 100 treated.	Cases.	Number of Deaths per 100 treated.
1	1842-43	3	1	33.33	28	...
2	1843-44	16	9	1 11.11
3	1844-45	29	2	6.89	9	...
4	1845-46	6	2	...
5	1846-47	28	6	21.42	1	1 100
6	1847-48	91	14	14.44	8	2 25
7	1848-49	49	1	2.04	10	2 20
8	1849-50	24	2	8.33	9	1 11.11
9	1850-51	38	1	2.47	9	2 22.22
10	1851-52	89	5	5.61	14	1 7.14
11	1852-53	30	2	6.46	23	2 8.69
12	1853-54	49	2	4.08	29	1 4.82
13	1854-55	57	33	...
		509	36	7.07	184	13 7.06

SPLENITIS.

Inflammation of the spleen, such as is indicated by the appellation given to the class of cases about to be considered, is a form of morbid action of exceedingly rare occurrence. Enlargement of the organ, however, and vitiated performance of its functions are of more common occurrence, and it is chiefly to these, that the following remarks are applicable:—

In India, cases of diseased spleen are of comparatively frequent occurrence, one fertile cause being Intermittent Fevers, during the cold stages of which, the blood thrown inwards from the surface, and finding a kind of compensation reservoir, so to speak, in the spleen, renders this organ particularly prone to become affected. The same holds good in the United Kingdom, where the diseased spleen is popularly called "Ague cake."

The Returns indicate the rate of occurrence of this disease in Bengal and the Upper Provinces of that Presidency to be as follows, *viz*:—

127 Among Soldiers, or 1.06 per 100 mean strength.

1	"	Officers	"	0.24	"	"	"	"	"
7	"	Women	"	3.59	"	"	"	"	"
13	"	Children	"	6.89	"	"	"	"	"

while among the two classes in which mortality occurred there were among men 5 or 2.52 per 100 treated, and " " children 1 " 7.69 " " "

The relative prevalence of this disease and Intermittent Fevers may, for the sake of illustration, be here noted side by side.—Thus there were—

Cases among,	Inter. Fevers.	Of Splenitis,	or per cent. of latter to former.
Soldiers,	1672	127	7.59
Officers,	48	1	2.08
Women,	130	7	5.39
Children,	74	13	17.56

This Table shows what we actually meet with in India; namely, the very great liability of children, compared with other classes to become affected with disease of the spleen, after fevers of the intermittent type. This too is particularly the case with children of all ages belonging to Officers, although statistics of a Regiment do not usually include them. In fact, a Medical practitioner in that country cannot do otherwise than feel surprise at the number of

Officers' children whose wasted forms, pale countenance, blanched compressed lips, tumified abdomen, and distinct enlargement in the left hypochondrium proclaim in unmistakable terms the existence of spleen disease, to such an extent, that recovery can only be looked for, or life saved by immediate removal of the little patient to a hill climate or to the United Kingdom.

That such cases occur in greater proportion among the children of Military Officers, Civil Servants, and other Members of "the upper classes" in India than among the children of soldiers brought up in all the discomfort of a barrack, and without that care or attention to health being bestowed upon them which are deemed so indispensable among gentlemen's children is undoubted, and probably depends upon causes of so obscure a nature that in a paper like this, they cannot be more than alluded to.

Thus we may presume, that a private Soldier only remains in India, while his health is robust, while the wives of men of this description must as a matter of course, proceed to Europe with their husbands; at the same time that those married to tolerably young men even should their own health begin to fail, cannot leave the country nor proceed to a Hill Station. Their health once broken therefore, they speedily succumb to the climatal influences. Thus women of this class, taken in the mass, reside a shorter time in India, and while they die off faster than ladies, are for the above reasons number for number more healthy.

The wives of gentlemen, on the other hand, like their husbands, remain in India, while in a comparatively bad state of health. Being averse to incur the expense and separation of a voyage to England, one or other, or both every now and then proceed to the Hills for a few months at a time, but this being insufficient to completely restore their impaired health, they thus pass on from year to year as Invalids.

It must be clear then, that the children of one class of persons must be more robust than those of the other, and also that the mortality of Soldiers' children, even great as it is in consequence of the want of necessary care usually bestowed on them, must, under opposite circumstances, have been even double what it is, while a bare look at the father and mother of many a gentleman's delicate child, is often more than sufficient to account in an instant for the unhealthiness

of an infant, notwithstanding all the care and attention bestowed upon it by its parents.

Enlargement of the spleen seldom exists among British Soldiers to the same degree as among Natives, very few of whom are free from some amount of it, and many are effected to such an extent that a slight accident occasions rupture, and almost instantaneous death. Yet even among our own countrymen out here, cases often occur in which the organ does attain a very alarming size.

Perhaps the real functions of the spleen are best ascertained by negative characters induced by disease. If this be the case, its direct connexion with the regulation of the quality of the blood is clearly demonstrated by the peculiarly pale and anæmic appearance of persons suffering from this disease.

In some, local dropsies occur, specially into the cellular tissue of the inferior extremities, and in others, direct hæmorrhages, while the general health gradually succumbs, and the functions of nutrition become affected, thus shewing the near relation between "Splentis" and those conditions of the system included in Returns under one or other of the denominations "Cachexia," "Scorbutus," "Purpura."

Nor must we omit to mention here, that many cases of spleen disease occur in Regimental Hospitals that cannot well, in accordance with the rules of nomenclature, be included under that head. For instance, a Soldier with a greater than ordinary predisposition to Intermittent Fever, is from time to time admitted on account of that disease. The severity, as well as continuance of each succeeding attack increase, and while subject to his ordinary paroxysms, his case is discovered to be further complicated by the occurrence of enlarged spleen. In such an instance, the disease can only be looked upon as an immediate effect of the continued action of whatever influence has given rise to Intermittent Fever, and therefore no alteration is made in the Register of Admissions.

In the same way, some fatal cases of "Splentis" are in great measure owing to the obstinate continuance of Intermittent Fevers; that is to say, death seems often to be induced by the repeated attacks of such fever; but it is nevertheless equally true, that when both affections co-exist, no remedy will be successful in checking the febrile returns, until such time as the state of the spleen has been in some measure improved.

But is not the spleen, it may be asked, subject to inflammation or enlargement, independent of Intermittent Fevers? In my Military experience in various countries, I have never met with such an instance. It seems, however, that the liability of the organ to become affected, varies much in different people, and also seems to increase with the length of time a person is resident in a malarious district; not unfrequently, in such cases, becoming a disease of greater immediate urgency than the original one.

Neither is the period required for the organ to attain a particular size, the same in all cases. In some, its bulk increases slowly, gradually, and almost without any symptom of sufficient urgency to attract the patient's attention. In others again, the rapidity of enlargement, the amount of local pain, and sensation of heat are such, that it might perhaps be deemed sufficiently active to constitute actual inflammation.

Although the rate of mortality from this disease cannot be said to be considerable, we have seen that various other morbid conditions arise as a direct result of it, which of themselves become more or less dangerous to the lives of the patients. There are other diseases, which although not directly induced by morbid alteration of the spleen, are nevertheless thereby rendered more fatal, than they would under other circumstances be.

The one, perhaps, above all others to which this particularly applies, is dysentery, and in some parts of India where the mortality from that disease is very great, it is often referred to Malaria, in the same way that "Spleen" is indirectly, through the medium of Intermittent Fever referred to that prolific cause of the disease.

In considering the treatment applicable to enlargement of the spleen, consequent on Intermittent Fever, and the cachectic habit of body thereby induced, it is necessary to observe that, among the Commissioned ranks it is seldom that any further treatment is necessary, than a bare removal of the patient to a more cool and healthy climate. It may however so happen that during the continuance of the rainy season, when this disease principally prevails, travelling may be impracticable, or the patient may, on account of some other disease or private circumstances, be unable to leave the Station, and then Medical treatment must be resorted to counteract the effects of the morbid poison with which the system

is more or less completely saturated, always bearing in mind, however, this important fact, that although our remedies may be so far successful as to relieve the disease for a time, still, a radical cure can only be hoped for by a removal of the patient from the locality for a considerable time; and that next to a visit to the United Kingdom, a sea voyage is the most effective remedy; a few months, visit to a Hill Station, although so far highly beneficial, being hardly ever sufficient to completely eliminate from the system the particular morbid influence on which the disease primarily depends.

Among Soldiers, immediate removal in this manner, is seldom practicable. In India, a few are annually permitted to be sent to the Hills, but the season at which they are allowed to go being just prior to the setting in of the intense heat, those who are unlucky enough to become affected with "Spleen" must, in the great majority of cases, remain in Hospital under treatment for a long time, unless they happen to be so seriously ill, or incapacitated by other causes of a medical nature, as to render them fit subjects to be sent to England as invalids.

In cases of what may be termed acute enlargement, that is, when the spleen in a patient who has suffered only in an inconsiderable degree from Intermittent Fever rapidly increases in size, while there is also present some amount of pain in its region, increased by pressure and sensation of internal heat, it will not be proper to employ the same active measures that would be demanded in a case of inflammation of any other of the large organs, the liver for instance. The presence of direct poison in the system must be borne in mind debilitating the powers, as well as vitiating the vital fluid, by inducing a decrease in the amount of red particles in the blood.

Active depletion, together with the free employment of Mercurials cannot be borne under such circumstances, although, perhaps contrary to the injunctions laid down by some authors, local abstraction of blood, as well as the moderate exhibition of Calomel may under certain circumstances be employed with advantage. Thus, when the pain under the left hypochondrium is of considerable severity, the disease of recent occurrence, and unattended by general cachexy or hemorrhagic habit of body, a few leeches applied will be of great advantage, while, if there is much dryness

and harshness of the surface, and the bowels have already been freely evacuated, a moderate dose of Calomel and James' Powder, by inducing a gentle diaphoresis will materially allay the internal congestion.

While such remedies may, under certain circumstances, be necessary, the general principles to be followed in the treatment of enlargement of the spleen as met with in India after Intermittent Fever, are to support the powers of the patient, to put an end to the paroxysms of fever upon which the disease seems directly to depend, and to supply to the system those materials of which the blood seems to stand most in need.

The first indication is carried out by the use of tonics, nutritious diet, a moderate allowance of malt liquor or wine, personal cleanliness, and as free a circulation of air as circumstances will permit.

The second, by Quinine, Arsenic, Zinc, Berberine, and the other medicines known to have a direct antiperiodic tendency, while the third object is best attained by a combination of one or other of the above vegetable preparations with Iron, of which, the best compounds are the Sulphate, or Carbonate.

A preparation of Sulphuret of Potass, Sulphate of Iron, and a gentle laxative, has for a long time been highly recommended in India. When the accessions of fever are distant or irregular, the Muriated Tincture of Iron is a very convenient form of administering the drug, and the compound Iron mixture of the Pharmacopœias is another highly useful form.

The local application of Iodine, either as the Tincture, or as Iodide of Potassium is also recommended, and under certain circumstances, as for instance, when the system of the patient has got into a state of torpidity, this is no doubt highly useful,—counter-irritation may also sometimes be required, but as already observed, removal, and for a considerable length of time to another climate, is the remedy to which we must look. If our treatment should prove effectual in completely removing the disease, good and well; but if not, we ought not to miss an opportunity of sending the patient away, knowing that once removed from the locality how rapid and complete is often the recovery, from what under other circumstances might become a fatal disease.

[From the MEDICAL MIRROR for April, 1866.]

On Indian Medical Practice. By THOMAS INMAN, M.D. Lond.,
M.R.C.P., Late Lecturer on Medicine, Physician to Liverpool
Royal Infirmary.

As an old, and somewhat enthusiastic, teacher of medicine, I have endeavoured to imbue my pupils with the same interest in their profession that I have had myself, and have done everything in my power to encourage in them original observation.

Those who have gone to a distance have occasionally communicated their experience to me, from one, however, in particular, who was originally my clinical clerk when I was working up the subject of myalgia, and who was an intimate friend and correspondent while I was writing my second work on "The Foundation for a New Theory," &c., and who has been now for many years in India, I have received such valuable information that I have urged him most strongly to prepare his experience for publication.

Circumstances are yet adverse to his doing so, yet I think that the results he has attained are so important, that it is a sort of duty to our countrymen residing in hot climates to make them known. I would willingly give the name of my informant did I think that I could do so without inflicting probable injury upon him. Army surgeons are just as much prejudiced as other people, and look upon innovation with a frown, a snub, and possibly something worse.

To any one who takes the trouble to think, the conclusion will be apparent, that the climate of India *does not* strengthen the constitution or increase a man's muscular power, and that it *does* impoverish the blood, weaken the vital energies and diminish the tone of the muscles.

As a corollary, muscular affections are likely to be far more frequent amongst Europeans in India and in hot climates generally, than in England.

In direct proportion to the weakness of the muscles is the

severity and duration of myalgia when it occurs, and the greater the tendency for the affection to be marked by fever and to run on into myositis.

Mistakes innumerable are daily made in England by medical men taking myalgia in one region or another for inflammation of the viscera below the affected muscles. More mistakes still are made in India, and the liver, spleen, kidney, &c., are supposed to be affected by inflammation, while the real seat of disease is in the parietes above them.

After long consideration, my friend assures me that *inflammation of the liver is a very uncommon affection amongst Europeans in India*. He has seen abundance of cases so reported and each one that he has been enabled personally to investigate have been simple cases of intense myalgia.*

If any one will take the trouble to investigate the symptoms said to mark acute hepatitis he will find fever, thirst, &c., intense pain increased on pressure over the hepatic region, and pain in the right shoulder. Still farther, if he will take the trouble to find in what classes the complaint is most common, he will find it amongst the artillery drivers, who ride on the left horse of a pair, and guide the other by the whip, and are necessarily obliged when wheeling to the left to bend their bodies to the right. He will find still farther, that the affection usually comes on after a heavy field day, when the men have been for hours in the saddle. The army surgeons have read all this as illustrating the history of hepatitis—nothing of the sort. The work has produced intense myalgia of the right side, and of the deltoid muscle which worked the whip ("voila tout"). This assertion of course is startling and requires proof, my friend has given me an account of all the cases he has had, and treated according to this view, and each one of them has recovered completely, by a few days rest, alone.

He has told me of other cases in which acute peritonitis or enteritis has been diagnosed, in which the whole of the symptoms have been due to long riding on camel back, or on horses peculiarly uneasy, or from long and rapid running, jumping or other unusual gymnastics, and I may note here, not only that I have met with similar mistakes in England, but that I have also seen them under the treatment adopted, run on to real peritonitis and enteritis.

He has told me of cases treated as acute inflammation of the kidney in which the real complaint has been intense myalgia, and possibly, myositis of the "longissimi dorsi," and of many others of a similar nature, the heart at one time, the lungs at another being the presumed seat of the disease. To any one familiar with muscular pathology, it must be at once apparent,

* Of course, there are many cases of abscess in the liver in dysentery, but these are not attended, during their formation, by those signs said to be characteristic of hepatitis.

that a tendency to myalgia implies a weak heart, consequently, he would expect to find that death from faintness or angina was a common occurrence on forced marches in hot weather, such is the case, and many a man falls down and dies, as it is said of sun-stroke, whose heart has simply been overtaken.

The practical results arising from the adoption of the above sensible and simple observations are so apparent, that I need not dwell upon them.

In the next place my friend very judiciously remarks, that in the climate of India there is a tendency to death by asthenia; consequently, all those medicines which have the effect of lowering the vital powers do far more harm in the hot countries than they do in England, and that many a death and shattered constitution are due more to extreme medication than to disease: and I can well understand this, for before he left our shores my friend had abundant evidence of myalgia in lying-in women, who had got up too soon after confinement, having been mistaken for peritonitis and treated accordingly with fatal results.

I would like to have the power of sweeping away tartar emetic and calomel from the medicine chest of the Indian Armies, as the physician general to the American forces did for his countrymen in the late wars, for I feel certain when the change was effected, that India would retain in her soil much less of Britain's bone and blood.

Another point to which my friend has called my attention is, the frequency with which fever and ague is brought on by exposing the body to sudden and violent alternations from heat to cold. He describes the change as being the most intense in marshy districts where between "sun up" and "sundown" the variation of the thermometer is some fifty degrees or more.

His first attack of fever was determined by visiting a house kept cool by all sorts of Indian appliances, after having had a rapid ride in the hottest part of the day.

I do not remember having yet seen this account given of the cause of certain fevers, but his observations to me have sufficed to demonstrate its truthfulness.

The effects of this would naturally be, to induce officers and men to shun a rapid change from a hot parade to artificially cooled rooms, and generally to avoid such extremes. It is not apparently the cold which does the mischief, but the suddenness of the change. Such a change in the wounded we have long known to be productive of lockjaw.

Entertaining these views, it is evident that my friend must wait a long time ere he ventures to enunciate them, I am influenced by no fears of "authorities," I write boldly what I believe firmly, and I trust that there are to be found some, high up in Indian Service, who will be found sufficiently liberal to put these ideas to the test.



“HEART DISEASE,”

ITS DIAGNOSIS AND TREATMENT;

A CLINICAL LECTURE,

DELIVERED BY

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MEDICAL WORKS BY DR. LYONS.

The following are some of DR. LYONS' Contributions to Medical Literature:—

- I.—“RESEARCHES ON THE PRIMARY STAGES OF HISTOGENESIS AND HISTOLYSIS.” (A Physiological and Pathological Inquiry into the processes of Growth and Development, Decay and Dissolution.) *Proceedings of Royal Irish Academy*, Vol. V. Part 3. 1853.
- II.—“ANNALS OF MICROLOGY,” (Anatomical, Physiological and Pathological.) A serial publication, of which Part I. appeared in the *British and Foreign Medico-Chirurgical Review*, April, 1853.
- III.—“A RETROSPECT OF THE PROGRESS OF MICROSCOPIC INVESTIGATION, &c.” *Dublin Quarterly Journal of Medicine*, August, 1850; (Reprinted in extenso in the *Philadelphia Medical Examiner*).
- IV.—“REVIEW OF RECENT RESEARCHES IN NORMAL AND PATHOLOGICAL HISTOLOGY.” *Dublin Quarterly Journal of Medicine*, May and August, 1852.
- V.—“OBSERVATIONS ON CIRRHOSIS OF THE LIVER WITHOUT ASCITES, and on the establishment of collateral Venous Circulation from the Vena Porta, through the Azygos and Rachidian systems.” *Dublin Medical Press*, December, 1849.
- VI.—“ON THE MOTIONS AND SOUNDS OF ANEURISM, the Mechanism of their production, and their diagnostic value.” *Dublin Quarterly Journal*, May, 1850. Translated in full in *The Archives Generales de Medicine*, August, 1850; abstracted in *Revue Medicale de Paris*; reported by VON LOBEL in *Comatatt's Jahrbuch*, 1851; *Boletin de Medicina*, No. 5, Madrid, 1851; also numerous English Journals.
- VII.—“AN APOLOGY FOR THE MICROSCOPE.” Introductory Lecture to the First Course of Microscopic Anatomy and Pathology ever delivered in Ireland 8vo. pp. 45. Dublin: Fannin and Co., 1851.

- VIII.—VARIOUS CONTRIBUTIONS to the Dublin Pathological Society.
- IX.—ESSAYS AND REVIEWS in *Dublin Quarterly Journal of Medicine, British and Foreign Medical-Chirurgical Review, &c., &c.*
- X.—“ON CERTAIN FORMS OF URINARY DISEASE.” *Dublin Quarterly Journal of Medicine.*
- XI.—INAUGURAL ADDRESS on the opening of the Medical Session of the Catholic University, 1856.
- XII.—“ON THE CASE OF M. GROUX; with Observations on the Motions and Sounds of the Human Heart.” Dublin, 1858.
- XIII.—VARIOUS CONTRIBUTIONS TO THE ATLANTIS.
- XIV.—COLLECTED ESSAYS AND REVIEWS on various subjects in Medicine and Pathology.
- XV.—“REPORT ON THE PATHOLOGY OF THE DISEASES OF THE ARMY IN THE EAST—CRIMEAN CAMPAIGN.” Printed for the War Office, London, 1856; and afterwards, on special motion of the House of Commons, presented to both Houses of Parliament by command of Her Majesty. *Blue Book. Folio. London, 1856-1857.*
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- XVII.—“HANDBOOK OF HOSPITAL PRACTICE.” 8vo. London, Longman, & Co. 1859.
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“HEART DISEASE,”
A CLINICAL LECTURE,

BY
DR. LYONS.

GENTLEMEN,

I wish to-day to engage your attention on the subject of “Heart Disease.” It is one fraught with alarm as well as interest to the public as well as the professional mind.

I am anxious to put before you a few general considerations, and some deductions of great practical importance which may be drawn from the study of the very numerous cases of Disease of the Heart which you have lately seen in the Hospital wards and at the Dispensary. These cases, with the numerous drawings, and the very ample series of pathological specimens I have exhibited to you, illustrate almost every possible stage and variety of cardiac affections. Some of these cases now under treatment are of recent origin, and still in the acute stage; some, on the other hand, are of old date. Some of them, as in the case of the poor fellow G. are attended with much suffering, great disturbance of the equilibrium of the circulation, and secondary lesions in vital organs, such as the liver and lungs. In others the heart acts tranquilly, the balance of the circulation is little affected, a comparative state of health is maintained, and the patient goes through the ordinary

avocations of life with but little and only occasional distress from the Heart, and the great organs in physiological connexion with it.

Our cases in Hospital, in conjunction with those attending the Dispensary, illustrate, in the most characteristic and instructive manner, the clinical history and pathology of the Weak Fatty Heart, the Nervous and Irritable Heart, the Dilated and Hypertrophied Heart, the Lesions of the Aortic Valves of several kinds, Lesions of the Mitral Valves, and Lesions of the Aorta itself.

In some of these cases we have the stereotyped signs and symptoms, which you will find detailed in the text-books, fully developed. In others there are features of a somewhat novel or unusual character, which I think may be studied with great interest and practical advantage.

We have studied the history, signs, symptoms, and pathology of these several cases in detail at the bed-side, and in the consultation room at the Dispensary. Let us now, for a moment, group them together, and see what conclusions of a general kind we may deduce from them.

In the first place, it will, I am sure, strike you as remarkable, when I point it out to you, that however various the actual state of health in these numerous cases of Heart Disease, and however they differ as to the amount of suffering entailed, *not one of them is dying*; indeed, with one exception, I think I may go so far as to say, that *not one of them is in much danger of dying for a considerable time to come*.

Some of the worst of these cases—or, I should rather say, with the stethoscopic signs, and the general symptoms usually supposed to indicate the most advanced and the worst forms of Heart Disease, are amongst those who attend the Dispensary. They still pursue their daily avocations, under all the deleterious influences of exposure to the weather, and the privations incident to scanty means or actual poverty. And as they return again and again to us, their now familiar faces, illumined by a cheerful smile, called up by a kind word or a friendly greeting from "his honor the Doctor," tell of a patience under suffering, and a Christian fortitude (and, I may add, a gratitude), that you will rarely see equalled,

never surpassed in the better ranks of life. Indeed, it is known to the Catholic clergyman, and the medical practitioner, better than to any others, how pre-eminent a virtue of our Irish poor is that full resignation to the chastening hand of God, and that patient tolerance of great suffering so often entailed in chronic disease, never under a more trying aspect than in Heart Disease.

Some of the cases now under treatment are, as we before said, of old date; some trace their origin to a period seven years back, others so far as ten years. I could cite you instances of Heart Disease of still longer standing, from my own experience in private practice.

Some of them that are of longest standing and with the most marked signs and loudest *bruits*, exhibit the least disturbance in the equilibrium of the circulation, the least distress referrible to the heart itself, and show a state of integrity of the general system, which is, at first view, quite surprising. All this, however, does not occur by haphazard, for all disease, and Heart Disease in an especial manner, as I think I shall be fully able to show you, exhibits an adaptation of means to ends, and a proportion of results to causes indicative of the design of a ruling hand.

Will it be too much to anticipate that, when the science of cardiac diagnosis is more fully advanced, we shall be able to estimate with something approaching to exactness, the probable duration of life in any given case of heart affection that comes before us? I for one, think not, and indeed I would venture to assert that, with full consideration of all the aids even now in our power, we ought to be able to distinguish those cases which admit of the hope of a moderate degree of longevity, from those in which a more unfavourable prognosis must be held. From the cases now before us we may, I think, draw the following deductions:—

- 1st. That advanced disease of the heart is quite compatible with the continuance of life for a more or less protracted period.
- 2nd. That advanced disease of the heart is quite compatible with a moderate and even very satisfactory degree of general bodily health, the mental and physical faculties, and the functions of the great organs being maintained in almost perfect integrity.

As a corollary to the foregoing, and also as a deduction based on a pretty wide range of experience in Heart Disease, I shall here add another proposition of equal and, in certain moral respects, of even superior importance, viz. :—

3rd. That, with certain exceptions, Heart Disease does not terminate life suddenly; that, on the contrary, in the great majority of instances, death ensues in Heart Disease after the lapse of many months, and it may be of many years, as a slow process.

I am anxious that you should enter on professional practice with minds disabused of the vulgar error, and I am sorry to say it is not confined to the public, that Heart Disease is at all likely, in any considerable number of cases, to end suddenly. *Sudden death is rare in Heart Disease.* Impress this on your own minds, impress it on your patients, and it will gradually find its way to the general public. You will thus be instrumental in removing a source of morbid apprehension, mental suffering, and paralyzing fears, which I hardly know to be so intimately, and, at the same time, erroneously associated with any other form of disease however appalling.

You can now, perhaps, hardly realise the importance of much of what I am endeavouring to impress upon you. But it will, I trust, come back upon your memory, when, in after life, you are awakened to a full sense of the cares and responsibilities of professional practice, by having thrust upon you the weighty duties of medical attendance on some highly prized, cherished, and valuable life; the hope or the stay of a large domestic circle, who hang upon your lips for the verdict that is, as they conceive, to doom or to spare the loved mother, the fond father, or the darling child.

Many of these contingencies we can illustrate for you from the life, from the cases that daily pass before your eyes in the Hospital and in our Dispensary consultation room. But what I find hardest is to impress upon you all the necessity of weighing the chances for the duration of life, and the other elements of the great life-problems that are being worked out before you. You are all eager enough to detect some *bruit de soufflet* of unusual intensity, and a *bruit de râpe* or a *bruit de scie*, is sure to awaken vigorous curiosity.

An aneurism of the aorta with its terrific sledge-hammer pulsations collects a wondering and intensely interested crowd. But observations on these and similar cases of advanced and hopeless disease are of far less consequence to you than inquiries of the kind I am now directing your attention to. In the one case the great drama of life is at its close, diagnosis presents no difficulties; the signs are such that "he that runneth may read." Our prognosis is necessarily a fatal one; *actum est* must be our verdict, and therapeutic skill is at fault, or, at best, can but momentarily palliate.

Let me then warn you again, as I often have done before, against running after the rare and the singular in disease, while you look on with *insouciant* airs and uninterested minds, when we are endeavouring to teach you the pathology, the diagnosis, and the treatment of the ordinary class of cases which will form the staple of your daily practice in after life; and in connexion with which your own success may be made or marred, by what you learn or neglect to learn here to-day or to-morrow.

One of our Dispensary cases exemplifies, in so striking a manner, the principles involved in our first and second propositions, that I think it will be useful to consider its features somewhat in detail. You all saw, and several of you examined the case of Mrs. M., amongst our out patients, on yesterday. She is a *healthy-looking*, fresh-coloured woman, of middle stature, comfortably clad, the wife of an artisan, in moderately easy circumstances for his position in life. Now mark, this woman sought advice for a trifling derangement of the stomach, with foul tongue, and of recent origin. She made no complaint or reference whatever to her heart, till she perceived, after she had explained her gastric symptoms, that I did not identify her. I had, however, recognised her face as one perfectly familiar to me. She then mentioned her name, and the fact of her having been under my care for "Heart Disease," two years and a half previously. This brought her case, with its history, symptoms, and pathology, to my mind, in all its vividness. Fresh inquiry elicited a repetition of her history, to the effect that some seven years since she had an attack of rheumatic fever, subsequent to which she com-

plained of heart affection. At the period of my last seeing her, two and a half years since, she went to the country for change of air, by my advice, and, she has since not only enjoyed very fair health, but has borne another child, now just weaned. She is perfectly aware that she has "the Disease of the Heart," as they generally term all cardiac affections. She suffers, but very slightly, and that only occasionally. Now let us see what are the stethoscopic signs in this case. We examined her, as you yourselves witnessed, with much care and interest, and many of you can confirm the observations then made. The pulse at the wrist was regular, and in no way remarkable for excess or deficiency of force or volume. The cardiac impulse is full and sensible over an increased surface of moderate extent; the præcordial dullness is likewise increased in extent to a moderate degree. On applying the stethoscope a very loud whirring murmur is audible, with the first sound of the heart, or rather replacing or drowning it. This murmur is very intense, much louder than in any of the other cases at present under observation. It is audible from the apex to the base of the heart; it is propagated up the line of the aorta, and is heard at the root of the neck and in the carotid arteries with great intensity; it can be traced as a very loud sound, and still of a whirring character in the descending thoracic aorta; it is audible between the scapulae and over the last dorsal vertebra; it is heard almost with the same intensity in the lumbar region; and it is perfectly audible and distinct when the stethoscope is carried to the lower end of the sacrum. And bear in mind, that in this case our examination was made through the numerous and thick folds of a female's dress. I have no doubt that the murmur is audible in this patient in the iliac and the femoral arteries, and perhaps, through the whole arterial tree. Now, note again the remarkable features of this interesting and instructive case. The patient makes no complaint of heart symptoms; she has for two years and a half enjoyed average health, with freedom from cardiac distress, except at rare intervals, and she then suffered but little. There is little, if any, disturbance of the equilibrium of the circulation. Not only have the functions generally, been performed with every indication

of integrity on the part of the great organs, but she has safely gone through that most wonderful of God's operations performed in the human economy, viz., that of child-bearing and the subsequent process of suckling her child.

Taking it all in all, you will rarely meet with a case more full of interest and instruction.

From the full exposition we have already given you of the principles of diagnosis applied to cardiac disease, you will, I trust, be able readily to understand the further observations we shall have to make in reference to this case.

But first of all, you will naturally ask, what is your diagnosis in this case? Well, my answer is—I regard it as an instance of obstructive disease at the mouth of the aorta, the result of vegetative growth on the semi-lunar valves, which offers a certain amount of impediment to the egress of the blood from the ventricle into the aorta, by which it is thrown into vibrations, and so the murmur is produced synchronously with the first sound of the heart. But observe, the second sound of the heart is, in this case, perfectly pure, clear, and distinct. From this we infer, that the lesion of the valves is such, that while it impedes the outflow of the blood from the ventricles, it in no way interferes with the integrity of the aortic valves, and they are, therefore as fully able as in the state of health to close the aortic orifice, and so prevent regurgitation from the aorta into the ventricle.

But we have in this case something special in the cardiac mechanism, under its new conditions of disease.

Compare for a moment the case of Mrs. M. with that of poor G. In the latter instance we have great cardiac suffering, the equilibrium of the circulation is profoundly impaired, the lungs and liver suffer in consequence, there is much œdema, general debility, and incapacity for exercise, not to say manual labour or active avocation of any kind. And yet the case of G. presents essentially the same stethoscopic phenomena. There is but a single murmur; it is systolic or audible with the first cardiac sound; it is propagated up the aorta. It is not, to be sure, of anything like the same intensity as that in Mrs. M.; it is not propagated one-tenth of the

distance; it has not the same whirring character. But all this, if we confined our attention to local and stethoscopic signs, would only mislead us; for we should be led to imagine that Mrs. M's was far the worse case of the two. Whereas, in practical reality, Mrs. M's state of health is one of comparative ease; poor G's is one of much suffering, and our prognosis must be of the most unfavourable kind, and implies a speedy issue in death. They are both cases of *non-regurgitant* obstructive disease of the aortic valves. Wherein do they so essentially differ? Are we to assume that it is a mere freak of disease, that in one there is tolerable health over a period of at least two years and a half, and this compatible with child-bearing and nursing; while in the other, the debility is extreme and the suffering great? No, gentlemen, it is not a freak of nature or disease; this is a term of a by-gone day, and implies only insufficiently or unsuccessfully explored causes.

Review with us the numerous morbid specimens I shall put before you, and I think I can satisfy you that there exist two very distinct forms of non-regurgitant obstructive disease at the mouth of the aorta. (The various proofs from examination and comparison of specimens cannot be given in print in the absence of illustrations.) In one of these forms you may observe that the aorta is constricted at its mouth, and the vegetations are so placed as to present a very serious obstacle and impediment to the egress of the blood from the ventricle. We shall call this variety obstructive aortic valve disease, with narrowing of the mouth of the aorta—for brevity sake we shall call it "obstructive stenosis." It is well exemplified in this specimen; it is what exists in the case of G. It is necessarily attended with great disturbance of the equilibrium of the circulation, and a permanently incomplete and ineffective circulation of blood. The lungs, the liver, and the other organs so directly implicated when the balance of the circulation is destroyed in the heart's chambers, become necessarily diseased in this form of valvular lesion.

Now contrast these other specimens. We find here obstructive disease, it is true, in the aortic valves, but though the vegetative masses are considerable, the mouth of the aorta is larger than

natural, and though the warty vegetations obstruct the blood as it flows from the ventricle, still the vessel is so capacious that no serious impediment to the egress of the blood takes place. The impediment is sufficient to throw the blood into vibrations, which cause systolic murmur, but the circulation is efficiently maintained, for the great arterial tube readily admits of the free passage of the blood through it. This latter condition we assume to exist in the case of Mrs. M. We may designate it obstructive disease at the mouth of the aorta, with a patulous, or it may be a dilated state of the vessel. We may, for brevity sake, designate it "obstructive patency." There is one difference in the signs in these two cases, to which I have not yet called your attention, and I presume it will hold for the two classes of cases. It is the condition of the radial pulse. In the one case, that of stenosis, it is a small weak pulse, that of an imperfect circulation. In the other case, that of Mrs. M. the pulse is natural in force and volume; it is the pulse of a vessel receiving its full charge of blood at each stroke of the heart. In fine, in the one case we have a deficient and wholly inadequate circulation; in the other the circulation is well and fully sustained, and quite as much blood finds its way at each stroke of the ventricles into the aorta, and thence into the peripheral vessels as in health, *malgre* the vibration given to it at the mouth of the aorta, as it issues from the ventricles by the vegetations aforesaid.

I have elsewhere taken an opportunity of showing you how the considerations to be derived from a study of the states of patency and stenosis apply to Mitral as well as Aortic Valve Disease. In my mind they give us a new light to aid in the explanation of the apparent anomalies and paradoxes which we have been accustomed to associate with our ideas of cases, bearing the same technical designation, but differing as widely in practical result to the patient as the cases of Mrs. M. and poor G. are seen to do.

In conclusion, I may call your attention, in a very brief and summary way, to the results of the various plans of treatment which you have had an opportunity of seeing carried out in detail at the bedside, and in our Dispensary practice. You may have

remarked how we have abandoned all the torturing methods of treatment, comprising setons, issues, cupping, mercurialization, starvation on bread and water diet, or rice and milk of a past day. Yet the case of Mrs. M. is one out of many, which I think very satisfactorily show that our milder plans, in one sense, and more liberal treatment in another, are not without practical benefit.

In our next Lecture I will call your attention to another class of cases of a most remarkable kind. They prove the possibility of the heart being restored to perfectly healthy action after years of protracted suffering. In one instance, a gentleman who, for five years, laboured under Heart Disease in one of its most aggravated forms, now enjoys perfect health, and has been accepted as a first-class life by three Insurance Companies.

8, MERRION-SQUARE, WEST,
DUBLIN, March, 1862.

Brief Extracts from the Notices of Dr. LYONS's Works in various Scientific and other Periodicals.

ESSAY ON THE DIAGNOSIS OF ANEURISM.—This Essay on an obscure and important form of Disease of the Chest was published in 1849, and immediately commanded attention. It was translated *in extenso* by the late eminent Physician, M. ARAS, of Paris, in *Les Archives G n rales de M decine*, August, 1850; abstracted in the *Revue M dicale de Paris*; reported by VOX LOEVI, in *Cunstat's Jahresbericht*, 1851; also in the *Bolet n de M dicina*, Madrid; and besides being noticed in various other standard periodicals, is referred to as an authoritative work in his "Treatise on the Practice of Medicine," by Dr. WOOD, an American writer of the greatest eminence.—See *Wood's Practice of Medicine*, Philadelphia, 1855, pages 227, 228, 229. This Essay is likewise quoted at large in Dr. STOKES' work *On the Disease of the Heart*, pp. 545, 608, 620, 621.

REPORT ON THE PATHOLOGY OF THE DISEASES OF THE ARMY IN THE EAST, first printed by the War Office, and afterwards presented to both Houses of Parliament. This work became immediately the subject of general and extended notice in the public and professional organs. Two several *Leading Articles* were devoted to its consideration by *The Times*.

The *Daily News* concludes a Lengthened *Leading Article* on this work with the following observations:—"We trust, therefore, that the valuable information obtained by Dr. LYONS, and his earnest recommendations, endorsed as they are by the most intelligent Officers in the Crimea, and corroborated as they are by the experience of all military men, will not be forgotten."

The *Advertiser* sums up its opinions thus: "Dr. LYONS has prefixed a general r sum  of his proceedings and the results of his experience, which demand the attentive consideration of all who are interested in the efficiency and welfare of our soldiers. This Report, as a contribution to the history of the late War

in the East, cannot fail to be of advantage should there be unhappily a necessity for sending another army to the same part of the world."

The Northern Times, Liverpool, says: "The Report is the production of Dr. ROBERT D. LYONS, who, though still a young man, has long acquired a distinguished position in his profession in Dublin, and who, from his deep study of the pathology of diseases, was eminently qualified for the onerous and important task intrusted to him. * * * The Report treats at considerable length of various types of disease which manifested themselves in the army in the East. * * * It exhibits on the part of Dr. LYONS a thorough knowledge of the characters of disease, and we are convinced, that the more it is studied the more will its value and importance be appreciated."

The British and Foreign Medico-Chirurgical Review says: "The fruits which Science, and we trust Humanity, will gather from the experience of the late War are beginning to ripen. One of the first that will claim our attention is the Pathological Report drawn up by Dr. LYONS, a Civil Medical Officer, who was specially appointed by Lord PAMMERE in April, 1855."

The same journal, in a subsequent number, concludes a lengthened and elaborate critique of this work, with the following observations: "The Report is a gain to science, and will always be a document of interest and authority to those who study the medical history of the Crimean Campaign."

REPORT ON THE PATHOLOGY, THERAPEUTICS, AND GENERAL ETIOLOGY OF THE EPIDEMIC OF YELLOW FEVER, WHICH PREVAILED AT LISBON IN THE YEARS 1854, 1857. Presented to both Houses of Parliament by command of Her Majesty.

The British and Foreign Medico-Chirurgical Review speaks in the following terms of this work: "This is a valuable addition to the history of Yellow Fever, and the thanks of the profession are due to the author for it, especially considering the disinterested manner in which he entered on the inquiry, going to Portugal, not as a Government employ  but * s sponte* as an unpaid volunteer; and moreover, taking into account the zeal and energy with which he conducted it and carried it out. Another praiseworthy quality belonging to him that requires mention is, that he engaged in the study of the disease with a mind, as he assures us, unfettered by any predetermined conclusion as to its origin and propagation—a merit this the more commendable the more rare."

"We commenced our notice of this Monograph in terms of commendation—we cannot but reiterate them in conclusion, believing that the author made the most of his brief opportunity, thereby affording an example of how much may be accomplished when zeal is accompanied by ability."

The Dublin Quarterly Journal of Medicine observes—"The 'Blue Book' before us is, what those ponderous tomes frequently are not—a valuable contribution on the subject of its report. As might have been expected from its learned compiler, it brings before the Profession and the public, in a clear and intelligent form, an evidently truthful and comprehensive account of the fatal epidemic of Yellow Fever which attacked the inhabitants of Lisbon in the year 1857, and of which we had such conflicting accounts in the Newspaper Press of the day. The Profession will receive this 'Report' as fresh evidence of its author's zeal in the cause of medical investigation; and while they honour him, and feel proud of his philanthropic efforts, the public will also estimate the man whose 'mission,' as he tells us, 'to that city, on the occasion in question, was one voluntarily undertaken in the interests of science and humanity.' Dr. LYONS is already well known for his valuable researches as Pathologist-in-Chief to the British Army in the Crimea, and we have no hesitation in stating that his Report on the Pathology, Therapeutics, and General Etiology of the Yellow Fever at Lisbon is not calculated to diminish

his reputation as an accurate observer and sound pathologist. * * * In taking leave of this excellent 'Report,' we beg to express to Dr. Lyons our admiration of the zeal which led him to undertake the great labour, and the energy and perseverance which enabled him to complete the task. We thank him for this valuable contribution to medical science; and to the members of our Profession who take an interest in the Pathology and Etiology of Yellow Fever, we recommend this 'Report' as one well worthy of their earnest attention."

This work has been extensively noticed, in most flattering terms, by the general Press in these countries, and it has likewise received the highest commendation from individual authorities of great eminence. Professor Virchow, of Berlin, has cited it in terms of flattering praise. Professor Gairdner, of Edinburgh, writes to the author: "I feel much obliged to you for clearing up Yellow Fever, and some other obscure parts of the subject." Professor Parkes, of the Army Medical School, Chatham, himself a most distinguished medical writer, says: "Let me, at the same time, congratulate you on your Yellow Fever Blue Book, which I have gone through very closely. It is, indeed, an admirable performance, and will be a leading book on this subject."

LYONS ON FEVER.

8vo., pp. 473, Price 12s. 6d. London: Longman, Dublin: Fannin.


The British Medical Journal observes—"Dr. Lyons is well entitled to write a book on Fevers. His experience in Ireland, where Fevers are at all times so prevalent, coupled with that obtained as Pathologist-in-Chief to the British Army in the Crimea, and during his admirable investigation of the Yellow Fever lately prevalent at Lisbon, have afforded him rare opportunities for observation which his industry and talents have turned to good account."

"We have great pleasure in recommending Dr. Lyons' work on Fevers to the attention of the Profession. It is a work which cannot fail to enhance the author's previous well-earned reputation as a diligent, careful, and accurate observer."

The Dublin Quarterly Journal of Medicine remarks—"The work on Fevers, now before us, consists of *extracts* from a course of Lectures. He gives therein the results of a large experience in the observation of Fevers, beginning with Ireland. Unhappily, this country possesses the unenviable distinction of being the land of Fever; as the author graphically describes it:—"Typhus, chief habitat, Ireland." He had also, by his appointment as Pathologist-in-Chief to the British Army in the Crimea, possessed many opportunities for studying the 'febrile felicitus,' or war-typhus, both in the Camp before Sebastopol and in the hospitals at Scutari. Lastly, the author visited Lisbon, in 1857, to investigate the epidemic of Yellow Fever prevalent there, and has furnished us with important records, taken on the spot, of the pathological anatomy of this group of Fevers. He is thus able, from personal observation, to sketch vividly the lineaments of these different, yet similar, forms of disease. * * * We cannot, however, dismiss the present work without observing that it needs no laboured panegyric from us. To the zealous student of febrile diseases, we say—Go, buy it, and judge for yourselves. When you have mastered its contents you will possess no mean knowledge of fever."

Besides other Reviews and Notices of this Work, it has been introduced to scientific circles in France and Germany by highly laudatory critical Notices; in the "*Gazette Hebdomadaire*" and the "*Union Medicale*" in the French Capital; the "*Deutsche Klinik*," in Berlin; and the "*Medicinisches Wochenschrift*," in Vienna.

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NOTES

ON THE

PROGRESS OF ACUPRESSURE

BY

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UNIVERSITY OF EDINBURGH

"Tut, a pin!"—MASTER SHALLOW.

SECOND EDITION.

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ADAM AND CHARLES BLACK

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NOTES
ON THE PROGRESS OF
ACUPRESSURE

*Extracted from the LANCET for February 23, 1867, with
additional annotations.*

NOTES ON THE PROGRESS OF
ACUPRESSURE.

AFTER Ambrose Paré—three hundred years ago <sup>*Diligent long
and strongly
resisted.*</sup>—proposed to surgeons that they should “bid eternally adieu to all hot irons and cauterics” in arresting the hæmorrhage attendant upon amputations and surgical wounds, and use in their stead threads to the cut arteries, to stem the flow of blood, his suggestion met with little or no support during his own age or for many long years subsequently. During the sixteenth and seventeenth centuries surgeons still persevered in arresting the flow of blood after amputation and other wounds by charring over their raw and bleeding surfaces with red-hot irons, or by applying strong potential caustics. It is difficult for the mind to realise the ruthless agonies of the surgical sufferers of those times, under the dreadful double ordeal of cutting and burning—of knife and fire. “The horrors of the patient,” exclaims John Bell, “and

Ancient Surgery.

his ungovernable cries, the hurry of the operator and assistants, the sparkling of the irons, and the hissing of the blood against them, must have made terrible scenes; and surgery must, in those days, have been a horrid trade.* Dionis, writing a century and a half after the time of Paré, tells us that in the Hôtel Dieu of Paris,—in the chief hospital of the very city in which Paré himself had lived and practised,—caustics were still in general use for the arrestment of hæmorrhage after amputations. And in the middle of last century Mr. Sharp, of London, states, in his *Critical Inquiry into the Present State of Surgery in England*, that some English operators still employed the ligature sparingly, from their “horrid apprehension of compressing the nerves.” Speaking of the slow progress of the introduction of the ligature of arteries into England, the same author observes that—after being known to surgeons some hundred and fifty or two hundred years—it only “by degrees crept into practice” in this country.

Conservation of Surgery.

Knowing these painful, strange, and startling phenomena in the History of Surgery, in reference to the deligation of arteries, I had little or no hope, when—some seven years ago—I ventured to suggest the substitution of the pin or needle for

* See his *Principles of Surgery*, vol. i. p. 151.

the ligature, that I should live to witness the introduction of acupressure into general surgical practice anywhere,—though it seemed to my mind certain that if it was honestly adopted and carefully applied, it would be found greatly superior in many respects to deligation. I feel, therefore, some degree of pride in being allowed to forward to the columns of the *Lancet* the following extract from a letter which I received a few days ago from Dr. Keith, the well-known surgeon and celebrated lithotomist of Aberdeen—showing, with other facts, that acupressure had entered a new era, seeing it is now established as the rule of practice in one of our three Scottish Medical Schools. Before citing the letter, allow me to observe that the accomplished Professor of Surgery in the University of Aberdeen, Professor Pirrie, and Dr. Keith, have lately published a most excellent and masterly practical inquiry into the whole subject of acupressure, with careful and candid details of all their most important cases. The operations referred to in the following incidental letter from Dr. Keith have occurred since the book was printed.

* *Acupressure, an Excellent Method of Arresting Surgical Hemorrhage, and of Accelerating the Healing of Wounds.* London, Churchill and Sons, 1867.

Dr. Keith's letter.

... "Acupressure," writes Dr. Keith, "here is *triumphant*—nothing else being dreamt of—and, I may say, in daily use. Within the last fourteen days I have employed it in the following operations:—In an amputation of the forearm acupressure by first mode at the elbow commanded the humeral before bifurcation; and the wound healed by first intention. I removed a large recurrent fibroid tumour for the third time in ten years. Deep dissection brought me in contact with the external carotid artery. And what of that? It was looped by the sixth mode in three seconds; and the case has gone on to a wish, the wound filling up with healthy granulations, as all the integument, being adherent and discoloured, was intentionally removed; the mass was like one's fist. On Wednesday last I amputated a thigh. Three vessels by your fourth mode, and three by the sixth mode, were secured. On Monday of this week I amputated two thighs. In one four vessels required acupressure; in the other, six. The fourth and sixth modes* were used in about equal pro-

Modes of Acupressure.

* These numerical references apply to excellent descriptions and plates of the different methods of performing acupressure given in the treatise of Drs. Pirrie and Keith. In their practice they do not notice one method which I have often employed to close minor vessels, and which I have seen my friend Dr. Heron Watson, Surgeon to the Edinburgh

portions. The speed with which the vessels were closed in each case hindered the loss of blood so entirely that the sand-box only showed a spot of blood in its centre. . . .

"Yours very faithfully,

"WILLIAM KEITH.

"Aberdeen, 15th February 1867."

Dr. Keith's letter attests the small quantity of blood that is lost when acupressure is properly used. All experience proves that it is, at least, as safe and good a *primary* hæmostatic agent as de-

Royal Infirmary and to Chalmers' Hospital, use. It consists in transfixing with a pin or needle one or two lines of the soft tissue by the side of the bleeding artery, the pin being laid almost flat in doing so, and with its head towards the bottom of the wound. The head of the pin is then raised up till it is at right angles to the surface of the wound, and then further turned over till the head comes to the outer edge of the wound, and its point is turned to the bottom of the wound. It is easily fixed in this new position by being thrust forward a few lines into the tissues beyond. In its movement the head of the pin is thus turned round half a circle, and in doing so closes the bleeding orifice, and overlaps it with a small quantity of neighbouring tissue. A quarter of a circle turn is sometimes quite sufficient. This kind of "over-twist" is one of the simplest and most expeditious modes of acupressure for smaller vessels. But further practice may point out many other modes of simplifying the application of the needle.

Primary and secondary hæmorrhage.

ligation. But it has been objected that it might lead to *secondary* hæmorrhage when the needles were withdrawn. On this point Dr. Keith, in the treatise referred to (p. 188), observes, in relation to his cases, about forty in number:—"All have been alike successful. . . . In not one instance have I had hæmorrhage at or after the operation; the removal of the pin or needle or wire loop has never in any one case occasioned hæmorrhage. A drop or two of blood may have come—in one or two instances—from an abraded granulation, but that is all I have ever seen. The period, on the average, which I think sufficient for the acupressure to continue is forty-eight hours, though I feel certain a shorter period would suffice."*

Rarity of secondary hæmorrhage.

* In the treatise of Drs. Pirrie and Keith, in addition to Dr. Keith's 40 cases, 10 capital operations with acupressure are reported (p. 135) from the practice of Dr. Fiddes of the Aberdeen Hospital. Dr. Fiddes writes me that in none of these cases was there any secondary hæmorrhage. Dr. Pirrie has used acupressure in 32 of the major operations of surgery, and in many minor ones. In only one case did he meet with secondary, or rather with intermediary hæmorrhage. It was a case in which the leg was removed in its upper third for medullary cancer. There was not a drop of oozing after the operation. Early—or twenty-four hours afterwards—Dr. Pirrie "relieved the anterior tibial artery from acupressure, and it instantly bled as energetically as if it had been that moment cut across." The femoral artery was immediately compressed,

"The confidence of the Aberdeen surgeons," adds Dr. Keith, "is now so firmly established in acupressure that its use is the rule by the hospital staff" (p. 189). Three of the operative surgeons to the Aberdeen Hospital—Drs. Pirrie, Keith, and Fiddes—all now employ the needle in preference to the ligature in every wound where union seems at all attainable. I had the pleasure of meeting some time ago, at a consultation in Aberdeen, my friend Dr. Kerr, the fourth or remaining surgeon, and he ingenuously and frankly told me that—

the wound reopened, and the artery again acupressed. "The blood lost did not exceed a dessert-spoonful, and the whole proceedings did not occupy above four minutes." "This," adds Dr. Pirrie, "is the only instance in which I had ever seen any hæmorrhage from an artery on its being freed from acupressure, and I attributed its occurrence in this case to the extreme exhaustion of the patient being unfavourable for adhesion. The whole of the pins were removed in other forty-eight hours, without their removal being followed by a single drop of blood" (p. 112). In one of Dr. Keith's cases of amputation of the thigh, the patient, a boy of twelve years of age, "amusing himself with the pin-heads protruding at the angles of the wound," withdrew the pin acupressing the femoral artery *four hours* after it was applied; and yet no hæmorrhage resulted (p. 174).

* Dr. Keith describes an amputation of the thigh performed on 16th November 1864, and he adds that on this occasion, for the first time in the Aberdeen Hospital, ligatures were not laid out (p. 163).

Establishment of Acupressure at Aberdeen.

Secondary hæmorrhage.

Acupressure removed in four hours.

Ligatures not laid out.

Needle supplementing ligature.

though he had no objections to acupressure—he was too old to adopt such a revolution in practice. He added that, though he did not use acupressure, it had, shortly before my visit, been the means of saving a patient of his in secondary hæmorrhage after amputation. The amputation was one of the thigh, and hæmorrhage had supervened some two weeks or more after the operation, and after the ligatures were separated. It was a case, he thought, where, to stem the hæmorrhage, it would have been necessary to cut down and tie the femoral artery nearer the groin than the site of the amputation. But Dr. Fiddes, who saw the patient before Dr. Kerr arrived, stayed altogether the bleeding by using a mode of acupressure to the femoral artery; and there was no return of the hæmorrhage.

Advantages over deligation.

The great pathological and practical advantages which acupressure possesses over deligation have always appeared to me to be simple and decided. When an artery is closed by a needle, or by any other form of temporary metallic compression, the walls of the arterial tube are placed, and held together in apposition, by a material which does not tear their coats or irritate, like thread or silk, and that can be withdrawn in a day or two, at the will of the operator,—leaving subsequently

no extraneous body whatever in the wound to prevent its speedy and complete healing. On the contrary, when deligation is employed—(1.) The two middle coats of the artery are systematically lacerated; and (2.) Its external coat is strangulated. (3.) This strangulation leads on inevitably to ulceration or molecular destruction of the external coat at the constricted part, and causes mortification of the artery at the tied point, and usually also below it,—just as a strangulating thread kills and removes a hæmorrhoid, a polypus, or a wart. (4.) The small mortified point of the vessel becomes a dead and foreign body, and hence requires to be eliminated and thrown out of the living system by ulceration and suppuration,—so that there are as many dead sloughs, however minute, in each wound, and as many consequent points of ulceration and suppuration are set up—as there are arteries ligatured in the wound. (5.) Besides, the ligature-threads, if of silk or hemp, rapidly imbibe animal fluids, which speedily decompose in them; and then, the ligatures, like so many single-thread setons,* irritate the contiguous tissues; and at last they

* I have seen my friend Mr. Walker, the excellent *Single-thread Ophthalmic Surgeon* to the Infirmary of Edinburgh, employ *setons* with great success *setons of single threads* in the temple, where they soon excite abundance of irritation and suppuration.

Morbific effects of ligature.

only become—more or less slowly—set free by ulcerating entirely through the strangulated portions of the tied vessels. (6.) Hence arises the difficulty, or indeed the impossibility, of wounds containing a series of ligatured arteries ever uniting completely by the first intention; for such wounds are in the same condition as if diminutive morsels of dead flesh had been methodically and intentionally affixed with threads to their sides and walls. But other dangers follow. (7.) No surgeon would deliberately leave a patient after the shock of amputation in the neighbourhood of a foul sewer, or where decomposing animal effluvia could be inhaled. But a wound containing a series of dead, decomposing, putrefying sloughs—however minute these foul sloughs are individually—places the patient in perilous hygienic conditions; for morbid septic poisons can be imbibed by the open and absorbing surfaces of wounds, as well as inhaled by the lungs. "A poison," sagaciously remarked the late Mr. Travers, "admitted by a wound or raw surface, and a poison admitted by the lungs, are equally excitants of a specific constitutional irritation."^{*}

Practical effects of Acupressure.

Professor Pirrie, in a series of most important observations and cases, maintains (p. 137) that

^{*} See his *Inquiry concerning Constitutional Irritation*, p. 257.

acupressure "is not only the *easiest* of application, but the *quickest* method yet devised for arresting bleeding;" and a pupil of Dr. Pirrie's—Dr. Will—last week informed me that the rapidity with which Dr. Pirrie secures the open vessels after amputation, etc., by acupressure is "something wonderful,"—the movements of his fingers not being easily followed by the eyes of the attendants.

In advocating the practice of acupressure, I have always claimed for it two advantages*—viz.

First, That it would yet be found the quickest and easiest mode of arresting surgical hæmorrhage; and

Secondly, That its use, combined with the total absence of all surgical dressings,† would *accelerate* the healing of wounds.

On the first of these points the evidence of Drs. Pirrie, Keith, and others, may now be looked upon

^{*} See the inferences which I drew of the effects of the *First* proposed practice of acupressure, when I first communicated it to the Royal Society of Edinburgh, in the *Edinburgh Medical Journal* for January 1860, p. 650.!

† In my volume on *Acupressure*, p. 116, I have, for *No dressings*, example, stated that "I believe that after the sides and edges of a wound are properly approximated and adjusted with its metallic stitches, the best dressing, as a general rule, is—nothing, absolutely nothing," etc. See also p. 128, etc. etc.

Rapid healing of wounds. as comparatively complete. But what of the other question—

DOES IT ACCELERATE THE HEALING OF WOUNDS?

In his—the largest—portion of the conjoint treatise on Acupressure by himself and Dr. Keith, Professor Pirrie has described, with the most conscientious and scrupulous exactitude, all the special cases in which he used acupressure in capital operations; and I will try here to summarise the results obtained. To understand these RESULTS,—which are far more important than the mere facility and rapidity of the process,—let me first adduce the opinion which Dr. Pirrie holds as to what constitutes union by the first intention or by primary adhesion. In reference to this point Dr. Pirrie states: "The use of the ligature is attended with an insuperable obstacle to obtaining perfect examples either of immediate union or of union by primary adhesion without the formation of some pus. I have never allowed myself to call any case a perfect example of either of these two methods of healing where a *single drop of pus* was seen. Neither of these two methods of healing, in this sense, can be perfect in any case where the ligature is used. The immediate effects of the ligature—the changes by which its removal is ren-

dered possible, and its presence acting as a seton *Ligature prevents healing.* in the wound—render more or less suppuration at the points of deligation and in the tracks of the ligatures inevitable. I never saw, in the experience of any surgeon or in my own, where the ligature was used, a perfect example of either of the two desirable methods of healing without any suppuration. I believe such a case NEVER *was and NEVER will be seen*" (p. 138).

We see in this paragraph how strict Professor Pirrie's definition of union by the first intention *Union of wounds under Acupressure.* is, he considering no case a perfect example of it where "*a single drop of pus*" is observable. Yet in the course of his work he has described above a dozen cases of large surgical wounds, consisting of amputation of the limbs, excision of the mamma, removal of tumours, etc., where perfect and entire union by the first intention was obtained under acupressure, metallic sutures, complete rest of the wounded part, and the avoidance of all dressings; and he relates a series of other cases where the results were highly satisfactory, though the primary healing was *not* thoroughly and entirely perfect, because a small amount of suppuration was seen.*

* Take, for example, the first case in which Dr. Pirrie tried acupressure—namely, on the 16th of March 1864. The

Dr. Pirrie's Results.

Dr Pirrie tells us (p. 66) that he has used acupressure in thirty-two cases where the "major operations" of surgery were performed, besides employing it in "many minor ones." In every instance (he adds) "its use has been *most* satisfactory."

His first case of Acupressure.

patient was a boy six years of age; the malady, disease of the knee-joint; and the operation, amputation at the thigh. Four arteries required to be acupressed. The needles were removed in forty-eight hours, and no bleeding followed. "The wound," observes Dr. Pirrie, "healed by primary adhesion, but as there were a few drops of pus, I do not consider the case a perfect example of that mode of healing without the slightest appearance of purulent secretion. There could not have been more than six or eight drops of pus in all, as there was not the slightest stain upon the linen, except on two occasions, when the stains were not larger than half the size of a sixpence. This (adds Dr. Pirrie) was the first time I had ever employed acupressure, and the result produced a great impression on my mind, as it was the *nearest approach* I had ever seen to perfect primary adhesion after amputation of the thigh, or after amputation of any kind. But for these few drops of pus, I would have considered this case an instance of what—previous to the introduction of acupressure—I in vain longed to see—namely, an example of healing of an amputation-wound either by immediate union or the first intention, or by primary adhesion without a drop of pus. I have not (concludes Dr. Pirrie) called this a *perfect* specimen of primary adhesion, as I have never applied the term to the healing of a wound of any kind where a single drop of pus was seen, however gratifying the conditions in all other particulars may have been; and they could not have been more so than in this case" (pp. 66-68).

Dr. Pirrie's Results.

Out of these thirty-two major operations,* which he has given in detail—including seven amputations of the thigh†—fifteen cases, at least, healed entirely by the first intention, or *more than one*

* In all the thirty-two cases primary union could not be expected; as one of the cases, for instance, of excision of the diseased mamma, had a *ligature* applied by the House Surgeon to a vessel some hours after the operation, which had not bled at the time the acupressure-needles were used (p. 75). In another it was found impossible to bring the whole of the opposite lips of the wound into apposition (p. 118.) In a third instance acupressure was applied to restrain hemorrhage produced by sloughing (p. 96); and in a fourth instance, where a cartilaginous tumour of the head of the fibula was removed, a vacancy was left necessarily preventing primary union (p. 105).

† Out of these thirty-two capital operations in which Dr. Pirrie employed acupressure, only three of the patients died—a very small mortality. One died six weeks after amputation of the thigh from chronic bronchitis (p. 70). The second, who, when in a very weak state, had the same operation performed, sunk with symptoms of shock in thirty-six hours (p. 72). The third, after amputation of the leg, died of diarrhoea, under which he had suffered before the operation to such a degree that it had already brought him into "a state of great exhaustion" (p. 112). On the general high rate of mortality among patients subjected to operations, and particularly to the major operations of surgery, see the *Medical Times and Gazette* for April 23, 1859. Dr. Pirrie states that not a single instance of *pyæmia*, or surgical fever, had as yet been seen, either by his colleagues or by himself, in any case where acupressure was employed (p. 141).

*Dr. Pirrie's
Results.*

in three. Out of his first eight cases of acupressure, one only of the wounds was cured entirely by immediate union, and without a drop of pus. Out, however, of his last twenty-four cases, fourteen of the wounds were healed by the first intention, and without a drop of pus—*or more than one in two*—showing, as was to be naturally expected, that he became more successful in his results as he became more extensively and intimately acquainted with the practice of acupressure. To state these important facts in another form—

Among his first 8 cases 1 completely united—or 1 in 8.
Among his last 24 cases 14 completely united—above 1 in 2.

He had eight cases—among these thirty-two—of excision of the diseased mamma. In ~~five~~ ^{six} of these eight cases the resulting mammary wounds closed at once, and without a single drop of pus.*

*First successful
mammary
case.*

* A case of removal of a very large mammary tumour was the first instance in which Dr. Pirrie saw complete union under acupressure. Three vessels were secured and "relieved"—says Dr. Pirrie—"from acupressure in forty-eight hours, without the slightest appearance of blood, and the wound healed entirely by immediate union, or the first intention—a result which (he adds) I had, up to that time, seen in two other cases only of excision of the mamma, but in them the vessels were so small as to require no ligature. The wound healed without a single drop of pus, or the slightest appearance of exudation of any kind" (p. 75).

Has any surgeon, living or dead, ever, in using *Dr. Pirrie's Results.* deligation, met with such marvellous success as Dr. Pirrie has thus attained under the use of acupressure?

Could all the Hospitals of Great Britain, or of Europe, or of the World—if grouped and collected together—furnish within the same period of three years, as great a number of cases of large operative wounds, where the ligature was employed, healed entirely by the first intention, and without the appearance of "a single drop of pus," as the one little Hospital of Aberdeen?

And let us not forget—under this question—what an immensity of mental and bodily distress, discomfort, and even danger to health and life, is necessarily avoided and averted when surgical wounds are thus closed and healed in days, instead of weeks and months; and without those continuous dreaded dressings, and drainings of pus, which deligation usually, and almost inevitably, involves among its consequences.

In the way of illustrating these results let me adduce an individual case—a kind of evidence which, to some minds, is more impressive than any mode or amount of reasoning. For this purpose I might cite several, but I shall content myself with an abridgment of the last of the thirty-two cases

*Illustrative
case of Thigh
Amputation.*

which Dr. Pirrie details, having, for another purpose, already given (*foot-note*, p. 15) a similar abridgment of his first case. This 32d case was, like his 1st case, one of Amputation of the Thigh, on account of extensive disease of the knee-joint, in a "delicate-looking" boy, six years of age. The femoral and two other arteries were acupressed. The pins were withdrawn in forty-four hours, without, as the patient said, "causing any pain." "After operation" (says Dr. Pirrie) "I thought it necessary to caution the little fellow not to touch the acupressure-pins, which he called the pins with the beautiful [glass] heads; and I promised to give them to him after their removal. He took great care not to touch them, or allow any person except myself to do so; and, to his great delight, sent them home by his father for preservation until his return." The stump healed perfectly, and throughout every part, by the first intention; and, as Dr. Pirrie expresses it, "either by immediate union or by primary adhesion: and I am inclined to think it must have been by the former, as no medium of union is discernible at the edges of the wound." No dressings were employed except a few metallic sutures, and three strips of isinglass-plaster, which were applied before the patient was removed from the opera-

tion-table. The sutures were all removed on the fourth day. After the operation the little patient slept well—was anxious for his food—had no uneasy sensation of any kind, local or general—and was in the highest spirits. A few days after the amputation, when, observes Dr. Pirrie, "I entered the ward with the surgical pupils of the hospital, he was whistling most beautifully the 'Braes o' Mar,' and amusing himself by passing a little model carriage over an inclined plane he had got constructed in his bed. He continued to whistle the above and other airs till it was his turn to be visited!"

"This"—Dr. Pirrie further observes—"is one *Modern Surgery* of the many instances we have lately seen in the Aberdeen Hospital of a capital operation and its after-treatment having been made *delightful* to the patients as well as to the surgeon. By the aids of Chloroform, Acupressure, no dressings, and the most perfect immunity attainable from every movement of the stump, many of the hindrances to these gratifying surgical scenes are removed. Since the use of dressings was discontinued in the Aberdeen Hospital, patients have looked forward to the visit of their surgeon as the happiest event of the day, instead of, as formerly, dreading the visit, and thankful when it was over" (p. 132).

Modern Surgery.

And why should these "delightful" results not be sought for and attained for the poor patients in every other Surgical Hospital as well as in that of Aberdeen? "Can"—asks Dr. Keith (p. 189)—"can anything but prejudice oppose a trial, when *such* present and prospective advantages are PROVEN TO ACCRUE?"

Three Surgical Propositions.

Perhaps the whole question between deligation and acupressure, as far as the evidence of Dr. Pirrie's cases is concerned, may be briefly stated in the three following surgical propositions:—

- | | |
|--|---------------------------------|
| 1. The COMPLETE primary union of
all Operative Wounds is highly | } DESIRABLE* |
| 2. But in the Surgical Hospitals
where DELIGATION is used, it
occurs very rarely, or | } NEVER. |
| 3. While in the Aberdeen Hospital,
where ACUPRESSURE is used, it
has latterly occurred—unat-
tended by a single drop of pus
—in 15† out of 32 "major ope-
rations" performed by Professor
Pirrie, or in nearly | } ONE IN
EVERY TWO
CASES. |

Two great points in Surgery.

* "In Wounds and Operations (observes that distinguished surgeon John Bell) there are BUT TWO GREAT POINTS to be

In contrasting on one occasion acupressure with deligation, I ventured to remark that, while the ligature "generally requires two persons for its application," acupressure "requires only one person."† Let me cite an interesting piece of evidence on this point. Dr. Davidson, the very able and zealous Medical Missionary to Madagascar, and surgeon to the Royal Court of

attended to: *first*, the securing the arteries, so that the patient may be in no danger from bleeding; and *then* the procuring a *speedy adhesion*, by which the pain, suppuration, waste of substance, and all the other bad consequences of the wound, are prevented. Upon this principle we are able to perform things in the regular way of Surgery as surprising as those which passed for miracles in the times when the sympathetic cures were in vogue." (See Bell's *Principles of Surgery*, Edit. of 1826, vol. I. p. 40.)

† Out of these 15 cases, in 13 the wound healed without a drop of pus anywhere in it or its neighbourhood. In the 14th case (p. 83)—one of excision of a mammary tumour weighing 61 oz.—there were "two or three drops" of pus at the site of an overdrawn suture, "but none whatever in the wound." Notwithstanding the great size of the wound, it "furnished (says Dr. Pirrie) a perfect example of primary adhesion." In the 15th case (p. 114), the wound "furnished a perfect example of healing by primary adhesion;" but when about to leave the hospital the patient took small-pox, and "a few drops" of pus appeared in the wound, as elsewhere—that disease often, as is well known, attacking any new or weak part.

‡ See my work on *Acupressure* (1864), p. 449.

*Facts of some
Surgeons.*

that kingdom, is at present on a brief visit to this country. He has kindly furnished me with the following statement with regard to his experience of acupressure at Antananarivo, and under circumstances where, it is to be remembered, he had no educated professional assistant to aid him in his operations. I have myself been told by more than one English hospital surgeon that they were afraid even to try the use of the pin or needle instead of the ligature, and this too though they were surrounded by a staff of able and skilled professional assistants. Dr. Davidson's experience and success should surely serve as a sufficient answer to that weak argument against acupressure.

*Dr. Davidson's
letter on Acu-
pressure.*

"During the last three years and a half," writes Dr. Davidson to me, "I have used acupressure in about thirty cases, chiefly amputations, and also in accidental wounds, and after the removal of tumours. Although I have had no professional assistance in any of my operations, I have never had the slightest difficulty in applying acupressure. Indeed, the much greater ease and quickness with which acupressure is applied, as compared with the ligature, are to me, in my isolated position, very strong recommendations in favour of the former.

"I have never had a single case of secondary hæmorrhage where acupressure was used. The wounds have healed more quickly and with less suppuration than when the ligature was employed. The method which I usually employ is what is described as the third in your work. I usually withdraw the needles in forty-eight hours, and, with most surgeons, I feel much relief when I have thus the wound free of all foreign bodies. In several instances I have applied acupressure to the largest artery which is ever cut in operations—namely, the femoral. I have never seen any cases of pyæmia; but this disease is rare in Madagascar. From the comparative facility and security with which acupressure can be applied, I have no doubt that when I come back from Madagascar—some years hence—I shall find it in common use in some British hospitals where it is not thought of at present; for it only requires a fair trial of it to prove to any unprejudiced mind its superiority over the ligature.

"Yours very truly,

"ANDREW DAVIDSON.

"EDINBURGH, Feb. 16, 1867.

"P.S.—I have used chloroform in hundreds of cases, and it is always given upon a folded

*Dr. Davidson
on Chloroform
in Madag-
ascar.*

handkerchief by one of the native assistants, none of whom have ever received a professional education. People come forty and fifty miles to be operated on; and they always ask for this 'rannomatory' or 'sleeping water,' as they poetically term it, and it is as well known in the capital of Madagascar as in the capital of England.—A. D."

*Number of
Acupressure
at Aberdeen.*

In the work of Professor Pirrie and Dr. Keith, above eighty capital operations are reported as treated by them and Dr. Fiddes with acupressure. As this sheet was going to press, I received a note from Professor Pirrie, dated February 26, 1867, telling me that up to this time he had himself, in major operations, successfully applied acupressure to arteries "in 137 examples, occurring under every diversity of circumstances," besides using it in many minor operations. Eight times he has acupressed the femoral artery.

ZEITSCHRIFT

FÜR

RATIONELLE MEDICIN.

HERAUSGEGEBEN

VON

DR. J. HENLE,
Professor der Anatomie in Göttingen.

UND

DR. C. v. PFEUFER,
*Königl. Bair. Ober-Medicinalrath und Professor der speciellen Pathologie
und Therapie und der medicinischen Klinik in München.*

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Dr. H. WELCKER

1861



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*Presented to the Army Medical
Library
by the Authors
24/1/80.
E.B.*

Die Kohlensäurespannung im Blute als proportionales Maass des Umsatzes der kohlenstoffhaltigen Körper- und Nahrungsbestandtheile.

Von
Dr. E. Becher.
(Hierzu Taf. VI. VII.)

Je allgemeiner und fester begründet die Ueberzeugung wird, dass ein wesentlicher Fortschritt der medizinischen Wissenschaft nur dadurch möglich wird, dass, wie an die physiologische Formenlehre die Lehre von den Functionen, so an die so vollkommen ausgebildete pathologische Formenlehre die Erforschung der krankhaften Veränderungen der Functionen sich anschliesst, — um so mehr richtet sich die Aufmerksamkeit auch der Pathologen vor Allem auf die Reihe von Erscheinungen, welche die wahre Grundlage des ganzen thierischen Lebens bilden, und gemeinhin unter dem Namen des Stoffwechsels zusammengefasst werden.

Den natürlichsten Ausgangspunct für die Erforschung der Schwankungen und Verschiedenheiten des Stoffwechsels inner- und ausserhalb der Breite der Gesundheit bildet die qualitative und quantitative Untersuchung der Endprodukte, die als Auswurfstoffe aus dem Körper ausgeführt werden. — Da es in der Natur der Sache liegt, dass die bezüglichen Untersuchungen in grosser Zahl und Ausdehnung angestellt werden müssen, so ist das unentbehrlichste Erforderniss, der wichtigste Schritt zu ihrer Ermöglichung, die Ermittlung und Einführung sicherer und nicht gar zu komplizirter Untersuchungsmethoden. Kaum ist es den Bemühungen Liebig's gelungen, eine Methode zu Bestimmung des Harnstoffgehaltes des Harns anzubilden, die es möglich macht, jeden Augenblick binnen wenigen Minuten die ausgeschiedene Menge dieses Endproduktes der Umsetzung der stickstoffhaltigen Körper- und Nahrungsbestandtheile zu bestim-

men, so weitfern Physiologen und Pathologen in der Erforschung der Schwankungen dieser Ausscheidung im gesunden und kranken Körper, und von Tag zu Tag nehmen unsere Kenntnisse über diesen Gegenstand an Umfang und Tiefe zu.

Warum hält nun die physiologische Untersuchung über das nicht minder wichtige Endprodukt der Umsetzung der Kohlenstoffatome, die Kohlensäure, mit der ebengenannten nicht gleichen Schritt? Warum wendet die pathologische Untersuchung sich geradezu von dem Gegenstande ab, mit Worten, wie: „— die Chemiker haben das Gebiet dieser Untersuchungen, was ihnen bei den grossen Schwierigkeiten derselben auch nicht verdacht werden soll, noch nicht betreten —“ oder: „— man darf dem Arzte nicht zumuthen, die im Blute gelösten Gase unmittelbar quantitativ zu bestimmen —“ (Henle)? Warum beschränkt sich Traube auf die Untersuchung der dyspnöetischen Erscheinungen an den Bewegungen der Inspirationsmuskeln? Warum haben die vielversprechenden Anfänge von Scharling und Hannover, desgleichen die Bemühungen von Andral-Gavarret nicht weiter geführt? Warum verstummt sogar der eifrige Vierordt mit den Worten: „Von grösstem Interesse für eine höhere Auffassung der Vorgänge des kranken Lebens müssten Beobachtungen über die Respiration in verschiedenen Krankheiten sein. Im Verein mit den zahlreichen in neuester Zeit angestellten Untersuchungen über die Veränderungen des Blutes und Harns in Krankheiten würden sich daraus ohne Zweifel für die Physiologie und Pathologie wichtige Konsequenzen ergeben“?

Doch wohl aus keinem andern Grunde als wegen der Unbrauchbarkeit der bisherigen Methoden, für die eben hiedurch der faktische Beweis geliefert wird. Vierordt selbst sagt über die von ihm mit so vielem Erfolg angewandte Methode: „sie hat den Nachtheil, dass sie erst nach sehr langer Uebung mit Erfolg angewandt werden kann, wenn man nicht Gefahr laufen will, die Athembewegungen zu schnell und zu tief zu vollführen. Aus diesem Grunde ist sie auch zu an einer grössern Anzahl von Personen vorzunehmenden vergleichenden Experimenten durchaus unbrauchbar, was die vielen hinsichtlich der chemischen Beschaffenheit der ausgeathmeten Luft von den älteren Beobachtern begangenen Irrthümer beweisen.“

Eine einfache auf ein neues Prinzip gegründete Methode aufzusuchen und auszubilden war seit längerer Zeit der Gegenstand unserer Arbeit; nachdem uns diess so weit gelungen, dass wir eine solche als sicher begründet betrachten können, ist ihre Mittheilung der Zweck dieser Blätter.

Wir fasteten zu dem Ende die Spannung der Kohlensäure im Blute ins Auge; unter ihr verstehen wir aber die Kräfte, welche zwischen den Kohlensäuretheilen wirksam, die Verdunstung derselben bewerkstelligen. Die Gewinnung eines absoluten oder relativen Maasses war unser Bestreben. — Auf dem geradesten Wege vorschreitend, fanden wir ein solches Maass in dem Barometerdruck einer Kohlensäureatmosphäre, welche, wenn sie über das lebende Blut gesetzt wird, gerade genügt, um den Austritt der CO_2 aus dem Blute zu verhindern und doch nicht hinreicht, um neue CO_2 in das Blut zu treiben: wenn diese Bedingung erfüllt ist, so halten sich offenbar die Triebkräfte der beiden CO_2 -Atmosphären, in der Flüssigkeit und in der übergeschichteten Luft, das Gleichgewicht.

Die Beziehungen, welche der Werth einer solchen Spannung zu unserer Aufgabe besitzt, sind nun folgende. Nach den bekannten Regeln über die Verbreitung der Gase in Flüssigkeiten wird sich der Werth, den diese Spannung bei gleicher Temperatur und gleicher Zusammensetzung annimmt, mehren mit der Dichtigkeit des Gases, oder, was dasselbe sagen will, mit dem Procentgehalt der Flüssigkeit an Kohlensäure. Wir würden in Folge der Bestätigungen, welche die Henry und Dalton'schen Regeln durch die tadellose Arbeit Bunsen's erhalten haben, sogar im Stande sein, das Genauere dieses Abhängigkeitsverhältnisses zwischen Spannung und procentischen CO_2 -gehalt für das Blut abzuleiten, wenn dieses letztere die CO_2 nicht in einer besondern Form, in einer alkalischen Lösung, gebunden enthielte, für die wie wir uns durch den Versuch überzeugten, die Beziehungen keine volle Giltigkeit besitzen, welche zwischen Kohlensäure und Wasser bestehen. Jedenfalls wird es aber dem Versuch gelingen, das Abhängigkeitsverhältniss zwischen der Spannung und dem Procentgehalt des Blutes an CO_2 zu ermitteln, und wir behalten es uns vor, später darauf einzugehen. — Setzen wir nun aber voraus, es sei diese Lücke ausgefüllt, und wir würden, wenn uns der Werth der Spannung gegeben wäre, auch unmittelbar

den Gehalt des Blutes an verdunstbarer Kohlensäure kennen, so würde aus einer Bestimmung der erstern für den Arzt sogleich derselbe wichtige Vortheil entspringen, der ihm erwächst aus der Kenntniss eines jeden andern das Leben verändernden Einflusses, derselbe Vortheil, den ihm die Erkenntniss der Werthe des Blutdrucks, der Temperatur u. s. w. u. s. w. bietet. Denn es würde ihm, vorausgesetzt, dass jene Spannung mit dem Gange des Lebens veränderlich gefunden würde, möglich sein, Störungen des normalen Lebens als Funktionen der gesteigerten oder verringerten CO_2 Procente des Blutes aufzufassen. — Nun lag aber das Bedenken nahe, dass diese Spannungen und damit die procentigen Mengen der Kohlensäure in dem lebenden Blute keine wesentlichen Verschiedenheiten zeigen. Denn die Menge der Kohlensäure, welche das Blut schwängert, ist ja bekanntlich nur der Unterschied der Massen dieses Gases, welche auf der einen Seite in die Capillaren der Aorta ein- und auf der andern Seite, in den Capillaren der Art. pulmonalis, aus dem Blute weggeführt werden. Unsere Hoffnungen beruhen also auf der Voraussetzung, dass der eindringende Strom nicht zu derselben Zeit und in demselben Maasse verändert werde, wie der austretende. Dieses, was vor Allem festzustellen war, ist in der That durch die folgende Versuchsreihe bestätigt worden; der Spannungswerth der Kohlensäure im Blute ist eine veränderliche Grösse, und namentlich in der Weise, dass mit der steigenden Zufuhr an Kohlensäure auch ihre Spannung im Blute wächst. — Die Spannungsbestimmungen leisten demnach das, was wir vor Allem zum Vortheil der ärztlichen Praxis anstreben, nämlich: ein relatives Maass ebenso wohl für die Bildung als auch für die Ausfuhr der Kohlensäure. Denn man wird, ebenso wie es bei Temperaturbestimmungen u. s. w. geschieht, mit Hilfe ganz einfacher Beobachtungen, wie z. B. der Zahl der Athemzüge oder gar ihres Umfangs, des Zustandes der Lungen, der Menge von genossener Nahrung und Aehnlichem, jedesmal aus der bekannten Spannung schliessen können, ob die Kohlensäure vermehrt oder vermindert gebildet oder ausgeschieden werde.

Wir gehen nach diesen einleitenden Bemerkungen zum Inhalt unserer Versuche über. Es stellte sich, wie erwähnt, uns als nächste Aufgabe die Beantwortung der Frage, ob die Spannung der Kohlensäure im Blute variabel ist oder nicht?

Die Lösung derselben musste sich einfach ergeben, wenn wir untersuchten, ob bei konstantem Querschnitt des Kohlensäurestromes, bei konstantem Volum (und Zusammensetzung) der darüber gesetzten Luft die Menge der in gleichen Zeiten übergegangenen Kohlensäure wechselt, mit andern Worten: ob gleichgrosse Luftvolumina von demselben Individuum (nach vorausgegangener gleichtiefer Expiration) inspirirt, gleiche Zeiten zurückgehalten und darauf möglichst vollständig expirirt, in verschiedenen Zuständen denselben oder verschiedene Grade der Sättigung mit Kohlensäure erreichen?

Die absolute Grösse der jeweiligen Spannung der Kohlensäure im Blute lässt sich durch den Versuch natürlich erst dann bestimmen, wenn es gelingt, das jeweilige Maximum der Kohlensäurespannung in der Lungenluft zu erreichen, das derjenigen im Blute das Gleichgewicht hält und somit jede weitere Diffusion verhindert. Lässt sich dieses Maximum nicht erreichen, so sind wir doch mindestens sicher, eine Kurve der zu- und abnehmenden CO_2 Spannung für verschiedene Zustände zu erhalten, die als proportionaler Ausdruck nicht weniger werthvoll wird, wenn auch die absolute Höhe ihrer Ordinaten unbekannt bleibt.

Die Anforderungen, denen unser Versuch zu genügen hat, sind kurz folgende:

- a) Der von der Inspiration in den Lungen befindliche Rückstand von Luft muss nach Menge und Kohlensäuregehalt möglichst klein und für alle Versuche möglichst gleich sein. — Wir suchten diess dadurch zu erreichen, dass wir der Versuchsinspiration jedesmal eine möglichst tiefe Inspiration mit unmittelbar darauf folgender möglichst grosser Expiration vorausschickten.
- b) Das inspirirte Luftvolum soll bei allen Versuchen gleich gross sein. — Mit absoluter Genauigkeit kann diess nur geschehen, wenn ein abgemessenes Volum eingeathmet wird, annähernd aber lässt sich's am einfachsten dadurch erreichen, dass jedesmal der Brustkorb auf das Maximum ausgedehnt wird. Der letztere Weg empfiehlt sich auch mit Rücksicht darauf, dass je grösser im Allgemeinen die Inspiration gemacht wird, und je länger die eingeogene Luft somit zurückgehalten werden kann, um so grösser

die etwaigen Differenzen erscheinen müssen. Bei unsern bisherigen Versuchen haben wir den letzteren Weg, wie die Versuche zeigen, ohne erheblichen Nachtheil eingeschlagen, nichtsdestoweniger erscheinen uns die Gründe für das Einathmen genau gemessener Volumina so überwiegend, dass wir künftighin ausschliesslich so verfahren werden.

c) Die Dauer des Zurückhaltens der inspirirten Luft muss in allen Versuchen dieselbe sein. Die Bestimmung derselben betreffend, so war sie entweder so lange zu wählen, dass das jedesmal inspirirte Luftvolum das Maximum seiner Kohlensäuresättigung erreichen muss, oder da der (unten beschriebene) Versuch zeigte, dass diess bei grösstmöglicher Inspiration nicht zu erreichen ist, schien es zweckmässig, dieselbe so lange zu wählen, als ohne erhebliche Beschwerden für den Experimentator möglich ist. Wir haben demgemäss durchaus die Dauer von 60 Sekunden angenommen.

d) Die Expiration betreffend, so ist für die Messung des Luftvolums zwar eine in allen Fällen möglichst gleich vollständige Ausathmung nothwendig, für die Bestimmung des Prozentgehaltes an Kohlensäure aber dürfte diese vollkommen gleichgültig sein, da wir uns durch die genauesten Versuche überzeugt haben, dass am Ende der 60 Sekunden eine Verschiedenheit des Kohlensäuregehaltes der Luft in den verschiedenen Lungenpartien nicht mehr besteht. Um diese Gleichheit um so sicherer zu erreichen, haben wir uns stets bemüht, während der Inspirationsdauer durch Bewegungen des Brustkorbes, so weit sie noch möglich waren, die mechanische Mischung der Gase zu unterstützen.

e) Das Auffangen der eingeathmeten Luft und die Bestimmung ihres Kohlensäuregehaltes sind endlich von der grössten Wichtigkeit.

Ohne uns hier auf eine Kritik der verschiedenen von den bisherigen Beobachtern benützten, zum Theil sehr ungenauen Methoden einzulassen, begnügen wir uns, die Haupterfordernisse: Auffangung des Gases in einem Gefässe, dessen Rauminhalt genau bekannt, Sperrung durch eine Flüssigkeit, die keinerlei chemische oder physikalische Verbin-

dung mit keinem der Gase eingeht, Bestimmung der Kohlensäure nach dem Volum und nicht nach dem Gewichte — hervorzuheben, und gehen sofort zur Darstellung des von uns angewandten Verfahrens über.

Die einzig mögliche Flüssigkeit zur Sperrung des Gases ist anerkanntermassen das Quecksilber; seine allgemeine Anwendung wurde nur verhindert durch die Schwierigkeiten der Beschaffung und Handhabung desselben in so grossen Massen, wie sie bei den gewöhnlichen Gasometervorrichtungen erforderlich waren. — Um diesen Uebelständen so gut wie möglich auszuweichen, konstruirten wir den zuerst von Despretz angegebenen Apparat, welcher in Taf. VI. Fig. 1 leer, in Figur 2 mit Gas gefüllt dargestellt ist; er hat sich in jeder Beziehung vollständig zweckmässig erprobt. Auf ein mit Stellschrauben versehenes Brett (A) ist ein solider, an seiner Spitze abgerundeter Holzcylinder (B) aufgeschraubt; über denselben ist eine unten offene, oben tubulirte Glasglocke (C) gestürzt, die ihn so eng, als ohne gegenseitige Berührung möglich, umschliesst; sie trägt eine Millimeterskala und ihr Inhalt ist durch Kalibrirung genau bestimmt. Diese Glasglocke wird von einem Mantel von Sturzblech (D) umgeben, dessen umgebogener unterer Rand mit untergelegtem Kautschuck auf das Brett aufgeschraubt ist. Derselbe ist bei *b* auf beiden Seiten mit korrespondirenden Glasfenstern versehen. Der Raum zwischen diesem Mantel und dem Holzcylinder, der eben gross genug ist, damit die Glocke (B) zwischen beiden auf und ab bewegt werden kann, ohne die geringste Reibung zu erfahren, wird nunmehr mit Quecksilber angefüllt (durch die Wellenlinie in der Figur bezeichnet), und zwar so weit, dass der Cylinder A bei jeder Stellung der Glocke vollkommen bedeckt bleibt, und somit das Gas immer durch den ebenen Quecksilberspiegel (a) abgesperrt bleibt. Die fabelhafte Leichtigkeit, mit der das Quecksilber — zumal unter so hohem Drucke — durch unsichtbare Oeffnungen, selbst in die Poren des Holzes dringt, erschwert die Herstellung des Apparates beträchtlich. — Durch den Hals der Glocke geht eine gut eingekittete Glasröhre (d): dieser ist ein kurzes Kautschoukrohr mit einem Ansatzstücke (c) verbunden, welches letzteres durch einen Hahn oder Kork luftdicht verschlossen werden kann.

Die Anwendung dieses einfachen Apparates: Anfüllung

der Glocke mit Quecksilber durch Niederdrücken derselben und Ansaugen des Quecksilbers in das Ansatzrohr einerseits; andererseits Öffnen des Hahns und darauf folgendes Emporsteiigen der Glocke, so lange Luft oben eintritt, bedarf keiner weitem Auseinandersetzung. Ohne weiteres leuchtet auch der grosse Vortheil ein, dass er sich zur Inspiration so gut wie zur Expiration benützen lässt. Zur Bestimmung des Volums des eingeschlossenen Gases wird durch die Fenster (bb) das Quecksilberniveau innerhalb und ausserhalb der Glocke an dem Masstabe abgelesen, und in bekannter Weise nach der Kalibrirungstabelle das Volum berechnet.

Zur Bestimmung des Kohlensäuregehaltes der Ansathungsluft wandten wir die einzig gute Methode von Bunsen — Absorption der Kohlensäure durch befeuchtete Kalikugeln, und Bestimmung der Volumsabnahme — an; behufs derselben leiteten wir das Gas durch eine enge Glasröhre, die bei c vorgesteckt wurde, aus dem Gasbehälter in die mit Quecksilber gefüllten Eudiometerröhren. Wir setzen das Detail der Methode als bekannt voraus, verweisen bezüglich der zahlreichen Fehlerquellen auf das in der Abhandlung über Kohlensäurediffusion ausführlich Gesagte, müssen aber doch einiger besondern Vorsichtsmassregeln hier erwähnen: Die Expirationsluft mag für die jeweilige Körpertemperatur mit Wasserdampf gesättigt sein oder nicht, die in dem Gasbehälter befindliche Luft ist es unter allen Umständen für die Temperatur der Umgebung. Wenn aber diese Luft auf dem Wege in die Eudiometerröhre eine Abkühlung erfährt, oder wenn dieselbe in dieser Röhre bis zur Volumbestimmung eine höhere Temperatur angenommen hat, so könnte leicht der Fall eintreten, dass zur Zeit der Volumbestimmungen keine vollständige Sättigung mit Wassergas mehr Statt hat, und somit bei der Reduktion des gefundenen Volums ein zu grosser Abzug für die Tension des Wasserdampfs gemacht würde. Darum ist es notwendig für alle Fälle in den Kopf der Eudiometerröhre einen kleinen Tropfen Wasser zu bringen. Die Kalikugeln müssen immer frisch bereitet und gut befeuchtet eingebracht werden, und mindestens 24 Stunden liegen, indem sonst, wie wir uns durch zahlreiche Versuche überzeugten, keine vollständige Absorption bewirkt wird. Dass auch bei etwa reichlichem Wasserbeschlag in der

Röhre durch die Kalikugel dennoch das Gas vollständig getrocknet wird, davon haben wir uns durch nachträgliches Einbringen von Chlorkalcium überzeugt. Die jedesmalige Volumbestimmung muss mit äusserster Genauigkeit gemacht werden; ein Ablesungsfehler von $\frac{1}{10}$ Millimeter kann schon bis in die $\frac{1}{10}$ der Prozentzahl wirken (das Ablesen kann deshalb nur aus grösserer Entfernung mittelst des Fernrohres geschehen). Die geringsten Temperaturschwankungen werden so störend, dass ein Lokal mit möglichst konstanter Temperatur ein unumgängliches Erforderniss zur Anstellung der Analysen ist. So lange wir darauf nicht die allersorgfältigste Rücksicht genommen, war es uns in der That nicht möglich, unter 6 Volumbestimmungen einer und derselben Gasmenge auch nur zwei übereinstimmende zu erhalten; wird aber nach dieser Methode wirklich genau gearbeitet, so rücken die Fehlergrenzen in die dritte Stelle der Prozentzahl zurück.

Als Beispiel der Analysen, und als Masstab für den Grad ihrer Genauigkeit führen wir hier zunächst zwei mit der Luft einer und derselben Expiration angestellten Analysen aus unserem Versuchsprotokoll auf.

Eudiometer b.

	Inner. Hg.-Niv.	Aeus. Hg.-Niv.	Temperatur.	Barometer.
Vonder Absorption	22,3	8,8	17,2	737,8
Nach „	56,7	33,0	19,0	734,7

Daraus berechnet:

	Volum.	Temp.	Druck.	Corrig. Vol (0°; 1000 ^{mm}).
Vor d. Absorpt.	123,43	17,2	724,3	82,404
Nach „	113,32	19,0	711,0	75,325

Demnach enthalten 100 Vol. des Gases = 8,590 CO₂

Eudiometer c.

	Inner. Hg.-Niv.	Aeus. Hg.-Niv.	Temp.	Barom.
Vor d. Absorpt.	39,0	22,7	17,2	737,8
Nach „	74,0	44,6	19,0	734,7

Daraus berechnet:

	Vol.	Temp.	Druck.	Corr. Vol.
Vor d. Absorpt.	132,432	17,2	721,5	88,064
Nach „	122,072	19,0	705,3	80,491

Demnach enthalten 100 Vol. des Gases = 8,599 CO₂.

Weiter mögen hier drei Versuche, die zu derselben Stunde unmittelbar hinter einander angestellt wurden, ihre

Stelle finden, um als vollständiges Beispiel des ganzen Verfahrens, und als Probe für die Genauigkeit des Versuchs zu dienen.

$\frac{20}{10}$ Morgens nüchtern, 9 ^h	
Zahl der Pulsschläge in der Minute	72
Zahl der Athemzüge in der Minute	15
Temperatur auf dem Boden der Mundhöhle	36,2 ^o
Temperatur des Zimmers	10,5
Barometerhöhe	724,5 ^{mm}

Versuch I.

Tiefste Inspiration; 60" angehalten.
Volum der expirirten Luft = 4601,25 C. C.

Analyse. Eudiometer II.

	Inner. Hg.-Niv.	Aeuss. Hg.-Niv.	Temp.	Barom.
Vor d. Absorpt.	40,7	27,7	10,0	716,0
Nach, „	70,0	28,7	10,0	722,8

Daraus berechnet:

	Vol.	Temp.	Druck	Corrig. Vol.
Vor d. Absorpt.	125,275	10,0	703,0	101,980
Nach, „	144,950	10,0	681,5	95,291

Demnach enthalten 100 Vol. des Gases = 6,562 CO₂.

Versuch II.

Tiefste Inspiration; 60" angehalten.
Volum der expirirten Luft = 4564,0 C. C. *

Analyse. Eudiometer a.

	Inner. Hg.-Niv.	Aeuss. Hg.-Niv.	Temp.	Barom.
Vor d. Absorpt.	64,0	46,0	10,0	716,0
Nach, „	86,9	47,0	9,9	719,0

Daraus berechnet:

	Vol.	Temp.	Druck	Corrig. Vol.
Vor d. Absorpt.	125,03	10,0	698,0	83,052
Nach, „	118,25	9,9	679,1	77,493

Demnach enthalten 100 Vol. des Gases = 6,693 CO₂.

Versuch III.

Tiefste Inspiration; 60" angehalten.
Expirirtes Volum = 4527,25 C. C.

Analyse. Eudiometer II.

	Inner. Hg.-Niv.	Aeuss. Hg.-Niv.	Temp.	Barom.
Vor d. Absorpt.	74,9	49,0	10,0	716,0
Nach, „	104,0	50,0	9,9	719,0

Daraus berechnet:

	Vol.	Temp.	Druck	Corrig. Vol.
Vor d. Absorpt.	163,908	10,0	590,1	107,62
Nach, „	156,051	9,9	665,0	100,28

Demnach enthalten 100 Vol. des Gases = 6,80 CO₂.

Anmerkung. Die Unterschiede der in den drei Versuchen erhaltenen Kohlensäurewerthe erklären sich aus den Verschiedenheiten der In- und Expirationsvolumina.

Aus den mitgetheilten Probeversuchen geht hervor, dass die Differenzen im Prozentgehalte an CO₂ der die Expirationsluft in unsern Versuchen angiebt, höchstens bis zu 0,25 in die unvermeidlichen Fehlergrenzen fallen, grössere Verschiedenheiten aber zur Annahme einer wirklich stattgehabten Ausscheidung verschiedener Kohlensäuremengen berechtigen. Wie sich nach Auführung der bis jetzt von uns angestellten Versuche ergeben wird, kommt zu dem Beweise, den diese Probeversuche für die Genauigkeit liefern, noch hinzu, dass die Uebereinstimmung mit den bisher bekannten Thatsachen über die Kohlensäureausscheidung nur geeignet ist, das Zutrauen zu unserer Methode zu bestärken.

In der Absicht, einmal den Einfluss der Dauer des Zurückhaltens einer gewissen Luftmenge in den Lungen auf die Ausscheidung der Kohlensäure zur Anschauung zu bringen, hauptsächlich aber um zu erfahren, ob wir im Stande sind, durch möglichst langes Zurückhalten der eingeathmeten atmosphärischen Luft das früher besprochene Maximum der Kohlensäureausscheidung zu erreichen, stellen wir den zunächst mitzutheilenden Versuch an:

$\frac{12}{10}$ Vormittag, 9—10^h, nüchtern.

Ein möglichst grosses Luftvolum wird eingeathmet (Dauer der Inspiration: 2 bis 3") und der Reihe nach 0, 20, 40, 60, 80, 100 Sekunden zurückgehalten (Dauer der Ausathmung 6 bis 8").

Zahl der Pulsschläge in der Minute (P) = 56.

Zahl der Athemzüge in der Minute (R) = 12.

Temperatur in der Mundhöhle (T) = 36,0^o.

Temperatur des Zimmers (A) = 14,5.

Barometerhöhe (B) = 729,0.

Erster Versuch: Dauer des Anhaltens = 0".

Expirirtes Volum. = 4821,72 C. C.

Analyse. Eudiometer I.

	Vol.	Temp.	Druck.	Corrig. Vol.
Vor d. Absorpt.	152,975	14,0	704,2	100,72
Nach „ „	147,22	11,9	688,1	97,047
Demnach enthalten 100 Vol. des Gases = 3,636 % CO ₂ .				

Zweiter Versuch: Dauer des Anhaltens = 20".
Exspirirtes Volum. = 4564,25 C. C.

Analyse. Eudiometer a.

	Vol.	Temp.	Druck.	Corrig. Vol.
Vor d. Absorpt.	132,69	14,0	697,8	86,557
Nach „ „	125,43	11,9	680,2	82,751
Demnach enthalten 100 Vol. des Gases = 5,552 % CO ₂ .				

Dritter Versuch: Dauer des Anhaltens = 40".
Exspirirtes Volum. = 4656,75 C. C.

Analyse. Eudiometer c.

	Vol.	Temp.	Druck.	Corrig. Vol.
Vor d. Absorpt.	132,165	14,0	721,7	89,011
Nach „ „	123,226	12,0	706,9	83,434
Demnach enthalten 100 Vol. des Gases = 6,265 % CO ₂ .				

Vierter Versuch: Dauer des Anhaltens = 60".
Exspirirtes Volum. = 4471,75 C. C.

Analyse. Eudiometer II.

	Vol.	Temp.	Druck.	Corrig. Vol.
Vor d. Absorpt.	169,011	14,0	708,0	111,89
Nach „ „	159,561	11,9	679,3	103,86
Demnach enthalten 100 Vol. des Gases = 7,176 % CO ₂ .				

Fünfter Versuch: Dauer des Anhaltens = 80".
Exspirirtes Vol. = 4471,75 C. C.

Analyse. Eudiometer b.

	Volum.	Druck.	Temp.	Corrig. Vol.
Vor d. Absorpt.	112,793	689,0	14,0	72,632
Nach „ „	104,149	675,1	12,0	67,343
Demnach enthalten 100 Vol. des Gases = 7,282 % CO ₂ .				

Sechster Versuch: Dauer des Anhaltens = 100".
Exspirirtes Volum. = 4285,75 C. C.

Analyse. Eudiometer d.

	Vol.	Temp.	Druck.	Corrig. Vol.
Vor d. Absorpt.	112,615	14,0	618,2	75,644
Nach „ „	104,653	11,9	697,8	69,973
Demnach enthalten 100 Vol. des Gases = 7,497 % CO ₂ .				

Dieselbe Versuchsreihe hat Vierordt¹⁾ angestellt und giebt darüber eine Tabelle von 40 Versuchen, die folgende Zahlen enthält:

Dauer der Hemmung des Athmens	Temp.	Druck.	Corrig. Vol.	% CO ₂
20"	14,0	704,2	100,72	4,80
40"	11,9	688,1	97,047	5,21
60"	11,9	680,2	82,751	6,06
80"	11,9	679,3	103,86	6,44
90"	11,9	679,3	103,86	6,50
100"	11,9	679,3	103,86	8,06

Stellen wir unsere Versuche (Taf. VII. Fig. 3) und die von Vierordt (Fig. 4) zur bessern Uebersicht in graphischer Darstellung nebeneinander, so geht aus beiden Kurven unzweideutig hervor, dass bei grösstmöglicher Inspiration das Maximum des Kohlensäuregehaltes innerhalb 100" sich nicht erreichen lässt, wenn gleich nach dem Gange unserer Kurve, die nach den Forderungen der Theorie die zuverlässigere sein dürfte, dasselbe unmöglich mehr weit entfernt sein kann. Künftige Versuche werden zeigen, dass mit allmählig kleiner werdendem Inspirationsvolum dasselbe in kürzerer Zeit näher rückt, allein wegen den bedeutenden Athembeschwerden, die gegen das Ende unerträglich werden, wird sich wohl auch dann ebensowenig sicher fest stellen lassen. Nach dem Ergebnisse dieses nahmen wir für die folgenden Versuche ein für allemal die Dauer des Anhaltens zu 60 Sekunden an, aus den schon oben angegebenen Gründen.

Wir theilen nun zunächst einige Versuche mit, die wir zu verschiedenen Zeiten, in verschiedenen Zuständen, mit gleicher Dauer und möglichst gleichem Volum der Inspiration anstellten.

Nr. I. $\frac{4}{9}$ Abends 5^h.

Tiefste Inspiration; 60" angehalten. Exspirirt. Vol. = 4434,75 C. C.

Zahl der Pulsschläge	60.
Temperatur des Zimmers	22,3.
Barometerhöhe	734,0.

Analyse. Eudiometer VII.

	Vol.	Temp.	Druck.	Corrig. Vol.
Vor d. Absorpt.	40,32	21,8	653,8	23,68
Nach „ „	36,544	18,8	641,3	21,92
Demnach enthalten 100 Vol. = 7,43 % CO ₂ .				

¹⁾ Wagner's Handwörterbuch, Art. Respiration p. 693.

Nr. II. $\frac{5}{9}$ Abends 6^h. 4 Stunden nach d. Mittagessen.
Tiefste Inspiration; 60". Exspirirt. Vol. = 4200,7 CO.
Puls 80.
Resp. 15.
Temperatur 21,8.
Barometerhöhe 734,0.

Analyse. Eudiometer VII.
Vol. Temp. Druck. Corrig. Vol.
Vor d. Absorpt. 61,376 21,8 709,2 39,217
Nach,, " 55,744 16,2 691,4 36,382
Demnach enthalten 100 Vol. = 7,229% CO₂.

Nr. III. $\frac{2}{9}$ Morgens 8^h. Nüchtern.
Tiefste Inspiration; 60". Exspirirt. Vol. = 4101 C. C.
Puls 64.
Resp. 12.
Temperatur 17.
Barometerhöhe 734,2.

Analyse. Eudiometer VII.
Vol. Temp. Druck. Corrig. Vol.
Vor d. Absorpt. 41,216 19,0 652,0 24,495
Nach,, " 38,464 22,3 643,2 32,871
Demnach enthalten 100 Vol. = 6,629% CO₂.

Nr. IV. $\frac{6}{9}$ Abends 5^h. Nüchtern.
Tiefste Inspiration; 60". Exspirirt. Vol. = 4508,75 C. C.
Puls 52.
Temperatur 22,4.
Barometerhöhe 732,5.

Analyse. Eudiometer VII.
Vol. Temp. Druck. Corrig. Vol.
Vor d. Absorpt. 55,720 22,1 697,1 34,300
Nach,, " 50,432 19,6 681,4 32,073
Demnach enthalten 100 Vol. = 6,492% CO₂.
Im Mittel aus mehreren Analysen = 6,581% CO₂.

Nr. V. $\frac{7}{9}$ Abends 5^h. 4 Stunden nach d. Mittagessen.
Tiefste Inspiration; 60". Exspirirt. Vol. = 4231,25 C. C.
Barometerhöhe 729,2.
Temperatur 22,2.

Analyse. Eudiometer V.
Vol. Temp. Druck. Corrig. Vol.
Vor d. Absorpt. 31,035 22,2 677,9 18,925
Nach,, " 28,200 20,2 669,4 17,486
Demnach enthalten 100 Vol. = 7,603% CO₂.
Im Mittel aus mehreren Analysen = 7,600% CO₂.

Nr. VI. $\frac{10}{9}$ Morgens 10^h. Nüchtern.
Tiefste Inspiration; 60". Exspirirt. Vol. = 4379,25 C. C.
Puls 72.
Resp. 10.
Temperatur 17,5.
Barometerhöhe 721,0.

Analyse. Eudiometer V.
Vol. Temp. Druck. Corrig. Vol.
Vor d. Absorpt. 33,870 20,5 686,7 21,073
Nach,, " 30,755 15,3 680,0 19,803
Demnach enthalten 100 Vol. = 6,027.
Im Mittel aus mehreren Analysen = 6,107% CO₂.

Nr. VII. $\frac{11}{9}$ Abends 5^h. 4 Stunden nach d. Essen.
Tiefste Inspiration; 60". Exspirirt. Vol. = 3546,75 C. C.
Barometerhöhe 732,0.
Temperatur 20,0.

Analyse. Eudiometer I.
Vol. Temp. Druck. Corrig. Vol.
Vor d. Absorpt. 137,925 19,8 664,9 83,290
Nach,, " 129,900 15,9 631,3 77,490
Demnach enthalten 100 Vol. = 6,963% CO₂.
Im Mittel aus mehreren Analysen = 6,939% CO₂.

Nr. VIII. $\frac{12}{9}$ Abends 5^h. 3 Stunden nach d. Essen.
Tiefste Inspiration; 60". Exspirirt. Vol. = 4379,25 C. C.
Puls 104.
Resp. 12.
Temperatur 20,7.
Barometer 732,5.

Analyse. Eudiometer I.
Vol. Temp. Druck. Corrig. Vol.
Vor d. Absorpt. 141,750 20,8 679,7 87,139
Nach,, " 133,550 18,4 646,4 80,874
Demnach enthalten 100 Vol. = 7,189% CO₂.

Nr. IX. $\frac{15}{9}$ Abends 6 $\frac{1}{2}$ 4 Stunden nach d. Essen
 Tiefste Inspiration; 60". Exspirirt. Vol. = 4397,75 C. C.
 Puls 92.
 Resp. 10.
 Temperatur 20,8.
 Barometerhöhe 732,5.

Analyse. Eudiometer II.

	Vol.	Temp.	Druck.	Corrig. Vol.
Vor d. Absorpt.	151,488	20,8	664,8	91,028
Nach,, "	142,821	18,4	631,5	84,494

 Demnach enthalten 100 Vol. = 7,178% CO₂.

Nr. X. $\frac{14}{9}$ Abends 4 $\frac{1}{2}$ 3 Stunden nach d. Essen.
 Tiefste Inspiration; 60". Exspirirt. Vol. 4305,25 C. C.
 Puls 80.
 Resp. 17.
 Temp. in d. Mundhöhle 37,0.
 Zimmertemperatur 20,7.
 Barometerhöhe 731,5.

Analyse. Eudiometer II.

	Vol.	Temp.	Druck.	Corrig. Vol.
Vor d. Absorpt.	157,401	20,8	685,0	97,535
Nach,, "	148,923	20,4	653,9	90,610

 Demnach enthalten 100 Vol. = 7,10% CO₂.

Nr. XI. $\frac{14}{9}$ Abends 6 $\frac{1}{2}$ 5 Stunden nach d. Essen.
 Tiefste Inspiration; 60". Exspirirt. Vol. = 4305,25 C. C.
 Puls 72.
 Resp. 12.
 Temperatur 36,8.
 Zimmertemperatur 21,0.
 Barometerhöhe 731,0.

Analyse. Eudiometer I.

	Vol.	Temp.	Druck.	Corrig. Vol.
Vor d. Absorpt.	148,00	20,8	703,8	94,296
Nach,, "	140,400	21,3	673,4	87,699

 Demnach enthalten 100 Vol. = 6,996% CO₂.

Eine Vergleichung des Ergebnisses der einzelnen hier aufgeführten Versuche genügt, um die Thatsache zu begründen, dass die Kohlensäurespannung im Blute in verschiedenen Zuständen eine verschiedene ist; denn für die Verschiedenheit der gefundenen Kohlensäurewerthe lässt

sich bei nur einigem Zutrauen in die Genauigkeit der Arbeit, eine andere plausible Erklärung nicht auffinden. Zudem aber zeigt sich sofort, wie die Zu- und Abnahme des Kohlensäuregehaltes zusammenfällt mit der durch frühere Beobachtungen gefundenen Zu- und Abnahme der Gesamtmenge der in gleichen Zeiten ausgeathmeten Kohlensäure.

Um jeden Zweifel unmöglich zu machen, erscheint Nichts geeigneter, als die Beobachtung der verschiedenen Spannungswerthe zu verschiedenen Stunden eines und desselben Tages; bestätigt sich bei diesem Versuche dieselbe Thatsache, so handelt es sich nicht mehr um die Frage, ob unsere Methode als solche gelten könne, sondern darum, ob die mit ihrer Hilfe gewonnenen Thatsachen nicht den allein richtigen Ausdruck für die fraglichen Vorgänge abzugeben geeignet sind.

Wir theilen darum sofort einige in der bezeichneten Weise angestellte Versuche mit:

Nr. XII. $\frac{20}{10}$ Morgens 9 $\frac{1}{2}$ Nüchtern.
 Tiefste Inspiration; 60".
 Im Mittel aus 3 Versuchen, die oben (pag. 261) als Beispiel mitgetheilt sind, enthalten 100 Vol. der Expirationsluft = 6,692% CO₂.

Nr. XIII. $\frac{20}{10}$ Morgens 11 $\frac{1}{2}$ Nüchtern.
 Tiefste Inspiration; 60". Exspirirt. Vol. = 4601,25 C. C.
 Puls 68.
 Resp. 12.
 Temperatur 36,1.
 Zimmertemperatur 10,5.
 Barometerhöhe 724,0.

Analyse. Eudiometer d.

	Vol.	Temp.	Druck.	Corrig. Vol.
Vor d. Absorpt.	107,257	10,0	701,1	71,563
Nach,, "	101,101	9,9	684,5	66,782

 Demnach enthalten 100 Vol. = 6,680% CO₂.

Nr. XIV. $\frac{20}{10}$ Nachmittags 2 $\frac{1}{2}$ 1 $\frac{1}{2}$ Stunden nach dem Essen.
 Tiefste Inspiration; 60". Exspirirt. Vol. = 4582,75 C. C.
 Puls 115.
 Resp. 18.
 Temperatur 36,7.
 Zimmertemperatur 10,5.
 Barometerhöhe 732,0.

Analyse. Eudiometer IV.				
	Vol.	Temp.	Druck.	Corrig. Vol.
Vor d. Absorpt.	81,200	10,0	708,5	54,760
Nach,, "	75,539	9,9	697,7	50,859.
Demnach enthalten 100 Vol. = 7,123% CO ₂				

Nr. XV. ²⁰ / ₁₀ Nachmittags 4 ^h .				
Tiefste Inspiration; 60". Exspirirt. Vol. = 4471 C. C.				
	Puls	72.		
	Resp.	13.		
	Temperatur	36,5.		
	Zimmertemperatur	10,5.		
	Barometerhöhe	721,0.		

Analyse. Eudiometer a.				
	Vol.	Temp.	Druck.	Corrig. Vol.
Vor d. Absorpt.	123,154	10,0	668,1	78,251
Nach,, "	115,184	9,9	649,3	72,232.
Demnach enthalten 100 Vol. = 7,047% CO ₂ .				

Nr. XVI. ²⁰ / ₁₀ Abends 6 ^h .				
Tiefste Inspiration; 60". Exspirirt. Vol. = 4508 C. C.				
	Puls	76.		
	Resp.	13.		
	Temperatur	36,4.		
	Zimmertemperatur	10,3.		
	Barometerhöhe	720,5.		

Analyse. Eudiometer b.				
	Vol.	Temp.	Druck.	Corrig. Vol.
Vor d. Absorpt.	111,911	10,0	687,4	73,192
Nach,, "	105,355	9,9	669,2	68,034.
Demnach enthalten 100 Vol. = 7,047% CO ₂ .				

Die Körpertemperatur wurde an diesem Tage von Stunde zu Stunde beobachtet, und dafür die nebenstehenden Werthe gefunden. Wir stellen der Uebersicht wegen die Curven für die Temperatur (Taf. VII. Fig. 5), Pulsfrequenz (Fig. 6) und Kohlensäurespannung (Fig. 7) nebeneinander.

²⁰ / ₁₀ Temperatur auf dem Boden der Mundhöhle.	
9 ^h	36,2
10 ^h	36,2
11 ^h	36,1
12 ^h	34,0

Um 11^h kaltes Bad von 13^o.

Um 12^h 30' Mittagessen.

2 ^h	36,7
3 ^h	36,6
4 ^h	36,5
5 ^h	36,4
6 ^h	36,4

Bemerkung. Wie die Beobachtungen über die Körpertemperatur gewonnen wurden, siehe unten.

Nr. XVII. ²⁰/₉ Morgens 11^h. Nüchtern.

Tiefste Inspiration; 60". Exspirirt. Vol. = 4564,25 C. C.				
	Puls	68.		
	Resp.	13.		
	Temperatur	36,2.		
	Zimmertemperatur	12,5.		
	Barometerhöhe	732,0.		

Analyse. Eudiometer a.				
	Vol.	Temp.	Druck.	Corrig. Vol.
Vor d. Absorpt.	130,354	13,0	696,9	85,313
Nach,, "	124,024	12,9	675,0	79,935.
Demnach enthalten 100 Vol. = 6,303% CO ₂ .				

Nr. XVIII. ²⁰/₉ Nachmittags 4^h. 2 Stunden nach dem Essen.

Tiefste Inspiration; 60". Exspirirt. Vol. = 4564,25 C. C.				
	Puls	76.		
	Resp.	16.		
	Temperatur	36,8.		
	Zimmertemperatur	13,0.		
	Barometerhöhe	730,0.		

Analyse. Eudiometer b.				
	Vol.	Temp.	Druck.	Corrig. Vol.
Vor d. Absorpt.	130,656	13,0	722,5	88,702
Nach,, "	123,746	12,9	698,0	82,472.
Demnach enthalten 100 Vol. = 7,023% CO ₂ .				

Nr. XIX. ²⁰/₉ Morgens 8^h. Nüchtern.

Tiefste Inspiration; 60". Exspirirt. Vol. = 4545,75 C. C.				
	Puls	76.		
	Resp.	11.		
	Temperatur	36,5.		
	Zimmertemperatur	14,0.		
	Barometerhöhe	734,0.		
		18*		

Analyse. Eudiometer a.				
	Vol.	Temp.	Druck.	Corrig. Vol.
Vor d. Absorpt.	131,284	14,2	718,6	87,946
Nach,, "	124,624	13,0	692,4	82,373.
Demnach enthalten 100 Vol. = 6,336% CO ₂ .				

Nr. XX. ²⁵/₁₀ Mittags 2^h. 1 Stunde nach dem Essen.

Tiefste Inspiration; 60°. Exspirirt. Vol. =	4397 C. C.
Puls	116.
Resp.	18.
Temperatur	36,8.
Zimmertemperatur	14,2.
Barometerhöhe	734,2.

Analyse. Eudiometer b.				
	Vol.	Temp.	Druck.	Corrig. Vol.
Vor d. Absorpt.	117,555	14,2	726,5	79,812
Nach,, "	110,588	13,0	702,8	74,187.
Demnach enthalten 100 Vol. = 7,048% CO ₂ .				

Nr. XXI. ²⁵/₁₀ Abends 4^h.

Tiefste Inspiration; 60°. Exspirirt. Vol. =	4545,75 C. C.
Puls	76.
Resp.	14.
Temperatur	36,8.
Zimmertemperatur	14,0.
Barometerhöhe	734,0.

Analyse. Eudiometer c.				
	Vol.	Temp.	Druck.	Corrig. Vol.
Vor d. Absorpt.	131,070	14,2	726,8	87,003
Nach,, "	120,486	13,0	703,2	80,877.
Demnach enthalten 100 Vol. = 7,057% CO ₂ .				

Die letzten drei Versuchsreihen (Nr. 12 bis 16, 17 bis 18, 19 bis 21) stimmen sowohl unter sich, als mit den einzelnen früher mitgetheilten Versuchen vollkommen überein; eine nähere Vergleichung mit den Ergebnissen der Beobachtungen über die Gesammtmenge der bei „ruhigem Athmen“ während gewisser Zeiträume ausgeschiedenen Kohlensäure (v. Vierordt) lassen wir zum Schlusse der hieher gehörigen Beobachtungen folgen; hier nur folgende Bemerkung: Durch unsere Methode wird jeder Einfluss der so unsichern Bewegungen des Respirationapparates ausgeschlossen, die Beobachtung wird geradezu auf das Blut zurückverlegt.

das Ergebniss derselben liefert den getreuen Ausdruck des jeweiligen Zustandes des Blutes bezüglich seines Kohlensäuregehaltes. Es kann darum kein Zweifel sein, dass sie weit geeigneter ist zur Beobachtung des Ganges der Bildung und Ausscheidung der Kohlensäure, als alle bisherigen Methoden.

Bedenken wir ferner, dass es nie möglich sein wird, beim Menschen die Gesammtmenge der ausgeschiedenen Kohlensäure während längeren Zeiträumen ohne Fehler zu bestimmen, so können wir nicht zweifeln, dass die Beobachtung der Kohlensäurespannung des Blutes es ist, die in Zukunft in die Reihe der übrigen Beobachtungen über den Stoffwechsel als anderwichtigstes Glied einzutreten habe.

Das wichtigste Glied derselben mag immer die Beobachtung der Bildung und Ausscheidung des Harnstoffes sein, aber auch die genaueste Kenntniss der Umsetzung der Stickstoffatome ist eben nie im Stande, ein „Maass des Stoffwechsels“ (Bischoff) abzugeben, ohne gleichzeitige Beobachtung derjenigen des Kohlenstoffs, und ebenso umgekehrt (da die Annahme, dass beide sich nach Menge und Zeit proportional giengen, mindestens nicht immer eintreffen kann).

Aus dem Grunde suchten wir beiderlei Beobachtungen zu vereinigen; die Schwierigkeiten, die eine so komplizierte Beobachtung für ein einzelnes Individuum hat, liessen sich in kurzer Zeit bewältigen, und nun glauben wir als schönstes Ergebniss unserer Arbeit betrachten zu können, dass wir im Stande sind, neben dem täglichen Gang der Körpertemperatur, der Frequenz der Herz- und Athembewegungen, der Harnstoffausscheidung — die Kohlensäurespannung im Blute — gleichzeitig in beliebigen Zeiträumen beobachten zu können.

Es erübrigt uns, ehe wir zu den betreffenden Versuchen übergehen, einige Worte über die Art der Bestimmung der genannten Werthe vorzuschicken.

Die Messung der Körpertemperatur bewerkstelligten wir auf dem Boden der Mundhöhle mit einem nach ¹/₁₀ Graden getheilten Thermometer, in der Weise, dass wir denselben circa 5 Minuten möglichst tief im wohlverschlossenen Munde liegen liessen, und darauf den Stand mittelst eines Spiegels ablasen.

Die Zählung der Pulsschläge geschieht, wie wir uns durch viele vergleichende Versuche überzeugten, am si-

chersten in der zweiten Hälfte der Inspirationsdauer; es wird nämlich, nachdem unmittelbar auf die Inspiration eine Reihe unregelmässiger rascher Herzkontraktionen gefolgt ist, der Puls vollkommen regelmässig, und zwar in demselben Rhythmus, wie vor dem Versuche, während wir selbst bei ruhigem Athmen unsern Puls nie zählen können, ohne durch vorübergehende Unregelmässigkeiten gestört zu werden.

Die Zählung der Athemzüge kann nicht weniger unzuverlässig gemacht werden, als ihre Frequenz überhaupt ist.

Die Bestimmung des Harnstoffs führten wir genau nach Liebig's Methode aus, durch Fällung mit einer filtrirten Lösung von salpetersaurem Quecksilberoxyd. Zu bemerken ist nur, dass, auch ohne allen Wassergenuss, die während einer Stunde in der Blase angesammelte Harnmenge vollkommen hinreicht, um eine sichere Harnstoffbestimmung zu machen. (Sämmtliche Bestimmungen wurden unmittelbar nach der Entleerung gemacht.)

$\frac{1}{10}$ Nüchtern bis Mittag. $12\frac{1}{4}$ Gewöhnliches Mittagessen.

Nr. XXII. 10^h.

Tiefste Inspirat. 60"; Exspir. Vol. = 4453,25 C. C.

Puls	68.
Respiration	11.
Temperatur	36,2.
Zimmertemp.	15,5.
Barometerhöhe	723,0.

Analyse. Eudiometer I.

	Vol.	Temp.	Druck.	Corrig. Vol.
Vor d. Absorp.	155,45	16,8	686,4	98,419
Nach, "	143,175	19,1	685,3	91,943.

Demnach enthalten 100 Vol. = 6,580% CO₂.

Nr. XXIII. 12^h.

Tiefste Inspiration 60"; Exspir. Volum = 4582 C. C.

Puls	68.
Respiration	11.
Zimmertemperatur	16,0.
Barometerhöhe	722,7.

Analyse. Eudiometer c.

	Vol.	Temp.	Druck.	Corrig. Vol.
Vor d. Absorp.	130,656	16,8	688,0	82,921
Nach, "	119,141	16,6	690,1	77,503.

Demnach enthalten 100 Vol. = 6,534% CO₂.

Nr. XXIV. 2^h. Inspir. 60". Expir. Vol. = 4619,75 C. C.

Puls	100.
Respiration	16.
Zimmertemperatur	17,5.
Barometerhöhe	722,5.

Analyse. Eudiometer II.

	Vol.	Temp.	Druck.	Corrig. Vol.
Vor d. Absorp.	172,035	16,8	691,3	109,715
Nach, "	158,481	16,6	682,0	101,88.

Demnach enthalten 100 Vol. = 7,141% CO₂.

Nr. XXV. 3^h. Inspir. 60"; Exspir. Vol. 4379,25 C. C.

Puls	84.
Respiration	14.
Zimmertemperatur	17,0.
Barometerhöhe	722,5.

Analyse. Eudiometer d.

	Vol.	Temp.	Druck.	Corrig. Vol.
Vor d. Absorp.	109,625	16,8	680,5	68,793
Nach, "	99,236	18,0	683,8	63,656.

Demnach enthalten 100 Vol. = 7,467% CO₂.

Nr. XXVI. 4^h. Inspir. 60"; Exspir. Vol. = 4638,25 C. C.

Puls	76.
Respiration	13.
Zimmertemperatur	17,8.
Barometerhöhe	722,5.

Analyse. Eudiometer b.

	Vol.	Temp.	Druck.	Corrig. Vol.
Vor d. Absorp.	117,320	16,8	676,9	73,227
Nach, "	106,531	18,0	678,7	67,826.

Demnach enthalten 100 Vol. = 7,375% CO₂.

Nr. XXVII. 6^h. Inspir. 60"; Exspir. Vol. = 4453,25 C. C.

Puls	68.
Respiration	11.
Zimmertemperatur	17,8.
Barometerhöhe	722,0.

Analyse. Eudiometer a.

	Vol.	Temp.	Druck.	Corrig. Vol.
Vor d. Absorp.	132,664	16,8	670,4	81,990
Nach, "	120,754	16,6	671,9	76,478.

Demnach enthalten 100 Vol. = 6,722% CO₂.

Nr. XXVIII. 8^a. Inspir. 60'; Exspir. Vol. 4323,75 C. C.
 Puls 64.
 Respiration 11.
 Zimmertemperatur 17,8.
 Barometerhöhe 720,2.
 Analyse. Eudiometer IV.
 Vol. Temp. Druck. Corrig. Vol.
 Vor d. Absorp. 82,532 16,8 683,2 52,004
 Nach,, " 73,245 19,1 706,2 48,469.
 Demnach enthalten 100 Vol. = 6,797% CO₂.

Harnstoffbestimmung.				
Zeit	Harnmenge in C. C.	Hg.-Lösung (192:0,20)	Harnstoff in % d.H.	Harnstoff-Gesammtm. in Grs.
2 ⁵ / ₁₀ 8 ^a (Morgenh.)	330 C.C.	39,5 C.C.	4,114	13,576 Grs.
9 ^a	55 "	31,3 "	3,280 "	1,793 "
10 ^a	55 "	26,4 "	2,750 "	1,512 "
11 ^a	50 "	26,8 "	2,792 "	1,395 "
12 ^a	42 "	31,0 "	3,229 "	1,356 "
1 ^b	40 "	31,5 "	3,281 "	1,312 "
2 ^b	72 "	21,0 "	2,708 "	1,950 "
3 ^b	150 "	13,8 "	1,437 "	2,156 "
4 ^b	76 "	25,3 "	2,635 "	2,002 "
5 ^b	110 "	27,0 "	2,812 "	3,093 "
6 ^b	100 "	25,8 "	2,687 "	2,687 "
7 ^b	74 "	29,0 "	3,021 "	2,235 "
8 ^b	63 "	36,0 "	3,750 "	2,362 "
9 ^b	55 "	34,8 "	3,625 "	1,993 "
11 ^b	348 "	12,6 "	1,312 "	4,565 "
2 ⁵ / ₁₀ 8 ^a	295 "	46,5 "	4,843 "	14,286 "
10 ^a	272 "	14,5 "	1,458 "	3,965 "
11 ^a	188 "	16,5 "	1,718 "	5,412 "
12 ^a	127 "			
2 ^b	140 "	27,5 "	2,864 "	4,009 "
5 ^b	90 "	33,5 "	3,489 "	3,140 "
7 ^b	65 "	30,0 "	3,125 "	2,031 "

Temperaturbestimmung.		
Zeit.	Körpertemp.	Zimmertemp.
2 ⁵ / ₁₀ 8 ^a	36,5° Cels.	16,0°
9 ^a	36,4 "	15,5

Zeit.	Körpertemp.	Zimmertemp.
10 ^b	36,3 Cels.	15,5
11 ^b	36,3 "	16,0
12 ^b	36,2 "	16,0
2 ^b	37,1 "	17,5
3 ^b	36,8 "	17,0
4 ^b	36,7 "	17,8
5 ^b	36,6 "	17,8
6 ^b	36,6 "	17,8
7 ^b	36,2 "	17,5
8 ^b	36,1 "	17,5

Bemerkungen: 2⁵/₁₀ bis 12^a nüchtern.
 12^a 30' Mittagessen.
 9^a Nachtessen.
 2⁵/₁₀ 8^a Frühstück.
 Kein Mittagessen.
 2—5^b starke Bewegung.

Zu Erleichterung der Uebersicht stellen wir sämtliche Beobachtungen dieses Tages in graphischer Darstellung neben einander. Fig. 8 giebt die Pulszahl, Fig. 9 die Temperatur, Fig. 10 die Zahl der Athemzüge, Fig. 11 die Kohlensäurespannung, Fig. 12 die Harnmenge in C. C. und Fig. 13 den Harnstoff in Grammen.

Es geht aus einer Vergleichung des Zeitpunktes, in welchem die verschiedenen Werthe ihr Maximum erreichen, hervor, dass 1) Temperatur, Puls- und Athemfrequenz, vielleicht unmittelbar mit dem Beginne der Verdauung, mindestens aber innert der ersten Stunde, 2) der Kohlensäuregehalt des Blutes 2—3 Stunden, 3) die Harnstoffausscheidung 2—5 Stunden nach der Einnahme der Mahlzeit ihr Maximum erreichen. Die Ausscheidung des mit dem Essen aufgenommenen Wassers erfolgt in der zweiten Stunde, und führt eine proportionale Vermehrung der Harnstoffausscheidung herbei (siehe die Erhebung der Harnstoffkurve um 3^b).

Bedenken wir, dass die Athemfrequenz nicht allein von der Menge der auszuscheidenden CO₂, sondern, wie hier, von der des aufzunehmenden O abhängig ist, so giebt diese Beobachtung ein äußerst getreues Bild von den Vorgängen während der Verdauungsperiode.

²⁰/₉ — ²²/₉ 48 stündiges Hungern.
²⁰/₉ Morgens nüchtern, 12^a circa $\frac{1}{2}$ \bar{u} Fleisch genossen; da-
 rauf Hunger bis ²³/₉ Mittags 1^a Mittagessen.

Nr. XXIX. ²⁰/₉ Morgens 6^a.
 Tiefste Inspir. 60"; expirirtes Vol. = 4323,75 C. C.

Puls 60.
 Respiration 11.
 Temperatur 36,0.
 Zimmertemperatur 17,0.
 Barometerhöhe 732,5.

Analyse. Eudiometer I.

	Vol.	Temp.	Druck.	Corrig. Vol.
Vor d. Absorpt.	151,275	18,2	712,9	98,899
Nach,, "	144,250	18,0	680,4	92,071.

Demnach enthalten 100 Vol. = 6,904% CO₂.

Nr. XXX. Morgens 8^a.

Inspir. 60"; expirirtes Vol. = 4656,75 C. C.

Puls 64.
 Respiration 13.
 Temperatur 36,3.
 Zimmertemperatur 17,3.
 Barometerhöhe 732,2.

Analyse. Eudiometer II.

	Vol.	Temp.	Druck.	Corrig. Vol.
Vor d. Absorpt.	152,596	18,2	669,6	93,998
Nach,, "	146,871	18,0	638,5	87,972.

Demnach enthalten 100 Vol. = 6,411% CO₂.

Nr. XXXI. Morgens 11^a.

Inspir. 60"; expirirtes Vol. = 4656,75 C. C.

Puls 60.
 Respiration 11.
 Temperatur 36,3.
 Zimmertemperatur 17,6.
 Barometerhöhe 731,5.

Analyse. Eudiometer d.

	Vol.	Temp.	Druck.	Corrig. Vol.
Vor d. Absorpt.	110,780	18,2	727,8	73,374
Nach,, "	104,564	18,0	703,0	68,356.

Demnach enthalten 100 Vol. = 6,783% CO₂.

Nr. XXXII. Mittags 1^a.

Inspir. 60"; expirirtes Vol. = 4471 C. C.

Puls 76.
 Respiration 15.
 Temperatur 36,6.
 Zimmertemperatur 17,9.
 Barometerhöhe 730,5.

Analyse. Eudiometer a.

	Vol.	Temp.	Druck.	Corrig. Vol.
Vor d. Absorpt.	135,124	18,2	721,7	89,455
Nach,, "	127,684	18,0	693,2	83,029.

Demnach enthalten 100 Vol. = 7,183% CO₂.

Nr. XXXIII. Mittags 3^a.

Inspir. 60"; expirirtes Vol. = 4508% CO₂.

Puls 72.
 Respiration 16.
 Temperatur 36,4.
 Zimmertemperatur 18,0.
 Barometerhöhe 730,0.

Analyse. Eudiometer b.

	Vol.	Temp.	Druck.	Corrig. Vol.
Vor d. Absorpt.	119,114	18,2	725,8	79,313
Nach,, "	112,352	18,0	699,0	73,671.

Demnach enthalten 100 Vol. = 7,113% CO₂.

Nr. XXXIV. Abends 5^a.

Inspir. 60"; expirirtes Vol. = 4379 C. C.

Puls 72.
 Respiration 16.
 Temperatur 36,5.
 Zimmertemperatur 18,0.
 Barometerhöhe 730.

Analyse. Eudiometer c.

	Vol.	Temp.	Druck.	Corrig. Vol.
Vor d. Absorpt.	121,864	18,2	702,1	78,437
Nach,, "	115,027	18,0	675,2	72,853.

Demnach enthalten 100 Vol. = 7,119% CO₂.

Nr. XXXV. ²¹/₉ Morgens 7^a.

Inspir. 60"; expirirtes Vol. = 4527 C. C.

Puls 76.
 Respiration 12.

Temperatur 36,0.
 Zimmertemperatur 17,0.
 Barometerhöhe 730.

Analyse. Eudiometer I.

	Vol.	Temp.	Druck.	Corrig. Vol.
Vor d. Absorpt.	147,20	18,5	695,0	93,630
Nach,, "	139,525	16,8	663,6	87,222.

Demnach enthalten 100 Vol. = 6,843% CO₂.

Nr. XXXVI. Morgens 11^h.

Inspir. 60"; exspirirtes Vol. = 4527 C. C.

Puls 56.
 Respiration 11.
 Temperatur 36,1.
 Zimmertemperatur 18,5.
 Barometerhöhe 730.

Analyse. Eudiometer a.

	Vol.	Temp.	Druck.	Corrig. Vol.
Vor d. Absorpt.	127,924	18,5	696,3	81,522
Nach,, "	120,424	16,8	670,6	76,070.

Demnach enthalten 100 Vol. = 6,687% CO₂.

Nr. XXXVII. Mittags 2^h.

Inspir. 60"; exspirirtes Vol. = 4545 C. C.

Puls 80.
 Respiration 11.
 Temperatur 36,7.
 Zimmertemperatur 18,7.
 Barometerhöhe 729,0.

Analyse. Eudiometer II.

	Vol.	Temp.	Druck.	Corrig. Vol.
Vor d. Absorpt.	167,607	18,5	717,3	110,11
Nach,, "	158,967	16,8	684,6	102,52.

Demnach enthalten 100 Vol. = 6,893 CO.

Nr. XXXVIII. Abends 5^h.

Inspir. 60"; exspirirtes Vol. = 4601 C. C.

Puls 60.
 Respiration 11.
 Temperatur 36,1.
 Zimmertemperatur 18,5.
 Barometerhöhe 728,5.

Analyse. Eudiometer b.

	Vol.	Temp.	Druck.	Corrig. Vol.
Vor d. Absorpt.	109,412	18,5	691,5	69,235
Nach,, "	102,591	16,8	667,6	64,517.

Demnach enthalten 100 Vol. = 6,814% CO₂.

Nr. XXIX. 2^h/3 Morgens 7^h.

Inspir. 60"; exspirirtes Vol. = 4693 C. C.

Puls 84.
 Respiration 10.
 Temperatur 36,2.
 Zimmertemperatur 16,8.
 Barometerhöhe 730,8.

Analyse. Eudiometer b.

	Vol.	Temp.	Druck.	Corrig. Vol.
Vor d. Absorpt.	108,353	17,8	685,4	68,169
Nach,, "	102,209	16,0	663,9	64,030.

Demnach enthalten 100 Vol. = 6,071% CO₂.

Nr. XL. 2^h/3 Morgens 11^h.

Inspir. 60"; exspirirtes Vol. = 4730,75 C. C.

Puls 72.
 Respiration 8.
 Temperatur 36,3.
 Zimmertemperatur 17,2.
 Barometerhöhe 731,2.

Analyse. Eudiometer a.

	Vol.	Temp.	Druck.	Corrig. Vol.
Vor d. Absorpt.	138,214	17,8	727,8	92,449
Nach,, "	130,864	16,0	703,2	86,924.

Demnach enthalten 100 Vol. = 5,976% CO₂.

Nr. XLI. Mittags 3^h.

2 Stunden nach d. Essen. Inspir. 60"; exspir. Vol. = 4490,25 C. C.

Puls 108.
 Respiration 2.
 Temperatur 36,8.
 Zimmertemperatur 17,4.
 Barometerhöhe 731,0.

Analyse. Eudiometer c.

	Vol.	Temp.	Druck.	Corrig. Vol.
Vor d. Absorpt.	128,643	17,8	730,7	86,406
Nach,, "	120,917	16,0	694,5	79,320.

Demnach enthalten 100 Vol. = 8,201% CO₂.

Nr. XLII. Abends 5^h.

Inspir. 60'; expirirtes Vol. = 4527,25 C. C.
 Puls 80.
 Respiration ?
 Temperatur ?
 Zimmertemperatur 17,0.
 Barometerhöhe 731,0.

Analyse. Eudiometer d.

	Vol.	Temp.	Druck.	Corrig. Vol.
Vor d. Absorpt.	104,682	17,8	704,2	67,707
Nach,, "	98,288	16,0	681,4	63,262.

Demnach enthalten 100 Vol. = 6,565% CO₂.

Harnstoffbestimmungen.

Zeit	Harnmenge.	Hg.-lösung.	Harnstoff in % d. Harns.	Harnstoffge-sammtenge.
20 ^h 6 ^h Morgenh.	283 C.C.	55,0 C.C.	4,701 Grs.	13,304 Grs.
" 8 ^h	55 "	48,5 "	4,144 "	2,279 "
" 11 ^h	140 "	32,7 "	2,794 "	3,911 "
" 1 ^h	85 "	42,6 "	3,641 "	3,095 "
" 3 ^h	110 "	41,0 "	3,504 "	3,854 "
" 5 ^h	150 "	38,0 "	3,247 "	4,871 "
" 11 ^h	295 "	46,0 "	3,931 "	11,596 "
21 ^h 7 ^h Morgenh.	180 "	60,0 "	5,128 "	9,230 "
" 11 ^h	70 "	?	?	?3,974 "
" 2 ^h	70 "	54,5 "	5,677 "	3,974 "
		(19,2:0,20)		
" 5 ^h	65 "	52,0 "	5,416 "	3,520 "
" 11 ^h	160 "	56,0 "	5,833 "	9,333 "
22 ^h 7 ^h	170 "	55,5 "	5,781 "	9,827 "
" 11 ^h	70 "	50,5 "	5,260 "	3,682 "
" 3 ^h	280 "	42,0 "	4,375 "	12,250 "
" 6 ^h	100 "	40,0 "	4,166 "	4,166 "

Harnstoffmenge am ersten Tage v. Morgens 6^h bis Morgens 7^h = 38,836 Grammes.

Harnstoffmenge am zweiten Tage v. Morgens 7^h bis Morgens 7^h = 30,628 Grammes.

Temperaturbestimmungen.

Zeit.	Körpertemperatur.	Zimmertemperatur.
20 ^h 6 ^h	36,0	17,0
" 8 ^h	36,3	17,3
" 9 ^h	36,4	—

Zeit.	Körpertemperatur.	Zimmertemperatur.
" 10 ^h	36,4	17,5
" 11 ^h	36,3	17,6
" 12 ^h	36,4	17,8
" 1 ^h	36,6	17,9
" 2 ^h	36,6	—
" 3 ^h	36,4	18,0
" 4 ^h	36,4	—
" 5 ^h	36,5	—
21 ^h 7 ^h	36,0	17
" 10 ^h	36,2	18,0
" 11 ^h	36,1	18,5
" 2 ^h	36,7	18,7
" 5 ^h	36,1	18,5
22 ^h 7 ^h	36,2	16,8
" 11 ^h	36,3	17,2
" 3 ^h	36,8	17,4
" 5 ^h	?	17,0

Die graphische Darstellung des Ganges der CO₂ Spannung während der drei Tage giebt Fig. 14. Leider sind die Beobachtungen über die Harnstoffausscheidung, den Gang der Temperatur, Pulsfrequenz etc. noch nicht so vollständig und gleichmässig, dass sie ausreichend mit Hilfe einer solchen Darstellung verglichen werden könnten. Wir verweisen darum bezüglich des Harnstoffes auf die Zusammenstellung am Schlusse. Ehe wir die einzelnen Kurven der CO₂ Spannung betrachten, versuchen wir, eine Kurve (Fig. 15) aus dem Mittel sämtlicher bisherigen Beobachtungen für die verschiedenen Tageszeiten zu konstruiren, mit Trennung der Beobachtungen während der Verdauung und während des Fastens. Zum Vergleiche fügen wir sogleich die Ergebnisse von Vierordt's Versuchen (Fig. 16) bei.

Beiden Kurven liegen folgende Werthe zu Grunde:

Stunde	1.		2.	
	CO ₂ Spannung.		Exspir.CO ₂ einer Minute in C.C.	
	Nüchtern.	W. d. Verdauung.	Nüchtern.	W. d. Verdauung.
6 ^h	6,904	—	—	—
7 ^h	6,639	—	—	—
8 ^h	6,401	—	—	—
9 ^h	—	—	—	264 C.C.

Stunde.	1. CO ₂ Spannung.		2. Exspir.CO ₂ einer Minute in C.C. Nach Vierordt.	
	Nächtl.	W. d. Verdauung.	Nächtl.	W. d. Verdauung.
10 ^h	6,287	—	—	282
11 ^h	6,458	—	—	278
12 ^h	6,534	—	270	(270)243
1 ^h	—	7,183	242	276
2 ^h	6,893	(7,104)	258	291
3 ^h	—	7,593	—	279
4 ^h	—	7,300	—	265
5 ^h	6,711	7,087	—	252
6 ^h	—	7,028	—	238
7 ^h	—	—	—	229
8 ^h	—	6,797	—	—

(NB. Das Mittagessen fällt zwischen 12^h 30' und 1^h.)

Betrachten wir die Zu- und Abnahme der Kohlensäurespannung während eines Tages, so zeigt sich, dass dieselbe Morgens, unmittelbar nach dem Erwachen, ziemlich bedeutend ist, bis 10^h allmählig abnimmt, gegen die Mittagszeit aber wieder zu steigen beginnt. Findet um Mittag die Nahrungsaufnahme statt, so steigt unmittelbar mit dem Beginne der Verdauung die CO₂ Spannung beträchtlich an, um ungefähr 2—2½ Stunden nach dem Essen ihr Maximum zu erreichen, von welchem sie dann allmählig wieder abfällt. Wird dagegen gefastet, so erreicht die um Mittag stattfindende Steigerung eine ungleich geringere Höhe. Diese geringe Erhebung geht proportional mit der Vermehrung der Pulsfrequenz und Temperatur, die man um Mittag unabhängig von der Verdauung beobachtet. Vierordt's Kurve der während einer Minute ausgeathmeten CO₂ mengen zeigt von unseren bemerkenswerthe Abweichungen. Eine verhältnissmässig grosse CO₂ menge unmittelbar nach dem Erwachen haben Vierordt und Prout in Uebereinstimmung mit unserer Spannungshöhe gefunden, sie ist aber in obiger Reihe nicht aufgezeichnet. Die Steigerung während des Vormittags rührt bei Vierordt wohl einzig von dem Genusse des Frühstückes her. Weiterhin fällt das Maximum der CO₂ ausathmung bei Vierordt circa 1 Stunde nach der Mahlzeit, während unser Spannungsmaximum eine Stunde später eintritt. Wir werden sogleich auf die Differenz zu sprechen kommen.

Während des Fastens will Vierordt keine Zunahme der CO₂ ausscheidung beobachtet haben, und rührt von seinen Versuchen, dass durch dieselben die „mystischen Ideen von einem Einfluss der Tageszeiten“ widerlegt seien. Wir können nicht umhin zu bemerken, dass diese Folgerung aus seinen Versuchen unstatthaft ist, da er nur zwei Versuche während des Fastens anstellte, und in diesen von 1—2^h eine ziemlich starke Erhebung fand, die er aber nicht weiter zu verfolgen für gut fand; wie wenig Zutrauen eine kleine Zahl von Versuchen bei seiner Methode überdies verdient, zeigt der oben verzeichnete Fall, dass er in seiner mittleren Tageskurve um 12^h = 243 C. C. CO₂ findet, in einer andern Tabelle aber 258 C. C. angiebt, in den zwei Versuchen aber, die er während des Fastens anstellte, zu derselben Zeit 270 C. C. aufzeichnet: Schwankungen, welche die Hälfte des ganzen Unterschiedes der Ordinaten in der Kurve umspannen. Ueberdies stehen mit unserer Behauptung die Beobachtungen über Pulsfrequenz und Temperatur im Einklang.

Was nun die erwähnte Verschiedenheit bezüglich der Zeit des Eintretens des Maximalwerthes in beiden Kurven betrifft, so zeigt sich, dass das Maximum der CO₂ ausfuhr mit der grössten Athemfrequenz zusammenfällt, als notwendige Folge des vermehrten Luftwechsels; diese ihrerseits ist aber nur zum kleinsten Theil von der dannzumal noch geringen CO₂ spannung bedingt, zum grössten aber von der mit dem Beginne der Verdauung notwendigen Steigerung des Sauerstoffbedarfes: wir erkennen darin also eine durch anderweitig veranlasste Vermehrung der Athemfrequenz gelegentlich herbeigeführte CO₂ ausfuhr, die weiterhin dazu dienen muss, das später eintretende Maximum der Spannung herabzudrücken. Der umgekehrte Fall scheint uns in der hohen Spannung am frühen Morgen gegeben zu sein: während des Schlafes Verminderung der Athembewegungen, somit Anhäufung der CO₂ und dadurch erhöhte Spannung am Morgen, und mit dem Eintreten lebhafter Bewegungen vermehrte CO₂ ausfuhr. Beiderlei Annahmen werden sich bewahrheiten, wenn demnächst anzustellende Versuche ergeben, dass durch längere Zeit willkürlich vermehrte Athembewegungen die CO₂ spannung herabgedrückt, durch Verminderung derselben aber erhöht werden kann. Wir erblicken in diesem Verhalten die voll-

ständigste Analogie mit der Harnstoffausfuhr: Wie die CO_2 -ausfuhr von der Menge der eingenommenen Luft, so ist, wie wir unten sehen werden, die Harnstoffausfuhr zu einem nicht geringen Theil von der Menge des eingenommenen Wassers abhängig (Bischoff).

Ausserdem aber liegt hierin der deutlichste Fingerzeig, wie nothwendig zu einer genauen Erforschung der Verhältnisse der CO_2 -bildung die Eliminirung des Einflusses der Athembewegungen ist.

Die Zahl der von uns bis jetzt vorgenommenen Variationen der Bedingungen, unter denen unsere Beobachtungen angestellt werden sollen, ist noch sehr klein; sie beschränken sich auf zwei Versuche mit übermässigem Wassergenuss, und mit Anwendung grosser Dosen Digitalis, erlauben deshalb nur sehr wenige weitere Schlüsse; der Vollständigkeit wegen führen wir sie hier doch auf:

²⁶/₁₀ Morgens nüchtern, 3 Flaschen Wasser getrunken.

1 Flasche = 775 C. C.

3 Flaschen = 2325 C. C.

Nr. XLIII. 8^h. Tiefste Inspir. 60"; Exspir. Vol. = 4619,75 C.C.

Puls 56.

Respiration 15.

Temperatur 35,0.

Zimmertemperatur 13,0.

Barometerhöhe 735.

Analyse. Eudiometer c.

	Vol.	Temp.	Druck.	Corrig. Vol.
Vor d. Absorpt.	122,042	12,8	707,1	81,122
Nach „ „	116,418	12,4	683,1	76,070

Demnach enthalten 100 Vol. = 6,228% CO_2 .

9^h bis 10^h 30'. 4 Flaschen Wasser = 3100 C. C. (Empfindliches Kältegefühl, heftiges Zittern.)

Summa 7 Flaschen = 5425 C. C.

Nr. XLIV. 10^h 30' Tiefste Inspir. 60"; Exspir. Vol. = 4323,75 C.C.

Puls 56.

Respiration 12.

Temperatur 35,0.

Zimmertemperatur 12,4.

Barometerhöhe 735.

Analyse. Eudiometer d.

	Vol.	Temp.	Druck.	Corrig. Vol.
Vor d. Absorpt.	102,166	12,8	702,9	67,498
Nach „ „	97,075	12,4	680,7	63,206

Demnach enthalten 100 Vol. = 6,357% CO_2 .

1^h Mittagessen.

Nr. XLV. 3^h. 2 Stunden nach dem Essen.

Tiefste Inspiration 60". Exspirirt. Vol. = 4416,25 C. C.

Puls 112.

Respiration ?

Temperatur 36,6.

Zimmertemperatur 13,3.

Barometerhöhe 734,5.

Analyse. Eudiometer a.

	Vol.	Temp.	Druck.	Corrig. Vol.
Vor d. Absorpt.	131,224	12,8	713,0	87,964
Nach „ „	124,774	12,4	686,7	81,931

Demnach enthalten 100 Vol. = 6,858% CO_2 .

3^h bis 5^h 7 Flaschen Wasser getrunken = 6200 C. C.

Summe vom ganzen Tag = 10850 C. C.

Nr. XLVI. 5^h Abends. Heftiges Frieren und Zittern.

Tiefste Inspiration 60". Exspirirt. Vol. = 4101,75 C. C.

Puls 60.

Respiration 17.

Temperatur 36,3.

Zimmertemperatur 13,3.

Barometerhöhe 734,8.

Analyse. Eudiometer b.

	Vol.	Temp.	Druck.	Corrig. Vol.
Vor d. Absorpt.	116,362	12,8	721,0	78,892
Nach „ „	110,382	12,4	695,7	73,454

Demnach enthalten 100 Vol. = 6,893% CO_2 .

²⁷/₁₀ Morgens 8^h. Nüchtern.

Nr. XLVII. 8^h Tiefste Inspir. 60"; Exspir. Vol. = 4564,25 C. C.

Puls 56.

Respiration 12.

Temperatur 35,0.

Zimmertemperatur 12,4.

Barometerhöhe 735.

Analyse. Eudiometer e.

	Vol.	Temp.	Druck.	Corrig. Vol.
Vor d. Absorpt.	102,166	12,8	702,9	67,498
Nach „ „	97,075	12,4	680,7	63,206

Demnach enthalten 100 Vol. = 6,357% CO_2 .

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Temperatur 34,2.

Zimmertemperatur 13.

Barometerhöhe 735.

Analyse. Eudiometer d.

	Vol.	Temp.	Druck.	Corrig. Vol.
Vor d. Absorpt.	102,166	12,8	702,9	67,498
Nach „ „	97,075	12,4	680,7	63,206

Demnach enthalten 100 Vol. = 6,357% CO_2 .

1^h Mittagessen.

Nr. XLV. 3^h. 2 Stunden nach dem Essen.

Tiefste Inspiration 60". Exspirirt. Vol. = 4416,25 C. C.

Puls 112.

Respiration ?

Temperatur 36,6.

Zimmertemperatur 13,3.

Barometerhöhe 734,5.

Analyse. Eudiometer a.

	Vol.	Temp.	Druck.	Corrig. Vol.
Vor d. Absorpt.	131,224	12,8	713,0	87,964
Nach „ „	124,774	12,4	686,7	81,931

Demnach enthalten 100 Vol. = 6,858% CO_2 .

3^h bis 5^h 7 Flaschen Wasser getrunken = 6200 C. C.

Summe vom ganzen Tag = 10850 C. C.

Nr. XLVI. 5^h Abends. Heftiges Frieren und Zittern.

Tiefste Inspiration 60". Exspirirt. Vol. = 4101,75 C. C.

Puls 60.

Respiration 17.

Temperatur 36,3.

Zimmertemperatur 13,3.

Barometerhöhe 734,8.

Analyse. Eudiometer b.

	Vol.	Temp.	Druck.	Corrig. Vol.
Vor d. Absorpt.	116,362	12,8	721,0	78,892
Nach „ „	110,382	12,4	695,7	73,454

Demnach enthalten 100 Vol. = 6,893% CO_2 .

²⁷/₁₀ Morgens 8^h. Nüchtern.

Nr. XLVII. 8^h Tiefste Inspir. 60"; Exspir. Vol. = 4564,25 C. C.

Puls 56.

Respiration 12.

Temperatur 35,0.

Zimmertemperatur 12,4.

Barometerhöhe 735.

Analyse. Eudiometer e.

	Vol.	Temp.	Druck.	Corrig. Vol.
Vor d. Absorpt.	102,166	12,8	702,9	67,498
Nach „ „	97,075	12,4	680,7	63,206

Demnach enthalten 100 Vol. = 6,357% CO_2 .

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Analyse. Eudiometer IV.

	Vol.	Temp.	Druck.	Corrig. Vol.
Vor d. Absorpt.	78,610	12,8	720,4	53,237
Nach,, "	73,911	12,4	702,7	49,680

Demnach enthalten 100 Vol. = 6,681% CO₂.

Harnstoffbestimmung.

Zeit.	Harnmenge.	Hg O-lösung.	Prozentgehalt an Harnstoff.	Harnstoffmenge.
²⁵ / ₉ Morgenharn	345			
" Mittagsharn	500	31,0	3,229	60,382 Grs.
" Abendharn	215			
" Morgenharn	810			
In 24 Stunden	1520			ungefähr 50,000 "
²⁶ / ₉ von Morgens 8 ^h bis ²⁷ / ₉ 8 ^h				
In 24 Stunden 12000 C. C.	5,7	0,593		71,160 "
10850 C. C. Wasser getrunken.				
²⁷ / ₉ - ²⁸ / ₉ (kein Wasser).				
In 24 Stunden 1550 C. C.	25,0	2,604		40,362 "
²⁸ / ₉ - ²⁹ / ₉ .				
In 24 Stunden 1550 C. C.	29,6	3,083		47,786 "
²⁹ / ₉ - ³⁰ / ₉ .				
In 24 Stunden 1520 C. C.	34,7	3,614		54,933 "

Die CO₂ spannung in diesem Versuche zeigt ausser einer nur geringen Erhebung während der wahrscheinlich gestörten Verdauung, nichts Bemerkenswerthes; insbesondere lässt sich eine bestimmte Beziehung zu der bedeutenden Temperaturerniedrigung nicht auffinden. In den Fig. 17 (Temperatur) und 18 (CO₂ spannung) sind beide zusammengestellt.

Die Harnstoffbestimmung zeigt in sehr bestimmter Weise, wie durch die grosse Wassermenge, die den ²⁶/₉ getrunken wurde, 11 bis 16 Grammes Harnstoff mehr ausgeführt wurden, als in der Norm, und wie darauf 3 Tage erforderlich waren, um diesen Ausfall allmählig wieder zu ersetzen.

³⁰/₉ Vormittags 9^h bis 9^h 30' ein Infusum hb. Digitalis genommen.

Aus 50 Grammes (= 80 Gran) Pulv. hb. Digital. 218,5 Grammes (= circa 7 Unzen) Aq. destill.

³⁰/₉ Morgens 7^h. Nüchtern.

Nr. XLVIII. Tiefste Inspir. 60'; Exspir. Vol. = 4471,75 C. C.

Puls	68.
Respiration	12.
Temperatur	36,2.
Zimmertemperatur	11,7.
Barometerhöhe	733,3.

Analyse. Eudiometer II.

	Vol.	Temp.	Druck.	Corrig. Vol.
Vor d. Absorpt.	165,771	15,0	703,4	108,52
Nach,, "	157,401	15,0	676,0	100,86

Demnach enthalten 100 Vol. = 7,059% CO₂.Morgens 11^h.

Nr. XLIX. Tiefste Inspir. 60'; Exspir. Vol. = 4471,75 C. C.

Puls	9 ^h 10 ^h 11 ^h
Puls	64 52 48.
Respiration	12 11 16.
Temperatur	36,0 36,0 36,0.
Zimmertemperatur	12,5.
Barometerhöhe	733,0.

Analyse. Eudiometer I.

	Vol.	Temp.	Druck.	Corrig. Vol.
Vor d. Absorpt.	151,70	15,0	704,6	99,478
Nach,, "	144,225	15,0	678,2	92,720

Demnach enthalten 100 Vol. = 6,793% CO₂.Mittags 2^h. Nüchtern.

Nr. L. Tiefste Inspir. 60'; Exspir. Vol. = 4490,25 C. C.

Puls	56.
Respiration	16.
Temperatur	36,4.
Zimmertemperatur	12,8.
Barometerhöhe	732,8.

Analyse. Eudiometer b.

	Vol.	Temp.	Druck.	Corr. Vol.
Vor d. Absorpt.	113,263	15,0	696,7	73,432
Nach,, "	106,560	15,0	677,5	68,433

Demnach enthalten 100 Vol. = 6,796% CO₂.Abends 6^h. Nüchtern.

Nr. LL. Tiefste Inspir. 60'; Exspir. Vol. = 4564 C. C.

Puls	48.
Respiration	16.

Temperatur	35,7			
Zimmertemperatur	12,8			
Barometerhöhe	732,5			
Analyse. Eudiometer IV.				
Vol.	Temp.	Druck.	Corrig. Vol.	
Vor d. Absorpt.	74,540	685,8	15,0	48,258
Nach „	69,360	685,2	15,0	46,050
Demnach enthalten 100 Vol. = 6,647% CO ₂ .				
1/10 Morgens 9 ^h . Nüchtern.				
Nr. LII.	Tiefste Inspir. 60"; Exspir. Vol. 4564 C. C.			
	Puls unregelmässig langsam.			
	Respiration	12.		
	Temperatur	36,1.		
	Zimmertemperatur	12,5.		
	Barometerhöhe	732,6.		

Analyse. Eudiometer d.				
Vol.	Temp.	Druck.	Corrig. Vol.	
Vor d. Absorpt.	111,964	15,0	723,2	75,392
Nach „	105,452	15,0	704,7	70,439
Demnach enthalten 100 Vol. = 6,569% CO ₂ .				

Harnstoffbestimmungen.				
Zeit	Harnmenge.	H ₂ O-	Harnstoff-	Harnstoffge-
50% Morgens 7-9 ^h	270 C.C.	14,0	1,458	3,936 Grs.
(1 Tasse Caffee).				
„ 9-11 ^h	365 „	11,0	1,450	4,179 „
„ 11-2 ^h	290 „	20,0	2,083	6,040 „
„ 2-6 ^h	110 „	33,0	3,437	3,781 „
1/10 Morgens 7 ^h	565 „	19,6	2,031	11,373 „
In 24 Stunden	1595 C.C.			29,309 Grs.

Die Zahlen sind zusammengestellt in Fig. 19 (Pulsfrequenz), Fig. 20 (Temperatur) und Fig. 21 (Kohlensäurespannung).

Den gänzlichen Mangel einer Erhebung des Kohlensäuregehaltes im Blute dürfen wir wohl füglich einer durch die Verminderung der Heizaktion bedingten Beschränkung des Stoffwechsels zuschreiben. Weniger deutlich spricht sich diese in der Harnstoffausscheidung aus; denn die Menge von 29,3 Grammes ist so ziemlich gleich zu setzen der oben während vollständigen Fastens ausgeführten Menge

von 30,6 Grammes; freilich muss auch für die eingeführten 7 Unzen Wasser im letzten Versuche eine proportionale Menge Harnstoff entzogen werden, deren genaue Bestimmung aber erst künftigen Versuchen vorbehalten bleibt.

Wir begnügen uns bis jetzt, die Methode der Bestimmung der Kohlensäurespannung im Blute begründet, und ihre Brauchbarkeit an Beispielen gezeigt zu haben. Weitere Folgerungen aus den mit ihrer Hilfe zu gewinnenden Beobachtungen bleiben billig ausgesetzt, bis wir über eine grössere Reihe von Beobachtungen in verschiedenen Zuständen gebieten können.

Ueber Diffusion.

Von

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Kürzlich habe ich in Poggendorff's Annalen¹⁾ die Resultate meiner bisherigen Versuche über Hydrodiffusion veröffentlicht. Da der Gegenstand auch den Leserkreis dieser Zeitschrift interessirt, so erlaube ich mir denselben in den folgenden Zeilen den Hauptinhalt jener Arbeit auszugsweise vorzuführen. Um sofort die Blicke des Lesers auf das Ziel zu richten, will ich gleich Eingangs bemerken, dass ich, von Brücke's Theorie so zu sagen eingenommen, dieser eine neue Stütze zu geben bestrebt war, indem ich sie mit der Diffusion ohne Dazwischenkunft von Membranen zu verknüpfen suchte.

Zu diesem Zwecke aber war eine Vorarbeit nöthig, nämlich zuerst das Gesetz der einfachen Diffusion oder der Verbreitung eines löslichen Körpers im Lösungsmittel aufzustellen; was in der bekannten verdienstlichen Arbeit von Graham²⁾ versäumt war. Wenn der reine Diffusionsvorgang ungetrübt durch Strömungen, die anderen Ursachen ihre Entstehung verdanken, Statt finden soll, so müssen folgende Bedingungen erfüllt sein: In der ganzen Masse von Lösung muss an allen Punkten einer horizontalen Ebene dieselbe Concentration herrschen, weil sonst kein hydrostatisches Gleichgewicht in der Masse sein könnte; ferner muss jede horizontale Schicht weniger concentrirt als die zunächst unter ihr liegende und mehr concentrirt als die zunächst

1) Jahrg. 1855. Januarheft.

2) Ann. d. Chem. und Pharmazie Bd. 77. S. 56.

über ihr liegende sein, denn mit der Concentration wächst das spez. Gewicht und wenn eine spezifisch schwerere concentrirtere Flüssigkeitsschicht über einer leichteren (weniger concentrirten) läge, so bestände ebenfalls kein hydrostatisches Gleichgewicht (wenigstens kein stabiles). Wir können also kurz sagen, damit in einer Masse von Lösung reine Diffusionströme die vollständige Ausgleichung der Concentration bewirken ohne Dazwischenkunft von rein hydrodynamischen Strömungen durch spez. Gewichtsunterschiede hervorgerufen, muss die Concentration bloss von der Höhe über dem Boden abhängen und zwar näher mit dieser Höhe stets abnehmen (natürlich nicht etwa nothwendig der Höhe geradezu proportional), die Abnahme braucht nicht einmal stetig zu sein, es kann z. B. auf einer absolut gesättigten Salzlösung unmittelbar reines Wasser liegen. Es fragt sich jetzt, nach welchem Gesetze findet die Ausgleichung statt, d. h. wie hängt der Austausch zwischen benachbarten Schichten von den variablen Umständen ab. Wir wollen die Concentration einer Lösung allemal die Anzahl von Gewichtseinheiten des gelösten Körpers nennen, welche in der Volumeinheit der Lösung enthalten ist, und wollen übrigens dabei absehen von der Contraction, welche bei Mischung ungleich concentrirter Lösungen regelmässig eintritt. Ist nun k die Concentration einer Lösungsschicht, k' die der nächst höher liegenden (also weniger concentrirten), ist der Abstand dieser beiden Schichten voneinander d — der unendlich klein zu denken ist, wobei dann aber auch der Konzentrationsunterschied $k - k'$ in der Regel unendlich klein ist — so geht während eines Zeitraumes θ , während dessen die Concentration beider Schichten constant bleibt, durch die Flächeneinheit eine Menge des gelösten Körpers aus der concentrirteren Schicht in die weniger concentrirte, welche ausgedrückt werden kann durch $C \cdot \frac{k - k'}{d} \cdot \theta$, wo C eine bloss von der Natur der Stoffe abhängige Konstante bedeutet. Diese kann noch so definiert werden dass man sagt: sie bedeutet die Menge des fraglichen gelösten Stoffes, welche durch die Flächeneinheit diffundirt während der Zeiteinheit, wenn während dieser die constante Konzentrationsdifferenz der beiden um d voneinander abstehenden Schichten erhalten wird, welche zu eben jenem d ein Verhältniss = 1 hat; wobei beide Grössen

als absolute Zahlen durch eine der Grössengattung entsprechende Maasseinheit ausgedrückt zu denken sind. Ist noch Q die Anzahl von Flächeneinheiten, welche die ganze Oberfläche der beiden Schichten umfasst, so hat man die ganze von der unteren zur oberen während des Zeitraums ϕ gelieferte Salzquantität $= Q \cdot C \cdot \frac{k-k'}{d} \cdot \phi$. Natürlicherweise wird eine an Volum gleiche Menge des Lösungsmittels ¹⁾ gleichzeitig von der weniger concentrirten Schicht an die concentrirtere abgegeben.

Ausgerüstet mit diesem Grundgesetz wollen wir einen speziellen Fall näher ins Auge fassen. Denken wir uns einen an beiden Enden offenen Zylinder A mit seinem unteren Ende eingekittet in ein Salzreservoir B , so dass die untere Endfläche desselben gewissermassen aus Salz gebildet ist.

Der Zylinder selbst soll mit Lösung angefüllt sein, deren Concentration von absoluter Sättigung, die am Boden Statt hat, nach oben hin der Höhe proportional abnimmt, so dass sie an der oberen Endfläche $= 0$ ist. Der ganze Apparat sei nun eingesenkt in ein grosses mit reinem Wasser gefülltes Gefäss C . Ist die Wassermenge des letzteren so gross dass man berechtigt ist anzunehmen die aus dem Zylinder heraus diffundirenden Salzquantitäten alteriren seine Eigenschaft als reines Wasser in mässigen Zeiträumen nicht wesentlich, so hat man eine Veranstaltung bei welcher in einem Gefäss A zwei Schichten beständig auf constanter Concentration erhalten werden, denn am Boden muss wegen der Berührung mit den vorrätigen Salzkristallen fortwährend absolute Sättigung statt haben, in der obersten Schicht muss wegen der Berührung mit dem reinen Wasser des Gefässes C fortwährend die Concentration gleich Null sein. Bei der gedachten ursprünglichen Anordnung der Concentrationen wird nun in A der Diffusionsstrom stationär sein oder es wird, wie man sich wohl auch auszu-



Fig. 1.

¹⁾ Wir wollen im Folgenden immer kurz an Kochsalz und Wasser denken und demgemäss den Ausdruck abkürzen.

drücken pflegt, ein dynamisches Gleichgewicht Statt haben d. h. es wird durch den Diffusionsstrom die Concentration jeder einzelnen Schicht nicht alterirt, sie bleibt zu jeder Zeit dieselbe. Nimmt man nämlich an einer beliebigen Stelle des Zylinders A drei aufeinanderfolgende aequidistante Schichten, so sind auch die beiden Concentrationsdifferenzen der ersten und zweiten, sowie der zweiten und dritten einander gleich nach der Voraussetzung, dass die Concentrationen den Höhen proportional abnehmen. Folglich sind aber auch nach unserem Grundgesetze die in einem sehr kleinen Zeittheilchen d ausgetauschten Stoffmengen einander gleich d. h. also nach Verfluss von d hat die mittlere Schicht von ihrer unteren Nachbarin ebensoviel Salz aufgenommen als sie an ihre obere abgeben musste, die untere bekam aber ihren Verlust aus demselben Grunde von ihrer unteren Nachbarin genau ersetzt und sofort bis zur Bodenschicht die ihre vollständige Sättigung durch Berührung mit dem festen Salze erhielt, indem sie ein gerade so grosses Quantum davon löste, als sie an ihre Nachfolgerin abgab; wenn aber eine Lösungsschicht ebensoviel Salz aufnimmt als abgibt, so bleibt ihre Concentration ungeändert. Alle Schichten in A haben also nach Ablauf der Zeit d noch genau die Concentration, welche sie bei Beginn derselben hatten. Dasselbe wiederholt sich in allen folgenden Zeitabschnitten, so dass unsere Behauptung damit gerechtfertigt ist: Bei der Anordnung, die Fig. 1 darstellt, bleibt die Concentration in allen Schichten zu allen Zeiten dieselbe. Es ergibt sich aber aus der ganzen Betrachtung unmittelbar noch das Resultat, dass die Salzmenge welche einen Querschnitt durchwandert, gerade so gross ist als die, welche einen beliebigen anderen in derselben Zeit passiert, also namentlich auch ebenso gross als die Salzmenge, die in der nämlichen Zeit den oberen Endquerschnitt des Rohres A durchsetzt und in das Wasser des Gefässes C übertritt. Ueberdies müssen die Salz mengen, welche während zweier verschiedener Zeitabschnitte austreten, sich verhalten wie diese Abschnitte selbst, denn in jedem unendlich kleinen Zeitabschnitte, sahen wir ja, was die Salz beförderung dieselbe. Wir haben dadurch ein Mittel in der Hand, die oben mit C bezeichnete Konstante für irgend eine Körperkombination z. B. Kochsalz und Wasser zu bestimmen. Denn messen wir die Quantität von Salz, welche während der Zeit

einheit aus der Röhre ins Wasser antrat und finden sie = a , so haben wir eine Grösse die gleich sein muss der Salzmenge, welche während der Zeiteinheit zwischen zwei Schichten von gegebener Oberfläche (dem Querschnitt von A) ausgetauscht wurde, deren Concentrationen konstant waren und sich um eine Differenz unterschieden, deren Verhältniss zu ihrem Abstände ebenfalls ein bekannter ist. Denn da die Abnahme der Concentrationen der Höhe proportional war, so muss die Concentrationsdifferenz zweier benachbarter Schichten sich zu ihrem Abstände gerade so verhalten, wie die Differenz der Concentration der Boden- und Oberflächenschicht sich verhält zu deren Abstände d , h. zur Länge des ganzen Rohres A . Nennen wir letztere I , so hat man das Verhältniss $\frac{k-k'}{d}$, was in den Ausdruck für das ausgetauschte Salzquantum eingeht = $\frac{a}{I}$, wo s die absolute Sättigung entsprechende Concentration bedeutet; der Subtrahend in der den Zähler ausmachenden Differenz ist nämlich = 0 (= der Concentration am obern Endquerschnitte von A). Man hat also zu setzen die gemessene Grösse $a = QC \frac{a}{I}$, wo Q den Querschnitt von A in Flächenmaass ausgedrückt bezeichnet; aus dieser Gleichung, in der alle übrigen Grössen bekannt sind, kann C gefunden werden.

Ich habe nun für Kochsalz und Wasser C aus einer Reihe von Versuchen bestimmt und zwar bestand jeder Versuch aus dreien die sich gegenseitig kontrollirten; es wurden nämlich allemal gleichzeitig 3 Röhren von verschiedener Länge hingestellt und die 3 Salzquanta gemessen die sie geliefert hatten. Beifolgende Tabelle giebt die Resultate dieser Versuche.

Temperatur während des Versuches.	C berechnet aus der vom längsten Rohre gelieferten Salzmenge.	C berechnet aus d. vom mittleren Rohre gelieferten Salzmenge.	C berechnet aus d. vom kürzesten Rohre gelieferten Salzmenge.
nicht bestimmt.	11,71	12,36	11,08
15,8—14,8 ⁰	9,67	9,7	9,3
15,5—16,		9,57	
16 —16,5		9,94	
17,5—18,5	10,97		
18 —19	10,71	11,08	10,50
20	11,14		11,02
19 —22	11,44	11,33	
20 —21	11,89		11,12

Ueber die diesen Zahlen zu Grunde gelegten Maasseinheiten ist zu bemerken, Zeiteinheit: 1 Tag, Längeneinheit: 1^m, Flächeneinheit: Oberfläche eines Kreises von 10^{mm} Radius, Concentrationseinheit: absolute Sättigung. Man sieht aus der Tabelle sofort was aus den Graham'schen Versuchen schon geschlossen werden konnte, dass die Konstante c von der Temperatur abhängt und mit dieser wächst.

Man könnte vielleicht noch fragen, wie ich dahin gelangte, in den Röhren die vorausgesetzte Anordnung der Concentrationen hervorzubringen. Nichts ist leichter als dies; denn wenn man Anfangs- und Endquerschnitt in der beschriebenen Weise auf constanter Concentration erhält, so stellt sich die gewünschte Anordnung und der stationäre Strom mit der Zeit von selbst her, mag die Anordnung der Concentrationen sonst im Anfang gewesen sein welche sie wolle, wovon man sich durch einfache Ueberlegungen überzeugen kann. Man braucht also nur ein Paar Tage den Apparat sich selbst zu überlassen, indem man nur durch Wechseln dafür sorgt, dass die Flüssigkeit im äusseren Gefässe immer reines Wasser bleibt; und kann dann sicher sein, die verlangte Anordnung zu haben.

Wir wollen nun sehen, ob wir von unserem Grundgesetze der Diffusion Gebrauch machen können zur Orientirung in den Fragen, die sich bei der Diffusion durch Membranen erheben. Brücke¹⁾ hat zuerst eine Hypothese über diesen Vorgang ausgesprochen, die hernach von Ludwig weiter ausgebildet und mit Versuchen gestützt wurde. Sie lässt die Diffusion durch Poren zwischen ganzen Massen von Membranmolekülen, nicht durch die Molekularinterstitien vor sich gehen und besteht wesentlich in Folgendem. Zwischen den Theilchen einer porösen Membran und dem Wasser besteht im Allgemeinen eine stärkere Anziehung, als zwischen Membran und Salztheilchen, daher muss an der Wand des Porus eine Schicht reinen Wassers sich befinden und in der Mitte ein Faden von Lösung, dessen Concentration mit der der umspülenden Flüssigkeit übereinstimmt. In der That hat Ludwig²⁾ durch Experimente erhärtet, dass die Tränkungsflüssigkeit einer Membran reicher an Wasser und ärmer an Salz ist

1) Pogg. Ann. Bd. 58. S. 77.

2) Pogg. Ann. Bd. 78. S. 307. auch in dieser Zeitschrift.

als die Lösung mit welcher sie getränkt wurde. Sind nun auf beiden Seiten der Membran verschieden concentrirte Lösungen, so geht ein Diffusionsstrom durch den centralen Flüssigkeitsfaden, der Salz nach der einen und Wasser nach der andern Seite schafft. Durch die Wandschicht kann bloss Wasser von der dünneren zur dichten Lösung treten, somit wäre das Phänomen erklärt, dass mehr Wasser zur dichten, als Salz zur dünneren Lösung geht.

Weitere Folgerungen waren bisher noch nicht aus Brücke's Anschauung gezogen; um dies zu können muss die Vorstellung noch mehr detaillirt werden. Denken wir uns der Einfachheit wegen die Poren zylindrisch und die Membran ganz in eine Lösung von überall gleicher constanter Concentration eingetaucht, dann wird, wenn in der That die Wassertheilchen stärker von der Porenwand angezogen werden, als die Salztheilchen, alsbald eine Vertheilung der Concentrationen innerhalb des zylindrischen Porus Platz greifen, vermöge deren in Zylindermänteln der Porenwand parallel allemal eine und dieselbe Concentration Statt hat, dass diese aber von einem zum andern, von der Wand gegen die Axe hinzunimmt, so dass unmittelbar an der Wand reines Wasser und in der Axe des Porus die concentrirteste überhaupt innerhalb desselben mögliche Lösung sich findet, die aber möglicherweise die Concentration der umgebenden Flüssigkeit noch nicht erreicht.

Man stelle sich nunmehr eine solche Membran in horizontaler Lage vor, über derselben gesättigte Salzlösung, unter derselben reines Wasser. Wir denken uns in derselben wieder einen beliebigen zylindrischen Porus, sein Radius wie der aller Poren der Membran, das sei hier noch erinnert, muss aber jedenfalls so klein sein, dass ein Durchfiltriren von Flüssigkeit durch deren Kohäsion in Verbindung mit der Anziehung der Membran selbst auch bei hohem Drucke verhindert wird, und dass auch Ausgleichung der Druckdifferenzen, wenn auf beiden Seiten der Membran Flüssigkeit befindlich, wo überhaupt, wenigstens nur in unverhältnissmässig langen Zeiträumen möglich ist, und dass endlich auch Mischungsströme durch spezifische Gewichts-differenzen innerhalb des Porus nicht vorkommen können. Nach den obigen Vorstellungen wird nun auch hier in jeder der Porenwand concentrischen ringförmigen Elementarschicht von sehr geringer Dicke, der Hergang gleichartig sein. Wir fas-

sen daher eine solche ins Auge und sehen in der Fig 2

ab und $a'b'$ die beiden Durchschnitte mit einer durch die Axe des Porus gelegten Ebene, die diesen selbst im Rechteck $ac\beta\delta$ schneidet. $ac\beta$ wäre also die Dicke der Membran und oberhalb hätten wir gesättigte Salzlösung, unterhalb reines Wasser.

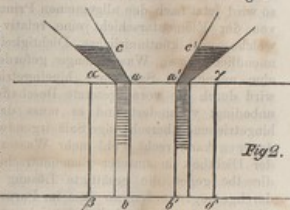


Fig 2.

Wäre die Membran ganz in gesättigte Salzlösung eingetaucht so würden nach unseren obigen Annahmen in unserer ganzen Schicht eine gewisse Concentration statt haben, jedenfalls kleiner als die vollständige Sättigung; diese wird nun im gegenwärtigen Falle bloss am oberen Ende bei a und a' Platz greifen, während am unteren Ende bei b und b' nothwendig die Concentration auf Null erhalten wird wegen der Berührung mit dem reinen Wasser. Offenbar stellt sich alsbald innerhalb der Elementarschicht ganz wie in dem vorhin beschriebenen Zylinder A ein stationärer Diffusionsstrom her mit einer gleichförmigen Abnahme der Concentrationen von oben nach unten, wovon die immer matter werdende Schraffirung zwischen a und b sowie zwischen a' und b' eine Anschauung geben soll. Dieser Diffusionsstrom schafft gleiche Quantitäten Salz und Wasser beziehlich nach unten und nach oben. Berechnete man die Salzquantitäten, welche vermöge dieses Stromes durch jede ringförmige Elementarschicht befördert werden, so hätte man in der That den genauen Ausdruck für die gesammte von einem Porus gelieferte Salzquantität. Eine besondere Betrachtung erfordert aber noch der Wasserübergang nach oben. Wir sahen nämlich soeben, dass die bei a und a' statt findende Concentration im Allgemeinen noch um eine endliche Grösse von vollkommener Sättigung verschieden sei. Es würde demgemäss hier ein plötzlicher Sprung in der Aufeinanderfolge der Concentrationen eintreten, da eben auf der Oberfläche der Membran eine (durch eingelegte Krystalle) relativ unerschöpfliche Menge gesättigter Salzlösung aufliegt. Nehmen wir

an, dies sei in der That für den ersten Augenblick der Fall, so wird jetzt nach den allgemeinen Principien der Diffusion¹⁾ von der Elementarschicht eine relativ (gegen die Menge, welche ein kontinuierlicher Dichtigkeitsübergang fordert) unendlich grosse Wassermenge gefordert und andererseits eine unendliche Salzmenge hineingetrieben. Das letztere wird durch die vorausgesetzte Beschaffenheit der Membran unbedingt verhindert und es muss das gegen den Porus hingetriebene überschüssige Salz irgendwie seitlich abgleiten; dagegen kann recht wohl mehr Wasser, als die Anordnung der Dichtigkeit in unserer Elementarschicht verlangt, durch dieselbe gegen die gesättigte Lösung hin gewissermassen durchgesaugt werden, so dass im Porus die Wassertheilchen mit grösserer Geschwindigkeit sich aufwärts bewegen als die Salztheilchen abwärts. Die überschüssige Wassermenge verbreitet sich nun in der gesättigten Flüssigkeit allseitig (da die Porenöffnungen in gewissen Entfernungen von einander liegen müssen) theils durch Diffusions- theils durch Mischungs-Strömungen wegen der spezifischen Gewichtsdivergenz. Es wird sich so allmählig auch hier ein stationärer Zustand herstellen, dadurch, dass sich auf den ringförmigen obern Endquerschnitt der Elementarschicht ein nach oben konisch erweiterter Raum stützt (sein Durchschnitt ist bei a e und a' e' in der Figur gezeichnet) in welchem die Dichtigkeit von der bei a und a' konstant bleibenden bis zur vollkommenen Sättigung stetig nach oben zunimmt, wie die Figur durch die nach oben immer schräger werdende Schraffirung andeutet. Die Anordnung der Dichtigkeiten in diesem Räume muss einen Diffusionsstrom von solcher Intensität bedingen, dass derselbe genau so viel Wasser nach oben schafft, als sich gerade in derselben Zeit von seinem oberen Ende in das Reservoir gesättigter Lösung verbreiten kann, ohne die Concentration zu alteriren. Denn offenbar würde der gedachte Raum sich sofort nach oben verlängern

1) Ein Sprung in der Ab- oder Zunahme der Concentrationen heisst ja nichts anders, als dass in zwei unendlich nahe aneinanderliegenden Schichten, endlich verschiedene Concentrationen statt haben, so dass in dem Ausdruck $c \cdot \frac{k-k'}{d}$ der Zähler $k-k'$ einen endlichen Werth hat, während d unendlich klein bleibt. Wodurch der Werth des ganzen Ausdrucks, der die ausgetauschten Salz- und Wassermengen misst, unendlich gross wird.

(und dadurch die Intensität des Diffusionsstroms verringert werden) sobald mehr Wasser hindurchginge und so die Concentration am obern Ende des Raumes noch alterirt würde, (es wird ja immer der Raum gerade bis dahin gerechnet, wo die absolute Sättigung der obern Lösung eintritt); umgekehrt ginge weniger Wasser nach oben, so müssten sofort Sprünge in den Concentrationsübergängen an gewissen Stellen eintreten, die gleichsam wieder einen unendlich starken Diffusionsstrom bedingen würden und so augenblicklich wieder die gehörige Menge von Wasser ansaugten. Nun hängt aber diese Wassermenge, welche sich ceteris paribus während der Zeiteinheit in der gesättigten Lösung verbreiten kann, ohne an der Stelle die Concentration noch merklich zu alteriren, ab von der leichten Beweglichkeit der Theile der Lösung. Es muss also auch ceteris paribus der Raum, in welchem die Ausgleichung geschieht, um so kürzer und deshalb der Strom des Wassers nach der gesättigten Lösung hin um so stärker sein, je leichter beweglich die Theilchen der Lösung sind, während durch diesen Umstand die diffundirende Salzmenge nicht afficirt wird. Wir ziehen somit die erste Folgerung:

1. Je beweglicher die Theilchen der gesättigten Lösung sind, desto mehr Wasser im Verhältniss zum Salze geht über d. h. desto grösser ist die nach Jolly's Vorgang mit dem Namen des endosmotischen Aequivalentes belegte Grösse.

Der Einfluss der stärkeren Anziehung der Porenwand zum Wasser auf die mögliche Lösungs-dichtigkeit an irgend einer Stelle nimmt natürlich mit wachsender Entfernung ab und man kann sich daher offenbar einen Porus so weit denken, dass er in seinen centralen Partien bereits ganz verschwunden ist. In thierischen Membranen müssten sie sogar in der That höchst wahrscheinlich soweit angenommen werden, denn nach den oben angeführten Versuchen von Ludwig ist die Imbibitionsfähigkeit derselben einer sehr hohen Concentration fähig, die doch ein Mittel aus den niedrigeren Concentrationen der Wandschichten und der noch höheren der centralen sein muss. Wenn aber dies wirklich der Fall ist, so etablirt sich so weit um die Axe des Porus herum, als die Möglichkeit vollständiger Sättigung reicht, ein Diffusionsstrom, wie in der oben beschriebenen einfachen Röhre A , der dem Volum nach gleich viel Wasser nach oben und Salz nach unten schafft. Das Plus von

Wasser wird alsdann durch die der Wand näher gelegenen Schichten allein bedingt, für welche unsere besondere Betrachtung (siehe Fig. 2) gültig bleibt. Es wird also in solchen Fällen offenbar nicht soviel Wasser im Verhältnisse zum Salze übergehen, als wenn die Poren so eng sind, dass darin nirgendwo gesättigte Lösung existiren kann und folglich in der ganzen Ausdehnung des Porus mehr Wasser als Salz übergeht. Wir ziehen somit die zweite Folgerung:

2. Das endosmotische Aequivalent muss für eine Membran mit engen Poren grösser sein, als für eine solche mit weiteren. Auf diesen Umstand hat schon Ludwig aufmerksam gemacht.

Denken wir uns jetzt unterhalb der Membran statt reinen Wassers ebenfalls eine Lösung des Salzes, das oberhalb der Membran in gesättigter Lösung vorhanden ist, dann werden eine gewisse Anzahl von Elementarschichten, die der Wand näher gelegen sind, für den Salzstrom ganz unthätig gemacht, nämlich alle diejenigen in welchen vermöge der Beschaffenheit der Membran keine höhere Concentration möglich ist als die der Lösung unterhalb der Membran. Das mögliche Maximum wird nämlich in der ganzen Ausdehnung einer jeden Platz greifen, Wasser wird aber immer noch durch dieselben von der dünneren zur dichteren Lösung übertreten, denn an beiden Enden einer jeden wird wegen der Concentrationsdifferenz eine Saugkraft wirksam sein, welche an dem oberen Ende offenbar stärker ist, denn hier ist die wirksame Concentrationsdifferenz der ganze Abstand von vollkommener Sättigung bis zu dem in der betreffenden Elementarschicht möglichen Maximum, während am unteren Ende der Unterschied zwischen letztgedachtem Maximum und der Concentration der unteren Lösung offenbar kleiner ist. Alle mehr nach der Axe des Porus gelegenen Schichten verhalten sich ganz in der zuerst beschriebenen Weise mit dem einzigen Unterschiede, dass in ihnen die Concentration statt von Null an, jetzt von dem in der unteren Lösungsmasse herrschenden Concentrationsgrade an bis zu den respectiven Maximis von unten nach oben wächst, und somit ein absolut schwächerer Diffusionsstrom zu Stande kommt. Insbesondere könnte noch (bei sehr engen Poren am leichtesten) der Fall vorkommen, dass die gedachte Concentration der unteren Lösung grösser als das selbst im Centrum des Porus mögliche Dichtigkeitsmaximum

wäre; dann würde das soeben von den Wandschichten ausgesagte auf den ganzen Porus auszudehnen sein und es würde dann der ganze Vorgang in einem blossen Wasserübertritt von der dünneren zur dichteren Lösung bestehen. Da jedenfalls hier an vielen Stellen, die in dem zuerst entwickelten Falle einem zweiseitigen Diffusionsstrom Durchgang gestatteten, im gegenwärtigen nur ein einseitiger Wasserübergang Platz greift, so können wir die Folgerung ziehen:

3. Hat man auf der oberen Seite der Membran gesättigte Lösung, auf der unteren eine Lösung desselben Salzes von der Concentration c , so müsste das endosmotische Aequivalent um so grösser ausfallen je grösser c ist und möglicherweise $= \infty$ werden.

Denken wir uns jetzt noch den Fall, dass unter der Membran wieder reines Wasser befindlich sei, über derselben aber eine Salzlösung, deren Concentration $= c$ (nicht vollständige Sättigung) sein mag. Wir wollen uns c so klein denken, dass mindestens die in einigen mittleren Schichten des Porus möglichen Dichtigkeitsmaxima grösser als c sind. Durch alle diese wird alsdann ein gewöhnlicher Diffusionsstrom gehen, der gleiche Volume Salz und Wasser nach entgegengesetzten Richtungen befördert, ohne dass sich jener nach oben erweiterte konische Raum bildet durch welchen noch eine überschüssige Wassermenge angesaugt wird (denn es fällt der Grund für dieses Raumes Bildung, der plötzliche Sprung in der Dichtigkeitsabnahme weg). Offenbar wird aber die Anzahl von Elementarschichten, die somit nicht mehr Wasser als Salz liefern, desto grösser, je kleiner c ist und man kann den Satz ableiten:

4. Befindet sich unter der Membran Wasser, über derselben Lösung von der Concentration c , so müsste das endosmotische Aequivalent mit c gleichzeitig rasch abnehmen, möglicherweise bis zum reciproken Werthe des spezifischen Gewichtes des Salzes.

Die vier numerirten Folgerungen suchte ich nun durch Experimente zu bewähren, die nach demselben Plane angestellt werden, wie die bekannten von Ludwig und Clotetta, ich brauche daher über die Methode gar nichts hinzuzufügen. Namentlich glaubte ich aber Nro. 1. zu einem Experimentum crucis für die Porentheorie ausbeuten zu können. Ich verminderte nämlich durch eingemengte feste Theilchen — Kreidepulver — die zu einer Art von Schlamm

mit Salzkristallen und Wasser angerührt wurden, die Beweglichkeit der oberen Lösung. Das endosmotische Aequivalent, das für die angewandte Membran sonst zwischen 5 und 6 lag, hielt sich aber auch bei der erwähnten Abänderung des Versuches zwischen denselben Grenzen.

Was No. 2 betrifft, so gestattet dieser Satz streng genommen keine direkte experimentelle Prüfung, denn man wird schwerlich hoffen dürfen, dass man zwei Membranen wird herstellen können, die sich lediglich durch die Porenweite unterscheiden. Inzwischen ist wohl die Vermuthung begründet, dass in einer strukturlosen glashellen Kollodiummembran die Poren in so kolossalem Verhältniss enger sein werden, als in einer thierischen, dass alle andern Unterschiede gegen diesen einen geringfügig sind. Solche Kollodiumhäute sind begreiflicherweise ein sehr verführerisches Objekt und habe ich mich lange Zeit hindurch mit Diffusionsversuchen an ihnen beschäftigt, bin aber leider so zu sagen zu keinem Resultate gekommen. Sind nämlich diese Häute dünn genug um einen namhaften Diffusionsstrom zu Stande kommen zu lassen, so sind sie gleichzeitig so zerbrechlich dass es selten gelingt zwei vergleichbare Versuche mit derselben Membran anzustellen. So viel glaube ich behaupten zu können, dass bei Kollodiumhäuten das endosmotische Aequivalent nie kleiner als 20 war, meist gingen nur Spuren von Salz über, und öfters konnte ich auch nicht einmal solche wahrnehmen, während namhafte Wasserquanta hindurchgetreten waren. Fast möchte ich glauben dass hier der einseitige Wasserdurchtritt normal ist und das in manchen Versuchen gefundene Salz auf andern Wegen als durch die Membran gewandert war (zwischen Klebstoff und Glaswand). Trennte ich durch eine Kollodiumhaut Lösungen von Chlorbarium und schwefelsaurem Natron, so zeigte sich in den meisten Fällen keine Spur von Fällung, zum Beweis, dass ebenfalls nur Wasser ausgetauscht wurde.

Die zur Prüfung des dritten der aufgestellten Sätze gemachten Versuche gaben ein in hohem Grade positives Resultat. Zwei Membranen deren endosmotisches Aequivalent für eine Kombination von reinem Wasser mit gesättigter Kochsalzlösung zwischen 5 und 6 lag, gaben beträchtlich höhere Werthe dieser Grösse, wenn das reine Wasser unter der Membran durch eine Lösung von der

Concentration 0,22¹⁾ ersetzt wurde. Es stieg das Aequivalent in einem solchen Falle auf 11,05, in einem andern sogar auf 17,05.

Der Satz 4. bewährte sich durch meine Versuche entschieden nicht, was mich um so mehr in Erstaunen setzte, als unter den Versuchen von Ludwig und Cloetta viele vorkommen, die ein positives Resultat höchst wahrscheinlich machen mussten. Der Widerspruch zwischen meinen und jenen Versuchen ist mir annoch ganz räthselhaft.

Es scheint mir aus dem Mitgetheilten nur so viel mit Sicherheit zu erschliessen dass die Porentheorie jedenfalls beträchtlicher Modifikationen bedarf. Was eine kritische Diskussion der angeführten Thatsachen betrifft, so will ich dem Leser nicht vorgreifen und spreche nur zum Schluss die Hoffnung aus, dass wenigstens eine auf physikalische Analyse des Hergangs nach einer bestimmten Hypothese gegründete Fragestellung, wie sie hier angestrebt wurde, die Sache um etwas weiter zu fördern geeignet ist.

1) d. h. 0,22 Gewichtstheile Salz waren in 1 Gewichtstheil Lösung enthalten.

Die Verbindung des Handgriffes mit dem Körper des Brustbeines.

Von

Prof. H. Luschka in Tübingen.

Hierzu Taf. VIII.

Wenn es erlaubt ist aus den in der Literatur über diese Knochenverbindung niedergelegten Ansichten einen Schluss zu ziehen, dann hat man wohl Grund daran zu zweifeln, dass sie jemals, wenigstens was den feineren Bau betrifft, zum Gegenstande einer sorgfältigern Untersuchung gemacht worden ist. Von den ältern Angaben, welchen eine scharfe Charakteristik der Gewebe im heutigen Sinne nicht zu Grunde liegt, ganz abgesehen, begegnen wir aber auch bei den Schriftstellern der Gegenwart durchaus keinen befriedigenden Aufschlüssen. Um statt vieler, ähnlich lautender Angaben nur Einer zu gedenken, so finden wir bei D'Alton¹⁾ die Bemerkung, „zwischen Handhabe und Körper des Brustbeines treffe man bei Erwachsenen gewöhnlich eine Knorpelschicht oder wirklichen Faserknorpel.“

Während die meisten Beobachter, eine, sei es durch was immer für ein Gewebe vermittelte, solide Verbindung jener Knochenstücke untereinander erkennen, will dagegen Maisonneuve²⁾ in der Mehrzahl der Fälle einen gegliederten Zusammenhang derselben aufgefunden haben. Das Bestreben, diese Widersprüche auszugleichen, ist für die Lehre Barkow's³⁾ bezeichnend, wenn er berichtet, „die Handhabe des Brustbeines und der Körper werden durch eine Bandmasse vereint, die beim Erwachsenen in der Regel aus Knorpel, seltener Faserknorpel, zuweilen aus blossem Fasergewebe bestehe; zuweilen zeige sich selbst eine Hemiarthrose zwischen jenen Knochen.“

1) Handbuch der menschl. Anatomie. I. Band. 1850 S. 83.
2) Archives générales. Paris 1842. p. 233.
3) Syndesmologie 1841. S. 38.

Die hier in Frage stehende Natur der genannten Knochenverbindung verdient unsere besondere Aufmerksamkeit nicht allein vom praktischen Gesichtspunkte aus, in Rücksicht auf die an ihr nicht selten vorkommenden Dislocationen, sondern es knüpft sich an ihre Betrachtung auch insofern ein bedeutendes morphologisches Interesse, als wir von ihr aus manche die Entwicklungsgeschichte der Gelenke betreffende Aufklärung gewinnen können.

Bei Untersuchung einer grössern Anzahl von Brustbeinen des Menschen aus verschiedenen Altersstufen überzeugt man sich alsbald davon: dass die zwischen Handhabe und Körper befindliche Verbindung sich anders verhalte beim Neugeborenen und im frühern Kindesalter, anders beim Erwachsenen, und dass bei diesem sehr mannigfaltige Abweichungen vom normalmässigen Typus statt finden.

Beim Neugeborenen ist es eine weissliche, schon für das blosse Auge deutlich gefaserte, lockere, 1 Millimeter hohe Substanz, welche einerseits die den Knochenkern des Handgriffes und jenen des ersten Körperstückes umgebende Knorpelmasse verbindet, und andererseits zwischen den die Gelenkfacetten der Sternalenden des zweiten Rippenpaares trennenden Kanten ausgebreitet ist. Dem feineren Baue nach besteht diese Fasersubstanz vorwiegend aus feinen elastischen Fibrillen, welche der Quere und der Länge nach sowie in bogenförmiger Richtung verlaufen. Ausserdem gewahrt man feingefaserte Bindesubstanz und spindelförmige, sehr schmale, der Entstehung jener elastischen Elemente zu Grunde liegende Zellen, aber keine Spur von Knorpelkörperchen.

Ausser zwischen Handhabe und Körper des Sternum findet sich am Brustbein des Neugeborenen eine derlei Fasermasse nur noch zwischen dem Schwertknorpel und dem über ihm befindlichen, von Knorpelmasse umgebenen Knochenkern, nicht aber zwischen den übrigen in ihrer Peripherie noch knorpeligen Stücken, welche im Gegentheile durch eine ganz hyaline Masse unter sich in Verbindung stehen. Es verdient gekannt zu sein, dass bei manchen Thieren, wie beim Hunde, durch das ganze Leben hindurch nicht allein Handgriff und Körper des Brustbeins, sondern auch die übrigen in grösserer Anzahl vorhandenen Stücke des Brustbeines durch echte Knorpelscheiben zusammengehalten sind.

Noch beim achtjährigen Kinde habe ich ähnlich wie beim Neugeborenen eine von der hyalinen Knorpelsubstanz der unteren Fläche des Manubrium und der oberen des Corpus sterni durch die weisse Färbung und durch das lockere Gefüge scharf absteckende und zum zweiten Rippenpaare im gleichen Verhältnisse stehende Substanz vorgefunden, welche aber verhältnissmässig viel weniger elastische Elemente, dagegen mehr Bindesubstanz und vor Allem im Gegensatz zu Neugeborenen zahlreiche kleinere Knorpelzellen enthielt.

Beim erwachsenen Menschen ist es, der vielfachen und häufigen Variationen wegen, nicht gerade leicht das Gesetzmässige dieser Knorpelverbindung herauszufinden, was denn auch ganz mit den abweichenden Angaben der Schriftsteller bezüglich der hier waltenden Regel im Einklang steht.

Nach dem, was mir bei Menschen in den Blütenjahren des Lebens am häufigsten zur Beobachtung kam, muss ich als Norm betrachten, dass die Verbindung der Handhabe mit dem Körper des Brustbeines realisiert werde: durch eine vier Millimeter hohe, aus zwei hyalinen Knorpelplatten und einer zwischen diesen befindlichen Faserknorpelmasse bestehende Scheibe.

Bei der Betrachtung scharfer senkrechter Durchschnitte mit unbewaffnetem Auge, vermag man häufig keinerlei Abgrenzung des Gewebes zu unterscheiden, indem das Ganze fast ein gleichförmiges, bläulichweisses Ansehen darbietet, und nur bei Bewegungsversuchen die mittlere Substanz durch ihre leichtere Verschiebbarkeit eine grössere Lockerheit zu erkennen gibt. Anderemale aber fällt es dem ersten Blicke auf, dass eine mittlere, deutlicher abgegrenzte, meist ein Millimeter hohe, saftigere, weisse oder blass gelbliche Substanz zwischen zwei Scheiben echten Knorpels eingeschlossen ist.

Von den Scheiben der hyalinen Knorpelsubstanz, von welchen jede $1\frac{1}{2}$ Millimeter hoch ist, gehört die eine dem Handgriff, die andere dem oberen Ende des Corpus sterni an. Die beiden Scheiben treten jederseits da, wo der Sinus costalis für das zweite Rippenpaar beginnt, unter spitzem Winkel auseinander, und setzen sich, die Gelenkknorpel jenes Ausschnittes darstellend, ohne Unterbrechung in ihm fort. Die Substanz dieser Knorpelscheiben differirt von andern echten Knorpel nicht, nur muss bemerkt werden, dass

in der Nähe seines Ueberganges in die mittlere, gefaserte Substanz, eine Anzahl grösserer Mutterzellen eingelagert sind.

Die mittlere oder Fasersubstanz ist zunächst dadurch auffallend, dass sie die Sternalenden des zweiten Rippenpaares unter einander verbindet. Man muss wissen, dass das Sternalende des Knorpels der zweiten Rippe, in der Art geformt ist, dass zwei Gelenkflächen unter Bildung einer scharfen, das äusserste Ende darstellenden Kante gegeneinanderlaufen. Diese Kante nun ist es an welche sich das Ende jener Faserung jederseits als Lig. sternocostale interarticularis (Krause) befestigt, und so eine Scheidung des Sinus costalis in zwei übereinander liegende Gelenkhöhlen vermittelt. Von diesem Verhältnisse zur zweiten Rippe findet aber häufig auf beiden oder auch nur auf einer Seite die Abweichung statt, dass der Knorpel nicht allein an der Kante im Sinus befestigt, sondern mit seiner ganzen oberen Fläche continuirlich mit der benachbarten des Rippenausschnittes verwachsen ist. Es wird daraus verständlich, warum bei Luxationen zwischen Corpus und Manubrium sterni die zweite Rippe der Ausweichung der Handhabe folgt.

Mag äusserlich und für das blosse Auge eine scharfe Grenze zwischen der faserigen und der hyalinen Masse bestehen oder nicht, dem feinem Baue nach ist sie niemals vorhanden, sondern es geht die Hauptmasse der Faserung so allmählig aus der Grundsubstanz jener Knorpelscheiben hervor, dass sie mit ihr durchaus continuirlich erscheint. In der That sind denn auch die meisten Faserelemente mit der Dignität von Zellstoffibrillen nur das Ergebniss des Zerfalles einer gewissen Partie von hyaliner Knorpelmasse. Die Faserung zeigt demgemäss auch mehrfache Verschiedenheiten, indem sie bald nur vereinzelte, wenig gefaserte Bänder oder Streifen in einer structurlosen Grundlage, bald reichlichere Bündel und Fibrillen, die zu einem unregelmässigen Maschenwerke angeordnet sind, darstellt. Stets finden sich zwischen diesen Bestandtheilen zahlreiche, oft ausgezeichnete grosse Knorpelzellen. Nie fehlt es an feinen elastischen Fasern, die aber ohne irgend eine Ordnung zwischen die andern Theile hineingestreut sind gleichsam so, als seien sie durch ein Hereinwuchern jener zwischen sie, aneinandergedrängt worden.

In der nächsten Umgebung jener Faserschichte begegnet

man bisweilen sehr wunderbaren Zerklüftungsweisen der Grundsubstanz des hyalinen Knorpels. Besonders ist mir in einem Falle eine eigenthümliche Canalisation aufgefallen. Es waren theils kurze, theils sehr in die Länge gezogene, dünnere und dickere, einfache und getheilte, vorwiegend gestreckt verlaufende Streifen, welche häufig wie von einer Knorpelhöhle ausstrahlten und so den Anschein von verästigten Knorpelzellen gewährten. Es ist mir auf keine Weise gelungen eine selbständige Wandung nachzuweisen und bin ich schliesslich zur Ansicht gekommen, dass es nichts Anderes als Spaltbildungen sein möchten, die sich zum Theil bis in die Knorpelhöhlen hinein erstreckt haben. Eine häufiger vorkommende Zerklüftungsart der Knorpelgrundsubstanz erscheint im Auftreten von breitem und schmalern einfachen und verästigten Bändern, welche sehr scharf quer gestreift sind, so dass man zwischen schmalen queren Substanzbrücken ihrer Breite entsprechende Lücken findet.

Als besonders bemerkenswerthe Abweichungen von jener regelmässigen Vereinigung der Handhabe und des Brustbeinkörpers findet man erstens einen die ganze Dicke derselben betreffenden Zerfall in Bindegewebsfaserknorpel. Dieser ist entweder immer auf eine kleinere Stelle beschränkt oder durchgreifend und bedingt im letztern Falle eine ausgezeichnete Weichheit, Succulenz und Beweglichkeit des ganzen Gebildes. Die Färbung ist dann bald grauweiss, bald gleichförmig oder auch nur stellenweise schmutzig gelb. Die mikroskopische Untersuchung lässt darüber keinen Zweifel übrig, dass ein successiver Zerfall der hyalinen Grundmasse der Knorpelscheiben bis an die knöcherne Grenze eingetreten ist, indessen die Knorpelzellen frei geworden sind. Die hier oft ungewöhnlich reichliche Anzahl der grössten Mutterzellen berechtigt zur Annahme einer neben jenem Zerfalle einhergehenden Neubildung.

Zweitens, es tritt unter Metamorphose sowohl des hyalinen als faserigen Knorpels jener Verbindung in Knochen- substanz eine gleichförmige Fortsetzung zwischen spongiöser Substanz des Manubrium und Corpus sterni auf. Es ist dies eine Erscheinung die nicht gerade das höhere Alter characterisirt, sondern auch, aus freilich nicht zureichend be-

kannten Gründen, gleich wie die Verknöcherung der Kehlkopfs- und Rippenknorpel, häufig bei jüngern Individualitäten getroffen wird.

Von höchstem Interesse sind drittens, diejenigen Abweichungen, bei welchen die Continuität der Verbindung aufgehoben ist. Hier gibt es alle möglichen Uebergänge vom kaum Merkbareren bis zu einer die ganze Dicke der Verbindung betreffenden Spaltbildung. Während ich es im kindlichen Alter bis jetzt nur Einmal gefunden habe, gehören kleinere Unterbrechungen der faserknorpeligen Substanz beim Erwachsenen nicht zu den Seltenheiten. Am häufigsten sieht man die Trennung in Form einer nur wenige Millimeter langen Spalte in der Nähe des Sinus costalis da, wo die Knorpelscheiben zur Auskleidung desselben aus einander zu treten im Begriffe sind. Aber auch in der Mitte, oder gegen die Angrenzung an die Membrana sterni anterior und posterior begegnet man Continuitätsstörungen bald in Gestalt enger Spältchen, bald als unregelmässige, zerklüftete, kleine Lücke.

Als ein sehr ausnahmsweises Vorkommen muss es angesehen werden, wenn eine durch die ganze Dicke der Verbindung zwischen Handhabe und Körper des Brustbeines sich erstreckende, eine Gelenkhöhle darstellende Spalte vorhanden ist. Der Zufall hat es gewollt, dass kurze Zeit nachdem ich eine die Entwicklung der Gelenke betreffende kleine Arbeit an die Redaction des Archives für Anatomie und Physiologie abgesendet hatte, zwei derlei Fälle zu meiner Beobachtung gelangten. Den einen, der Leiche eines 40jährigen Mannes entnommenen Fall verdanke ich der Güte meines Collegen Bruns; den anderen habe ich selbst bei einer männlichen Leiche gefunden. Die Spalten waren in beiden Beobachtungen so enge, dass sie bei scharfen senkrechten Durchschnitten im geraden und queren Durchmesser, eine kaum bestimmbare Höhe und eine directe Berührung der sie begrenzenden Flächen zeigten. Gegen die Membr. sterni ant. et post. waren dieselben durch eine von diesen verschiedene Fasermasse abgegrenzt. An dem Sinus costalis mündete die Spalte in dem einen Falle in das ungetheilte zweite Sternocostal-gelenk aus; in dem andern in die untere Hälfte desselben, indessen die dem Handgriffe angehörige Knorpelscheibe mit der Kante des zweiten Rippenknorpels in Verbindung stand, so dass also

das über diesen gelegene Gelenk von jener Spalte vollständig abgeschlossen war.

Die jene Spalten begrenzenden Flächen zeigten sich nicht glatt, sondern liessen theils festere Ungleichheiten, theils weiche, etwas verschiebbare, kürzere und längere Hervorragungen erkennen, welche letztere unter Wasser deutlich flottirten und an ihren freien Enden in eine Anzahl zarter Anhängsel auseinanderwichen.

Führte man scharfe Schnitte durch die ganze Dicke der jene Gelenkspalten von oben und unten her begrenzenden Knorpelplatte, dann liessen sich Objekte gewinnen, an welchen sehr überraschende Eigentümlichkeiten des feinern Baues befriedigend zur Ansicht kamen. Es liessen sich zunächst zweierlei morphologisch verschiedene Schichten unterscheiden. Die eine mächtigere, und mit dem Knochen in Berührung stehende, war aus echtem Knorpel gebildet und ging ohne scharfe Grenze in die andere hinüber. An der Uebergangsstelle fielen sehr umfangliche, zum Theil 0, 1^{mm} breite, an Tochterzellen reiche Knorpelkörperchen auf, welche sich übrigens bis in die zweite Schichte hinein erstreckten. Diese letztere ist eine Faserlage, welche bei den mit Essigsäure nicht versetzten Objekten durch ihre Lockerheit und ihr trübes, schmutzig gelbes Colorit von dem hyalinen Knorpel absteht und den höchst verworrenen Faserverlauf noch nicht genügend zu erkennen giebt. Sobald aber jenes Mittel eingewirkt hat, dann zeigt sich ein Fasergerüste mit zum Theil sehr dicken, in der mannigfaltigsten Art untereinander verbundenen und unregelmässige Räume einschliessenden Balken, welche theils homogen, theils gestreift und im Zerfalle zu feinem Fibrillen begriffen sind. Sowohl in ihnen selbst, als auch in den Maschen finden sich grössere und kleinere, häufig mit sehr dicken, doppelt contourirten Wänden versehene Knorpelzellen. An elastischen Fibrillen fehlt es nirgends. Sie gehören zu der feinsten Art, und bieten einen häufig auffallend gestreckten Verlauf dar, auch ohne dass ein Druck auf das Objekt ausgeübt wurde.

Von dieser Faserschichte gehen zahlreiche in die Gelenkspalte hineinragende, und so mannigfaltig gestaltete Fortsätze aus, dass es kaum möglich wäre, eine erschöpfende Schilderung ihrer Formen zu geben. Sie stellen in meist pflanzenblattähnlichen Gestalten eine äusserst üppige

Vegetation dar, deren Mutterboden oben jenes Fasergerüste ist. Es sind aus Binde substanz bestehende, theils homogene, theils fein gestreifte oder auch Längsfasern besitzende Auswüchse desselben, die mitunter auch elastische Fibrillen und da und dort eine Knorpelzelle enthalten. Die Auswüchse sind platt, scharf contourirt. Die meisten ähneln den Blättern des Rohrschilfes, laufen mit ebenen Rändern sehr spitz aus; andere zeigen fein gekerbte Ränder oder sind mehrfach gelappt oder auch sanduhrähnlich eingeschnürt, oder sie bieten eine kolbige Endigung und dgl. dar. Besonders ziehen diejenigen Formen die Aufmerksamkeit auf sich, welche wie von einem Kanale durchzogen erscheinen, der aber bei näherer Untersuchung nichts Anderes ist, als ein ursprünglich dünner stielartiger Auswuchs des Fasergerüsts, über den secundär homogene Binde substanz hinweggewachsen ist.

Die Begrenzungsflächen der kleinern in der Verbindung substanz zwischen Manubrium und Corpus sterna öfters vordringlichen Lücken, sind gleichfalls nicht glatt, sondern mit zum Theil blattartigen Binde substanzfortsätzen, vorwiegend aber mit unregelmässigen und wie in der Auflösung begriffenen Zellstofffasern und - Bündeln besetzt.

Da es nun eine Thatsache ist, welche denn auch Maisonneuve anerkennt, dass eine Lückenbildung in jener Knochenverbindung hauptsächlich beim Erwachsenen auftritt, im frühern Jugendalter höchst selten, beim Neugeborenen aber, soweit ich bisher gefunden habe, gar nie besteht, sondern hier zwischen hyalinem Knorpel eine solide Faserung befindlich ist; so sieht man sich geradezu genöthigt jene Lückenbildung als einen spätern Versuch der Natur ein Gelenk hervorzubringen, anzusehen. Welcher Vorgang dabei stattfindet, kann nicht zweifelhaft sein. Es ist ein Process des Zerfallens und der Schmelzung der Gewebelemente, welche der Bildung der Höhle und ihres Inhaltes zu Grunde liegt. Dieser Process bleibt bald auf eine kleine Stelle beschränkt, bald greift er durch die ganze Breite hindurch und erzeugt so eine wirklich gegliederte Verbindung. Während die Schmelzung des Gewebes langsam fortschreitet, findet einerseits ein weiter gehender faseriger Zerfall der nunmehr als Gelenkknorpel erscheinenden Scheiben statt und anderseits das Auswachsen von blattartigen Fortsätzen aus der faserig zerfallenen Grundsubstanz.

Es ist von selbst verständlich, übrigens bisher noch nicht zu meiner Beobachtung gekommen, dass die Verfügsung auch diese neuen Productionen, sowie die neu aufgetratene Faserung betreffen, und so eine umfanglichere, von glatten Knorpelflächen begrenzte Gelenkhöhle entstehen könne.

Erklärung der Abbildungen.

Fig. 1. Handgriff und oberer Theil des Körpers vom Brustbeine eines 20jährigen Mädchens (Senkrechter Durchschnitt im Querdurchmesser, zur Darlegung der normalmässigen Verbindungsweise jener Knochen.)

- a. a. Schreiben aus hyalinem Knorpel, welche sich jederseits in den Sinus costalis fortsetzen.
- b. Faserige Zwischensubstanz, welche die Knorpel des zweiten Rippenpaares untereinander verbindet.
- c. c. c. c. Gelenkhöhlen des 2. Rippenknorpelpaares.

Fig. 2. Handgriff und oberes Ende des Körpers vom Brustbeine eines 40 J. a. Mannes. (Senkrechter Durchschnitt im geraden Durchmesser.)

Die Verbindung zwischen beiden Knochen stellt anomaler Weise ein Gelenk dar.

- a. a. Die Gelenkknorpel, welche gegen ihre freie Fläche hin in eine faserige Substanz übergehen und die spaltenähnliche Gelenkhöhle b. umgrenzen.

Fig. 3. Senkrechter Durchschnitt eines Gelenkknorpels vom vorigen Präparate. 300 Mal vergrössert. Man sieht:

- a. Hyaline, die Verbindung mit dem Knochen vermittelnde Knorpelsubstanz;
- b. Eine Knorpelkörperchen haltige Faserschichte;
- c. Aus ihr hervorgewachsene, und in die Gelenkhöhle hineingewucherte, blattähnliche Bindegewebsfortsätze.

Ueber quantitative Bestimmungen des Zuckers im diabetischen Harn.

Von

Dr. Wilh. Wicke, nebst Zusatz von Prof. Listing.

Die quantitative Bestimmung des Harnzuckers geschieht bekanntlich auf chemischem Wege entweder mittelst der Gährung oder mittelst der bekannten Trommer-Fehling'schen Kupferlösung. Wie gewöhnlich angenommen wird, sollen beide Methoden „brauchbare, Resultate geben. Es lag mir daran zu erfahren, wie genau die auf dem einen oder dem andern Wege gefundenen Zahlen übereinstimmen, weshalb ich eine Reihe vergleichender Versuche anzustellen mir vornahm. Ich hielt es für zweckmässig, meine Bestimmungen durch die mittelst des Polarisationsapparats gefundenen Werthe kontrolliren zu lassen, weshalb ich Herrn Prof. Listing ersuchte, sich mit an der Arbeit beteiligen zu wollen.

Das Material zu unsern Untersuchungen lieferte uns ein im hiesigen Hospitale behandelter Diabetiker, dessen ausführliche Krankheitsgeschichte Hofr. Fuchs wohl später veröffentlichen wird. Hier nur so viel, dass Patient, ein Zimmermann von 45 J., mittlerer Grösse und untersetzter Statur, schon seit 8 Monaten krank war, als er am 20. Novbr. v. J. in die Anstalt kam, wo er nach 224 tägiger Behandlung — am 22. Juni — starb. Bei seinem Eintritte schied er in 24 St. nur 2900 CC. Urin von 1046 spec. Gewicht aus: er litt damals an Diarrhöen. Später betrug die täglich gelassene Harnmenge 4—6000, ja vorübergehend über 7500 CC., wog aber dagegen nur 1036—1028, ja zuweilen nur 1024—1020. Der Zuckergehalt oscillirte, einzelne Tage abgerechnet, zwischen 5 u. 6%. Der Harn war blassgelb gefärbt, dünnflüssig, klar, sauer, ohne urinösen Geruch, ohne Eiweis: Die Harnsäuremenge normal. Einmal gelang es mir, ohne vorhergegangene Concentration, nur durch Zusatz von Salzsäure die schönsten Hippursäure-Cry-

stalle darans abzuschneiden und so Lehmann's Beobachtung, dass im diabetischen Harn die Hippursäure vermehrt sei, zu bestätigen. So beträchtlich die anhaltende Ausscheidung von Zucker auch war, behielt der Kranke doch während seines ganzen Aufenthalts im Hospitale eine gute Gesichtsfarbe und nahm während der ersten 4 Monate selbst zu: bei seiner Aufnahme nur 107 \bar{u} schwer, wog er am 31. März 113 \bar{u} . Die letzte Wiegung vor seinem Tode aber (am 10. Mai) ergab 108 \bar{u} , immer noch ein \bar{u} mehr als bei seinem Eintritte. Er starb an diphtheritischer Colitis (Dysenterie?), die ihn ohne bekannte Veranlassung in der Nacht vom 17. auf den 18. Januar mit sehr frequenten blutigen Stühlen, heftigem Schmerze nach dem Verlaufe des Colon descendens, trockner, reiner Zeuge, kleinem frequenten Puls u. s. w. befiel und am 22. Abends tödtete.

Der Harn, welchen wir zur ersten Reihe unserer vergleichenden Versuche benutzten, wurde vom 15. auf den 16. Juni, also 2 Tage vor dem Eintreten der tödtlichen acuten Krankheit, der hingegen, mit welchem wir die 2. Versuchsreihe anstellten, vom 20. auf den 21., am Tage vor dem Tode des Patienten gelassen und hieraus erklärten sich die Differenzen des procentischen Zuckergehaltes, welchen die ersten gegen die zweiten Untersuchungen ergeben. Noch grösser als auf den verhältnässigen Zuckergehalt war aber, wie aus den mir mitgetheilten Hospitaldiarien hervorgeht, der Einfluss des acuten Erkrankens auf die in 24 St. gelassene Harnmenge und während derselbe v. $\frac{19}{16}$ 5500 C. C. betrug, wurden vom $\frac{20}{21}$ nur 550 C. C. secernt. Am ersteren Tage producirt der Kranke somit (nach den Ergebnissen der Fehling'schen Solution) 384,6, am letzteren nur 32,34 Gran Zucker. An Harnstoff dagegen war, wie ich gleichfalls dem Hospitaldiarium entnehme, der Harn vom 21. reicher als der vor dem Eintritte der Dysenterie gelassene (1,73%: 0,82%), während sich von Chloratrium nur 0,056 statt 0,094% (am 15.) fanden. Auch trat gleichzeitig mit der Ruhr (am 18.) im bisher immer eiweissfreien Urine eine beträchtliche Menge Albumin auf, verschwand aber vom 19. an wieder. Alle diese Verhältnisse gehören übrigens mehr in die Krankheitsgeschichte als hieher, wo es sich nur um einen Vergleich der verschiedenen Methoden, den Harnzucker quantitativ zu bestimmen, handelt.

Bedauern muss ich, dass uns der unerwartet rasche Tod des Diabetikers verhinderte, unsere Untersuchungen weiter auszudehnen. Unsere Absicht ist jedoch bei späterer Gelegenheit die Arbeit wieder aufzunehmen, um wo möglich die Fehlerquellen, welche in den Methoden selbst liegen, kennen zu lernen, resp. fest zu stellen.

Die Bestimmung mittelst der Fehling'schen Kupferlösung betreffend, will ich nur bemerken, dass meine Probestoffigkeit richtig bereitet war und, wie Versuche mit reinem Traubenzucker zeigten, den Normalgehalt an Kupferoxyd hatte.

Was die Ausführung der Methode angeht, so halte ich für besser nicht direkt über der Flamme, sondern im Wasserbade zu erwärmen. Der ganze Process verläuft viel gleichmässiger, so dass man schärfer beobachten kann. Es tritt alsdann ein Zeitpunkt ein, wo die Flüssigkeit sich schmutzig grün färbt — ein Zeichen, dass die Reduction von Kupferoxyd zu Oxydul vor sich geht. Es wird dann von da an durch blosses Erwärmen fast aller Zucker zersetzt werden. Meistens fehlen jetzt nur noch wenige Tropfen, um den Process vollends zu Ende zu bringen.

Als Beweis, dass kein Kupferoxyd mehr gelöst sei, kann man die von Heine Gericke — Chem. Pharm. Central-Blatt Nro. 13. 1855 — angeführte Löthrohrreaktion sehr gut benutzen. Sie besteht darin: die auf Kupfer zu prüfende Flüssigkeit mit Chlorsilber in Verbindung zu bringen und dieses dann an einem dünnen Eisendraht in die Löthrohrflamme zu bringen. Man sieht bei Gegenwart von Kupfer einen grün umskünten Flammenkegel. Ich fand die Angabe von Gericke bestätigt, dass mittelst dieses Versuchs in einer Flüssigkeit, welche durch Ammoniak nicht mehr wahrnehmbar blau gefärbt wird, auf diese Weise das Kupfer doch noch zu erkennen ist. Ein Gewinn für unsern Zweck liegt besonders darin, dass, wenn es erforderlich ist, mit derselben Flüssigkeit weiter operirt werden kann. Zudem erfordert der Versuch eine kürzere Zeit, als die bekannten Reaktionen mit Blutlaugensalz in der zuvor mit Salzsäure angesäuerten Lösung oder die mit Schwefelammonium.

Ueber die Gährungsversuche habe ich Nichts weiter anzuführen. Ich kann versichern, dass sie ebenfalls mit aller Vorsicht angestellt wurden. Um der Genauigkeit vollständig zu genügen, wurde — wie es G. Orup verlangt — bei jedem Versuch auch die Kohlensäure bestimmt, welche die in An-

wendung genommene Menge Hefe für sich entwickelte. Die gefundene Menge — nur einige Milligramme betragend — wurde dann von der aus dem Zucker entwickelten Gesamtmenge Kohlensäure in Abrechnung gebracht. In den Gährungsapparat wurde Weinsäure gegeben, um die Zersetzung des Zuckers zu befördern, die der albuminösen Stoffe aber zu verhindern.

Aus den gefundenen Zahlen erhellt, dass die durch die Kupferlösung gefundene Procentmenge Zucker um 1 p. c. und darüber mehr beträgt, als die durch Gährung erhaltene, und dass die Listing'schen Werthe den letzteren ungleich näher liegen als jenen. Auch frühere Versuche haben mich überzeugt, dass die durch Titriren erhaltene Menge fast immer zu hoch ausfällt. Es muss späteren Versuchen überlassen bleiben zu ermitteln, welche Umstände davon die Schuld tragen. Nach meinem Ermessen scheinen Zeiterparnis und Genauigkeit bei den beiden Methoden in umgekehrtem Verhältnis zu stehen.

Die Versuche habe ich mit Herrn L. Brummerstädt aus Rostock, meinem fleissigen Schüler, gemeinschaftlich ausgeführt.

a. Durch Gährung. Juni 15.

I. Genommen wurde Harn:	19,019 Grm.
Hefe:	2,827 "
Der Apparat hatte nach der Gährung verloren	0,470 "
Die Hefe hatte entwickelt Kohlensäure	0,002 "
blieben Kohlensäure aus dem Harnzucker	0,468 "
Der Harn enthielt demnach, wie der bekannte Ansatz ergibt: 5,033 p. c. Zucker.	
II. Harn	11,4485 "
Hefe	1,843 "
Verlust an Kohlensäure	0,302 "
Kohlensäure aus der Hefe	0,001 "
Demnach Kohlensäure aus dem Harnzucker	0,301 "
Folglich enthielt der Harn 5,381 p. c. Zucker.	

b. Durch titrirte Kupferlösung.

10 CC. der Kupferlösung entsprechen 0,05 Grm. trockenem Krümelzucker.
5 CC. des Harns wurde mit 95 CC. Wasser verdünnt, also auf 100 CC. gebracht.

Zur Reduktion der Kupferlösung wurden 14,3 CC. des verdünnten Harns verbraucht.

Der Harn enthielt demnach 6,993 p. c. Zucker.

Die zweite Reihe von Versuchen wurde sechs Tage später angestellt.

a. Durch Gährung. Juni 21.

I. Harn	13,891 Grm.
Hefe	3,503 "
Verlust an Kohlensäure	0,245 "
Kohlensäure aus der Hefe	0,011 "
Folglich Kohlensäure aus dem Harnzucker	0,234 "
Ergibt 3,441 p. c. Zucker im Harn.	
II. Harn	16,4535 "
Hefe	1,4895 "
Verlust an Kohlensäure	0,2627 "
Kohlensäure aus der Hefe	0,0048 "
Demnach Kohlensäure aus dem Harnzucker	0,2579 "
Entsprechen 3,206 p. c. Zucker	
III. Harn	8,2947 "
Hefe	1,8585 "
Verlust an Kohlensäure	0,1405 "
Kohlensäure aus der Hefe	0,006 "
Folglich Kohlensäure aus dem Harnzucker	0,1345 "
Entsprechen 3,316 p. c. Zucker.	

b. Durch titrirte Kupferlösung.

Die Mischungsverhältnisse waren ganz dieselben, wie bei der ersten Bestimmung. Gebraucht wurden 17 CC. des verdünnten Harns, der demnach 5,88 p. c. Zucker enthalten musste.

Wie sich zu diesen Zahlen die vom Herrn Prof. Listing gefundenen Procente verhalten, wird der folgende Theil der Arbeit weiter ausführen. Vorläufig sei nur bemerkt, dass die durch die letzten drei Gährungsversuche erhaltenen Werthe mit jenen sehr gut übereinstimmen.

Z u s a t z.

Es mag nicht überflüssig erscheinen, bei Gelegenheit dieser Mittheilung des Herrn Wicke, trotz der ausführlichen Untersuchungen namentlich von Biot und Clerget, auf die optisch saccharimetrische Probe des diabetischen Harns

zurückzukommen, welche im vorliegenden Falle der chemischen Controlle von neuem unterworfen worden.

Bei dem hierfür benutzten Polarisationsapparate habe ich, wie bereits seit geraumer Zeit für ähnliche Zwecke, die unter dem Namen des Savart'schen Polariskops bekannte Verbindung zweier gleich dicken, schiefe gegen die optische Axe geschnittenen Quarzplatten und einer parallel zur Axe geschnittenen Turmalinplatte als Analyser angewandt, dessen Einstellung auf dasjenige Azimut, in welchem die farbigen Interferenzstreifen verschwinden, meines Erachtens eine grössere Sicherheit gewährt, als die Hervorrufung der sog. „teinte de passage.“ Gelegentlich bemerkt, hoffe ich unter Anwendung dieser Art von Analyser ein für den Chemiker wie für den Arzt brauchbares, leicht zu handhabendes und dabei wesentlich wohlfeiles Instrument zu Stande zu bringen, behufs Bestimmung der Rotationskraft flüssiger Substanzen, so wie, mittelst eines einfachen additionellen Bestandtheils, des Brechungsvermögens derselben. Die von uns gleichzeitig gemachten Zuckerbestimmungen des Harns vom 15. und 21. Juni des seinem Leiden am 22. Juni unterlegenen Diabetes-Kranken des hiesigen Hospitals ergaben folgende Ziffern (Gramme Zucker in 1 Liter Urin):

Juni 15. durch Gährung	50,33	(Wicke)
	53,81	
Durch Kupferlösung	69,93	
Durch Polarisation	56,152	(L.)
Juni 21. Durch Gährung	34,41	(Wicke)
	32,06	
	33,16	
Durch Kupferlösung	38,80	
Durch Polarisation	35,778	(L.)

Die durch Kupferlösung erhaltenen Daten werde ich, als mit einem Fehler in Plus behaftet und von Herrn Wicke selbst als minder zuverlässig betrachtet, bei der gegenwärtigen Besprechung unseres Resultats ausser Acht lassen, und will in dieser Beziehung nur noch daran erinnern, dass (Comptes rendus XXVI. p. 305) Lespiaux hervorhebt: „le procédé de M. Barreswil, qui est très-convenable pour la détermination du sucre dans les dissolutions aqueuses, ne reçoit pas ici d'application, à cause des matières organiques contenues dans l'urine, matières qui précé-

ptent le protoxyde de cuivre dans la liqueur d'épreuve.“ Ich stelle somit die Mittel aus zwei und bez. drei Gährungs-Bestimmungen des Hrn. Wicke mit den beiden von mir erhaltenen optischen Messungen zusammen, nämlich

	(W.)	(L.)	Diff.
Juni 15.	52,07	56,152	+ 4,082
21.	33,21	35,778	+ 2,568

Die beigefügten Differenzen zeigen den Betrag, um welchen das optische Resultat gegen das chemische zu gross ist, und es scheint mir nicht ohne Interesse zu sein, unter der Voraussetzung, dass diese Differenzen ausserhalb der Grenzen der unvermeidlichen Beobachtungsfehler liegen, auf die Rechnungselemente zurückzugehen, auf welchen die auf optischem Wege gewonnenen Zahlen beruhen.

Ich bezeichne mit p den in Grammen ausgedrückten Zuckergehalt eines Liters der Lösung, d. i. die sog. Dosis, eine Zahl, welche mit 1000 α der Biot'schen Bezeichnung übereinkommt, bei welcher α das in der Lösung stattfindende Gewichtsverhältniss der rotatorisch activen Substanz (Zucker), und δ die Dichtigkeit der Lösung (die des Wassers im Normalzustand als Einheit gesetzt) bedeutet. Dann ist die Vorschrift, nach welcher aus der in einer Flüssigkeits-Säule von der Länge l beobachteten Drehung α der Polarisationsebene in Sexagesimalgraden der Gehalt p der Lösung berechnet wird, darstellbar durch den Ausdruck

$$p = C \cdot \frac{\alpha}{l}$$

Der constante Factor C ist nach Biot'scher Bezeichnung gleichbedeutend mit $\frac{m}{[\alpha]}$, wo $[\alpha]$ das der gelösten activen Substanz eigenthümliche moleculare Rotativvermögen, und m den von Biot durchweg $= \frac{2}{3}$ gesetzten Dispersiv-Factor zur Reduction des mittelst der Uebergangsfarbe bestimmten hellsten (gelben) Strahls im weissen Lichte auf denjenigen rothen Strahl bedeutet, welchen das mit Kupferorydul gefärbte rothe Glas durchlässt, und welcher nahezu der Fraunhofer'schen Linie C des prismatischen Spectrums entspricht. [vgl. Comptes rend. XXV. 624.]

Die Zuverlässigkeit der optischen Saccharimetrie hängt nun, wenn nicht lediglich, doch wesentlich von der Sicherheit in der numerischen Feststellung des erwähnten constanten Factors C ab. Aus den seit einer Reihe von Jah-

ren diesem Gegenstande gewidmeten Arbeiten von Biot stellt sich der Werth dieser Constante für den Rohrzucker = 1399, für den Harnzucker = 2176, während aus Clerget's sorgfältigen Untersuchungen [Ann. de Chim. (3) XXVII. 175 — 207] für den Rohrzucker $C = 1391$, für den diabetischen Zucker $C = 1905,7$ hervorgeht. Die letzteren Werthe habe ich aus den in ganz anderer Form gemachten Angaben Clerget's deducirt, um sie mit den Biot'schen unmittelbar vergleichen zu können. Diese Ziffern zeigen, dass die optische Saccharimetrie in der Harnzuckerbestimmung noch lange nicht bis zu dem Grade der Sicherheit gelangt ist, welchen sie hinsichtlich der Rohrzuckermessung besitzt.

Zur Bestimmung von C concurriren zwei Grössen $[\alpha]$ und m , die erstere hängt am unmittelbarsten mit der chemischen, die zweite mit der optischen Natur der rotativen Substanz zusammen. Beim Rohrzucker, dessen Lösungen leicht wasserklar wie farblos krystallisirter Quarz herzustellen sind, bietet der Reductionsfactor m nur geringe Schwierigkeiten. Anders verhält es sich bei dem im Urin gelösten Harnzucker, wo die von der Röhrenlänge l abhängige und im Harn an sich veränderliche gelbe Tingirung Ungleichheiten einführt, welche sich in ihrer Wirkung auf den Werth von C in den von Biot [Comptes rend. XV. 634] aufgestellten Zahlen 2176 und 2340 abspiegeln, von welchen erstere für den Fall der im geklärten Urin beobachteten Uebergangsfarbe, die zweite für den praktisch-wichtigeren Fall der dunkelsten Farbe im natürlichen orangefarbenen Urin gelten soll, so dass, während für jenen $m = \frac{23}{30} = 0,767$, für diesen $m = \frac{24,17}{30} = 0,806$ zu setzen wäre. Nach meinen bisherigen Erfahrungen kann man den Werth von m auch für den von mir gebrauchten Analyseur trotz der grünlichen (aber hellen) Färbung des daran befindlichen Turmalins bei Röhrenlängen zwischen 150 und 200 Millimeter und der somit sehr hellgelben Färbung des bloss filtrirten diabetischen Urins = $\frac{23}{30}$ setzen, oder, mit andern Worten, die Neutralstellung dieses Analyseurs auf die von Biot angewandte teinte de passage beziehen, so dass die mittelst dieses Parallelstreifen-Analyseurs erhaltenen Werthe von C einen directen Schluss auf die spezifische Rotationskraft $[\alpha]$ gestatten, welcher — bei gleichbleibendem

m — die Constante C umgekehrt proportional ist. Die Biot'schen Constanten geben nun für das Verhältniss der Rotationskräfte von Harnzucker und Rohrzucker

$$\frac{1399}{2176} = 0,643$$

Clerget gibt dieses Verhältniss [a. a. O. 197] zu 0,73 an, so dass nach ihm die Constante C für Harnzucker den oben erwähnten Werth

$$\frac{1391}{0,73} = 1905,7$$

statt der Biot'schen Zahl 2176 erhält. Clerget schreibt also dem diabetischen Zucker im Verhältniss von 73 zu 64 eine stärkere Rotationskraft zu, als Biot.

Die oben angeführten auf optischem Wege erhaltenen Zuckergehaltzahlen beruhen auf der Clerget'schen Constante $C = 1905,7$. Sie liegen dem chemischen Ergebniss ziemlich nahe. Aber die Abweichung in Plus deutet darauf hin, dass wenn die Clerget'sche Constante noch einer Abänderung bedürfen sollte, sie vermindert werden müsse, und ihr Werth sich noch weiter von demjenigen entfernen dürfte, welchen ihr die Biot'schen Messungen beilegen. Das Verhältniss der Ablenkungsfähigkeit des diabetischen Harns zu der der Rohrzuckerlösung würde dann noch grösser als 0,73 ausfallen. Versuchen wir diese neue Bestimmung unter Zugrundlegung unserer Beobachtungen vom 15. und 21. Juni, so muss auf die Ablenkungen und die Länge der Flüssigkeitssäule zurückgegangen werden. Jene mit α , diese mit l bezeichnet, war

1.	Juni 15.	$\alpha = 5^{\circ}275$	$l = 179,0$	$p = 52,07$
2.	21.	$\alpha = 3,361$	$l = 179,0$	$p = 33,21$

Durch die oben erwähnte Relation, aus welcher

$$C = \frac{pl}{\alpha}$$

folgt, erhält man mittelst dieser numerischen Daten

$$1.) C = 1766,9$$

$$2.) = 1768,7$$

Dem ersten Werth das Gewicht 2, dem zweiten das Gewicht 3 belegend, erhalten wir aus unseren Beobachtungen den neuen Mittelwerth

$$C = 1768,0$$

welcher an die Stelle des Biot'schen Verhältnisses 0,64 oder des Clerget'schen 0,73 das neue 0,78685 setzt.

Worauf diese Discongruenzen in den Bestimmungen von C für den diabetischen Urin beruhen, lässt sich zur Zeit noch nicht mit Bestimmtheit sagen. Möglich sogar, dass der Werth von C gar nicht constant ist wegen der im Zuckerharn wechselnden Antheile anderer rotativ activen Nebenbestandtheile oder der wechselnden Natur des Zuckers u. dgl. Bekanntlich haben Biot und Ventzke zuckerhaltigen Harn von ganz anomalen Rotationsverhältnissen begegnet¹⁾.

Die von Mitscherlich [Lehrb. d. Chemie 4. Aufl. I. S. 366] mitgetheilten Zahlen ergeben das Verhältniss der Ablenkungskräfte des Rohr- und des Stärkezuckers = 0,682, welches zwischen den Zahlen von Biot und Clerget mitten inne steht.

Trotz der guten Uebereinstimmung der oben ermittelten beiden Werthe von C will ich auf das Ergebniss bloss zweier sorgfältig angestellten Messungen kein grosses Gewicht legen. Der Zweck dieser Bemerkungen sollte nur sein, auf die Wichtigkeit der Wiederholung sorgfältiger Controllen aufmerksam zu machen, in einer speciellen Frage der Saccharimetrie, welche für den Arzt ein ganz vorzügliches Interesse in Anspruch nimmt, und in Betreff deren er wünschen muss, durch das vereinigte Studium des Chemikers und des Physikers in Besitz wo möglich sicherer und leichtausführbarer saccharimetrischer Methoden zu gelangen. Dem Chemiker seinerseits aber liegt es ob, die etwaigen constanten Fehler seiner Methoden, auf welche ich hier nicht eingehen durfte, zu studiren und zu evaluiren.

Ein zu grosser Werth von C (wie bei Biot) könnte darauf beruhen, dass die chemische Analyse (wie z.B. die Trommer-Fehling'sche) in Plus irret, ob die hier als platzgreifend aufgeführte Verkleinerung im Werthe von C auf einem gegentheiligen Fehler der Gährungsmethode beruhe, werden weitere Untersuchungen festzustellen haben. Unmassgeblich erlaube ich mir auf die Möglichkeit eines zu kleinen als Kohlensäure in Rechnung kommenden Gewichtsverlusts in Folge von in den Apparat eintretendem atmosphärischen Wasser im Verlauf der Gährung aufmerksam zu machen²⁾.

1) Die von Boucharadat gefundene Inactivität des Harnstoffs habe ich an Lösungen von 10 und 25 pro Cent, deren Darstellung ich Herrn Prof. Bödeker verdanke, bestätigt gefunden.

2) Darauf bezügliche Versuche behalte ich mir vor. Wicke.

Bleiben wir wenigstens für unseren vorliegenden Fall bei dem gefundenen Werthe $C = 1768$ stehen, und berechnen den Zuckergehalt von neuem, wie er sich aus der optischen Beobachtung ergibt, mittelst der Vorschrift

$$p = 1768 \frac{a}{l}$$

so finden wir nunmehr folgende Zahlen:

	chem.	opt.	Diff.
1.	52,07	52,101	+ 0,031
2.	33,21	33,197	- 0,013

Ob dieser auffallende Parallelismus mehr als zufällig ist, werden künftige zahlreichere Wiederholungen derartiger Vergleichen herausstellen.

Noch mag die Bemerkung hier Platz finden, dass unser Patient während seines Aufenthaltes in hiesigen Krankenhause, etwa 8 Monate, die erhebliche Quantität von mindestens 52 Kilogramm Zucker (reichlich das eigene Körpergewicht) secretirt hat.

Göttingen 1855 Juli 10.

Listing.

Nachtrag

zu den vergleichend saccharimetrischen Bestimmungen

von

Dr. Wicke und Prof. Listing.

Die vom Herrn Prof. Listing ausgesprochene Vermuthung, dass während der Gährung die Schwefelsäure eine, auf das Gewicht der gefundenen Kohlensäure influirende Menge Feuchtigkeit aus der Luft aufnehmen könne, hat sich als richtig bestätigt. Man wird den Verlust an Kohlensäure, welchen der Apparat erlitten, zu gering finden, wenn dieser nicht durch eine besondere Vorrichtung gegen die äussere Luft geschützt wird. Die unten angeführten Versuche beweisen dies. Es ist aber leicht, durch ein Chlorcalciumrohr, das mittelst Kautschuck's dem für die entweichende Kohlensäure bestimmten Röhren vorgelegt wird, hinreichenden Schutz zu erlangen. Das Rohr, der beque-

meren Form wegen U förmig gebogen, wird, wenn nach der Gährung Luft durch den Apparat gesogen wird, zum Trocknen dieser auf der andern Seite des Apparates verwendet. So eingerichtet hat man eine dem beabsichtigten Zwecke vollkommen entsprechende Construction.

Die Versuche haben ergeben, dass die Apparate A u. B, welche auf die beschriebene Weise geschützt waren, durchaus dasselbe Gewicht an Kohlensäure verloren hatten. C u. D, nicht geschützt, hatten ein ungleiches Gewicht und einen geringeren Verlust.

Je nach der Weite des offenen Röhrchens war eine ungleiche Menge Wasserdampf aufgenommen.

Bei unsern ersten Versuchen ergaben sich die durch die Gährung gefundenen Zahlen zu niedrig gegen die optischen Werthe. Dieser Umstand ist jetzt aufgeklärt, da die damals benutzten Apparate ohne Chlorcalciumrohr angewendet wurden.

Die der Gährung unterworfenen Zuckermenge war bei allen vier Versuchen gleich.

A.	Apparat vor der Gährung	8,1875
	Nach der Gährung	7,947
	Kohlensäure:	0,2405
B.	Vor der Gährung	29,002
	Nach der Gährung	28,765
	Kohlensäure:	0,2405
C.	Vor der Gährung	36,740
	Nach der Gährung	36,505
	Kohlensäure:	0,235
D.	Vor der Gährung	21,789
	Nach der Gährung	21,565
		0,224

Wilh. Wicke.

Um die vorstehenden von Herrn Dr. Wicke erhaltenen Ergebnisse für eine weitere Discussion über die optische Constante C nutzbar zu machen, deren Werth nach Biot 2175, nach Clerget 1905,7 und nach unsern früher mitgetheilten Beobachtungen 1768 war, muss der hieraus hervorgetretene Einfluss des aus der Atmosphäre aufgenommenen

nen Wassers auf den Kohlensäurebetrag — soweit es die vorliegenden beiden Paare von Gährungsversuchen gestatten — ermittelt werden.

Betrachten wir den mittelst der beiden Apparate A und B übereinstimmend = 0,2405 gefundenen Gewichtsverlust als das wahre Kohlensäurequantum, so ist der in C und D gefundene Verlust als die um die aufgenommene Wassermenge verringerte Quantität Kohlensäure anzusehn. Die hygroskopische Differenz (für die gleichmässig bei allen vier Versuchen angewandte Quantität Zucker) beträgt also

	bei C	0,0055
	bei D	0,0165
	in Mittel	0,0110

Diese Zahlen durch 0,2405 dividirt, ergeben den auf die Gewichtseinheit der entwichenen Kohlensäure reducirten Fehler in der Kohlensäure-Messung

	bei C	0,02287
	bei D	0,06861
	im Mittel	0,04574

und mithin, da 22 Aequivalente Kohlensäure 45 Aequivalenten Traubenzucker (Harzucker) entsprechen, den auf die Gewichtseinheit Zucker reducirten Fehler in der Zuckerbestimmung

	bei C	0,04678
	bei D	0,14034
	im Mittel	0,09356

Diese Zahlen besagen, dass man bei der gewöhnlichen Art der Anwendung der Gährungsmethode (nämlich ohne vorgelegtes Chlorcalciumrohr) einem Fehler in Minus von 5 bis 14 pro Cent in der Bestimmung der Dosage ausgesetzt bleibt, und dass also zur Vermeidung dieses Fehlers die Anwendung einer die eintretende atmosphärische Feuchtigkeit abhaltenden Vorlage unerlässlich ist.

Bezeichnen wir diesen Fehler durch ϵ und $\frac{\epsilon}{1-\epsilon}$ durch f , so würde aus dem mittelst der Gährungsmethode ohne Vorlage erhaltenen Zuckergehalt p der wegen des hygroskopischen Einflusses verbesserte Gehalt p' durch die Vorschrift

$$p' = p + fp$$

erhalten werden, wo f den hygroskopischen Correctionfactor bedeutet, dessen numerischer Werth sich findet

für $e = 0,04678$	$f = 0,0491$ oder etwa $\frac{1}{20}$
$= 0,09356$	$= 0,1035$. . . $\frac{1}{10}$
$= 0,14034$	$= 0,1632$. . . $\frac{1}{6}$

Es müsste demnach zur uncorrectirten Dosage p der zwanzigste bis sechste Theil derselben addirt werden, um die correctirte p' zu finden.

Die Wicke'schen Bestimmungen vom 15. und 21. Juni, nämlich

$$p = 52,07$$

$$= 33,21$$

in dieser Weise modificirt und auf dem in unserer früheren Mittheilung erörterten Wege mit den beobachteten Drehungswinkeln combinirt, liefern nun unter Anwendung verschiedener Werthe des Factors f folgende übersichtliche Werthe der optischen Constante C :

für $f = 0$	$C = 1768$
$= 0,0491 = \frac{1}{20}$	$= 1855$
$= 0,0725 = \frac{1}{14}$	$= 1906$
$= 0,1035 = \frac{1}{10}$	$= 1951$
$= 0,1632 = \frac{1}{6}$	$= 2056$

Hieraus ist ersichtlich, dass die ohne Feuchtigkeits-Correction abgeleitete Constante 1768 zu klein ist, dass ferner die Clerget'sche Zahl 1906 aus der Annahme des Correctionfactors $= \frac{1}{14}$, d. h. eines Fehlers e von etwa 7 pro Cent entspringt, und nicht nur zwischen den hier auftretenden Grenzwerten 1855 und 2056 enthalten ist, sondern auch dem mittlern Werthe 1951 so nahe liegt, dass die Beibehaltung der Clerget'schen Zahl, bei der Unsicherheit, welcher die Feuchtigkeits-Correction offenbar unterworfen ist, der unwesentlichen Erhöhung auf den Werth 1951 noch so lange vorzuziehen ist, bis eine directere Prüfung am diabetischen Harn wird vorgenommen werden können, und dass demnach durch Mitherrücksichtigung des Feuchtigkeitsinflusses die Gährungsmethode mit der optischen Analyse unter Anwendung von $C = 1905,7$ bis innerhalb der unvermeidlichen Beobachtungsfehler in Concordanz gebracht ist. Es darf hinzugefügt werden, dass die Biot'sche Zahl 2176 entschieden zu gross ist.

Die feinere Controlle der Constante C wird also ins Künftige nicht ohne Anwendung der hier besprochenen Cau-

tel gegen den Einfluss des in der umgebenden Luft enthaltenen Wassergases vorgenommen werden dürfen, deren Nothwendigkeit vor Augen zu legen der Hauptzweck dieser nachträglichen Bemerkungen war.

Güttingen 1855. Aug. 28.

Listing.

Kleine Bemerkung zur Oculologie des
Prof.
Dr. H. Listing.

Das wichtigste Resultat der Untersuchungen über die optische Wirkung des menschlichen Auges ist die Entdeckung, dass das Auge ein optisches System darstellt, welches die einfallenden Lichtstrahlen so gebrochen und gebrochen wird, dass sie in einem Punkte, dem sogenannten Hauptbrennpunkte, vereinigt werden. Diese Entdeckung ist von Listing zuerst gemacht worden, und hat seitdem die Grundlage der Oculologie gebildet. In der vorliegenden kleinen Bemerkung soll die Aufmerksamkeit auf einige Punkte gelenkt werden, welche bei der Untersuchung der optischen Eigenschaften des Auges zu berücksichtigen sind. Insbesondere soll die Frage erörtert werden, inwiefern die optischen Eigenschaften des Auges durch die Beschaffenheit der Hornhaut, der Linse und des Netzhauts bedingt sind. Die optische Wirkung des Auges ist eine sehr interessante Erscheinung, und hat seitdem die Aufmerksamkeit der Naturforscher auf sich gezogen. Die Untersuchungen über die optische Wirkung des Auges sind von Listing zuerst gemacht worden, und hat seitdem die Grundlage der Oculologie gebildet. In der vorliegenden kleinen Bemerkung soll die Aufmerksamkeit auf einige Punkte gelenkt werden, welche bei der Untersuchung der optischen Eigenschaften des Auges zu berücksichtigen sind. Insbesondere soll die Frage erörtert werden, inwiefern die optischen Eigenschaften des Auges durch die Beschaffenheit der Hornhaut, der Linse und des Netzhauts bedingt sind.

Die meisterhafte Darstellung der Osteologie des menschlichen Brustkorbes, insbesondere der Rippenkrümmungen, welche Henle in seinem soeben erscheinenden Handbuche der systematischen Anatomie gegeben hat, rief mir eine am menschlichen Thorax von mir gemachte kleine Bemerkung in das Gedächtniss zurück, die ich in jener Darstellung und auch sonst nicht erwähnt finde, und deren Mittheilung hier gestattet sein möge.

Kleine Bemerkung zur Osteologie des Thorax,

von Dr. H. Weicker.

Betrachtet man den menschlichen Thorax, am besten bei noch erhaltenen Intercostalmuskeln, von der Seite, so sieht man (die 1., 11. und 12. Rippe ausser Acht lassend) die spongiosen Enden der obern Rippen mehr oder weniger aufwärts, die unteren abwärts gebogen; in allen den zahlreichen von mir beobachteten Fällen nun war stets die sechste Rippe die letzte der aufwärts gebogenen, während sich die siebente in der Regel deutlich abwärts gebogen zeigte. Der 6. Intercostalraum fällt, in Uebereinstimmung hiermit, in der aus der Achselhöhle gezogenen Senkrechten ganz besonders schmal aus. Betrachtet man eine getreue Profilzeichnung des senkrecht gestellten Thorax — Augenpunkt der Mitte des Thorax gegenüber — so scheinen die 6., 5. und 4. Rippe in Kreislinien zu liegen, deren Mittelpunkt (auf dem Papiere) etwa in das Manubr. sterni fällt; die 7. Rippe entbehrt dieser concentrischen Stellung.

Die angedeutete Formation ist, wenn auch an und für sich wenig auffallend, doch so charakteristisch, dass ich bei Sectionen, wenn irgend ein Befund seiner Lage nach durch Nennung einer Rippe näher bezeichnet werden sollte, erste

oder 12. Rippe aber, als Ausgangspunkt für die Zählung, nicht sichtbar war, in der Erkennung der 6. und 7. Rippe, deren vordern Enden sich fast immer dem Blicke bieten, niemals irren konnte. Auch am Lebenden ist es leicht, die 6. und 7. Rippe nach der Richtung des knöchernen Endes zu unterscheiden; da nun aber die 1. Rippe unter die Clavicula versteckt, die Zählung von ihr aus ein tief einkneifendes Umgreifen oft zahlreicher Rippen fordert, die Betastung der 12. und 11. Rippe den zu Untersuchenden öfters unangenehm, der Sitz der Brustwarze als Anhaltspunkt häufig nicht zu benutzen ist, so dürfte jenes Merkmal möglicherweise auch bei Brustuntersuchungen zur Orientirung beitragen können.

Am Skelet ist das erwähnte Verhalten, in Folge von Abweichungen der Rippen aus ihrer richtigen Stellung, oft weniger deutlich; ebenso lassen die meisten Abbildungen nichts davon erkennen, wie ja so viele Formverhältnisse, so lange sie nicht als constant und wesentlich erkannt sind, in der Zeichnung ohne Ausdruck bleiben. Doch zeigen verschiedene Abbildungen, namentlich Albin's (z. B. Alb. musc. tab. XIV, fig. 2 und tab. ossium XII), ganz deutlich die Aufwärtskrümmung der 6. und die Abwärtskrümmung der 7. Rippe; ja noch in der verkleinerten Copie der Albin'schen Seitenansicht des Skelets, welche u. a. in Valentin's Lehrb. der Physiologie (2. Aufl., II, 284) sich findet, lässt sich jene Bildung deutlich erkennen.

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Ueber die Fascien und Schleimbeutel der Fusssohle.

Von
 Prosector Dr. Dursy in Tübingen.

Nachdem ich vor drei Jahren eine Reihe von Beobachtungen über die Muskeln und Fascien der Hand in dieser Zeitschrift mitgetheilt hatte, so lag es nahe, ähnliche Untersuchungen am Fusse anzustellen, zumal dessen anatomische Beschreibung, durch einfache Verweisung auf analoge Theile der Hand, in mancher Beziehung eine oberflächliche genannt werden kann.

Dergleichen Arbeiten in dem Gebiete der gröbern Anatomie gehören heut zu Tage gerade nicht zu den dankbarsten, zumal bei zu einseitiger, zu streng anatomischer Richtung, mit welchem Vorwurfe man mit vollem Rechte meine oben citirte Abhandlung über die Anatomie der Hand belasten kann.

Nachdem ich aber, besonders durch die Arbeiten des Herrn Prof. Luschka gelernt habe, was in dieser sogenannten gröbern Anatomie auch heute noch geleistet werden kann, wenn man Praxis und Wissenschaft in richtigem Maasse verbindet und berücksichtigt, und die gewonnenen Resultate durch gleichzeitige mikroskopische, vergleichend-anatomische, physiologische und pathologische Studien für die gesammte medizinische Wissenschaft von bleibendem Werthe zu machen versteht, seitdem suche ich mit dem anatomischen Messer auch geistige Resultate zu erlangen, und halte derartige Arbeiten für dankbar, indem auf diesem mit so glänzendem Erfolge angebahnten Wege der Medizin eine solide Grundlage gesichert wird.

Wie sehr ich von der Richtigkeit dieser Methode überzeugt bin, so bin ich es nicht minder von deren Schwierigkeit; und wenn ich es dennoch versuche, in ähnlicher Weise einen Körpertheil zu bearbeiten, so bleibt es eben ein An-

fangsversuch, für den ich die Nachsicht der Leser in Anspruch nehme, und betrete ganz bescheiden mit der Fusssohle den neuen Weg der anatomischen Forschung.

Durch genaue und zahlreiche Untersuchungen ergab sich mir ein bisher noch nicht näher gewürdigtes Verhalten der Fascia superficialis und profunda plantae pedis, wobei ich gelegentlich einen von den gleichen Muskeln der Hand abweichenden Ansatz der Mm. interossei entdeckte.

Ferner bin ich durch diese Untersuchungen in den Stand gesetzt, weitere Angaben über das Vorkommen und die Beschaffenheit der noch streitigen Schleimbeutel in dem Fettpolster der Fusssohle mitzutheilen. Ich wende mich zunächst zur Beschreibung der Fascien.

1. Fascia superf. et prof. plantae pedis.

a. Anatomie der Fascia superf.

Die Fascia superf., welche nach den Untersuchungen von Velpeau und Anderen als continuirliche, theils zellige, theils fibröse, subcutane Bindegewebsschichte die ganze Oberfläche des Körpers überzieht, hat schon von jeher die Aufmerksamkeit der Chirurgen auf sich gezogen, wie die chirurgischen und chirurgisch-anatomischen Handbücher zur Genüge, und oft zum Ueberdusse beweisen. Den letztern will ich gerade nicht vermehren, wenn ich eine genaue Beschreibung der superficiellen Fusssohlenfascie der Öffentlichkeit übergebe, und bemerke gleich von vorn herein, dass meine Fascie keine von denen ist, deren Existenz man mit gleichem Rechte behaupten und verwerfen kann, je nachdem man fette oder magere oder wasserüchtige Leichen vor sich hat. Die oberflächliche Fusssohlenbinde fand ich an den zahlreichen Leichen, die ich besonders zu diesem Zwecke untersuchte ohne Ausnahme, und jedesmal in solcher Stärke, dass sie darin alle bisher beschriebenen oberflächlichen Binden übertrifft, und schon deshalb eine nähere Würdigung verdient. Auch entspricht die Anordnung der sie constituirenden Gewebtheile durchaus nicht den herkömmlichen Angaben über Entstehung der superficiellen Fascien.

Bekanntlich sind die oberflächlichen Binden verdichtetes, membranöses gewordenes Unterhautzellgewebe, häufig nur zufällig durch Schwinden des Fettes aus den zelligen Räumen des Panniculus adiposus entstanden; daher zusammengefallene, ihres Fettes beraubte Bindegewebträume, welche mit der Zeit eine continuirliche, dünnhäutige oder auch derbe

Schichte bilden. Dies ist bei der oberflächlichen Sohlenbinde nicht der Fall, und schon aus dem Grunde nicht, weil das Fett der Fusssohle nie in dieser Art bis zum Zusammenfallen der Bindegewebsmaschen schwindet.

Die oberflächliche Sohlenbinde verdankt ihre Entstehung nicht dem weichen, blättrigen Bindegewebe der *Tela cellulosa subcut.*, sondern den sehnigen Fäden, welche, den *Panniculus adiposus* durchsetzend, Haut und tiefe Muskelbinden mit einander verbinden, und bereits mehr den Bau der Sehnen haben (Kölliker: *Gewebelehre* II, 1, Seite 9). Diese zwischen Haut und tiefer Fascie ausgespannten Sehnenfäden bilden im *Panniculus adiposus* der Fusssohle ein weit geflochtenes Netzwerk, von *Velpeau* und Anderen bereits beschrieben, dessen Maschenräume das weiche, mit Fettzellen erfüllte Bindegewebe des *Panniculus adiposus* enthalten. Ich fand nun nahe unter der Haut, durch eine dünne Fettschichte von ihr getrennt, eine flächenhafte Ausbreitung obiger Sehnenfäden in der Weise, dass dadurch eine Art *Lamina cribrosa* von dem Fersenhöcker bis zum Fussballen gebildet wurde. Die oberflächliche Sohlenbinde ist eine durch Verdichtung des oben erwähnten sehnigen Netzwerkes entstandene, siebartig durchlöchernte Platte, durch ihren Bau, ihre Stärke und ihre Lage in physiologischer und chirurgischer Hinsicht gewiss nicht ohne Interesse. Es liegt die Vermuthung nahe, dass diese Siebplatte nur der Ausdruck des von der Fläche gesehenen sehnigen Netzwerkes sei, und somit den Namen einer selbständigen Platte nicht verdiene. Als Einwendung gegen diese Anschauungsweise lasse ich eine genaue anatomische Beschreibung nebst Angabe der Darstellungsmethode folgen.

Nach sorgfältiger Abtragung der Haut, was an der Fusssohle nur durch allmähliche Entfernung einzelner kleiner Hautstückchen gelingt, erscheint ein zartes, reichlich verzweigtes Venennetz. Hierauf folgt eine dünne $1\frac{1}{2}$ –2^m starke, röthliche, feinkörnige Fettschichte, nach deren Entfernung eine glänzende, starke, zwischen Fersenhöcker und Fussballengegend straff ausgespannte, fibröse Platte erscheint, die *Fascia superf. plantae pedis*. Sie besteht aus flächenhaft angeordneten, in querer, schräger und longitudinaler Richtung einander schneidenden, dicken Sehnenfäden, wodurch eine Art Siebplatte gebildet wird, durch deren enge Maschen die gleich zu beschreibende tiefere Fettschichte

sich hügelig hervordrängt, was besonders deutlich bei abwechselndem Niederdrücken der Siebplatte mit dem Finger geschieht. Sie steht mit der Haut und mit der *Fascia profunda* durch starke Sehnenfäden in Verbindung, welche letztere, den *Panniculus adiposus* durchsetzend, das von *Velpeau* beschriebene Netzwerk bilden. Wäre die *Lamina cribrosa* nur der Flächenausdruck dieses Netzwerkes, so müsste ihre Darstellung in jeder beliebigen Richtung und in verschiedener Entfernung von der Haut gelingen. Ich trug deshalb die als eine besondere Platte leicht darstellbare *Lamina cribrosa* mit flachen Messerschnitten ab; aber das darauf folgende Netzwerk war viel lockerer, die einzelnen Fäden viel weniger an einander gedrängt, und erschienen deutlich nur als einzelne zwischen *Lamina cribrosa* und der *Fascia profunda* ausgespannte Sehnenfäden, aus denen man auch bei der sorgfältigsten Präparation keine ähnliche *Lamina cribrosa* darstellen konnte. Es ist somit meine *Lamina cribrosa* kein Artefact, was auch durch einen senkrechten Querschnitt des Fusses bewiesen wird. An einer solchen Schnittfläche unterscheidet man als deutliche Schichten von unten nach oben die Haut, die oben erwähnte dünne, röthliche, feinkörnige Fettschichte, hierauf die ohne weitere Präparation deutlich als besondere, starke, fibröse Binde erkennbare *Lamina cribrosa* oder *Fascia superf.*, hierauf das mächtige Fettpolster zwischen ihr und der *Fascia profunda*, welches durch sein mehr grobkörniges Gefüge und durch die gelbe oder gelblich weisse Farbe von der unter der *Lamina cribrosa* liegenden, feinkörnigen, röthlichen Fettschichte deutlich zu unterscheiden ist. Zwischen *Fascia superf.* und *profunda* findet man einzelne, straff gespannte Sehnenfäden, welche zum Theil bis zur Haut gelangen.

Wer sich durch eigene Präparation nach den beiden angegebenen Methoden von der Existenz dieser als *Lamina cribrosa* erscheinenden *Fascia superficialis* überzeugt hat, der wird sich mit mir nur darüber wundern, dass man ein so grobes Gebilde bisher nicht gekannt hat, zumal seine physiologische und chirurgische Wichtigkeit keine geringe sein kann, wie ich nachher angeben werde.

Fassen wir das bisher Gesagte übersichtlich zusammen, so finden wir:

Die *Fascia superf. plantae pedis* verdankt, wie die gleichnamigen Fascien anderer Körpertheile, ihre Entstehung

einer flächenhaften Verdichtung des Unterhautgewebes. Da aber das letztere an der Fusssohle, ähnlich wie an der Hand, neben dem weichen, blättrigen, mit Fettzellen erfüllten Zellgewebe noch besondere derbe Sehnenstränge enthält, so sind es nur die letztern, durch deren flächenhafte Verdichtung an einer bestimmten Stelle der Fusssohle die *Fascia superficialis* gebildet wird. Dadurch, sowie durch das siebförmige Ansehen, und durch die auffallende Derbheit unterscheidet sich diese subcutane Binde von den gleichnamigen anderer Körpertheile. Sie entspringt mit zahlreichen sehnigen Fäden von dem Fersenhöcker, und verschmilzt in der Fussballengegend, sowie an beiden Fussrändern mit der aus der Tiefe hervorkommenden *Fascia profunda*.

6. Die *Fascia superf. plantae pedis* in physiologischer und chirurgischer Hinsicht.

Betrachtet man das Verhältniss der *Fascia superf.* zum Fettpolster sowie zur *Fascia profunda plantae*, so lässt sich eine Aehnlichkeit mit den gleichnamigen Gebilden der Dammgegend nicht verkennen. Wir sehen ein massenhaftes Fettpolster zwischen zwei Fascien, welche zugleich dessen peripherische Ausbreitung durch stellenweise Verwachsung begrenzen. Am Damm finden wir eine Verschmelzung der *Fascia superf.* und *profunda* zwischen den beiden *tubera ischii*, wodurch das Fettpolster der Dammgruben nach vorn abgegrenzt, und der Ansammlung von Eiter u. s. w. ein weiteres Vordringen unmöglich gemacht wird.

Aehnlich finden wir an der Fusssohle durch Verschmelzung der *Fascia prof.* und *superf.* in der Fussballengegend und längs den Fussrändern, das dazwischen liegende Fettpolster eingeschlossen. Man hat diese Anordnung bisher noch nicht berücksichtigt, und wenn einige Anatomen, z. B. Hildebrandt, von einer dicht unter der Haut liegenden *Fascia plantaris* (worunter die tiefe verstanden ist) sprechen, so ist dies gewiss unrichtig, indem diese Fascie von der Haut durch die *Fascia superf.*, und von der letztern wieder durch das starke Fettpolster getrennt ist. In der Fussballengegend ändert sich dieses Verhältniss, indem die in fünf Schenkel sich spaltende *Fascia prof.* der Haut sich nähert, und zum Theil mit dieser und der *Fascia superf.* verschmilzt, während jetzt das Fettpolster über sie zu liegen kommt.

Auch dieses gerade entgegengesetzte Verhältniss des Fettpolsters zur *Fascia prof.* in der hintern und vordern Fussballengegend wurde bisher noch nicht beschrieben.

Durch diese in der Fussballengegend und längs den beiden Fussrändern stattfindende Verschmelzung beider Fascien wird das dazwischen liegende Fettpolster viel compacter und elastischer, als wenn es nur einfach zwischen Haut und der *Fascia prof.* angehäuft wäre. Man fühle nur an seinem eignen Fusse die zwischen Ferse und Fussballengegend befindliche zarte Haut, welche allein wohl nicht im Stande wäre, das mächtige Fettlager zu jenem prallen, elastischen Polster zusammen zu halten.

Beide Fascien verhalten sich zum Fussskelet, wie Sehnen zum Bogen; beim Gehen werden durch Abflachung des Fussbogens beide Fascien gespannter, sie werden einander selbst näher gebracht, drücken dadurch das zwischen beiden befindliche Fettpolster zusammen, und bringen somit die dem Fusse beim Gehen eigenthümliche Elasticität zu Stande.

Daraus lässt sich ferner der Schmerz bei Blutüberfüllung in Folge angestrenzter Fussbewegungen erklären, sowie die Unbrauchbarkeit des Fusses bei auch nur geringen Entzündungen oder Anschwellungen im Fettpolster, namentlich der zwischen beiden Fascien befindlichen Schleimbeutel.

Aus dem Gesagten erhellt, dass die entzündeten oder geschwollenen Theile gerade nicht an den mit dem Boden in Berührung kommenden Abschnitten des Fusses zu liegen brauchen, um Schmerzen oder Unbrauchbarkeit des Fusses hervorzurufen, indem ja bei jedem Schritte schon durch stärkere Spannung und Annäherung der beiden Fascien die dazwischen liegenden Theile hinlänglich gedrückt werden.

Ferner wird man zur Erklärung des schwierigen Aufbruchs der Abscesse nicht immer die Dicke der Sohlenhaut beschuldigen müssen, was auch nur für die Fersen- und Fussballengegend gültig wäre, indem die dazwischen liegende Haut meist sehr fein und zart ist.

Wenn ferner das Eintreten in Glasscherben oder Nägel die heftigsten Zufälle hervorrufen kann, selbst Tetanus und Tod, so braucht wohl nicht immer die *Fascia prof.* verletzt zu sein, indem durch Verletzung der derben *Fascia superficialis*, sowie durch Entzündung, Anschwellung und Ein-

schnürung der durchtretenden Theile Grund genug zu schlimmen Folgen gegeben ist.

Man giebt gewöhnlich an, dass die *Fascia profunda* die von ihr überzogenen Weichtheile gegen schädlichen Druck bei Unebenheiten des Fussbodens in Schutz nehmen soll. Ich glaube aber vielmehr, dass diese Rolle die *Fascia superf.* übernimmt, welche ohne Schaden in das darüber liegende Fettpolster eingedrückt werden darf, damit die *Fascia prof.* und die von derselben dicht und ohne Fetttage überzogenen Weichtheile vor Druck geschützt werden.

Wenn ferner nach der Angabe von Velpeau bei langsamen Entzündungen die Haut zerstört und abgelöst wird, so muss der Grund in mangelhafter Ernährung zu suchen sein, wenn die durch die derbe *Lamina cribrosa* hindurchtretenden Gefässe eingeschnürt werden.

c. *Fascia profunda pedis.*

Was die *Fascia profunda* betrifft, so gebe ich hier keine vollständige und in das Einzelne gehende Beschreibung, sondern suche, ausser einigen neuen Angaben, hauptsächlich den Beweis zu liefern, dass man die *Fascia prof. plantaris et dorsalis*, die *Ligamenta vaginalia digitorum*, die sehnigen Ausbreitungen auf der Rückenfläche der Zehen, sowie die sehnigen Streifen an deren Seitenflächen u. s. f., als Theile einer und derselben, Fuss und Zehen umhüllenden *Fascie* anzusehen hat.

Frägt man nach dem Ende der sonst so genau beschriebenen Gliederfascien, so findet man darüber keine weiteren Angaben in den Handbüchern; man beschreibt das Verhalten derselben von Anfang bis gegen aber nicht zu dem Ende, indem man Finger und Zehen unüberzogen lässt; die Gliederfascien enden in der Nähe der ersten Finger- und Zehen-Gelenke wie Handschuh ohne Finger, und höchstens spricht man noch von einem sich Verlieren der *Fascia palmaris* und *plantaris* an den Sehnenscheiden und der Haut der ersten Fingerglieder.

Nach meinen Untersuchungen finde ich eine ununterbrochene Fortsetzung der *Fascia palmaris* und *plantaris*, sowie der *Fascia dorsalis manus et pedis* bis zu den Finger- und Zehen-Spitzen. Der Nachweis dieser unmittelbaren Fortsetzung findet seine Schwierigkeiten, indem die Längs- und Quer-Faserlagen der *Fascie* sich gleichsam von einander abheben; die Längsfasern gelangen dicht unter

der Haut, besonders zu beiden Seiten der Finger und der Zehen als unmittelbare Fortsetzung der *Fascia palmaris* und *plantaris* bis zu den Spitzen der Finger und Zehen, und schützen einiger Massen Gefässe und Nerven; die quer und schräg verlaufenden Fasern liegen tiefer, sind besonders an den Seiten und an der Beugefläche der Finger und Zehen durch Fett, Gefässe und Nerven von den Längsfasern getrennt, und dienen zur Befestigung der Beuge- und Streck-Schnen, für die sie Scheiden bilden.

Die oberflächliche Längsfaserlage habe ich bereits für die Hand in einem frühern Aufsätze (Vergl. a. a. O. S. 20, und Tab. III, Fig. 8.) beschrieben. Sie wird gebildet theils durch directe Fortsetzung der Längsfasern der *Fascia palmaris*, theils durch veränderte Richtung einiger Querfaserzüge, welche in der Gegend der Köpfchen der Mittelhandknochen sich bogenförmig gegen die Finger wenden, um als Längsfasern die Fingerspitzen zu erreichen.

Ein gleiches Verhalten finde ich am Fusse. Zu beiden Seiten der Finger und Zehen finde ich diese Längsfaserlage zu besondern bandartigen Streifen verdichtet, und zum Theil mit den vorspringenden Gelenkenden der einzelnen Fingerglieder in Zusammenhang, so dass sie sich als besonders lange Seitenbänder ohne Mühe präpariren lassen.

In meinem Aufsätze über die Hand beschrieb ich sie bereits als besondere Bänder unter dem Namen: *Ligamenta lateralia longa* (a. a. O. S. 20). Auch an den Fusszehen sind sie brückenartig zwischen den Gelenkanswellungen der zweiten und dritten Fingergelenke ausgespannt, und schützen somit die darunter liegenden Gefässe und Nerven, was ihre besondere Erwähnung nöthig macht. Sie sind auch am Fusse regelmässig vorhanden und stark ausgebildet; sie wurden bis jetzt noch nirgends beschrieben.

Die quer und schräg verlaufenden Faserzüge sichern, wie bereits erwähnt, die Lage der Streck- und Beuge-Schnen, und bilden Scheiden, welche für die Beugeschnen schon längst als *Ligg. vaginalia* bekannt sind. Man beschrieb diese Bänder in der Art, dass man sie von den Knochenrändern der Finger entspringen lässt, jedoch finde ich neben diesem Knochenansatze noch einen directen Zusammenhang mit der Rückenaponeurose der Finger und Zehen, wodurch diese einen continuirlichen, quer und schräg gestreiften Ueberzug erhalten. Der der Rückenfläche ange-

hörige Abschnitt dieser sehnigen Hülle wird gewöhnlich als fibröse Ausbreitung der Strecksehnen, der *Mm. interossei* und *lumbricales* beschrieben. Diese Angabe finde ich nicht ganz richtig, indem die Strecksehnen nicht eigentlich einen Bestandtheil dieser Fascie ausmachen, da die queren und schrägen Faserzüge dieser Rückenfaszie deutlich über und unter den Sehnen hinweglaufen; die letztern erhalten dadurch einen dicht anliegenden, vollständigen Ueberzug, welcher ihre Lage sichert, ohne jedoch eine eigentliche durch einen Zwischenraum getrennte Scheide zu bilden. Man findet daher die Strecksehnen auf beiden Seiten deutlich quer gestreift (Vergl. a. a. O. Seite 13.)

Was die Betheiligung der *Mm. interossei* an dieser Rückenaponeurose betrifft, so finde ich an der Hand und dem Fusse eine verschiedene Anordnung. Wie ich schon früher bei der Hand angegeben habe (a. a. O. S. 15 und 17), so wird dort die Rückenfaszie gebildet: 1) durch seitliche Verlängerung der *Ligg. vaginalia* über die Knochenränder hinaus (Siche oben), 2) durch eigne Faserzüge, welche an den *Tuberculis capitulorum* der ersten und zweiten Phalangen entspringen, und sich membranartig ausbreitend, theils über theils unter den Strecksehnen in querer und schräger Richtung hinwegziehen, 3) durch die *Mm. interossei* und *lumbricales*, deren hautartig ausgebreitete Sehnen sich gleichsam in die bereits vorhandene Fascie verlieren, wie dies ja von andern Muskeln schon längst bekannt ist.

Diese Anordnung finde ich am Fusse zum Theil wieder, was nämlich den ersten und zweiten Punkt betrifft; der dritte Punkt ist nur für die *Mm. lumbricales* richtig, nicht aber für die *Mm. interossei*, welche, abweichend von der Anordnung an der Hand, am Fusse nicht in die Rückenaponeurose ausstrahlen, was allen bisherigen Angaben der Handbücher widerspricht. Die *Musculi interossei pedis* finden in den von mir untersuchten Fällen an der Gelenkkapsel der ersten Zehngelenke und an der Basis der ersten Phalangen ihr Ende. An derselben Stelle, an der sie sich am Knochen ansetzen, entspringt selbständig eine sich als Rückenaponeurose ausbreitende Faserhaut, welche bei oberflächlicher Betrachtung als directe sehnige Ausbreitung der Zwischenknochenmuskeln erscheint. Jedoch besteht durchaus kein Zusammenhang zwischen diesen Theilen; die *Mm. interos-*

sei, soweit meine Beobachtungen jetzt reichen, gelangen somit nicht zur Strecksehne, die Rückenaponeurose ist ganz selbständig, und dient in diesem Falle nur zur Befestigung der Strecksehnen, sowie zum Beweise einer selbständigen fibrösen Umhüllung der Zehen.

Vergleichen wir jetzt die *Fascia plantaris* und *dorsalis*, so weit sie die Fusswurzel und den Mittelfuss bedecken, mit dieser Umkleidung der Zehen, so lässt sich eine gewisse Analogie nicht verkennen. Fusswurzel, Mittelfuss und Zehen werden von einer aus longitudinalen, queren und schrägen Fasern gebildeten, continirlichen Fascie umgeben. An der Sohlenfläche der genannten Theile finden sich Scheidenbildungen in der Art, dass dadurch Kanäle mit knöchernen theils fibrösen Wandungen für die hier liegenden Muskeln und Sehnen gebildet werden. Der Rücken des Fusses und der Zehen wird von einer Fascie bekleidet, welche seitlich mit den eben genannten Scheiden der Sohlenfläche zusammenhängt, und sich in zwei Blätter spaltend, theils über theils unter den Strecksehnen vorbeizieht. Jedoch unterscheidet sich die Fascie der Zehen von der des übrigen Fusses durch die Trennung der Längsfasern von den queren und schrägen, wobei erstere oberflächlicher, daher dicht unter die Haut zu liegen kommen, und dadurch ihre Beziehungen zu den Streck- und Beuge-sehnen aufgeben. Sie dienen zum Schutze der darunter liegenden Gefässe und Nerven, sowie zur Fixirung der Haut.

Schliesslich führe ich noch ein bisher nicht näher gewürdigtes Verhalten der *Fascia plantaris* an, soweit sie Fusswurzel und Mittelfuss bedeckt. Die *Fascia plantaris* besteht bekanntlich aus einer mittleren stärkern und zwei seitlichen, sehr viel schwächern Portionen. Wie an der Fascie des Ober- und Unter-Schenkels, so unterscheidet man auch hier theils Längsfasern, theils quere und schräge. Die beiden letztern befinden sich überwiegend in den seitlichen Hälften, die Längsfasern hauptsächlich im mittleren Theil. Derselbe besitzt neben den Längsfasern einige an bestimmte Gegenden gebundene Querfaserzüge, deren nähere Anordnung und sonstige Beziehung nicht näher berücksichtigt wurden. Sie finden sich nämlich an der Ferse, sowie in der Fussballengegend, daher an solchen Stellen, welche beim Gebrauche des Fusses mit dem Boden in Berührung kommen; zwischen Fersen- und Fussballen-Gegend finden sich nur die

und da einzelne, die Längsfasern quer durchschneidende Fäden. Bei genauerer Untersuchung ergab sich mir Folgendes:

In der Fersenengegend finden sich über dem Ursprunge der Längsfasern der *Fascia plantaris* zahlreiche, starke, quer und schräg verlaufende Fasern, welche, grössten Theils von der untern Fläche des Fersenhöckers entspringend, nach allen Richtungen den *Panniculus adiposus* durchsetzen, um in der oben beschriebenen *Lamina cribrosa*, theils in der Haut ihr Ende zu finden.

Untersucht man die Fussballengegend, so findet man dort die schon längst bekannte Querfaserlage zur Verbindung der in fünf Zipfel gespaltenen *Fascia plantaris*. Bei genauerer Betrachtung ziehen diese Querfasern oberflächlich unter den erwähnten Schenkeln hinweg, zum Theil mit denselben verwebt (a. a. O. Tab. III, Fig. 8). Bei sorgfältiger Entfernung der Haut fand ich die Endigung derselben in der Art, dass die einzelnen Fäden nach kurzem Verlaufe sich mit einem oder beiden Enden bogenförmig gegen die Haut wenden, um dort ihre Befestigung zu finden. Es wird durch diese Fäden die Haut gleichsam mit weiten Stichen an die fünf Schenkel der *Fascia plantaris* genäht.

Daraus folgt, dass zwar die Sohlenhaut in ihrer ganzen Ausdehnung durch sehnige Fäden theils mit der *Fascia superficialis* theils mit der *profunda* zusammenhängt, hauptsächlich aber in der Fersen- und Fussballen-Gegend, also an beiden Enden des Fussbogens, woselbst die Haut beim Gehen am meisten der Verschiebung ausgesetzt ist.

2. *Bursae mucosae (?) plantae pedis.*

An den zahlreichen Leichen, die ich zu diesem Zwecke untersuchte, entblöste ich zuerst die oben beschriebene *Fascia superficialis*; zwischen ihr und der Haut liegt die bereits erwähnte dünne, röthliche, feinkörnige Fettschicht, worin keine Spur von Schleimbeuteln oder Lücken zu finden war. Hierauf entfernte ich mit flachen Messerzügen diese Fascie, jedoch sorgfältig, um etwaige dicht über ihr liegende Schleimbeutel zu schonen, gelangte jedoch trotz der sorgfältigsten Durchforschung des *Panniculus adiposus* zu keinem befriedigenden Funde, nämlich zu wirklichen, mit Flüssigkeit erfüllten, und mit eigener Wandung versehenen

Hohlräumen. Nur ein einziges Mal geschah es, dass mir beim Abziehen der *Fascia superficialis* etwas Flüssigkeit im Strahle entgegensprang, was offenbar eine angestochene, prall gespannte Cyste sein musste, deren zusammengefallene Wandungen ich sogleich untersuchte. Dieser Fund floss mir wieder etwas Math ein, indem er zugleich einen Wink zu andern Untersuchungsmethoden gab.

Bei oberflächlicher Betrachtung dieses Hohlraumes fand ich zwar dessen innere Wandung glatt und glänzend, aber sie unterschied sich durchaus nicht von ähnlichen Bindegewebsmaschen, aus denen ich die betreffenden Fettklümpchen hervorzog; ja ich konnte durch Auseinanderziehen der letztern an beliebigen Stellen ähnliche, mit weissen glänzenden Wandungen versehene Hohlräume erzeugen. Ich begann nun mit dem Messer einige dieser Bindegewebräume abzuschaben, und erhielt fast an jedem der von mir untersuchten Füsse eine grössere oder geringere Menge sehr deutlichen Pflasterepitheliums, wie solches an der Fusssohle bereits von Luschka beschrieben und abgebildet wurde (Vergl. Luschka: *Structur der serösen Häute* 1851).

Ich fand zart contourirte, etwas körnige Plättchen, manche mit einem länglichen, dunkler umrandeten Kerne; mitunter erhielt ich ganze Partien membranartig zusammenhängender Plättchen, meist jedoch nur einzelne derselben. In manchen Fällen fand ich nur Kerne in einer fein granulirten Masse liegen, hie und da mit einer Andeutung einer Zellencontour, welche Bildungen nach Luschka für junges Epithel zu halten sind, was auch Henle (Vergl. *Canstatt's Jahresberichte* 1852) bestätigt. Was die Grösse dieser Plättchen und Kerne betrifft, sowie ihr Verhalten gegen Reagentien, so kann ich davon nur das bestätigen, was bereits von Luschka über dieses Epithel in dessen Monographie der serösen Häute mitgetheilt wurde.

Im Allgemeinen muss ich hier bemerken, dass dieser Epithelialüberzug gewöhnlich sehr spärlich, daher wohl nur stellenweise vorkommt. Es wird zwar fast an jedem Fusse gelingen, bei sorgfältigem und längerem Durchsuchen wenn auch nur einige, aber jedenfalls charakteristische Epithelialplättchen zu finden, und zwar nach meinen jetzigen Untersuchungen in der Fersen- und Fussballengegend. Nebenbei untersuchte ich einmal eine gerade zur Disposition liegende Hand, und fand in der Volargegend der Köpfchen der

Mittelhand durch Abschaben des Bindegewebes dieselben Plättchen.

Nachdem ich nun mit aller Entschiedenheit das Vorkommen von Epithelium bald nur als einzelne, sparsame Plättchen, oder in grösserer Menge und mitunter als ganze zusammenhängende Hautstücke bestätigen konnte, so war ich jetzt um so mehr darüber erstaunt, trotz dieses Erfundes keine als Hohlräume bestehende und mit Flüssigkeit erfüllte Schleimbeutel zu finden. Den einzigen oben genannten Fall muss ich meinen übrigen zahlreichen Untersuchungen gegenüber als Ausnahme, und wie ich nachher zeigen werde, als pathologische Erscheinung bezeichnen.

Ich glaube aber, dass man im normalen Zustande sich weder die *Bursae mucosae subcutaneae* noch die *musculares* als wirkliche mit Flüssigkeit erfüllte Blasen denken darf; es ist hier wohl eine allseitige Berührung der Wandungen anzunehmen, indem man zu Demonstrationen immer erst die darüber liegenden Theile etwas aufhebt, wodurch erst die darüber liegenden Theile etwas aufhebt, wodurch erst nachträglich ein Hohlraum zum Vorschein kommt.

Nachdem ich zu dieser Ueberzeugung gelangt war, so suchte ich von nun an in dem Fettpolster der Fusssohle nicht mehr nach wirklichen mit Flüssigkeit erfüllten Hohlräumen, sondern nach glatten, feuchten, einander berührenden Flächen, da die Gegenwart von Epithelium offenbar dergleichen voraussetzen liess. Zieht man ein Fettklumpchen mit der Pincette aus dem *panniculus* hervor, so gelingt es sehr häufig, zwei in Berührung gestandene, feuchte, glatte, glänzende Membranen zu entfalten, und dadurch künstlich eine Lücke zu erzeugen, aus der man hier und da einzelne, seltener grössere Mengen von Epitheliumplättchen abstreifen kann. Durch fortgesetzte Untersuchungen kam ich zu der Ansicht, dass je eine Gruppe von Fettklumpchen durch solche glatte Membranen von benachbarten, ähnlich gegrenzten Gruppen abgeschieden wird. Man findet somit zwischen den grösseren Fettpartien der Fusssohle doppelte, einander genau anliegende, und an der Berührungsfäche mit Epithelium versehene Scheidewände. Man gelangt schliesslich zu der Vermuthung, als ob ein grosser, zwischen oberflächlicher und tiefer Sohlenbinde liegender Bindegewebsack durch einzelne, gleichsam von aussen nach innen vorgeschobene Fettgruppen vielfach in sich selber eingestülpt würde. Wir hätten demnach, ähnlich wie bei

dem Bauchfelle, ein System von mit Fett erfüllten Taschen, welche letztere sich dicht anliegen, und an einander hin und her gleiten. Wäre das zwischen den sehnigen Fäden des *Panniculus plantae* befindliche weiche Bindegewebe ein einziger, vielfach durch Fettkörner eingestülpter Synovialsack, so wäre dies gerade nicht ohne Beispiel; ich erinnere nur an die durch Fettmasse vielfach eingestülpte Synovialhaut des Kniegelenkes, sowie an das mitunter vorkommende Verhalten der *Bursa patellaris profunda*, in welcher *Luschka* mit Fett erfüllte Fältchen beschreibt. Jedoch habe ich bis jetzt meine Untersuchungen noch nicht auf die histologische Beschaffenheit des fraglichen Bindegewebes ausgedehnt, um es als besonderes Gewebe ansprechen zu können. Was ich dagegen bestimmt angeben kann, ist das stellenweise Vorhandensein von Epithelium, wie es *Kölliker* wenigstens für die *Bursae mucosae musculares* zugiebt. Dasselbe findet sich zwischen den glatten, mit einander in Contact stehenden Scheidewänden der grösseren Fettgruppen, wie ich oben bereits angegeben habe. Dagegen konnte ich mich in den meisten der von mir untersuchten Fälle durchaus nicht von der Existenz eigentlicher, isolirter, mit Flüssigkeit erfüllter Schleimbeutel überzeugen, finde aber in dem oben angegebenen Verhalten des Fettes zu dem umgebenden Bindegewebe den Schlüssel zur Erklärung für das Vorkommen solcher Räume unter ungewöhnlichen Verhältnissen. Wir haben in dem Fettpolster der Fusssohle glatte, an einander hin und hergleitende an der Berührungsfäche mit Epithelium versehene Bindegeweblamellen, welche durch vermehrte seröse Ausschwitzung in Folge heftiger oder anhaltender Reibung, wie es in der Fersen- und Fussballengegend leicht vorkommen kann, auseinander weichen, und dadurch mancherlei Beschwerden und Veränderungen bei übermässiger Anstrengung des Fusses hervorrufen können. Ich halte daher dergleichen mit Flüssigkeit erfüllte Räume für nachträglich, auf die oben angegebene Weise durch Druck und Reibung entstandene.

Das im *Panniculus* der Fusssohle befindliche Epithelium ist im Allgemeinen sehr spärlich; Gründe eines stellenweisen Defectes (daher wohl auch eines gänzlichen Mangels) sind nach *Luschka* theils sehr langsame Regeneration nach der Abstossung des dünnen Ueberzugs, theils Entzündungsprocesse.

Die zwischen obige Bindegewebblamellen ergossene Flüssigkeit kann sich bei fortschreitender Entzündung abgrenzen, wodurch allseitig geschlossene Cysten (sogenannte anomale Schleimbeutel, Henle) von verschiedener Grösse entstehen können; auch führt Rokitansky (Lehrbuch der pathol. Anatomie 1855, I, S. 230) diese anomalen Schleimbeutel unter den Cysten auf.

Die schlimmen Zufälle, die durch sie an der Fusssohle hervorgebracht werden, finden ihre Erklärung in der Lage der Cysten zwischen zwei derben Fascien (superf. und prof.). Bei fortgesetzter Einwirkung von Druck und Reibung können sich ihre Wandungen verdicken, wodurch die Entstehung einer selbstständigen, abziehbaren Membran an der innern Oberfläche der Cystenwandung möglich wird.

Sind also meinen Untersuchungen zufolge diese Cysten oder Schleimbeutel als anomale, unter ungewöhnlichen Verhältnissen entstandene zu betrachten, so fällt damit auch hinweg die denselben zugeordnete physiologische Bedeutung der Vermehrung der Elasticität und Verminderung des Druckes beim Gehen, welche Eigenschaften durch das reichliche, zwischen zwei Fascien kapselartig umschlossene Fettpolster (siehe oben) hinlänglich erklärt wird.

Ein Fall von angeborener Spalte der Clitoris.

Mittheilung von **J. Henle.**

(Hierzu Taf. IX).

Unter den angeborenen Spaltungen in der Mittellinie des Körpers scheint die Spaltung der Clitoris eine der seltensten zu sein. Die meisten Handbücher gedenken ihrer gar nicht; Otto (Handb. der path. Anatomie. Breslau 1814. p. 354) sagt, man habe die Clitoris beim Menschen in 2 Spitzen auslaufen sehen und citirt dazu eine Note Meckel's in der Uebersetzung von Cuvier's vergleichender Anatomie (Bd. IV. p. 518). An dieser Stelle ist von der Clitoris der Beuteltiere die Rede, welche, gleich der Eichel der Ruthe in dieser Thierklasse, in 2 Spitzen gespalten sei, eine Bildung, die beim menschlichen Weibe „bisweilen als regelwidrig vorkommt.“ Offenbar ist auch hiermit eine andere Form, vielleicht ein geringerer Grad des Bildungsfehlers gemeint, den unsere Tafel darstellt.

Die Tafel zeigt die äussern Genitalien eines 17jährigen, noch nicht menstruirten und überhaupt in der körperlichen Entwicklung zurückgebliebenen Mädchens, welches wegen Incontinentia urinae im akademischen Hospital Hilfe suchte. Bei der Untersuchung der Genitalien entdeckte Hr. Prof. Baum ausser einem Harnröhrenpolypen, welcher Ursache der Incontinentenz war, die abnorme Gestalt der Clitoris und der kleinen Schamlippen, und hatte die Güte, mir die getreu in natürlicher Grösse gefertigte Abbildung zur Mittheilung in dieser Zeitschrift zu überlassen. Sie bedarf kaum einer Erläuterung. Man sieht die vollkommen in 2 warzenförmige Körper getheilte Clitoris an der vordern und Seitenfläche von dem ebenfalls getheilten Praeputium umgeben, welches nach hinten gegen die Nymphen durch einen leichten Einschnitt abgesetzt ist, vorn und einwärts in die Seitenhälfte

des Frenulum clitoridis übergeht. In der Tiefe zwischen den beiden Hälften des Frenulum liegt die Harnröhrenmündung.

Störende Druckfehler
im 6. Bande Heft 3.

S. 153	Zeile 15 v. o.	lies fest	statt fast
— 154	— 13 —	— folglich	— plötzlich
— 159	— 5 —	— 800	— S + 800
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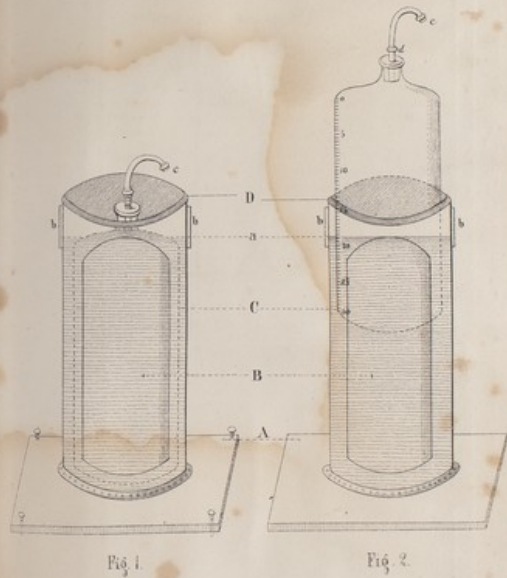
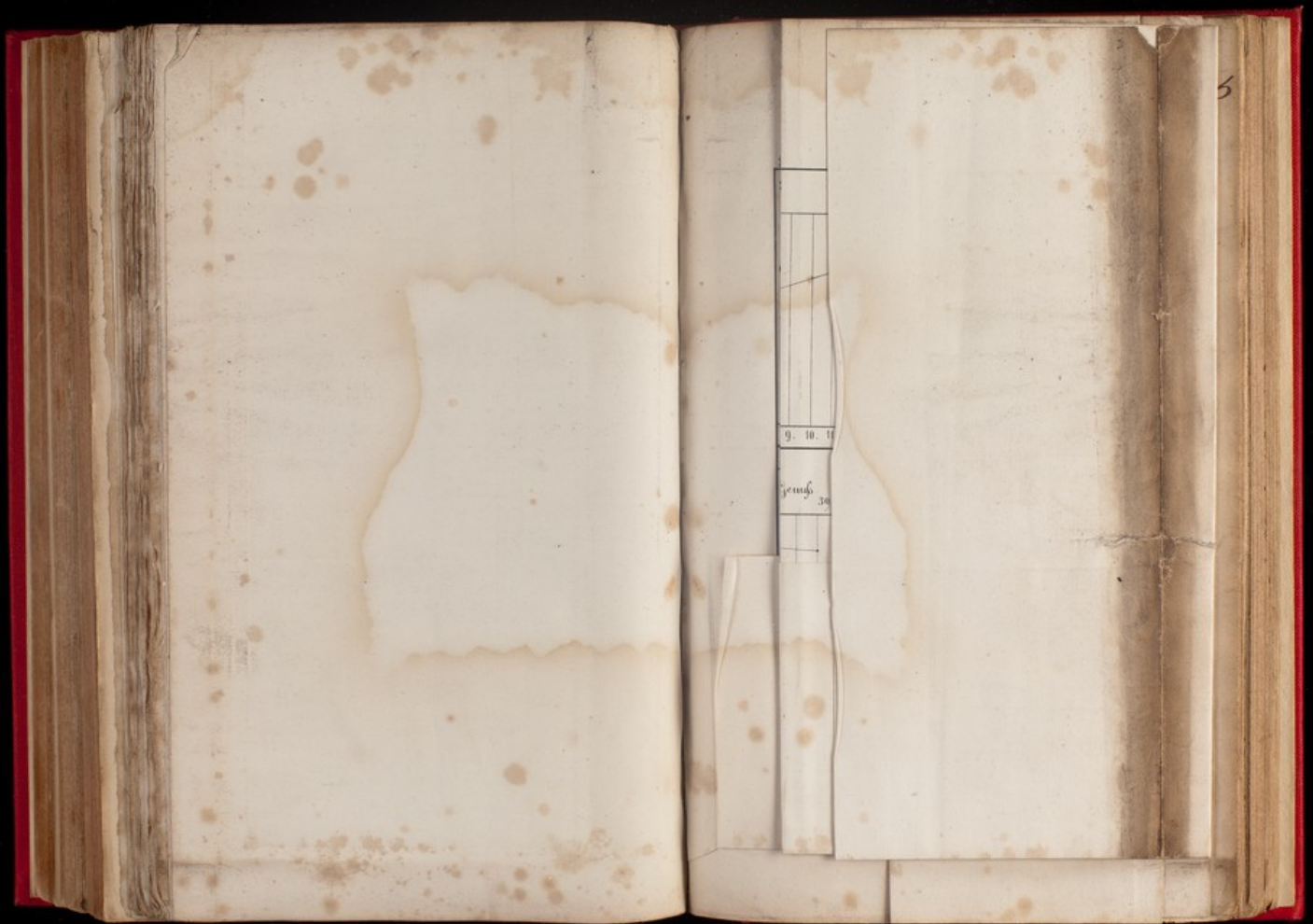


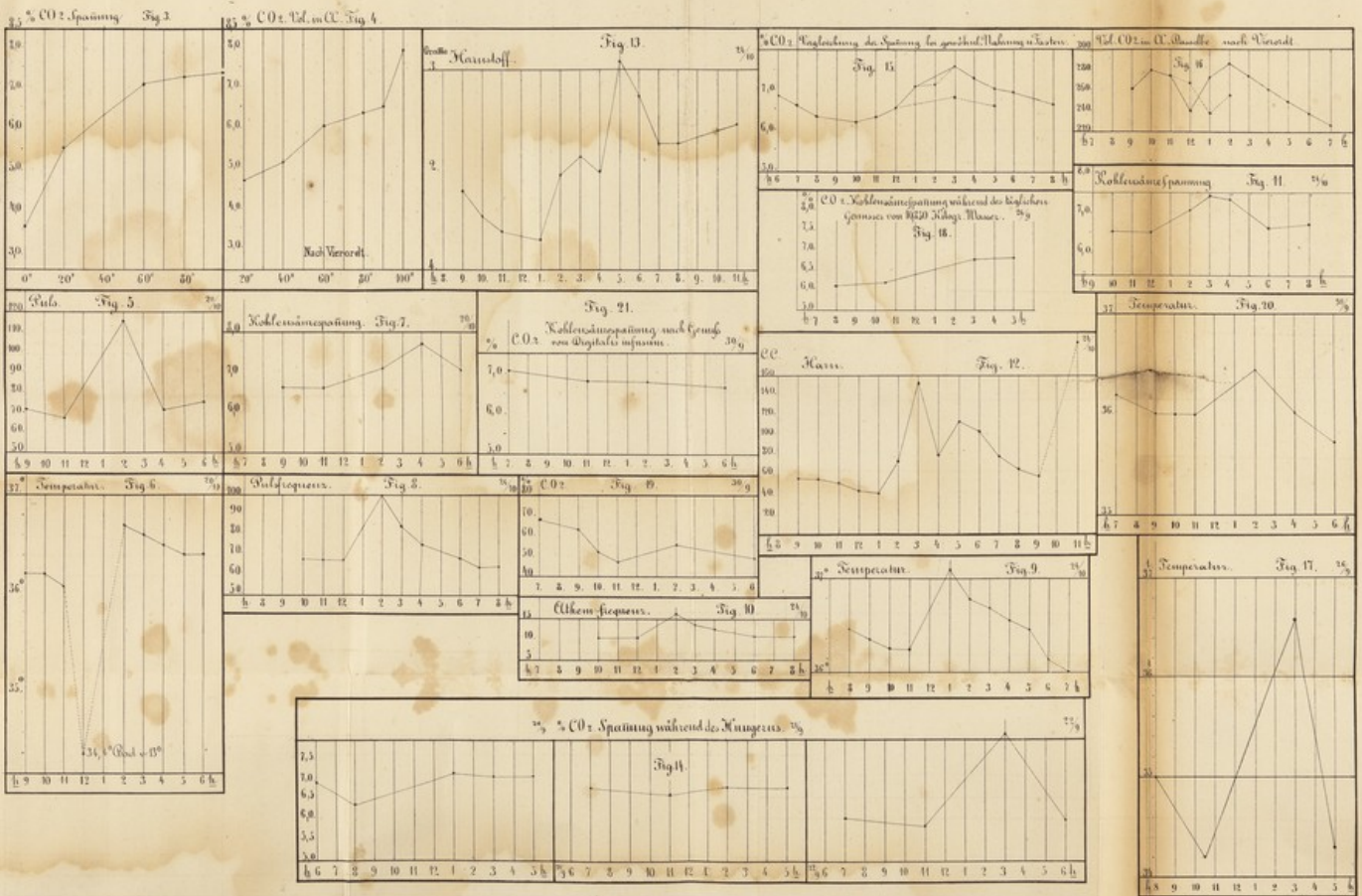
Fig. 1.

Fig. 2.



9	10	11
pounds		
30		

5





Handl. u. Pankr. Zoster. N. 18. Pl. Taf. III.



F. K. Schradl.

Hand u. Phosfor Zeitschr. N. F. Bd. VI. Taf. IX.

Hand u. Phosfor Zeitschr. N. F. Bd. VI. Taf. IX.

3



O. Eberlein sculp.

F. Kuchardt del.

*J. Andrew Smith
Superintendent Army Medical Department*

REMARKS

ON THE USE OF THE

INDIAN BAEI OR BELA

IN

DYSENTERY, DIARRHŒA, &c.

BY

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REMARKS
ON THE USE OF THE
INDIAN BAEL OR BELA
IN
DYSENTERY, DIARRHŒA, &c.

*History of the Bael—Botanical Description—Chemical Analysis—Bael Preserves—
Bael in Dysentery—Diarrhœa—English Cholera—Renal and Vesical Disorders—
Chronic Skin Diseases—Preparations and Doses of the Bael.*

1. It is now about two years ago that the *Bael* or *Bela*, a fruit well known and long celebrated in India as a remedy for Dysentery, was imported into England by Mr. Pound, of Oxford Street, and by him brought before the Medical Profession. But although many physicians prescribed it pretty extensively, both at hospitals and in their private practice, and derived considerable advantage from its employment, yet want of sufficient confidence in a comparatively new medicine, and of experience in its effects, have hitherto prevented its general adoption; and the majority of the profession in this country at the present time, with the exception of those who have met with it in India, or become acquainted with it accidentally, are probably unconscious of its existence.

*History
of and
Introduction
of the
Bael.*

[The following remarks may be interesting as a brief notice to those of the Profession who have not used the Bael—a medicine approximating to the class of Vegetable Astringents, but possessing, in addition, peculiar properties which are strongly recommended to their more extensive consideration and employment.]

Having lately had frequent opportunities of administering it, and contrasting its effects with those of other similar medicines, I am enabled to give a brief description of the fruit, and a favourable report of its medicinal value.

I am the more disposed to believe that the following remarks will be found advantageous, from the prevalence of Dysentery lately in many parts of England, and the mortality it has occasioned; and although of a far milder nature than in tropical climates, yet even in this country it occasionally rages with considerable violence, (especially in hospitals, asylums, workhouses, and on board ships, where large assemblies of human beings are congregated,) and it then becomes a matter of importance to the practitioner to have some remedy at hand which has been known to succeed after ordinary measures have proved unavailing. This the Bael has done; and although Dysentery is, comparatively speaking, not a disease of this country, and Diarrhoea also ordinarily sufficiently manageable, yet both the former occasionally occurs as an epidemic of uncontrollable violence and fatality, and the latter assumes an obstinate chronic form which resists all remedial measures.

It is upon these grounds that I feel myself justified in recommending very strongly a medicine which has been highly successful in my hands, and long extolled in countries where the disease assumes its most malignant forms.

2. Botanical descriptions of the tree which yields the Bael fruit will be found in the works of Ainslie, Persoon, Roxburgh, Sir W. Jones, Lindley, Royle, and others, who also remark upon its medicinal effects.

It is the *Egle* or *Crataeva Marmelos* of Linnæus, and

although commonly termed the Bengal Quince, this name is incorrect, as it is one of the *Aurantiaceæ*, or orange tribe.

Lindley says, speaking of this fruit and others of the same tribe: "A decoction of the root and bark of *Egle Marmelos* is supposed, on the Malabar coast, to be a sovereign remedy in hypochondriasis, melancholia, and palpitation of the heart; the leaves in decoction are used in asthmatic complaints; and the fruit, a little unripe, is given in diarrhoea and dysentery. Roxburgh adds, that 'the Dutch in Ceylon prepare a perfume from the rind; the fruit is most delicious to the taste, exquisitely fragrant and nutritious, but laxative; the mucus of the seeds is a good cement for some purposes.' The leaves of *Bergera Konigii* are considered by the Hindoos as stomachic and tonic; an infusion of them toasted stops vomiting; the green leaves are used raw in dysentery; the bark and root internally as stimuli."—*Lindley's Vegetable Kingdom*, Order clxx.

Royle further remarks: "The astringent rind of the fruit of *Egle Marmelos* (*Bel* of the Hindoos) is used in dyeing yellow, while the glutinous and tenacious matter which surrounds the seeds is considered an excellent addition to mortar, especially in well-building. *Bergera Konigii* is cultivated near villages, as its leaves are considered in the northern, as in the southern provinces, a pleasant addition to curries and other dishes in use among the natives of India."—See "*Illustrations of the Botany of the Himalayan Mountains*," vol. i. page 130.

A full botanical account of it is given also in the Transactions of the Medical and Physical Society of Calcutta, vol. iv. p. 110.

It grows very abundantly in the country around Bengal and other parts of India, where the juice of the unripe fruit is the part used for Dysentery; but as we cannot here obtain

the fresh juice, the whole of the fruit is dried and exported to England, and from this dried fruit is prepared the medicinal decoction.

Chemical description of Bael. 3. The examination of the Bael chemically shews it to contain *tannin*, either pure or in combination, a large amount of a peculiar mucilage, a concrete essential oil, and an aromatic as well as a bitter principle; and in one or other of these there appears to exist either a sedative or narcotic property; but as yet so small an amount of it has been procured separately, that its exact medicinal virtues have still to be determined.

The smell and taste of the decoction are aromatic and agreeable, slightly bitter and astringent, gummy and mucilaginous; something like a mixture of decoction of quince and pomegranate, but bearing an aroma peculiar to the Bael. It possesses, however, the important property, different from most vegetable astringents, of not subsequently confining the bowels, but appears rather to strengthen and regulate their action.

The *fruit* arrives in this country broken into dry hard chips, like pomegranate chips, the rind being, like the pomegranate's, very hard and dry.

4. There is also imported a preparation of the ripe fruit, the Bael Jelly for Constipation. *Bael preserve or jelly*, which has an opposite effect from the unripe fruit, being a gentle but effectual laxative. It is eaten at meals, like marmalade, every morning; and acts moderately on the bowels during the day. This is found very useful to ladies and children, whose bowels are frequently habitually torpid, and whom it is injurious to accustom to continual purgation.

5. It is, however, the unripe fruit to which I shall at present confine my remarks as a remedy especially for Medical use of Unripe Fruit. Chronic Diarrhoea and Dysentery.

Much has been lately said of the value of *dilute sulphuric acid* in these cases, and I can speak from experience of its highly beneficial results. Its employment is not new, having been extolled as an almost sovereign cure for Dysentery by Moseley, who wrote on Tropical diseases some sixty years ago. He prescribed it in conjunction with alum in what he called the "*Vitriolic Solution*," and, while deprecating the use of it as a *specific*, pointed out the benefit that might be obtained from its judicious and discriminate use. The great objection to it which I have found, has been that it obstinately confines the bowels, which the Bael does not do, and occasions sometimes, in consequence, cramps and colics worse than the original complaint.

Tannin also, alone and in combination, has been highly recommended by Professor Alison, and the value of *oxide of silver* in intestinal fluxes as well as in gastric disorders insisted upon by Sir James Eyre; so that it has been urged by many that there is no need of a medicine like the Bael, for that Dysentery can either be cured without it, or an artificial Bael compounded of Tannin and mucilaginous ingredients.

As far as I have observed, however, I cannot say I have found this to be the case. The Bael, like many other vegetable medicines, possesses properties peculiar to itself, and does not owe its efficacy to the mere presence of tannin in its composition; what these precise properties may be, is at present out of our research, but they certainly impart to it somewhat of the character of a *specific*.

It is the same with other drugs whose active principles have been extracted by chemistry, but which, thus isolated, produce dissimilar effects from the original natural compound.

This is strikingly the case with opium, whose place its alkaloid morphia cannot be made to supply by any artificial combination whatsoever; and the delicate distinctions between the effects of the foetid gums, and of many other substances, between which there is very little appreciable difference, are instances of these peculiar and special characters of natural productions. Moreover, I deny that it is to the mere property of astringency, or to the mere presence of tannin, that are to be ascribed the curative virtues of this class of drugs, for neither are the most powerful astringents the best medicines to give in Dysentery, nor are those fruits found to be the best which yield the largest amount of tannin. The astringency of the Bael is slight, and the quantity of tannin it contains inconsiderable; but its action is no less certain than that of much more powerful astringents. It is also safer in its operation, agreeable to the taste, and does not constipate.

In acute cases of Dysentery the treatment of the physician is naturally guided by the pathology of the disease; consequently free leeching in the course of the colon, the administration of Castor Oil with Dover's Powder, gently to cleanse away any irritating fœcal or other matters, and to soothe the over-excited membrane of the bowels; calomel, if necessary, in moderate doses, varied according to circumstances; opiates judiciously administered; dilute sulphuric acid as a grateful febrifuge and tonic,—constitute the most rational and proper treatment, and in many cases are sufficient to effect a cure.

But inasmuch as the symptoms of Dysentery, especially in the epidemic form, evidence peculiarities beyond what are caused by mere inflammation of the colon and rectum—for this condition frequently exists without the occurrence of that frightful tenesmus and constant bloody evacuations which characterise Dysentery—so I think we may, with advantage, bestow some attention to the examination of a medicine which promises peculiar powers of adaptation to meet the exigencies of this disease.

In the acute stage, although many hesitate to give astringents, and treat the disease entirely as an inflammation, yet the use of the Bael, if merely as a demulcent to lubricate the irritable or ulcerated intestines, is productive of the utmost benefit. But, independent of its other properties, I believe myself that it has some peculiar principle which is antagonistic to the poison which produces Epidemic Dysentery, as well as some anodyne, or perhaps narcotic, which acts powerfully upon the inflamed bowel; and should this supposition be found to be correct, it may readily be conceived of what vital consequence is the possession of such a medicine, not only during a severe occurrence of that form of the complaint, but in numerous other diseases, which it is not necessary to specify.

7. In *chronic Diarrhœa* the effect of the Bael may perhaps be even more striking when administered after the successive failure of other medicines, for it is in this Chronic Diarrhœa. form of the disease particularly that our treatment most frequently disappoints us, and our remedies most frequently fail. In that uncertain, irregular state of bowels so constantly met with, where the looseness is scarcely sufficient to constitute

diarrhoea, and yet the motions are never of their proper consistence; when ordinarily there exists no pain, but yet even a cup of warm tea or coffee is sufficient immediately to produce severe tormina and desire to go to stool; when this irregularity and uncertainty of bowel is productive of the greatest distress and inconvenience to the patient, while its long continuance is sure to lay the foundation for more serious mischief;—in this state of things, I am not saying too much to assert that there is no remedy equal to an habitual use of the Bael; and if we consider what our treatment of such cases ought to be, we shall find, that, even if it possesses no other qualities than those which art can imitate, it at least supplies us with exactly what we require in a natural form.

But the apparent inconsistency of its properties, at one time acting as a laxative, at another having an opposite effect; and the different modes of action of the ripe and unripe fruit, so accurately *regulating* the bowels; relaxing without purging, checking diarrhoea without constipating,—shew that we must look to other than the ordinary ingredients of kino or catechu to explain the remedial agency of this medicine.

In *English Cholera* it must be taken very freely, more as a dietetic drink than a medicine, mixed with a little wine or brandy, preceded by castor oil, and combined with dilute sulphuric acid and opium.

It will not be necessary for me to particularize the numerous cases in which a relaxed state of bowels occurs as a complication of other disorders, and not as the primary disease. Thus, in Diarrhoea from teething of children, or accompanying consumption; in disorders of the stomach occasioning flatulence; in chronic dyspepsia and heartburn, the greatest benefit has been derived

from its use; and in the two latter the Jelly is an excellent preparation, and may be taken with the greatest possible advantage.

The Bael may perhaps be found, like the Uva Ursi or Pareira, to exercise a beneficial influence in *renal* or *vesical* disorders; but I have had no experience in its employment in these cases.

8. Lastly, as a vehicle for the administration of irritating minerals which produce intestinal inflammation and excite diarrhoea, as arsenic, bi-chloride of mercury and cantharides, it recommends itself very strongly to those who especially study *chronic skin diseases*.

9. The Bael may be had in the dried chips, in powder, in the form of *wine*, concentrated *decoction*, and *extract*. The two latter are the preferable preparations, and the decoction the best of the two, as the evaporation, not having been carried so far, more of the aromatic principle is retained. It is the one I usually employ, and was first made by Mr. Pound of Oxford Street, who devoted much time and many experiments to obtain it in the best state.

To make a simple decoction of the Bael, four ounces of the chips may be simmered in a pint of water in a covered saucepan for about two hours, or until the chips have become soft. The liquor should then be *pressed off* and strained, and made up to half a pint. This may be sweetened with sugar and acidulated with dilute sulphuric acid, but must be made fresh every second or third day, according to the state of the atmosphere, as the quantity of mucilage it contains favours its early decomposition.

The concentrated decoction (as before mentioned) is four times the strength of the above, and of this the dose is one or two tablespoonfuls for an adult, three or four times a day, in cases of Dysentery or Diarrhoea, and one or two teaspoonfuls for flatulence and indigestion. The extract may be made into five-grain pills, and two taken twice or thrice a day.

I believe I have now said all that is necessary upon the subject of the Bacl. Those who have obstinate chronic cases of Diarrhoea or Dysentery under their care, will find it of especial value; provided they administer it steadily and confidently—not expecting immediate or wonderful results from it at first, but a certainty of permanent relief from its continuous administration.

When all other means have been tried without effect, and the Pharmacopœia vainly ransacked for relief, the physician will be pleased to meet even with a useful palliative, (should the case have proceeded too far to admit of cure,) and in other cases, if given in time, and in sufficient doses, it has never disappointed me in its results.

33, ORCHARD STREET, PORTMAN SQUARE.
January, 1853.

HISTORICAL
RESEARCHES
ON COUNTER-IRRITANTS
(EXUTORIES)

HISTORICAL
RESEARCHES
ON COUNTER-IRRITANTS
(EXUTORIES)

ALBESPEYRES'S BLISTERS
AND
BLISTER PAPER

Given a grave lesion, to produce an artificial
one elsewhere, more active but less dangerous, in
order to attenuate the former.

TROUSSARD and PIBOUT.

BY FUMOUBE

HONORARY PRESIDENT OF THE APOTHECARIES' SOCIETY OF THE SEINE,
HONORARY MEMBER OF THE IMPERIAL PHARMACEUTIC SOCIETY
OF ST-PETERSBURG,
AND OF THE PARIS SOCIETY OF THE FRIENDS OF SCIENCE

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HISTORICAL
HISTORICAL
ON COLICULAR IRRITANTS
ALBESPEYRES'S BLISTERS
ALBESPEYRES'S BLISTERS
BLISTERS HANDBOOK
BLISTER PAPER

connected with the fact of its discovery, but as the
evidence that the same result was obtained by
other than vesicant means, and that the history of
the use of blisters is not a new discovery, but
that the question of observation of the fact is
lighted up by the history of the country, and that
leaving out the history of the vesicant, and which
is well known, and that the history of
the use of blisters is not a new discovery, but

As the pupil, son-in-law, and successor of
M. Albespeyres, who was the first to make epi-
spastic paper, and thereby brought about a complete
revolution in the dressing of artificial sores, we
have been, from our very position, under the ne-
cessity of manipulating considerable masses of epi-
spastic compositions, and it is therefore not at all
surprising that we should have taken an interest in
tracing out all the facts connected with the history
of vesicants.

In the course of our reading on medicine and
therapeutics, we have met with many curious facts

connected with the matter in question, but so disseminated that the mind could with difficulty perceive their precise bearing.

We have therefore collected and grouped together the opinions and observations of the most enlightened scientific men of all countries, a task conferring but little honour on the writer, but which he trusts will prove useful, and that will satisfy his ambition.

HISTORICAL

RESEARCHES

ON COUNTER-IRRITANTS

(EXUTORIES)

I

ON COUNTER-IRRITANTS

(EXUTORIES)

Exutories are secreting surfaces artificially produced on the human body and kept open by proper dressings, in order to obtain a derivation useful to health. They are produced by means of moxas, incisions, caustics, but more especially epispastics, or blisters. They are an imitation of the processes which Nature herself employs when she produces a gathering, a discharge, etc.

Exutories are also employed as preservatives, as auxiliaries in various treatments, and as heroic remedies. They thus play an important part in therapeutics, having been employed in all ages, in all nations, and by all the schools, without distinction of system or method, which have in turns held sway in the medical world.

They constitute a unique order of derivatives.

The use of blisters may be traced back to the most remote period of medical practice in Greece. Asclepiades, Archigenes, Aëtius, Caelius Aurelianus, Aretæus, Galen, etc., frequently employed them. Among the moderns,

Fernel, Ambrose Paré, Dupuytren proved them very effective. Rivière sometimes applied as many as five blisters in malignant fevers with complete success. Sydenham held them in great esteem, especially for affections of the brain and of the chest.

At the very outset of our undertaking we experienced great embarrassment. Should we divide *Exutories* into several series, and devote our attention equally to all? That would have diverted us from the end we had in view, which was far less ambitious and altogether special. We consequently preferred limiting our investigations to blisters properly so called, but of course considering them in connection with the other agents of the same order whenever we could do so without unduly generalizing the question. Having once decided on this principle, our path lay straight and plain before us.

For the sake of greater assurance and authority, we shall give numerous quotations from authors of high repute, instead of stating the facts in our own language.

“During last century, the use of topics (blisters), was still limited to a narrow circle of complaints, and was considered by a great many physicians as an extreme resource. It is only since the beginning of the present century that they have come into such general use as to be no longer regarded with apprehension, that physicians prescribe them for a multitude of slight affections; and that, in fine, many persons apply them, without the intervention of a physician, to obtain relief from head-ache, sore eyes, colds, etc.”

(Extract from GERDY'S *Traité des Pansements*.)

“Vesicatories constitute the collection, the whole body, the treasury of all the means which the medical art employs externally, with a view to extract, or draw to the surface of the body, or turn from one part to another, whatever may endanger health

or prevent its restoration. It is in this sense that the word *vesicatory* ought to be understood.

“This term comprises not only blisters, which are commonly applied in the form of a plaster, but extends also to all the acrid, irritant, stimulant, exciting, and caustic substances, which, when applied to the surface of the body, or even in some cavity regarded as a continuation of that surface, produce more or less promptly redness, tumours, slight inflammations, blisters, irritation, eschars, etc.

“Thus generalized, the system, of vesicatories has supplied physicians with the most extensive resources and the most striking successes.”

(Dr. M. H. FOUQUET, in the *Encyclopédie générale*.)

“He who first conceived the idea of drawing towards the outside of the body an inflammation which was raging within; of displacing or generalizing, in some sort, the centre of morbid irritation, by dispersing its elements and distributing them over a greater number of organs, discovered one of the most important dogmas of our art. That idea first presented itself to Hippocrates, and it supplied most extensive resources to therapeutics in the palmiest days of the Greek school of medicine.”

(ALIBERT'S *Nouveaux éléments de thérapeutique*.)

“The theory founded on the hydraulic law has always been at fault when it was adduced to explain how a blister applied on the left arm promptly dissipated a congestion, an inflammation, or a violent irritation caused by a repelled herpetic humour. As it was thought that the humour could only be drawn to the outside or to its primitive seat by means of the circulation, the success of the remedy excited wonder, but was never understood; the mystery has now been fully explained!

“There are a great many chronic affections in which the utility of epispastics is fully recognized: it is always advisable to draw outwards whatever threatens or affects any parts essential to life.”

RETZ.

"Blisters draw out the putridity which causes the disease. As to the pain they cause, it is greatly exaggerated; and, besides, the remedy rather calms than irritates a patient, by the benefit resulting from revulsion."

MERCURIALS.

"When any noxious humour has entered or been produced in the body, we ought to correct the bad qualities, or expel them through the skin or the natural evacuations, and if those natural issues will not effect the purpose, we must open artificial ones to supply the deficiencies of nature."

"The ancients, who, with little theory and few scientific pretensions, were nevertheless great observers and successful practitioners, frequently employed issues and blisters, because they had witnessed their curative and preservative effects."

RAYMOND.

"The name of revulsives is given to the different means employed in medicine to turn aside the principle of a disease, or a humour, towards a more or less distant part."

NYSTEN.

"It is indispensable that revulsive stimulants should be more powerful than those to which they are opposed."

BROUSSAIS.

"In men who have a venomous skin, the simplest lesions suppurate on the slightest occasion; an exutory kept constantly active may remove that tendency."

BRETONNEAU.

"Vesicating substances acting on the skin augment its secretions and rouse its energies; they revive the diminished power of the organism; they produce, if extensive, a signal reaction, a kind of fever well known to practitioners, the action of which, spreading and generalizing, in some sort, the seat of the disease, may aid the development of the local affections for which they are most frequently prescribed, and work a cure."

"Blisters are especially prescribed in affections, whether acute or chronic, for which it is necessary to arouse the natural powers and produce derivation; and then their use is temporary or continuous. They are more particularly employed in complaints caused by repercussion or metastasis; in flying or changing diseases, rheumatic and neuralgic pains; cutaneous eruptions, which do not come out freely or have been repelled; inflammations of every kind, etc."

(Dictionn. de Matière médicale, by MÉRAT and DE LEUS.)

"Blistering is a non-mechanical irritation which determines the formation of a sero-albuminous vesicle on the surface of the skin."

"There is no part of the body to which blisters have not been used; if there be no danger of aggravating the disease, the remedy may be unhesitatingly applied as near as possible to its seat; if there be any danger of irritating the diseased organ, it is advisable to operate at some distance, and then the parts most commonly selected are the back of the neck, the inner side of the arm, the inner and middle surface of the legs and thighs."

GERDY.

"Vesicants form a section of external irritants, and act as stimulants and revulsives. They differ from issues, setons, cupping, electricity, galvanism, which are also external irritants and revulsives, but their action is of quite another kind, since they do not produce a blister nor any sort of evacuation, like vesicants."

MÉRAT and DE LEUS.

"As a spoliator, the *Exutories*, from the slowness and continuity of its action, and the facility of measuring and graduating its effects, will always hold the first rank among the agents of spoliative medication."

(Traité de Thérapeutique, by TROUSSEAU and PIDOUX.)

HISTORICAL SKETCH
OF
VESICATING SUBSTANCES

The number of vesicants is considerable, and it will not be without interest to give a brief notice of them here, were it only to aid those who, like ourselves, wish to form an accurate opinion on the value of the different bodies, which have been in turns vaunted as epispastics, then neglected, and again resumed with a character of factitious novelty by persons who perhaps knew no better.

They are found in the three kingdoms of nature: most numerous in the vegetable kingdom; most violent in the mineral kingdom; most complete and defined in the animal kingdom.

Among the mineral vesicants are certain salts, especially the emetic—most of the acids, the caustic alkalis, boiling water, etc. Acids and ammonia require extreme precaution, and can only be used with safety by medical men; nor ought they to be used, except in rare cases when an instantaneous effect is necessary: and even then vesi-

cation by means of the actual cautery, or of a hammer held in boiling water till the head is thoroughly heated, is generally preferred. This last process, which was long in common use throughout the United States, is now nearly always replaced by cantharidized collodion or blister plaster.

Entire classes of plants supply vesicants, or might supply them: such as the alliaceæ, the aroideæ, the euphorbiaceæ, the ranunculaceæ, the piperiteæ, the urticeæ, several thymeleæ, and especially the garou (*daphne gnidium*); a certain number of crucifera, as mustard-seed, horse-radish, cochlearia leaves, also ginger, tobacco, the wild cucumber, Spanish pellitory, etc.

It is to be remarked that the most active of these substances known to the ancients, were also simultaneously administered internally, as purgatives or emetics, and externally as irritants, excitants, and vesicants, which led the humoralists to call *Exutories* external purgatives. For instance: the concrete, resinous, or oily juices of the euphorbiaceæ, employed in a thousand ways, in as many different cases, from the days of Melampus, who, 4530 years before Christ, cured the insanity of the daughters of Proetus, king of Argos, with either white or black hellebore; the *thapsia* (the *syphion*, of Dioscorides, the *laser* of the Greeks and Romans), thus named by the ancients from the isle of Thapsos, whence it was procured: this substance was employed by Hippocrates as an evacuant (Sprengel), and by the Arabs as an epispastic, against herpes and other skin diseases (see POIRER's *Voyage en Barbarie*); the *rhus toxicodendron*, which we get from North America, appears to have been used by the Indians for the same purpose: in 1788, Dr Du Fresnoy, physician to the army, made known the properties of this plant, which were accidentally discovered in France by a gardener, who was cured of a herpetic affection after an eruption caused by this species of sumach.

All these and many other substances, of the vegetable kingdom, are now but little employed, owing to their feeble action as compared with cantharides.

In the animal kingdom many substances have been tried, vaunted for awhile, and then rejected. Ancient writers mention as epispastics, ants, certain excrements, as the droppings of the wood-pigeon, ox-gall, the flesh of snails, etc. The cantharis, or blister-fly, appears to have been first employed as a vesicant by Aretæus: "We use," says Archigenes, his imitator, "cataplasms containing cantharides, because they produce great effects, provided the little ulcers remain open and keep on running." Ælius informs us that Galen also used cantharides with great success.

Cantharides have been employed down to the present day, and science had not yet discovered to what principle their virtues must be attributed. The ingenuity of modern chemists has drawn from them a greenish oil, which was at first supposed to contain their active principle; but Robiquet subjected them to a more complete analysis: he separated from this oil a new substance, to which he gave the name of cantharidine, and he proved this to be the real epispastic agent of these coleoptera. Many chemists have since corroborated this discovery, among whom we may mention Messrs. L. Gmelin, Thierry, Henri and Plisson, Regnault, Beauvois, and Hatchett. Cantharidine has thus been thoroughly examined and defined: the least particle of this substance will raise blisters on the skin; its vapour is very dangerous for the eyes, the nose, and respiratory organs; taken internally, its ravages equal those of the most violent poisons.

Here the question naturally arises: what cantharides contain the greatest proportion of the active principle? And by what tokens are they to be distinguished?

The labours of our chemists have taught us nothing po-

sitive on this point, and yet it is a matter of some importance, even in a commercial point of view, to be able to judge of the value of the article, by sight and touch, since they are always bought at first hand in open market, especially at the Leipsic fairs, where the purchaser has to judge of their quality on the spot. Long experience is evidently the best guide. The following is the result of our own observations: in the trade, cantharides are divided into three sorts — small, middling, and large. All are caught in the same localities; they are, more or less, of a yellowish-green tint, with a bright metallic lustre. When the yellow predominates, and the insect is very dry and but little broken, you may buy boldly, whether large or small; if the green is in excess, hesitate; if the flies, when pressed in the hand, feel rather greasy, have nothing to do with them; for it is certain that they have been dipped in oil, to increase their weight and give them a more showy appearance, but they will prove deficient in quality, as part of the cantharidine may have been extracted by the oil. This deceptive kind has long been sold under the designation of *northern cantharides*. We have been cheated by them, to our great annoyance, and it was then we discovered the fraud and resolved to expose it. (*Journal de Chimie, Pharmacie, et Toxicologie*, 1851, p. 653.)

We have also had to reject large quantities of cantharides spoiled by sea-water. Cantharidine being insoluble in sea-water, it might be inferred that we here acted with excessive precaution; but it must be observed that these cantharides had acquired a peculiar appearance, which rendered it difficult to judge of their quality, and they had certainly gained additional weight; hence the impossibility of dosing them accurately; lastly, this damage by sea-water might perhaps conceal certain tricks of trade well-known to fraudulent dealers.

Setting aside all fraudulent practices, it may be asserted that the cantharides caught in France, Sicily, certain provinces of Spain, and in the Ukraine, are the best ¹.

In general, cantharides lose from 10 to 15 per cent. of their weight during the first year after they are collected, and from 4 to 7 per cent. in each following year, a fact which must never be overlooked by those who wish to have their preparations always of the same strength.

The ancients made their blisters with inert plasters, or any salve-like substance, which they powdered with pulverized cantharides; this was then applied to the skin and the blister was raised in about twenty-four hours. Very frequently some portion of the powder would become detached, and, spreading to the adjacent parts, would produce small blisters where they were not wanted.

The plaster itself was also liable to shift about and make a blister much too large. The protracted application was also apt to produce great irritation in the bladder.

At a later period, blisters were prepared by incorpo-

¹ Cantharides are very abundant in France, where clouds of them may be seen in May and June to settle on ash, lilac, and other trees, which they soon strip of every leaf. It is then time to catch them, for they live only eight or ten days. Men with masks on their faces shake the boughs early in the morning, while the flies are still benumbed by the chilly night air, and they fall on sheets spread on purpose to receive them. They are then thrown into diluted vinegar, dried in the sun or over a stove, and are ready for market. For some years past, collecting these flies has been too much neglected in France, although considerable sums might be gained without any hinderance to other work. It is highly desirable that the inhabitants of the regions visited by these insects should catch all they possibly can.

rating the powder, an insufficient amelioration, as the plaster would still shift about, whatever precautions were taken, and only operated slowly.

The discovery of cantharidine induced many practitioners to prepare blistering silk with a more or less perfect solution of that substance in alcohol or ether; but this was not adhesive, and in a few days lost part of its strength, which, added to the danger of handling so powerful a substance as cantharidine, soon led to the relinquishment of that expedient.

M. Albespeyres, who had devoted himself to a profound study of all the epispastic substances, then introduced his adhesive blister plasters, submitted them to the medical profession, for experiments in private practice and in hospitals, and gradually attained the most perfect result, which we now request the practitioners of all countries to submit to any trial they may deem conclusive.

A good remedy carries its own recommendation, and has nothing to fear from the severest tests.

ALBESPEYRES'S BLISTERS

Blisters are the epispastics in most frequent use.

ALBESPEYRES.

Albespeyres's blisters constitute a real vesicating spadrapp. To apply them, nothing more is necessary than to cut a piece in the form required and lay the black side next the skin, the green side, bearing the name "Albespeyres," being quite inert; a compress and a bandage to maintain the whole in its place complete the operation. In cold weather, it must be held a moment near the fire before laying on, and at all times it is indispensable to press firmly with the hand over the whole surface, so as to make it adhere well in every part. Should there be any hair on the place where the blister is to be applied, it must be shaved off.

In six or eight hours, the vesication will be complete, and a vesicle full of serous fluid will be formed; some

times, however, it will happen that the place is only very red, and the skin will not rise till after the first dressing, which should be made either with fresh lutter or hog's lard.

When the blister has risen, cut the loose skin all round, but do not remove it till twenty-four hours afterwards, in order that the natural irritation of the wound may not be over-excited by the immediate contact of the air. After one or two sedative dressings, all the subsequent ones may be made with Albespeyres's paper, as will be explained hereafter.

In certain circumstances, the skin absolutely resists the action of the most powerful vesicants, as we have had the misfortune to experience on our own person.

FLYING BLISTERS

If it is wished to apply a flying blister or to produce a slight irritation, the blister must be left on the skin half an hour, an hour, or two hours, according to the end in view. It will act at first as a mustard plaster, and will gradually become more irritating until the vesication is complete. In certain rheumatic pains, which are to be followed up wherever felt, we have seen the same blister shifted to all the painful parts, and constantly producing the same effect. This persisting activity of Albespeyres's blisters is explained by the homogeneity of the whole adhesive emplastic mass, and to the great quantity of the vesicating principle contained in it.

Albespeyres's Blisters are not liable to spoil.

Country practitioners may carry them in their case of instruments; inclosed in metallic envelopes, these blisters retain all their virtues during the longest voyages by sea, and will keep any length of time in all climates. This invaluable quality has been often proved, and was fully

established during the late war in the East; for they were then constantly employed in all the hospitals of the allied armies; and when peace was concluded, the stock remaining on hand was transferred to the military hospitals of the Ottoman Empire. Lastly, and this will suffice to characterize their superiority, Albespeyres's Blisters have their place in the hospitals of our armies¹.

We will conclude this chapter with quoting the opinions of the Medical Press, the vigilant sentinel of the

¹ "Ministry of War—Department of Hospitals and Invalids.

"28th May 1859.

"To M. Fumouze-Albespeyres, Paris.

"By your letter of the 4th instant, which I have had the honour of receiving, you were pleased to place at my disposal, to be employed in the hospitals of the army of Italy, 500 metres (537 yards) of the Epispassic plaster, of which you are the inventor.

"The Board of Health, to which I submitted your offer, having expressed a favourable opinion on the use of your preparation in our active armies, I accept your offer, and thank you for the generous sentiments which induced you to make it. You will please to inform the Pharmacia-in-chief at the Central Military Pharmacy, No. 160, Rue de l'Université, when it will suit your convenience for him to send and fetch the article.

"Accept, sir, the assurance of my profound respect.

"For the minister and by his order.

"DARRICAT,

"Councillor of State, Director of the Administration."

"8th June 1859.

"To M. Fumouze-Albespeyres.

"I have the honour to acknowledge the reception of the 500 metres of Vesicating Tissue.

"ANDRÉ,

"Pharmacia-in-chief."

public health, and by the specimen of our label which has been adopted to prevent counterfeits.

"To M. ALBESPEYRES.

"Sir, it is for me at once a duty and a pleasure to inform you that your epispastics satisfy all requirements. The action of your blisters is more prompt, and at the same time more gentle, than that of the ordinary plasters; and for dressing blisters, your Paper is everything that can be desired.

"I may add, to show how highly I appreciate your valuable preparation, that I have purchased a roll of the plaster and a box of the paper, for use in my own family if necessary.

"GULLAUMS,

"Physician at Chambéry."

"We beg to call the attention of our professional brethren to *Albespeyres's Adhesive Blisters*. They adhere to the skin like sparadrap, and raise a blister in a few hours, without causing the least irritation. This is one of the few inventions of which the physician should take note. We may also mention here that Albespeyres's Paper is the best preparation for keeping blisters open, and promoting an abundant discharge without smell or pain."

(*Institut médical.*)

"We have published a paper by M. Fumouze-Albespeyres, containing interesting information on cantharides. That document naturally led us to examine the various epispastics employed to raise blisters and keep them open.

"The old-fashioned plasters sprinkled with powdered cantharides were attended with numerous and sometimes serious inconveniences. They have been very generally neglected since the invention of Albespeyres's blistering tissue, which produces vesication in a few hours.

"Among the salves, prepared silk, and other epispastics for dressing blisters, *Albespeyres's Paper* stands so preeminently superior, that we are really astonished to see any practitioners persist in the old routine."

(*Abeille médicale.*)




ON DRESSING AND KEEPING OPEN BLISTERS

ALBESPEYRES'S EPISPASTIC PAPER

Till the beginning of the present century, blisters were always dressed with epispastic ointment, spread on vegetable leaves. According to the quantity of the ointment applied, the blister was more or less excited, and the secretion more or less abundant; but as it is impossible for the most practised hand always to spread precisely the same quantity of the ointment on a given surface, the result was sometimes an over-excitement of the *Exutory*, at others its complete inertia. Besides, the vegetable leaves being liable to decomposition, blisters dressed with them often smelt offensively, and sometimes became intolerable. Such inconveniences rendered this *Exutory* a very disagreeable remedy, which could be applied only in extreme cases, and the healing art was thus deprived of an important resource against many affections.

In 1817, however, two men began to devote themselves with great perseverance to the discovery of sure, simple, constant, easy, and convenient means for the dressing of these artificial sores. M. Mauvage made the first epispastic silk at the very time when M. Albespeyres was preparing his first epispastic paper.

These two preparations were received as most useful inventions, were patronized and recommended by the most eminent members of the profession (Vauquelin, Richerand, Dupuytren, Lallemand, Marjolin, Récamier,

VÉSICATOIRES D'ALBESPEYRES.

FABRIQUE
A L'ILE SAINT-DENIS
 PRÈS PARIS.

VENTE ET EXPÉDITION
FAUB. SAINT-DENIS, 78 ET 80
 A PARIS.

J. Mauvage

Nota. La grille Albespeyres ayant été apposée sur le côté vert des vésicatoires, il ne sera plus possible de les confondre avec les imitations et contrefaçons plus ou moins défectueuses. Chaque signature forme un carré de 5 centimètres.

Ces Vésicatoires, disposés en forme de sparadrap sur un tulle ciré vert, sont d'une extrême commodité pour les médecins, les pharmaciens et les malades. Ils adhèrent à la peau et produisent la vésication en quelques heures (six heures en moyenne). Les vésicatoires, il ne sera plus possible de les confondre avec les imitations et contrefaçons plus ou moins défectueuses. Chaque signature forme un carré de 5 centimètres.

Rostan, Elisée, Pinel, Esquirol, etc.). One of these epispastics was even admitted into the pharmacy of King Louis XVIII., and allowed to be sold in envelopes bearing the royal arms.

But this *silk tissue* had serious drawbacks, which well nigh over-balanced all its good qualities: being impermeable, it allowed the suppuration to escape from beneath, and soil the patient's linen; being hard and dry, it irritated and inflamed the sore, sometimes even making it bleed, and producing pimples, fleshy excrescences, etc.

Albespeyres's Paper, on the contrary, being made from the best lint, soft and porous, coated and impregnated with fatty epispastic substances, always in the same proportion, gives up its active principle to the exulory gradually and without irritation: the suppuration is produced naturally and passes through to the compress, which completely absorbs it. M. Mauvage saw this, and ceded to us the entire property of his processes and invention; so that Albespeyres's Paper thus came to combine all the best qualities of both inventions, now sanctioned by more than forty years of successful use, under the eyes of physicians of all countries, who are the persons best qualified to judge of the efficacy of remedies.

Albespeyres's Paper is made of four degrees of strength, respectively marked No. 1 weak, No. 1, 2, and 3.

The No. 1 weak, having scarcely any ointment on it, is the weakest of all, and adapted for children and persons easily irritated;

No. 1, with rather more ointment, is for persons whose blisters discharge freely;

No. 2 is employed for those whose blisters are sluggish and discharge but little;

No. 3, the most active, is only used when the blister shows a tendency to heal.

PROPERTIES OF ALBESPEYRES'S PAPER.

This paper, preferable to all other epispastics, is the *only* article of the kind recommended for nearly fifty years past by the head-physicians and surgeons of hospitals in France and foreign countries, by members of learned societies, and professors in the schools of medicine and pharmacy. Its most remarkable properties are; 1. without any addition, it keeps up an abundant and uniform suppuration, unattended with pain, redness, or inflammation of the skin or denuded parts; 2. it prevents the formation of false membranes and white pellicles which often prevent suppuration; the denuded surfaces are constantly of a fine red tint, smooth, and without fleshy excrescences; 3. it produces no irritation in the urinary passages, and is therefore very suitable for nervous and irritable persons; 4. it exhales no unpleasant smell and presents the advantage of extreme cleanliness; 5. this paper, very fine, supple, transparent, and extremely soft, adheres to the edges of the blister, never shrinks or shifts from its place, and retains its softness while remaining on the suppurating surface. It is the only preparation with which a person can dress his own blister easily, *even while in bed, or travelling*, etc.

MANNER OF DRESSING A BLISTER

Cut a sheet of the paper to the size of the blister, and apply to the sore its most glossy side, which is always laid uppermost in the box. If the blister is larger than the sheet of paper, lay two of them side by side; then put a compress on them and over that a bandage to keep every thing in its place. For the first dressing after removing the blister, a little butter is usually applied, for

all the following ones, the paper alone. Care must be taken not to tie the bandage too tight, as that would be apt to impede the free circulation of the blood, and might cause inflammation, fleshy excrescences, etc.

It will sometimes happen that the blister becomes angry without any evident cause and even gets covered with a kind of false skin : this is most frequently caused by derangements of the natural functions, change of season, etc.; it is then necessary to apply softening poultices (linseed meal, crum of bread, etc.); and dress with a weaker number; if the weakest was the one already used, it must be continued with the other side of the paper downwards. When, on the other hand, there is atony of the part, a higher number must be employed; sometimes it is advisable to alternate with two numbers of different strength. All the moisture must be carefully taken off the blister with a soft linen rag, and the edges wiped, but washing must be avoided as far as possible.

THE MOISTURE MUST NEVER BE SUFFERED TO DRY IN THE COMPRESS : it is therefore necessary to have two dressings daily (morning and evening). If the discharge is copious, put on several compresses at once, and dress three times a day. Dressings too long delayed cause irritation, make the humours acrid, and may close the *Exutory*.

Blisters thus managed will remain quite fresh and continue their discharge imperceptibly.

MOST IMPORTANT OBSERVATION

Wishing that his Paper should leave nothing to be desired, M. Albespyres consents to change all boxes deteriorated by carriage or otherwise.—Be careful to avoid spurious and dangerous counterfeits. As his blisters ought always to produce their effect without pain, should the contrary happen, or doubts be entertained as to the genuineness of the article, the parties concerned

are requested to write to M. Albespyres, to learn whether the paper really comes from Paris, from the manufactory of the inventor, whose name is water-marked in each sheet.

Albespyres's Paper may be obtained of all Chemists and Vendors of Patent Medicines in France and foreign countries, in boxes accompanied by instructions, deposited in conformity with the law, and is never sold otherwise than by the box. By an imperial ukase, it has been admitted into all the Russias.

* We must say a few words on *dressing issues*, which hold the second rank as *Exutory*, and ought never to be made except by a professional man.

We have already stated that, in order to combine the manufacture of the most renowned epispastics, M. Albespyres purchased the invention of M. Mauvage. With the elements of that invention and those peculiar to his own, he has composed a *dulcifying paper*. This paper was all the more necessary, as for a long time past nearly all the paper for issues had been made by speculators who knew little or nothing of medicine, and used resinous substances for the purpose.

The choice of issue peas is not of much importance, as their function, altogether mechanical, is simply to keep the edges of the opening apart. All peas thoroughly dry are equally good, provided they are perfectly inert; but the elastic peas cannot be too scrupulously avoided, as their swelling may cause great suffering. To prevent the enlargement or diminution of the hole, care should be taken to select peas always of exactly the same size, just touching the edges of the opening without pressing against them.

To keep an issue in good order, it must be dressed twice a day; put the pea in the hole, cover it with half a sheet of *dulcifying paper* or an *ivy-leaf*, then add a compress of linen or spongy paper, and fasten the whole with a bandage of any kind, but not so tight as to impede the blood by indenting the muscles.

Notwithstanding the patronage of eminent physicians, Albespyres's Paper, like all inventions, trifling or important, met with great opposition from the partisans of the old routine, who could not endure to see their practices set aside. They protested against it as a quack medicine!... A secret remedy! And yet it is evident that Albespyres's Paper is not a medicine in any way: it falls within the category of sparadraps and dressings. The Blister is the real and powerful remedy, the Paper only an auxiliary. However, after science had spoken, the courts of justice issued their fiat, and Albespyres's Paper has long since taken legal rank, without soliciting it, in the therapeutic codex published by order of the government.

JUDGMENTS.

The following are the dates of the principal judgments which prove that Albespyres's Paper, far from being a secret nostrum or quack medicine, is a valuable addition to the pharmacopœia:

In certain cases, it is necessary to have recourse to special means. For instance, when the epidermis tends to reappear at the bottom of the issue, or when the suppuration is not sufficient, the pea must be covered with a little epispastic ointment before it is put in; the fatty substance of the ointment will mitigate the exciting principle, and the suppuration will increase without causing any pain, an advantage not to be obtained with the so-called suppurating peas.

If a blow or another cause should happen to render the issue painful, the flesh must be brought back to its natural softness by the application of an emollient poultice, before the compress. In this case again, the patient must be careful not to change his usual peas; for it is not the hole only which is inflamed, but all the parts around on which the poultice acts.

1. July 7th, 1838, judgment of the Royal Court of Orleans;
2. April 27th, 1841, judgment of the Royal Court of Montpellier;
3. June 30th, 1841, judgment of the Royal Court of Rennes;
4. January 22nd, 1842, judgment of the Court of Cassation;
5. March 27th, 1844, judgment of the Royal Court of Douai.

COUNTERFEITS.

These repeated judgments, preceded by the chemical analyses which the tribunals always require in cases affecting the public health, constitute a judicial title which has given Albespyres's Paper a special rank among the most esteemed medicinal preparations. But success is ever attended with certain drawbacks; it generally excites jealousy and heartburnings, which, in trade, often lead to dishonest acts. We have had to prosecute unprincipled persons, who, finding that their own names did not suffice to attract the notice of practitioners, impudently used the name of Albespyres, as a lure to cheat the public into purchasing their spurious and worthless preparations.

"There is no property more sacred," said Count Chaptal in the Chamber of Peers (Report on the law of 28th July 1824) than the name of a manufacturer, who, by persevering application, straightforward honesty, and useful discoveries, has obtained an honourable position among the benefactors of his country and the founders of his industry. Preparations bearing such names are everywhere received with confidence, and that confidence is such that in general they are received without examination."

These words alone would justify our claims to respect for the name of Albespeyres.

In its sitting of the 17th June 1840, the tribunal of Péronne condemned M. C^{***}, a counterfeiting chemist, to heavy damages, a fine, and the insertion of the judgment in several journals, at our choice and at his expense. (See *Gazette des Tribunaux*, 10th July, 1840.)

At Brussels, a still more audacious piracy was conducted on a large scale, and inundated Belgium and the adjacent countries with a spurious paper bearing the name of Albespeyres and sold at a very low price. Numerous complaints from parties who had been deceived by it soon enabled us to discover the maker, who was a person we should hardly have suspected, being no other than *our own principal agent!* The inquiry set on foot soon proved the co-operation of a pharmacist of Paris, who had already been prosecuted for a similar offence. It was ascertained that he only performed the first part of the manufacture, which was completed in Brussels.

“ M. Fumouze-Albespeyres,” says the judgment of 21st July 1854, “ manufactures by a peculiar process known only to himself, an episptic paper sold to the public under the name of Albespeyres’s Paper. This paper, which enjoys a great reputation, has the same appearance as the episptic papers made by several chemists according to the prescription in the Pharmacopœia, though their effect may not be the same, etc.

And further on :

“ M. B. L..., chemist, of Brussels, wishing to take advantage of the name of Albespeyres’s Paper, has counterfeited it.”

¹ We only put the initials, as we are willing to let by-gones be by-gones; but we might give the name in full, if we pleased.

Under these circumstances our course was clear : we appealed to Belgian justice and obtained complete satisfaction. We cannot do better, in order to give a complete account of a matter which so essentially interests the public health, than to quote the language of one of the most influential organs of the judicial press, the *Droit*, of the 13th June, 1855.

“ Royal Court of Brussels, 30th May 1855.

“ We have reported several decisions of foreign tribunals which have enforced the just appeals made to them by Frenchmen. We remarked that these examples would no doubt find imitators, and that there was reason to hope that the rights guaranteed to our countrymen, either by foreign laws or special treaties of commerce, would be faithfully observed by both parties, and that the shameful frauds practised to the loss of respectable tradesmen, and to the injury of consumers, would soon be brought to a close.

“ The Royal Court of Brussels has just resolutely entered on this course, by condemning to a year’s imprisonment a tradesman of Brussels who had counterfeited the products of one of our principal chemists.

“ The following are the facts of the case :

“ M. Fumouze-Albespeyres is the proprietor of *Albespeyres’s Paper*, which has an extensive sale in France and foreign countries.”

“ M. B. L..., chemist, of Brussels, thought proper to manufacture and sell an episptic paper, which he offered to the public as Albespeyres’s. The paper not having produced the usual effect, several purchasers denounced the fraud to the public prosecutor at Brussels.

“ Four hundred and ninety boxes of the counterfeit paper were seized, and after a strict investigation, the cause was tried by the Tribunal of First Instance; and then, on appeal, by the Royal Court, presided by M. Corbisier de Meulstær; M. Kaeman, drew up the report of the inquiry; M. Vieminkx was counsel for the plaintiff, and M. X..., for the defendant.

After hearing the Advocate-general Heyndrick, the Court gave judgment as follows :

“ Whereas it is proved, by the evidence and documents produced that the defendant, after having caused to be printed counterfeitings of the prospectuses, instructions, labels, marks and signatures, which envelope a medicament favourably known to the medical profession under the name of *Albespeyres's Epispastic Paper for dressing blisters*, did, for the sake of gain, sell to many persons an epispastic medicament which he had prepared or got prepared, and did fraudulently wrap it in the printed papers above-mentioned, in boxes likewise counterfeited, and offered and sold it under the name of *Albespeyres's Epispastic Paper*;

“ Whereas the paper employed by Albespeyres, and expressly made by him, is of a soft texture, adapted to the sensitiveness of the skin; and the paper sold by the defendant does not possess that advantage, but is, on the contrary, far inferior to the genuine paper, etc.

“ Whereas it is just, in fixing the punishment, not to lose sight of the fact that the counterfeit may prove injurious to the public health, and as the defendant has already been condemned, etc.”

And with regard to the plaintiff :

Whereas the conduct of the defendant as above shown, has necessarily thrown injurious discredit on the property of the plaintiff, on the one hand owing to the defendant's extensive business, and on the other to the plaintiff's numerous connection; and as it is natural to think that the public attributed to the latter all the defects and inconveniences caused by the defendant's counterfeit;

“ Whereas the injury is material and the plaintiff is entitled to claim compensation, in conformity with Art. 1382 of the Civil Code, and Art. 1. of the Code of Criminal Procedure :

“ Jean-Népomucène Adolphe B. L*** is declared guilty of the offence punished by Art. 423 of the Penal Code, etc.

“ And, in conformity with Arts. 423, and 42, of the Penal Code and Art. 194 of the Code of Criminal Procedure, the said Jean-Népomucène-Adolphe B. L*** is condemned to one year's imprisonment, and to pay a fine of 200 fr.;

“ He is also condemned to pay the plaintiff the sum of 4,000 fr. as damages, with all costs of suit.

“ Fumouze-Albespeyres is authorized to insert the present judgment, at the defendant's expense, in one Paris journal and one Brussels journal, to be selected by the plaintiff.”

M. B. L*** appealed to the Court of Cassation against this judgment, but when the cause was called in court he withdrew his appeal, so that the sentence became definitive and was carried into execution.

DOCUMENTS PRODUCED IN COURT.

1. Specimens of the paper made on purpose to receive the epispastic ointments, by means of M. Albespeyres's machines.

2. Certificate of the deposit of M. Albespeyres's products at the Prud'hommes' secretary's office.

3. Copies of all the judgments mentioned in the preceding pages.

4. GENERAL ADMINISTRATION OF PUBLIC ASSISTANCE.

Hospital of St. Antoine.

“ We the under-signed, physicians of the Hospital of St. Antoine, hereby certify that *Albespeyres's Blisters* have always given us great satisfaction both for the rapidity of their application and the certainty of the effect produced, and that it would be advantageous to employ them in the hospitals instead of the ordinary blisters.

“ BOURDON, ARAN, HÉRARD, RICHARD, *Physicians and Surgeon in chief.*”

“ *Albespeyres's Blisters*, prepared by M. Fumouze, are an excellent pharmaceutical preparation, of rapid and certain effect, which is not always the case with the ordinary blisters.

“ TORDOS, *pharmacist in chief;*

“ PAILLARD, *directeur.*”

5. THE MAYOR OF ST. DENIS, to M. Fumouze-Albespeyres.

"The administrative committee of the Civil and Military Hospital of St. Denis has been informed by Dr. Leroy des Barres and M. Meuridefroy, the dispensing chemist of that establishment, that for some time past you have gratuitously supplied our dispensary with your vesicating tissue for the use of the hospital.

"The committee received this communication with a feeling of just gratitude towards yourself, and have authorized me to inform you of the extreme satisfaction which this act of generosity on your part has caused them.

"Accept, etc.

"Got, mayor."

6. Professor Chevallier, when requested to give his opinion on the right of property in the name of Albespeyres, replied as follows:—

"In my opinion, no one can justly use the name of any man, living or dead, for the purpose of deceiving the public; no one, as I think, can abuse the name of a pharmacist, living or dead, by substituting gross preparations for efficacious products, and offering them for sale under the name of the genuine article; so doing is to rob the inventor and the public at the same time.

"I do not think that it can be morally right for any one to disgrace the names of Pelletier or Albespeyres by offering to the public, under the names of those inventors, imperfect preparations, which are not the same as those sold by those pharmacutists, who owe their fame to their science and probity.

"A. CHEVALLIER,

"Professor of the Superior School of Pharmacy, member of the Academy of Medicine, etc."

7. A certificate in the following terms:

"We the undersigned hereby certify that Albespeyres's Epispastic Paper, of which great quantities are sold, is exclusively

manufactured by M. Fumouze Albespeyres, sole proprietor of that preparation, as the pupil, son-in-law, and successor of the inventor, M. Albespeyres.

"The name of Albespeyres, which M. Fumouze alone has the right to use and apply to his epispastics, is considered by the trade a guarantee of superiority.

SIGNED: "Truelle freres, druggists, Paris; — Pégnière et Perret, do.; — Ph. Lefebvre, do.; — Ch. Garnier, do.; — L. Foure, do.; — Ménier et Cie, do.; — Provost freres, do.; — Doreoult, director of the Central Pharmacy; — Dr L. Ducom, professor at the School of Pharmacy; — Fardos, pharmacist-in-chief of the Hospital St. Antoine; Dr Foy, do., of the Hospital St. Louis; — Pelouze, member of the Institute; — Gélis, assistant professor of chemistry at the College of France; — J.-B. Caventou, prof. at the School of Pharmacy, member of the Imperial Academy of Medicine; — J. Personne, pharmacist-in-chief of the Hospital du Midi; — Aear, first pharmacist to the Emperor."

The judgment of the Brussels Tribunal was hailed as a boon by all the medical and pharmaceutical press, as the following extracts will suffice to prove.

Union médicale, of 20th June, 1855.—"The Royal Court of Brussels has just delivered an important judgment, a knowledge of which may interest a certain number of our readers. This decision, indeed, seems to us destined to protect, in an efficient manner, a species of property held by many pharmacians in France, and which, in accordance with the principles we have always developed in this journal, and contrary to opinions which appear to us far too rigid, we are not at all disposed to exclude from the benefit of the laws which secure other possessions. We think it quite fair and perfectly moral, that a chemist who invents any really useful preparation, and obtains for it the sanction of competent scientific authorities, should be permitted to find in the sale of the said product some compensation for the sacrifices he made in effecting the discovery.

We are happy to see the judgment of the Royal Court of Brussels put an end to the shameless piracies of which French inventors have been the victims, especially in pharmaceutical products.

“ LATOUR.”

The *France médicale et pharmaceutique*, after publishing the judgment, adds :

“ The judgment we have published above will certainly open the eyes of all counterfeiters of pharmaceutical preparations. The courts of law, indeed, can hardly be too severe upon them, since the only means of preventing these commercial frauds is to prosecute all dealers who deceive their customers as to the nature or origin of their merchandize.

“ Dr. ROUBAUD.”

The *Gazette des Hôpitaux*, the *Abeille médicale*, the *Journal de Chimie, Pharmacologie et Toxicologie*, etc., all expressed themselves to the same effect, and in equally positive language.

USURPATION OF THE NAME OF ALBESPEYRES.

Another description of offence, well calculated to impose upon the public, soon made its appearance, but was at once punished by the tribunals.

MM. H. and C., pharmacians of Paris, had founded an establishment where they pretended to make improved pharmaceutical products. So far as we were concerned, the improvement consisted in selling ordinary epispastic paper with labels and handbills, in some cases bearing the words according to the process of *Albespeyres*, in others *S. L. F. d'Albespeyres* only.

We immediately brought the affair before the Tribunal of Commerce, in Paris, and the pretended improvers

were enjoined to cease their manœuvres to pay us damages and all costs of suit, including the insertion of the judgment in five journals to be chosen by ourselves.

Messrs. H. and C. appealed, but the judgment was confirmed by the Imperial Court of Paris, on the 12th January 1857.

We thus see that dishonest competition assumes all forms, hides itself under any mask, and thinks no trickery to be despised that will draw money out of the pockets of the public. But if counterfeiting and such proceedings are contemptible in all kinds of trade, they become dangerous in pharmaceutic products. Good medicine cures; bad only aggravates the disease.

All countries are equally interested in the adoption of severe measures against pharmaceutical pirates, as the talented editor of the *Union médicale* very properly designates them. Is not public health the sovereign good of nations?

CASES

PROVING THE VALUE OF COUNTER-IRRITANTS
(EXUTORIES).

Epidemics. — Many persons have been preserved from epidemic diseases by the use of *Exutory*, and such means have been recommended by a great number of physicians. As no author, so far as we know, has treated the subject with greater sagacity than *D^r Carrère*, we here insert the more important passages of the paper he read before the Royal Medical Society, of Paris, on the 28th of March, 1783.

“ Epidemic diseases often make extensive ravages : they depopulate towns and villages; they carry off great numbers of citizens whose lives are valuable to the community; they spread consternation wherever they prevail. The terrified inhabitants neglect their business to think only of the danger which threatens them. Some flee from the place of their birth and seek in distant regions a purer air exempt from contagion; others, frightened at the condition of their neighbours, dare not approach them; they are afraid of sharing their fate, and abandon them in their need; the sufferers cannot obtain the necessary attendance; and their situation becomes worse and worse. The ministers of health, more exposed to contagion by coming into close contact with their patients, run the

greatest risks; often, in their efforts to restore others to health, they perish themselves.

“ The means of preventing such complaints are therefore quite as important as those of curing them; I might even say that they are more important, and ought to be especially studied...

“ One of the means most commonly employed by practitioners, whenever they suspect the presence of a heterogeneous humour in the mass of the blood, consists in preparing a vent for it to escape. For this purpose they successfully apply blisters, setons, and issues. If this expedient suffices when the mass of the blood is already infected, there is reason to believe that it will be still more efficacious in preventing infection; it may at least hinder any impure miasms which enter the body from being assimilated with our fluids, impairing our constitution, communicating the virus they carry, or even depositing it in the viscera. In proportion as these deleterious miasms penetrate into the mass of our fluids, they will much more easily escape by these artificial emunctories : the infection of the fluids will become more difficult; consequently the body will be less exposed to receive contagion.

“ These reasons have been long familiar to many practitioners; they have sufficed to convince them of the necessity of providing a way of escape for the morbid matter which might gain admission to the body : medical men have therefore advised blisters, issues, and setons; they have recommended the most vigilant care in keeping up the runnings and other evacuations which existed in the body when an epidemic made its appearance; they have regarded these means as calculated to prevent contagion.”

Then he proceeds to say that this method is already ancient, and has been long practised in different nations :

“ It was used among the ancient Egyptians, according to *Prosper Alpin*; it was adopted by the Chinese, as stated by *Kempfer*; and we know from *Linnaeus* that it was familiar to the people of the North.

“ Reason and authority are supported by observation; a great number of practitioners have recorded the good effect of these means, and their testimony is founded on experience...”

Having thus introduced the subject, *Carrère* quotes the opinions of numerous authorities, some few of which we will here transcribe.

"Such are," says he, "*Alexander Massaria*, who advises, during the plague, the application of blisters, as a preservative against that fell disease; *Roderic Vega*, who, at the same time and with the same views, recommends the keeping open of all abscesses and ulcers already existing...; *Vicq-d'Azyr*, who considers the opening of an artificial drain as the most effective preventive ever employed against that disease; *Sennert* and *Zacutus Lusitanus*, who declare that during the prevalence of violent plagues, several persons were preserved from contagion by means of issues; *Diemerbroek* found the same means successful during the plague at Nimeguen, where he also saw several persons who escaped owing to inveterate ulcers on the thighs, which served the purpose of issues.

"*Galen* had already recorded the same fact; he noticed that all those who had ulcers on different parts of the body, escaped the pestilential infection.

"*Duchesne* and *Liddell* state that, to their knowledge, many physicians and surgeons who attended the plague-stricken found issues to be the best preservatives, and had braved all the worst chances of infection without any other precaution.

"*Le Paulmier* declares that he saw many persons who, when long exposed to pestilential contagion, escaped uninjured by means of artificial ulcers produced on their lower extremities.

"*Fabricius de Hilden*, who devoted himself to the treatment of the sufferers by the plague which raged at Lausanne in 1613, had two issues made on his own person, and to them he attributes his escape from the disease. He further declares that of all the persons who had issues, he knew only one or two die of the plague, and they were suffering from vitiated humours.

"*Forestus* saw many people preserved from contagion by the same means, and particularly the men employed in burying the victims of the plague. He also mentions instances of a monk, a surgeon, and a servant maid employed in a pest-house during the plague at Delft in 1557, who never took the infec-

tion, which he attributes to the running sores they had on different parts of the body.

"*Mercurialis*, in his account of the destructive plague at Venice and Padua in 1575 and 1576, asserts that he never knew any one who had an issue to be attacked, nor did any of his medical friends. *George Garnerus* made the same observation during the plague at Venice; *Massaria* likewise attests the same fact.

"The experience of *Van der Linden* fully supports that of *Mercurialis*, *Garnerus*, and *Massaria*; he relates that one of his uncles, who happened to be in Morocco, when the plague carried off 85,000 persons in five weeks, owed his preservation to an issue; that he recommended the same means to a Jewish physician, who attended the plague-stricken, and he was also preserved, as were all the other persons to whom they applied the same remedy. *Van der Linden* further states that he himself experienced its efficacy.

"We learn from *Bonnet*, that a doctor employed in attending persons sick of the plague preserved himself and many others by means of a seton in each groin; the same expedient was equally successful with *Prenestus*, a Hungarian physician, a fact recorded by *Walter Harris*. *John Baptiste Alprun* had recourse to similar means with the like result; having to attend patients during the plague which ravaged Germany in 1679, he made a slight incision in each groin with a lancet and inserted a pledget to keep the wound open; these wounds kept up a continuous and abundant discharge, to which he ascribes his escape from infection, although he ventured upon the dangerous experiment of analyzing the pus taken from a bubo on a patient who had the plague. He applied the same remedy to two of his friends, and both were saved.

"I will also mention here the case of that consul of Aix, who, when the plague was raging in Provence at the beginning of the eighteenth century, exposed his life continually and without any precaution to the contact of the pestilential virus, and nevertheless escaped; he had long been suffering from an ulcer in the nose, the suppuration of which most probably saved him.

"*M. Delpech*, surgeon-major of a regiment in the Polish

service, related to me a fact which confirms what I have already said. He was with his regiment at Kiminieck, in Podolia, where the plague was raging; he noticed that none of the soldiers who had runnings or suppurations took the disease, although it was most destructive.²⁷

Carrère concludes this long enumeration with full particulars of a case observed by himself.

“ On the return of the French troops from Portugal in 1763, a malignant fever of the worst type invaded the military hospital at Perpignan; no fewer than 3200 persons had it. The disease attacked all who approached the patients or breathed the same air; two young physicians who attended the hospital and all the officials, chaplains, surgeons, apothecaries, and wardsmen caught it, and the majority died. The ward for the wounded adjoined the principal fever ward. Nearly all the wounded were assailed by the same disease; none escaped, but those whose wounds were suppurating favourably. It was even noticed that if the suppuration happened to cease, they soon had the fever; it therefore became indispensable to remove them to another place when their wounds showed a tendency to heal and the suppuration began to diminish; from that moment they all escaped the contagion.

“ This observation is very important; the suppuration of the wounds appears to have been a real preservative for those patients in whom it existed: its suppression, on the contrary, was soon followed by an attack of the fever; consequently this case appears to demonstrate the efficacy of any discharge in preserving from contagious epidemic diseases.²⁸

These facts led Carrère to the following conclusions:

“ It results from all these observations that an evacuation provoked and maintained by the application of blisters, issues, setons, or by any other means, is an excellent preservative against epidemic diseases; but this same means is also well adapted to prevent the progress of the said diseases, when it has been employed from their first appearance.²⁹

Carrère, in conclusion, supports this last inference by the opinion of Galen, Simon Jacoz, Jacobus Lind, and relates remarkable instances of the preventive and curative action of blisters during the plague which ravaged France in 1628 and 1629.

Epizooties. — After showing the efficacy of blisters on the human species as a preservative and curative in epidemic diseases, Carrère asks whether the same means might not have equal success in contagious diseases among animals.

“ Might we not,” says he, “ apply to epizootic diseases what I have just remarked with regard to epidemics? Their effects are often most destructive; they frustrate the hopes of the farmer and deprive us of an essential part of our daily food. Their course seems to be the same as that of epidemics. Why should we not employ the same means to preserve animals? Issues, setons, rowels, blisters applied on certain parts of the animal, as the neck for instance, the ears, the shoulders, the corners of the eyes; boring holes in the horns, proposed by Fantasti, and other similar means, might protect them from contagion.

“ This means has long been known: Columella, Vegetius, Gesner recommended it; M. de Secondat asserted its efficacy; Lancini extolled it as being often the surest of all remedies; M. de Montigny and M. Vicq-d'Azyr advised it as the most efficacious, and we learn from M. Bertin that it was successfully applied by a planter at Guadeloupe to preserve cattle during a murrain which ravaged that island. By what fatality has it been nearly always neglected? Its benefits were generally recognized during the epizootic disease which raged all over France in 1774 and 1775. Des. Vicq-d'Azyr, Herment, Drouin, La Coste, Dufau, Prot, Larise, and Blechet advised issues, blisters, setons, acupuncture, scarification, and piercing of the horns; but these remedies were only prescribed and employed for cattle already attacked by the disease. It should be the very first means employed when an infectious cattle disease makes its appearance in any given locality; it might result in the saving

of an immense number of animals by preserving them from contagion.

“ My sole object in this paper has been to collect all the reasons and cases calculated to prove the efficacy of the means in question; I attach to it no farther importance than that of inducing practitioners to try it and make known the results. I should think my labours amply rewarded if I should thereby contribute to diminish the number of persons whom epidemics snatch from their families and the state.”

To the facts cited by Carrère, we will add the observation of Raymond, who, when the plague was at Marseilles, experienced sudden and very copious perspiration in the arm-holes, kept it up with great care, notwithstanding its fetid odour, and attributed to it his good fortune in escaping the contagion, although living in the midst of the plague-stricken, to whom he devoted his whole care.

Do not all these examples seem to indicate, how dangerous it would be during an epidemic to diminish or suppress suppurations or natural secretions? Would it not be wise, on the contrary, to create them by artificial sores, blisters, issues, setons, etc.? Blisters are now preferred on account of their more speedy action and the facility of applying them.

Cholera.—If we observe the course followed by cholera within the last half century, and follow it in its terrible visitations, from nation to nation, from town to town, from province to province, everywhere striking the people with terror, it will be impossible not to recognize, in its irregular, capricious, intermitting course, the characteristics of all the great epidemics, whose gloomy visitations are recorded in history.

Perhaps we might escape the cholera, as others have escaped the plague, by promptly having recourse to ex-tortories and keeping them in a state of active suppuration.

“ In malignant and pestilential fevers, it is necessary to apply as many as five blisters at once on different parts of the body.”

RIVIERE.

“ In adynamic, ataxic, and other fevers, blisters must be employed with the utmost promptitude at the very commencement of the attack. How many patients die through delay! It is true that the physician is often misled by the gastric symptoms which accompany the onset of adynamic fever, but then it is advisable to follow up the emetic with a blister more promptly than is usual. Far from doing this however, many practitioners fatigue their patients by purging. It is chiefly in the contagious typhus that I have had occasion to see the disastrous results of this practice. Hence the degenerescences, gangrenous eschars, etc., which supervene when blisters are applied too late.”

(ALIBERT'S *Thérapeutique*.)

“ King Charles V., surnamed the Wise, had been poisoned by an infamous conspiracy; the poison brought him to death's door, and from that time his health remained extremely delicate. What chiefly contributed to keep him alive was a suppuration which a skilful physician from Germany, sent by the Emperor, had produced in his arm; telling him that if it should ever happen to dry up, he would not have long to live.”

DE BARANTE.

“ Napoleon, when commanding the artillery at the siege of Toulon, caught a very malignant itch, in one of the batteries. After a short treatment, it disappeared; but the virus had only

gone inwards¹; it long affected his health, and well-nigh cost him his life. It was not till long afterwards, at the Tuileries, that Corvisart completely restored him to health by means of numerous blisters on the chest."

(Mémorial de Sainte-Hélène.)

"Blisters, as evacuants and stimulants, are the best remedies that can be employed in hydrocephalus; they are attended with no inconvenience, and I have, in a manner, almost covered a patient with them. I have kept them open as long as possible, and never found any reason to suppose that they had any bad effect."

ODIER.

"When the humour forming the callus in fractures is too copious, it is advisable to apply a blister on the opposite limb to attract a part of the humour to that side."

CELSUS.

"Blisters are employed with benefit for diseases of the chest, head-aches, ophthalmia, inflammation of the face or ears; epilepsy, catalepsy, inflammation of the brain; small-pox when the eruption is difficult; purple fevers, rheumatic pains, sciatica, the gout, pestilential and malignant fevers, etc."

PROSPER ALPIN.

"In pleurisies, it often appears, whether there is loss of blood or not, that a difficulty of breathing and expectorating intervenes, which places the patient in peril of losing his life. This danger is to be avoided by applying blisters on the legs and thighs."

BAGLIVI.

"The resorption of the serous collections which are formed under the influence of meningitis, pleurisy, peritonitis, gout, etc.;

¹ In the course of this work we have sometimes allowed ourselves to quote opinions which have since been exploded, as we did not like to mutilate the text.

the resolution of these phlegmasies and that of the lungs, is singularly accelerated by the action of blisters.

"They are perhaps still more beneficially applied against rheumatic pains unattended with inflammation or fever, against certain neuralgic pains, and especially against sciatica."

GERDY.

"At the beginning of an inflammation, when it is not positively declared, and is, as it were, in a state of doubt and hesitation, a blister applied on some distant part may prevent it, by operating a revulsion. If, on the contrary, it has attained its second stage and is fully declared, the impetuosity of the *ruptus humorum* may be diminished by applying irritants near the inflamed organ."

GUILLOT.

"I have been troubled with a most persisting and violent cough which I could not relieve till I applied blisters."

DR. WILLIS.

"Dr. Velveau dissipates diffused flegmons in the abdominal region by covering it almost entirely with a blister.

"In all the stages of pleurisy and local inflammations, Dr. Gendrin covers the chest with an immense blister, which soon dispels the inflammation."

"In a suffocating bronchial catarrh, a small blister is sometimes applied on the leg or the breast, and if the symptoms are not removed, the remedy is condemned as worthless, when the ignorance of the prescriber is alone in fault. How, indeed, can it be naturally expected, that a bronchial inflammation which occupies probably a surface of several square yards, should be dispelled by a blister of only a few square inches, any more than a serious inflammation of the chest would be, by taking away an ounce or two of blood?

"The only means is that which Velveau applied so successfully. He saw that the remedy must be proportioned to the

intensity and extent of the inflammation, and we have known him, by means of blisters which covered the whole surface of a limb, dissipate deep phlegmons which threatened to produce the most disastrous results. To the same cause also must be ascribed the success of M. Gendrin, who, at the first appearance and at the crises of a pleurisy and a pneumonia never hesitates to cover all one side of the chest with a continuous blister."

TROUSSEAU and PIDOUX.

"A woman, aged 24, of a sanguine and robust constitution, whom an ignorant doctor had most improperly treated, was brought to the Hospital St. Louis on the nineteenth day of a confluent small-pox. She was in a desperate state: her whole body presented the aspect of a crust irregularly cracked and sunk, exhaling an intolerable stench; her breathing was hurried and laborious, her voice gone, her eyes hollow, etc. I immediately applied two large blisters on the patient's thighs. The next day, to our great astonishment, she was not quite so ill. We redoubled our cares and attentions. I prescribed two more blisters on the legs, and from that time she kept gradually mending, and at last quite recovered, after a very long and painful convalescence."

ALIBERT.

"Drs. Bermond and Nonat have found no mode of treatment so effective as blisters in phlebitis, after bloodletting, and in spontaneous phlebitis of the limbs."

(Gazette de Méd. et de Chim.)

"A pork-butcher, aged 40, received a hurt over the left hypochondrium from a violent kick by a horse, which knocked him backwards and left him insensible on the ground. Acute and persisting pain inside immediately above the part struck; small and intermitting pulse; comatous state, on reviving from which the patient began to complain of the severe pain he felt. There was reason to fear the formation of a gathering or an extravasation in the abdomen. Several cupping-glasses were

applied and blood drawn, then two blisters on the legs and two more on the hypochondria. On the seventh day, the patient voided with his urine a quantity of blackish bilious matter: he recovered."

BROUSSONET.

"A clergyman had a heavy fall in the street, owing to his foot slipping, and was picked up by the passengers in a state of hemiplegia, or paralysis of one side. He recovered feeling and movement only by the use of flying blisters, applied alternately on the paralysed arm, side, and thigh."

ARIBERT.

"In intercostal neuralgia, Valleix obtained beneficial results only from flying blisters; but if the pain persists, salt of morphia must be applied to the part, beginning with one-fifth of a grain. The blister must be laid on the most painful point. Sometimes one blister will remove or relieve the suffering on the two other points: however, when the latter persist, as most frequently happens, each of them must have its blister. The treatment is the same in lumbo-abdominal neuralgia."

(Extracted from Gaissole's *Traité de Pathologie*.)

"If sciatica persists, it will be necessary to attack it at once by the mode of treatment which, as Cotugno has proved, is most certain against that kind of affection — I mean blisters."

"The blister must be laid on the most painful part: if the pain is felt next day in other parts, it must be followed up with the same remedy. For intense sciatica it may thus be necessary to apply simultaneously, or at short intervals, a blister on the top of the thigh close to the abdomen, others at the head of the fibula, on the inside of the calf, on the ankle, and on the top of the foot, all of which places were equally approved by Cotugno. The subsequent treatment must depend on the effects produced."

GAISOLLE.

“ In Raymond's opinion, the only means of curing retrocedent or displaced gout, besides the appropriate internal medication, consists in the application of drawing topics, on the joints previously attacked, beginning by the weakest and gradually advancing to blisters. If these means produce redness, humour, or pain in the part, it may be inferred that the gout is tending towards them, and will soon quit its internal seat and relieve the patient.”

GIRAUDY.

“ A soldier, aged 24, had been attacked with an eruptive affection which extended to all the thoracic and abdominal region, to the neck and the face. Bleeding and the use of an ointment caused the pimples to disappear; but soon after, his lungs became affected; he was troubled with a short dry cough, extreme weakness, obstinate diarrhoea, and became quite emaciated. At the Hospital St. Louis, this man was wonderfully relieved by blisters, which were successively applied on the different parts where the eruption had appeared.”

ALBERT.

“ *Dujuytren* spoke highly of blisters as a means of arresting and curing flying erysipelas; and some physicians have entirely surrounded the affected parts with blisters to stop the migrations of the disease.”

“ A tailor, aged 24, had long suffered with a scaly tetter which broke out sometimes on the face, at others on the hands, and at last just above the knee. He had tried all kinds of remedies in vain. From the knee, it flew to the breast, and from that time his breathing became difficult, the mucous membrane of the larynx affected; his voice was hoarse, and he expectorated mucous sputa either purulent or streaked with blood. Blisters applied on the different parts diminished the oppression in a most decided manner.”

ALBERT.

“ A boy, aged 40, was threatened with the loss of sight by the retrocession of a furfuraceous tetter, which first made its appearance on the left side of the head. He was promptly relieved by a small blister at the back of the neck.”

ALBERT.

“ After the application of blisters, I have always found diarrhoea arrested, to the great relief of the patient.”

BAGLIVI.

“ *Raymond*, to whom the medical profession is indebted for many valuable observations on cutaneous diseases, used to cure tetter, scald-head, and all diseases of the skin by the employment of *Evutorics*. In repelled tetter, the use of large blisters on the seat of the disease appeared to him the only remedy.

A. Paris has recorded a case of a young girl who had a suppurating tetter on the face, and was cured by the application of a blister upon it. This bold experiment of the great surgeon has been successfully repeated by many practitioners.”

“ The principle of military fever, when once it has found admission into the body, must be expelled by the exhaling vessels. Experience demonstrates that they are the best adapted to carry off morbid miasmata; nature always gives them the preference.

“ The means adopted for the purpose are diaphoretics, proper diet, and blisters.

“ Frictions, next to blisters, produce the most decided effect. It is not unusual to see all alarming symptoms at once disappear. When they are employed at the onset of the disease, the sweats do not appear, the eruption is less abundant, and often does not come on. They draw the morbid matter outward, and are especially advantageous in malignant military fevers; these exutorics should be then freely applied, and always kept open.”

BARRAILON.

"The salutary effects of blisters are nowhere more frequently observed than in the Hospital Saint Louis, which is the great receptacle for chronic diseases. They are there daily applied to displace inveterate inflammations, to counteract morbid tendencies of the tonic powers, etc."

ALBERT,
Head physician of the Hospital St. Louis.

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We might continue our extracts without end : such passages abound in all the best medical works, and a patient reader might soon accumulate enough of them to make a large volume; but these we have already given amply suffice for our purpose.

Our object has been to call attention to a curative agent which holds an honourable place in all schools of medicine, and in all nations; equally esteemed by Hippocrates and Galen; always placed in the first rank by the greatest medical authorities of ancient and modern times.

We have scrupulously respected the text of the authors quoted, in order that our readers, if we should be honoured with any, may perceive the differences in language and principles all leading to the same result : the efficacy of Exulorics.

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PARIS. — ACADEMY OF MEDICINE

Extract from the report of Messrs. BOULLAY, PLANCHE, CULLERIER and GUENEAU DE MUSSY, adopted in the public sitting, in favor of Raquin's Capsules of Pure Balsam of Copaiba.

“ GENTLEMEN,

“ Mr. Raquin has presented to the Academy a Memoir on a new mode of administering Copaiba, accompanied by specimens of capsules, which, though made several years ago, are quite tasteless, and from their oval form are extremely easy to swallow, so that persons who have the greatest repugnance to all medicines can take them without the slightest difficulty.

“ In order to acquire a perfect knowledge of the nature of the envelopes and of their contents, your committee requested Mr. Raquin to prepare, in their presence, a certain number of the said capsules. That gentleman acceded to this request, and the operation took place in the laboratory of the Academy.

“ Mr. Raquin thickens the Copaiba with a thirty-second part, and sometimes even a twenty-fourth part of magnesia, which renders the use of very pure Copaiba absolutely necessary, and offers an important therapeutic guarantee; then, by long and difficult manipulations which require no little tact and skill, and occupy several hours, he envelopes it with a layer of the purest gluten.

“ Mr. Raquin having attributed to his capsules peculiar effects which distinguish them from all other preparations, we have administered them to a hundred patients and have obtained a hundred cures of most inveterate cases.

“ We have shown (in Arts. 1 to 6 of the Report) that Raquin's Capsules are very easy take, especially if first soaked a few moments in sugared water; they occasionally purge when first taken, but not so much as to fatigue the patient; they cause no disagreeable sensation whatever in the stomach, and are never followed by eructations, like gelatine capsules and other preparations of Copaiba. It is probable that they only soften in the stomach, and that the Copaiba remains within the envelope till it reaches the intestine; their efficacy has been without exception.

“ Art. 7. The doses varied from 2 to 4 drams in 24 hours, half in the morning fasting, half one hour before dinner; in most instances 2 ounces were sufficient, but a trifle more was found necessary in a few inveterate cases. They may be taken longer if requisite, as the patients do not feel that insurmountable repugnance excited by all other preparations of Copaiba.

“ Raquin's Capsules are an important service rendered to the art of healing.

“ Their regular oval shape, the great quantity of Copaiba they contain in a small volume, the lightness and imperviousness of the

envelope, distinguish them from the gelatine capsules, which are of irregular form; heavier, larger, imperfectly filled, more difficult to swallow, and too frequently allow the Copiba to exude, so that the peculiar odour of that substance is perceptible on opening the box.

"Your committee proposes that your thanks shall be given to Mr. Raquin for the important results he has obtained, accompanied by a request that he will take the necessary steps to enable medical practitioners to profit by his invention."

Adopted the 27th June 1857.

"E. PARISET,
Perpetual Secretary."

Note by the Inventor. — This Report so peremptorily establishes the superiority of our Capsules over all other preparations of Copiba *scholæcor*, that we should feel ourselves failing in the respect due to that learned body, if we did not add a few words of our own, to which we request the reader's particular attention.

The difficulties of the manipulations mentioned by the Academy have frequently rendered us unable to meet the demand for the Capsules, but now that our efforts to improve the apparatus used in their manufacture have been crowned with success, we can immediately execute orders to any extent.

Manner of taking Raquin's Capsules.

However mild the *disease* may appear, it is always advisable to consult a doctor at first. When this cannot be done (at sea or travelling) the Capsules should be taken as described in the above quoted Art. 7 of the Report to the Academy; that is to say (after the inflammation is over), from 15 to 20 per day at two or three times, morning, noon, and night, one hour at least before or after a meal. Their virtues remaining the same, however long they may have been made, just soak them a few moments in a little sugared water or any other fluid that may be preferred, and they are swallowed as easily as soap.

Every bottle containing an ounce (eight drams), two bottles in 6 or 8 days are usually sufficient, though more will be required in some rebellious cases (see Art. 7 above). The patient should adopt a moderate diet, and every thing either stimulating or debilitating must be carefully avoided.

Important Notice. — Our bottles, wrapped in the Report of the Academy of Medicine, sealed with our seal, and completely enveloped in white paper bearing our label on a black ground, may be obtained of the principal chemists and patent medicine venders in France and abroad.

All bottles not sealed and enveloped as above described must be regarded as spurious preparations which may be attended with most dangerous consequences.

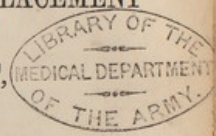
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With the writers Compl.

ON

**MALPOSITION OR DISPLACEMENT
OF THE HEART,
THE RESULT OF COLLATERAL DISEASE**



OR

VISCERAL DERANGEMENT.

From the Indian Annals of Good Luck

By

WILLIAM CURRAN, Esq., L.R.C.P., EDIN., M.R.C.S. LON.
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AND DISEASES OF THE CHEST, BROMPTON, LONDON.

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1862.

ON MALPOSITION OR DISPLACEMENT OF THE
HEART, THE RESULT OF COLLATERAL
DISEASE OR VISCERAL
DERANGEMENT,

BY WILLIAM CURRAN, ESQ., L.R.C.P., EDIN., M.R.C.S. LON.

ASSISTANT SURGEON, H. M.'S 88TH REGIMENT, (THE CONNAUGHT RANGERS,) FORMERLY RESIDENT SURGEON, ROYAL FREE HOSPITAL, GRAYS-INN ROAD, AND CLINICAL ASSISTANT, HOSPITAL FOR CONSUMPTION AND DISEASES OF THE CHEST, BROMPTON, LONDON.

"Medicine," says a distinguished Physician, "is a strange mixture of speculation and action. We have to cultivate a science and to exercise an art. The calls of science are upon our leisure and our choice, the calls of practice are of daily emergence and necessity; science may minister to practice much or little, but whether science help us or fail us, whether its instrumentality be sufficient or defective, still we must act. We are bound to the constant endeavour of doing the best we can, whether upon a perfect or an imperfect knowledge."

Acting upon this suggestion, and disclaiming, for the time, all pretence to scientific refinement in my investigation, I am anxious to communicate some particulars of the following cases of cardiac displacement which have come under my notice. Some of which, though of comparatively frequent occurrence, are introduced for the purpose of illustration; and while others, I would fain hope, have greater claims to public notice, they are, I think, collectively calculated to enhance the interest felt in the subject of heart pathology, and will therefore, perhaps, prove not altogether unacceptable to the general reader.

I submit them simply on their own merits, as presenting features of novelty and interest, in the aggregate, which will, at least, repay the trouble of perusal, and without reference to any ulterior deduction, which, indeed, in the absence of a more extended enquiry on the subject, and lacking the necessary material for illustration, it would be scarcely possible for me to compass. I have, however, less reason for regret

on this score, when I consider the teeming redundancy of kindred information so easily attainable in our public libraries, in face of which I may well content myself with dry detail, embodying only with, or appending to, each case, such comment as its peculiarities may seem to require—and leave inference and speculation to the future; and, in thus recording the results of experience, I am not merely adding to the literature of an already exhausted subject without an equivalent in the way of rarity, but am at the same time, I apprehend, supplying material and support to the great store-house of truth, whose investigation is confessedly difficult, but which is nevertheless greatly promoted by a faithful, "candid search by each of us for himself" according to his opportunities, by a ready communication of ideas among one another, and by a comparison of the conclusions to which we may have been individually led.

It will not be necessary for our purpose to enquire very minutely into the mechanism of cardiac displacement. Some of its causes are, indeed, sufficiently obvious and may be easily disposed of, but while merely glancing at the fact of its production by a solid, liquid, or seriform effusion into either sac of pleura by extensive tumours—the fungus *Hæmatodes* of Hope—in the lungs or mediastinum by aneurism of the arch, transverse or ascending aorta—or, as in a case recorded by Dr. Holland; of the *innominata*; by emphysema, as noticed by Laennec, hepatic enlargement, curvature, or exostosis of the spine or hypertrophy of the organ itself; there are others of rarer occurrence, but graver significance, to which it is here intended to call the reader's attention. Among these, besides those referred to in connection with the general subject as detailed below, we could especially claim precedence for such as interfere with or impair the functions of one *lung* being in preference to that of the other, for these, whether congenital or consequent on the progress of disease, generally bespeak such an amount of mechanical deformity or progressive decay as must seriously affect the nutrition of neighbouring structures, and ultimately be found incompatible with the continuance of life itself.

As causation in this case, Dr. Stokes enumerates chronic tubercle, gangrene of either lung, atrophy with dilatation of the bronchial tubes—the cirrhosis of Corrigan and others, and diminution of volume from the absorption of an empyema, and it will, I think, be easily seen that these are,

at least, sufficiently serious to warrant our selection without any reference to the question of mechanical *thrombosis* or *infarctus*. Every one is familiar with its occurrence in connection with acute pleuritis. Dr. Watson, I think, says that he has seen some of such cases; similar experience must, however, be elsewhere rare, though the researches of Drs. Hughes, Roe, Townshend, Williams, and MacDonnell have copiously illustrated its relationship to, and frequent supervention on, empyema, hydrothorax and other thoracic accumulations. Acute inflammatory turgescence of one side with sudden out-pouring of blood as from a punctured or gunshot wound have been known to produce it; and cases are on record wherein it was due to violence, to diaphragmatic hernia as related by Bouilland and Graves, to neighbouring visceral derangement as described by myself, and deformity of the chest as noticed by Latham, Bouvier, Brook, and others. With regard to this latter cause, I may say for myself, that—having very carefully at different times examined the chests of persons suffering from various forms of spinal curvature and deformity, including some of a very aggravated character indeed—I never could detect any positive cardiac displacement; and, though the sounds were generally audible to a much greater extent and over a larger area of surface than usual, I could not succeed in coupling these with any organic mischief as arising from pressure or transposition, and believe they were, in the majority of cases, the result of mere functional derangement, and easily to be accounted for on other grounds. This, indeed, does not accord with the statement of Dr. Latham, who, in treating of the causes that are regarded as capable of producing structural disorganization of the heart itself, says, "deformity of the chest, resulting from curvature of the spine, is partly reckoned among these," and further adds, "the whole chest being distorted and narrowed and the lungs straitened and imprisoned, and the heart itself displaced, and the aorta tortuous, and the liver bearing hard with its external pressure, lead upon the whole to as large an amount of hurtful encroachment of organ upon organ as can possibly be conceived. And this encroachment cannot be without mechanical impediment, and this impediment cannot be without hurt and hindrance to the functions, and then to the structure of such organs as the heart and lungs, yet in that may it continue the offices of life for years, provided care be taken to avert from it all casual influences of

an injurious sort." This is, however, more likely to obtain in cases of congenital ectopia, or when it follows early tubercular or traumatic pleurisy, for I have seen instances, and heard of others, in which it was audibly to the right of the mesial line, and yet the patient seemed quite unconscious of its cause or consequences.

On this point, Hope says, "slight displacements occasion little inconvenience. When considerable, they may create serious functional derangement, especially palpitation." Lancisi, translated by Forbes, after describing its mechanism and causes, confirms this, and further says, "these various kinds of displacements produce no perceptible inconvenience, where they exist in a slight degree; when more marked, they may produce bad effects, but in this case they are themselves consequences of lesions much more serious." The testimony of Stokes as to the absence of danger from malposition, is equally conclusive. After expressing his conviction that, even at its height, it "does not cause any alteration in the natural sounds of the organ," he adds, finally, "it may be stated, that we have found no instance of organic disease of the heart, which could be traced to the circumstance of its displacement." It is, however, but fair to observe, that the minute anatomy or morbid changes of its structure could not have been submitted to microscopic examination by either of the above-named writers, excepting probably the last, and I am not aware that the researches of Ormerod, Paget, or Quain, and their illustrations of fatty degeneration, were known at the date of publication of the editions of their works to which I have had access. It is not easy to infer from the complex and laboured phraseology of Walshe, what precise share he assigns to physical deformity in the production of heart displacement; he, however, enumerates *curvature* and *distortion* among its causes, but gives no facts or cases in point, and somewhat extended and careful enquiry conducted by myself at the Orthopedic Hospital, with the sanction and occasional kind assistance of Mr. Brodhurst, lead to the negative result recorded above; and I am not aware that the subject is even alluded to in connection with malformations or monstrosities in the monographs of Montgomery, Vrolik, or Simpson.

And touching the question mooted above, and the opinions expressed thereon, anent the comparative infrequency or absence of dangerous symptoms in connection with cardiac

malposition, I must say that neither my own experience as expressed below, nor that of the other authorities I have been able to consult, accord therewith. Doubtless, mere malposition is not, *per se* and irrespectively of other considerations, of sufficient import or urgency to excite alarm or beget misgivings as to the cause; but, when considered in connection with the collateral conditions which more usually accompany or give rise to it, and especially with such as either tend to impair its vitality, to obstruct the neighbouring circulation, or affect the constitution generally, there are then grave grounds for anxiety; then indeed are those influences at work which, it must be confessed, the resources of our art but too frequently fail to alleviate or restrain. Blakiston always found stridulous breathing as if the trachea had been interfered with, and ascribes its occurrence to twisting of the aorta—the result of displacement. Fuller states, that this form of stridulous wheezing is frequently symptomatic of thoracic aneurism, and we can easily conceive the amount of injury likely to be sustained by the heart in a case described by Dr. Holland, wherein aneurism of the innominata, besides inducing an appearance of displacement of that organ, compressed the trachea, and gave rise to dislocation of the clavicle. Hope acknowledges to having heard a murmur in a case of right lateral detrusion, whereupon Walshe comments as follows:—

"I once found, for many successive days during the height of left pleural effusion, both sounds of the heart, which was pushed to the right of the sternum, more or less masked by blowing murmurs; these murmurs when the heart was restored, or very nearly restored, to its natural position, almost completely disappeared—a fact the more remarkable as from the slowness of convalescence, plenty of time had elapsed for the formation of the systolic basis murmur of spasmia. The diastolic murmur disappeared the more perfectly of the two, and must, probably, have depended, probably through torsion of the aorta, on the malposition of the organ. Larrey has related a case in which extreme feebleness of the pulse coincided with cardiac displacement; there can be no certainty, however, that there was any mutual dependance between the two circumstances."

I fancy, indeed feel satisfied, I have recognised in some of the following cases, and in others to which my reading has extended, symptoms very similar to, if not

identical with, those described by Parry, Quain, and Scott, in connection with syncope,—angina^{is} fatty—degeneration, and empyema, and the condition as regards the aortic outlet and the narrowing and obstruction of that passage involved in the existence of twisting or dislocation, would seem to imply, on the part of its owner a liability to that form of syncope suffering or sudden death, and those other casualties ascribed by Cockle to insufficiency of the aortic valves.

Notwithstanding the opinion expressed by Dr. Stokes, to the effect that in "chronic tubercle the transverse displacement will be rarely observed, and when occurring, after the formation of cavities, may be regarded as a measure of their cicatrization," I think it will be found that a perusal of the following cases will tend rather to disprove than confirm that statement, and lead to the inference that the farther the organ is removed to the right, the greater is the amount of pulmonary disorganization that induced it. And, respecting that condition of displacement to which the heart is liable in consequence of adventitious circumstances the result of disease or deposit within or upon its structure, there is a symptom which, though not very particularly insisted on in the following notes, and somewhat differently interpreted by its own exponents, is, however, worthy of primary notice. Divested of some of the importance attached to it by the older writers, and simplified in regard of the somewhat superfluous and circumlocutory phraseology employed in its description, it may be at once alluded to under the head of epigastric pulsation. Though of rather anomalous and questionable import as regards the subject of our inquiry, and very summarily disposed of, if even noticed at all by modern writers, it furnished material for some keen controversy and speculation in days of yore; and in reproducing some scanty notes illustrative of the views of its supporters, I do so more in deference to the past, than with any serious view to its practical bearing or future revival. Baillie, however, did not consider it beneath his notice to inquire into its pathology and causation, and, after some discussion, arrived at the conclusion that it was due to gastric irritation or anemia. Ruder, on the contrary, assigned various causes, without specifying any one as more immediately concerned—and, among others, enumerates adhesion of the pericardium, enlargement of the substance of the heart itself,

regurgitation of blood into the inferior vena cava, induration of the pancreas, scirrhus of the pylorus, and tumours at the root of the mesentery. Lancisi referred it, in some instances, to encysted tumours of the pericardium chiefly affecting that portion of it which is attached to the diaphragm. Senac describes a case in which violent epigastric pulsation was due to enormous dilatation of the vena cava consequent on similar enlargement of the right auricle. Morgagni relates one or two cases in which the throbbing in the epigastrium was occasioned by some irregularity in the action of the abdominal vessels themselves, and in which a similar condition obtained in the carotid, temporal, and radial arteries—whose pulsations he described as loud and vibrating. He further adds that, in true prolapsus of the heart, the diaphragm is sometimes so greatly depressed that it seems to embrace it in a kind of hood, as was observed in the case of the Marquis de Palais by the physician of the French King. Cooke, the translator of the last-named writer, in a note appended to the above, mentions the case of an old man whose heart, after a sudden seizure of violent coughing, was suddenly and violently displaced downwards, and was felt pulsating in the epigastrium, while no trace of it could be found in its old situation. Finally, a remarkable example of vertical pressure, complicated with loud and troublesome epigastric pulsation, is given by Boerhaave, in which an enormous tumour within the thorax so compressed the diaphragm as to thrust the heart downwards and cause it to exist only beneath the umbilicus. Judging from analogy and contrasting the progress and inquisitiveness of the present with the stagnation and subserviency of the past, we will not, I think, be accused of undue prejudice or partiality to our own times and teaching, if we assume that in those days of imperfect science and diagnosis, more importance was attached to these anomalous manifestations than their site or significance deserved; and though they are not to be confounded with the palpitations and faintings described by Corvisart in connection with polypus of the heart, we will not, I apprehend, be far wrong in regarding them as akin to the "phantom tumours" the borborygmi and other strange developments of flatulent and hysterical women.

And considering the absence of danger and immunity from pain and other inconvenience enjoyed by some of the cases referred to above, as well as by others included in the series

below, we cannot, I think, too much admire the beneficial sway of our *alma mater* Nature, who seems to hold in special guard the fountain head of her ministrations to watch over, and provide for, the requirements of altered position and faulty mechanism, that she may still distribute thence as from a stronghold the materials of life and vitality to the remoter parts of the organism. And though, to use the words of Mr. Olmsted, it is hard to avoid being sentimental on a subject which so closely concerns us, I have no wish to exaggerate its importance, to dwell immeasurably on its peculiarities, or further enhance the unattractiveness of its details, by prolonging the discussion of them, I will therefore pass to particulars by way of contrast; and, as illustrative of the preceding remarks, will briefly introduce a few instances which, while they tend to exemplify her kindly forbearance, will at the same time serve to point a moral by the occasionally terrible suddenness of her seizures, and the penalty she exacts when the limits of her endurance are encroached upon, or unduly trifled with.

Dr. Thompson, Surgeon of Her Majesty's 80th Regiment, mentioned to me a case wherein death supervening on the exhaustion of protracted diarrhoea, afforded an example of the amount of mischief which may occur to the substance of the heart itself without thereby interfering with its functional activity, or the active progress of life. In this instance a hole, as large as a shilling, was found to communicate through the wall of the left ventricle with the cavity of the pericardium, which latter having contracted attachment or adhesion to its margins, stopped the escape of its contents, and formed a pouch of sufficient size and capacity to include at least three ounces of ordinary fluid. In a recent number of one of our professional periodicals, I find the following notice of the postmortem examination of a cyanotic child three months old. "The auricular septum was pierced by a circular hole with smooth round edges. There was another opening between the two ventricles, the aorta arising from both, with its orifice lying across the septum, and there was no trace whatever of any pulmonary artery." Mr. Rogers, of the Adjutant General's Office, in his little work on the "Curiosities of War," relates the following:

Samuel Evans, a private in the Grenadier Company of the 2nd Foot, was carried off amongst the wounded

at Corunna. He arrived in England, and died in the Military Hospital at Plymouth, on the 30th of January, 1809. On a postmortem examination being made, it was discovered that he had been shot through the heart, and yet had survived sixteen days. His heart is preserved in the museum of the above Hospital." I extract the following curious paragraph from the columns of the *Medical Times and Gazette* for August of the present year. "A bullet lodged in the heart for twenty years. Dr. Balsh, of North Lawrence, New York, details the following rare case. In 1840, a boy aged about fourteen was shot in the right shoulder, the bullet entering through the upper part of the trapezius, about two inches from the acromion. The ball, by those who examined him, was supposed to lie about four or five inches from where it entered, in a direction towards the cavity of the chest near the sternal end of the clavicle. In about six weeks he resumed his work, and gradually quite regained his health. In 1845 he was attacked with severe pneumonia on the right side, and then was first noticed a tumultuous action of the heart, which continued to increase after his recovery—its pulsation being seen and heard at a distance of ten or twelve feet. Another attack of pneumonia carried him off in 1860. At the autopsy the right internal jugular was found enlarged, and the external was entirely closed about a quarter of an inch from where it entered the internal jugular. In the right subclavian artery at the thyroid axis, was a large ossific deposit. The heart, two or three times its natural size, was very soft and flabby; and although on the outside no scar was discernible (the pericardium was however very adherent on the right side), on opening the right ventricle a leaden bullet was found embedded in its walls."

Dr. Richardson, of London, has, I think, somewhere published a paper in which he gives examples of the deposition of pins, needles, pieces of steel, and other foreign material in and upon the substance of the heart without their producing for the time any unusual symptoms. Another gentleman has, recently, given the particulars of a case of congenital *ectopia cordis* in a child which, I believe, survived its birth some days; its heart being meanwhile quite uncovered and pulsating feebly outside the chest; and numerous instances of a similar deformity are on record. Dr. Scott Alison assured me that, while in search of some pathological speci-

mens at Smithfield Market, he was informed by the butchers that they frequently found pieces of wood, iron, and the like embedded in the muscular coats of the heart; and my own experience of the knacker shops and dog-fanciers of Islington fully accords with this statement. A writer in the *Field* for August the 3rd, 1861, says, with reference to our Indian tiger:—"Sometimes a single lucky shot kills him, and at others he seems to bear a charmed life, receiving any quantity of lead without immediate effect. I have seen a tiger receive forty bullets before he fell, seven of which were from a 2 oz. smooth bore of my own, and he retained his wonderful agility to the last. * * * A tiger shot through the lungs lives a long time, and they have been known to execute determined charges after a bullet passing through the heart." Similar, though perhaps not equally cogent, testimony as regards our purpose, is borne by Messrs. Rice, Dunlop, and Shakespeare, in their interesting works on the tiger hunting and other wild sports of India and the hills. An equal, if not greater, amount of vital tenacity and endurance are ascribed to the lion and the wild buffalo of the American prairies, and I had the curiosity to enquire into the circumstances attending the last struggles of some of the horses that were gored in a Spanish bull-fight, and satisfied myself by personal observation, that one at least whose aorta had been penetrated—the laceration extending into and involving a portion of the heart's structure—was able not only to perform several circuits of the arena, and uphold his rider in another encounter with the bull, but actually to walk firmly from the ring, and survive several hours in a neighbouring enclosure. Abscesses have been found burrowing in its septum, and hydatids have been known to wriggle and disport themselves within the sacred precincts of its walls; fatty degeneration has supplanted its muscular fibre; ossific deposits have compressed its outlets, and encysted tumours have encroached upon its narrow enclosure; still the weary wheel of life jogs on apace, and, mayhap, will continue on its way if no unforeseen event interpose to arrest its progress. Truly, may we apply to it the words of an old writer, *Omnia corporis aut incommoda aut pericula per me transeunt nullum mihi videtur molestius*. Yet withal, will it not bear rude handling, and some who miscalculate their strength and live

"As if the flesh which walls about their life,
Were brass impregnable,"

find occasionally, to their cost, how difficult it is to trifle with, and how dangerous to ignore, the still small voice of warning from within, which so often bespeaks conscious insecurity and progressive decline. Indeed, so many instances in point have occurred of late, as to remove all doubt on this head, and obviate the necessity for any lengthened comment. Several well-known members of our own profession have, within a comparatively short space of time, afforded evidence to the purpose. A military gentleman ascending a hill for the purpose of shooting, feeling indisposed, sat down to rest himself, lay back and died; a gentleman well-known on the conservative benches to the left of Mr. Speaker, as well as in certain military circles beyond, was, I think, found dead in a cab; another, while taking leave of a friend, fell into his arms and died without a struggle; and even the pugilistic powers and burly frame of a Ben Caunt had to succumb before the insidious ravages of central decay. And, as if to mock at all precautions and show the facility with which one may end the heart-ache, anticipate the law's delay and "shuffle off the mortal coil," itself an ingenious but not very novel mode of accomplishing all at a blow, has been recently practised by a Mademoiselle Le Provost, who died suddenly at Caen a few weeks ago, the very day before she was to take her trial for embezzlement when postmistress at Cheruelles, and who committed suicide by thrusting pins and needles into her chest. The heart had been pierced in many places, and two needles were also found crossing each other in her throat; she had used the cover of a prayer book to force them into her body. A somewhat similar, but perhaps more culpable instance of reckless self-destructiveness on the part of a high personage is given by Mr. Guthrie in his Commentaries on the Surgery of the Peninsular War. I believe the annals of Revolutionary France could supply many more, and we may not unreasonably fancy, that the great dramatist and painter of human passion had some intuitive knowledge of the process, when he alludes to the facility of making one's quietus with a bare bodkin. However that may be, it certainly bespeaks no small amount of stoical indifference or endurance on the part of its perpetrator, and will in that respect, I think, bear favourable comparison with the cowardly desperation and studied barbarity of more fashionable performances. It has the further advantage of simplicity as regards execution, and an acquaintance with its

efficiency could not fail of proving highly acceptable to any Chinese Mandarin, who may for any offence be allowed, by way of special indulgence, and as a mark of his Emperor's impartiality and justice, to select the most convenient plan of taking himself out of the way. In this respect, and considered without reference to its ulterior bearings, it may contrast not unfavourably with the poison cup of the ancients with all its anticipatory horrors and dismal accompaniments, with the haphazard strychnos bean or charm test of Madagascar or Central Africa as described by Burton and Madame Pfeiffer, with even the witch torturing of Medieval Europe, and it is, I think, far more dignified and philosophic to end one's trouble at once than to invest the process with the solemn formality and ceremonious leave-taking, which are ascribed by Mr. Oliphant to the "happy despatch" of more civilized and modern Japan.

In conclusion, should these prefatory remarks be supposed to exceed the limits originally contemplated, or seem to contain more quotations and references than are required for the purpose of legitimate illustration, I can only plead the words of Dr. Whewell, who says in the preface to his *History of the Inductive Sciences*, in justification, "I presume, of his numerous extracts from other authors—"I have done this without scruple, since the novelty of my work was intended to consist, not in its superiority as a collection of facts, but in the point of view in which the facts were placed," and leave the issue to the candour of my reader, if any such there may be.

Should the subject, otherwise, present no features of attraction, be found to have suffered by my rendering, or appear to exhibit an undue amount of irrelevancy and dullness, I must only recall attention to my opening statement, observe that a topic of such bearings may not be satisfactorily discussed without some foreign admixture, and shelter myself behind the words of a well-known theological writer, as more appropriate and pertinent than any I can myself supply. "When we travel over ~~the~~ tracts of sterile and uninteresting country, the scenes around us not only depress our spirits and throw over us a spell of unsocial silence, but they impart something of their own barrenness even to our understandings." So it is here: the question itself—however extensive its relations—is not susceptible of much original discussion, and is more likely to be improved and understood by an accumulation of

facts than any adaptation of theory, by the deductions of others than any lucubrations of self. The *genius loci* is not calculated to inspire much interest or enthusiasm for composition, speculation is not my forte, and I have no wish to run counter to the voice of experience or the dictates of common sense. Finally, though these cases or some of them have been drawn up with somewhat more elaboration and detail than I should be now disposed to employ, they are not the less true and faithful records of some striking facts. In this sense alone are they offered for publication, further pretensions they decidedly have none."

Case I.—G. B., Æt. 19.—single, omnibus conductor, native of Walworth, engaged for last few years in the above capacity. Of regular habits, stout, thick-set, rather bloated in the face, of medium stature, and asthmatic tendency. Looks apparently in the enjoyment of average health, the finger nails are markedly clubbed, but there is complete absence of the characteristic reddish marginal streak at the edge of the gums, and his exterior is, otherwise, unfavourable to the assumption that he is the subject of a disease, the extent and gravity of which may be inferred from the following outline.

History.—Was, at one time, engaged as a cab-driver, and subsequently, at intervals, but continuously so for the last eighteen months, in the omnibus line, in which capacity was much exposed to changes of weather, night air, and other depressing influences; could not obtain much sleep, was irregular in his eating hours, but lived well and temperately, and states that, though his mother died of bronchitis, the other members of his family are long-lived, active, and free from any tendency to phthisis.

Previous Symptoms.—Has been troubled with a cough of variable severity for the last six years, and was laid up about sixteen months ago with pain in the right side, dyspnoea, headache and fever; has had some thick yellow expectoration for some period, night perspiration for the last month, and occasionally with distressing palpitation as to cause him to discontinue his work and seek for advice at this hospital, where he was admitted, complaining of a constant beating or pulsation at the epigastrium, great restlessness, vertigo and distressing shortness of breath. Cough is very troublesome at night; expectoration unmmulated, scanty, and tenacious, profuse night sweats, hectic, insomnia and constipation.

Pulse 96; respiration 28. Was recently under treatment at another hospital, and was repeatedly cupped and blistered on the *right* side, which on "inspection" is flattened, depressed, and contracted, measuring above mamma 16 inches, while the left is 16½, and at xyphoid cartilage this is reduced to 15½, and the corresponding left increased to 16½.

Percussion.—Dulness well marked all over; either aspect of right chest hard and tympanic; at apex where only some trace of healthy breathing, rough and tubular in character—is still heard,—elsewhere it is noisy, harsh, and clicking, intermixed with occasional dry rubbing crackle in mammary region, very weak towards base and obscured throughout by heart's impulse, whose apex beat is seen, distinctly tilting forward chest under nipple of same side. On posterior surface, the breathing is almost absent, in lower two-thirds, coarse, cavernous gurgling, grating, friction sound and whispering pectoriloquy thence upwards as far as spine of scapula, indicate the presence of a vomica, of such magnitude as to involve the greater portion of lung, and entirely destroy its aerating capacity.

As previously stated, the heart occupied a position entirely to the right of the sternum, and was most probably pushed over by the hypertrophied and emphysematous left lung which, in this case, discharged the function of both. This man had but *very slight* hæmoptysis before and after his admission, and was so much improved by the tonic and soothing treatment employed as to gain weight, and be able to leave the hospital with little or no dyspnoea, cough, or expectoration, and otherwise constitutionally better. This, however, was of short duration, for though still treated as an out-patient, under the care of Dr. Alison, and promising for some time to hold out, and perhaps survive the winter, yet, from some accidental cause, he caught a cold, and began rapidly to exhibit signs of decline.

After an attack of rigors, he suffered from sore-throat, and this was quickly followed by livid and puffy tumefaction of the face, œdema of lower limbs, abdominal tympanitis and other signs of general anasarca, the breathing simultaneously became more oppressed and troublesome; the urine more albuminous and scanty, and the heart's action, which, by the way, was always free from any valvular or other murmur, more irregular, feeble, and intermittent, and

death soon closed the scene. By the kind permission of Dr. Alison, I performed the *post mortem*, and noted the following particulars, which, having especial reference to the condition of the heart and lungs, will include little more:

On removing the sternum and its cartilages, the heart was seen quite displaced to right of spine, large, flabby, and pendulous, covered with opaque and greasy fat, presenting some patches of atheroma on free surface of aorta, having its orifices very patulous and open, its cavities empty and flaccid, and bound down posteriorly by old and tough adhesions. The right lung was almost completely obliterated, or rather reduced to a hard gristly cartilaginous mass situated at the apex of the same chest, and so closely adherent to the spine as to require considerable force to liberate it. Was quite impervious to air, sunk readily in water, and was regarded by Dr. Alison as a good example of that rare form of disease *yelept cirrhosis* by Corrigan and others. The left was spongy and crepitant throughout, with a few crude tubercles scattered sparingly here and there, its vesicular structure dilated, and its tissue generally in a state of hypertrophous emphysema. The liver occupied the greater part of abdomen, weighing nearly 10lbs, cutting as hard as preserved bacon, and feeling greasy, hard, and flesh-like to the touch. The kidneys and most of the other viscera were similarly disordered, and presented together an accumulation of disease rarely met with.

Case II.—L. M., Æt. 19.—A farm servant, single, residing in the country, engaged a good deal in the open air, but not over-worked or restricted as to exercise generally, well fed and otherwise cared for. Is disposed to phthisis on the father's side, but did not manifest any leaning thereto for many years. Was always an active, healthy girl up to the age of sixteen, when she began to menstruate with difficulty. Was subsequently much troubled with headache and sleepiness, hard hacking dry cough, and complained of languor, wasting, and failing strength. Began to improve apace on the approach of winter, and continued to progress so favourably during the colder months, as to be able to attend to her ordinary duties and encourage hopes on the part of her friends that her convalescence would be permanent. This, however, was destined to be otherwise, as the return of summer brought back the former symptoms in an aggravated

form to be modified again by the access of winter, and point a moral by illustrating the value of regulated temperature and its application to the alleviation or arrest of incipient phthisis. Sometime in August or July, of the following season, while taking some tea, she was suddenly seized with hæmoptysis, of a light red color, to the amount of a cup full, this recurred at intervals subsequently, and gave rise to much anxiety and alarm.

The catamenia, simultaneously, became more irregular, scanty, and difficult. Had the whites very bad between each period, and complained of recurrent stitchy pains under left mamma, sweats, weakness, and morning cough. In this state, she was admitted under Dr. Cursham, and, on examination, presented some deficiency of movement, clavicular dulness on percussion, increased vocal resonance and dry crackle on deep inspiration at left apex. The treatment consisted of tonics, such as iron, the syrup of which with cod oil, acids, occasional counter-irritations and anodynes to relieve cough, gave much relief and enabled her to leave the hospital improved in general health, but without any material change as regards the physical signs. Having, soon after, had another attack of hæmoptysis and subsequently severe persistent pain in left side, with bulging and enlargement of its lower-third, hectic sweats, dyspnoea, and other indications of pleural effusion, she was sent to Burnemouth, and, when admitted there, found to exhibit the following symptoms:

"Left side bulges outwards and downwards perceptibly, but not very extensively, lateral motion impaired and vocal fremitus perceptible through the hand. Heart beats completely on the right side, the apex being exactly on the corresponding site on the right side to what it occupies on the left normally, namely, between fifth and sixth ribs, its impulse diffused and undulating—as if from pericardiac fluid—its action weak, tremulous, and muffled, with softly blowing systolic murmur, and distantly intermittent rhythmical tendency. The murmur with the first sound is probably due to a twist from dislocation and the cardiac displacement itself consequent on pleuritic effusion following chronic tubercular pneumonia. On auscultation, there is a kind of indescribable mixture of voice and breathing sounds at left base, being loud superficial and bronchophonic in one place, shrill whispering and ægophonic in another, the respiratory murmur is, otherwise, regular and perillie elsewhere, and in this her general condi-

tion is better. Sleeps well and relishes food, and, in the absence of any other unfavourable symptom or evidence of progressive disease, it was expected she would not only escape further attack, but, ultimately, regain a fair amount of health, and, perhaps, realize the best anticipations of her friends.

Case III.—C. T., *Æt.* 27.—Single, of slender, weakly habit. Says her face is the "best part of her," and presents in a marked form the other outward characteristics of chronic phthisis. States that she has always lived well, being chiefly engaged as a house-maid in a respectable family, and although much harassed with up and down-stairs work, exposed a great deal to "heat and cold," and often obliged to keep late hours, was in good health and able to attend to her duties up to about July 1851, at which time she began to suffer from pain and uneasiness about the chest, looseness of the bowels, headache, cough, giddiness, and expectoration, and was generally so unwell, as to be obliged to give up her employment and seek relief in the country. Felt weakly and ailing ever since, complained of weakness and weight in the back. Was, at times, much troubled with sweats and recurrent attacks of diarrhoea, stinging sensations about the præcordia, frontal aching, tightness and phlegm. These symptoms increasing in severity about two years ago, again required abstinence from work, and were soon followed by severe pungent pain in right side, urgent dyspnoea, hard paroxysmal cough, viscid blood-stained sputum, and such other appearances as would seem to indicate an attack of pleurisy on the right side.

Was relieved by some expectorant medicines, counter-irritatives and leeches, but, continuing to lose ground slowly and grow weaker, was induced to come to this hospital, when she was admitted under the care of Dr. Cursham, and left three months after, her general condition being much improved, but without any arrest of her disease, or other material mitigation of her sufferings. Was treated as an out-patient for some time, and when re-admitted, appeared more hypocondriacal and nervous than before. The chest was flattened and sinking towards clavicles, giving out the crack-pot sound on percussion, and loud diffuse cavernous gurgling in either supra mammary region.

There was no particular costal bulging or prominence, and lateral measurement seemed pretty equal on both sides.

She was very liable to attacks of palpitation. Suffered much from dyspnoea and globus, but gave no evidence of any valvular or other organic disease of the heart, whose apex beat was heard between second and third ribs, somewhat above and to the outer side of mamma, and entirely right of sternum. In this region, dulness was absolute; in the normal cardiac space undue resonance obtained. Its sounds were quite audible over either aspect of chest, and together with whispering pectoriloquy, diffuse excavation-bubbling, and intercurrent pleural grating, clearly indicated an almost entire absence of healthy breathing, and made it a matter of surprise how life could be prolonged under such a complication. Notwithstanding the evidence of a pleuritic or quasi pleuritic seizure afforded by some of the symptoms alluded to above, I have, from various sources, satisfied myself that they were, in this and other similar cases, due to the irritation of softening tubercles, and were by no means necessarily associated with, or followed by the ordinary sequelae of that complication. This view is more confirmed by the characteristic conditions of the attack itself, the absence in many instances of all fluid or pleural retraction, and the state of the lung and affected side as seen after death.

This poor creature having struggled gallantly for some time, and regained somewhat of her former resignation and self-control, died rather suddenly soon after my leaving the hospital, and I was assured by those competent to judge and who were present at her autopsy, that the appearances ^{observed} thereby, were perfectly compatible with the diagnosis arrived at during life, and tended, by their extent and gravity, rather to increase and confirm, than diminish or modify, the feelings of sympathy and surprise excited by her condition during life.

The two following cases of cardiac displacement, consequent on extensive tubercular excavation, are peculiarly interesting in that respect, and also in as far as they evidence that inherent tenacity of life, that *vis vitæ* which is implanted in the human constitution, and in the strength of which it is enabled to contend with the ravages of disease, and often even survive the outward manifestations of its own decay.

The details which scarcely exceed the mere enunciation of a few facts, are entirely from memory, but not the less reliable on that score, or in so far as they involve this inquiry.

Case IV.—I. B., *Æt.* 40.—A tall, thick-set, muscular man, of exsanguine pasty aspect, and cheerful easy temperament; lived well, being chiefly engaged as a rat-catcher, in which pursuit he frequently spent hours in watching on the grass under trees, and in such other places and occasions as exposed him to the varying influences of the weather, and frequently gave him a slight "touch" of a cold. Was not particularly abstemious in his habits, and often forgot himself and his calling to gratify the ruling passion and indulge in "such stuff, such unsubstantial nothingness as dreams are made of," whereby he suffered severely from winter coughs, and was much troubled with sore-throat, expectoration, and other catarrhal symptoms; had also some slight hæmoptysis, sweats, and occasional fits of faintness, but was never obliged to discontinue his work or lay up until within a short time of my seeing him, when he presented the following appearances:

General aspect as above: outline of chest tolerably uniform and symmetrical. Slightly contracted and receding towards the left, somewhat unduly sunken under clavicle. Breathing loud, blowing and amphoric all over and on either aspect of right chest with occasional grating crackle near apex sounding and superficial elevation of voice, and hollow cavernous cough—on left 'tis harsh and bronchial, indicating some consolidation in the upper portion, in which signs of softening subsequently appear, but not otherwise very seriously affected.

The heart occupies a position entirely to right of sternum being much more audible behind than in front, and quite free from any murmur and independent as regards force and regularity of rhythm on any postural change. Notwithstanding such an amount of structural disorganization, this man was able to be up and almost to enjoy his food and exercise like other people; and in other respects, save, perhaps, the above-mentioned appearances, to enjoy as much vigour as is usually allotted to persons of his age and habits.

Case V.—A. B., *Æt.* 8.—A weakly, failing, diminutive child, of waxy strumous aspect, and looking as if "unfinished and sent before her time." Is described as being thin, spare, and delicate from her birth, very hard to rear. Not playful like other children, but always ailing, irritable, and languid. Had most of

the ordinary diseases of infancy, and was generally subject, in winter, to cough, headache, and burning heats; but never had any acute seizure, or evinced any particular deficiency as regards the chest, until within a very recent period, since which has had an increase of the cough, more frontal pain, and irritability of temper, with evening exacerbation, wasting, sweats, restlessness, and sensorial disturbance. When seen by me, the child presented all the outward characteristics of advanced and progressive phthisis, including the emaciated and narrow chest, prominent sternum, sunken intercostals, and flat depressed sub-clavicular spaces; to these were superadded the leaden dulness and coarse cavernous gurgling of extensive vomica on the right side, and harsh tubercular breathing of indurated but marginally emphysematous left lung, whereby the diminished and flabby heart was pushed to opposite side, where it beat feebly—but without intermission or murmur, being somewhat more audible in front than behind, and not influenced in rhythm or frequency by slight exertion or sudden movement.

This child, after pining for some time, daily decreasing in strength and vitality, and as it were looking death in the face, died suddenly of hæmoptysis, and was removed before any examination could be made.

TUBERCULOUS EMPYEMA.

Case VI.—Rob. T., *Æt.* 32.—Farm-servant, of medium stature; stout, pale, cheerful temperament, and free from any hereditary tendency. The history of his case, which in every essential very nearly corresponds to the preceding, amounts to this, that he had a cold and suffered from cough, expectoration, tightness, dyspnoea, and subsequent pleuritic pain in left side which, on admission, only rises not expands on inspiration, is very dull on percussion, giving evidence of a splashing sound on succussion and occasional metallic tinkle on postural change. This side exceeds the right by nearly two inches. Was obliged to lie on it continuously, as any alteration, however temporary or trivial, affecting the gravitation of the fluid, is liable to induce most distressing dyspnoea and compel a hasty retreat.

Having been under treatment for some time, and alteratives and other approved means having failed to afford relief, he was tapped between fifth and sixth ribs, and freed from the

incubus of four and a half pints of thick gelatiniform purulent fluid whereby considerable relief was procured; the pulse fell in twenty-six hours, from 96 to 70; and he was able to lie and sleep on his back. This improvement was not, however, destined to continue, as on some exertion, he was seized with a desire to vomit, and soon after discharged a large quantity of thick offensive puriform matter, which he thought came from the left side and was "near to choke him." Was subsequently much troubled with cough, headache, stinking, shreddy expectoration, tympanitic flatulence, and most distressing sickness. Moans a great deal and looks drowsy and comatose, had occasional twitches about the face, some lateral traction of mouth, rumbling involuntary motions and convulsions, which were only terminated by death.

Post Mortem.—Showed the left lung to be a mass of softened tubercle, completely shrivelled and reduced to the size of a cocoon, the pleural cavity is lined by a thready membrane and coated with pus, which when mixed with water and blown from the trachea, rises in distinct bubbles. The right side is occupied by the lung of that side, the heart and its membranes. Some atheromatous deposit on aortic and semilunar valves, entire substance large, flabby, and very friable. Cerebral mass soft, pulsatious, and containing more fluid than natural in either ventricle, whose lining membrane is rough and thickened, as if from tubercular inflammation. Some hard and granular matter formed on vessels in the fissure of Sylvius, and the hinder part of the left lobe, was shown, by microscope, to contain all the elements of tubercle and exhibit the usual characteristics of that deleterious product.

With regard to, and by way of explanation of, the strange escape of purulent matter noticed in connection with this case, and which would have probably "proved its saving clause" had not a graver complication been, meanwhile, developing itself, I subjoin the following particulars of a similar occurrence from a paper on *Paracentesis*, by Dr. Hamilton Roe, published in the *Transactions of the Medico-Chirurgical Society of London* for 1844-5.

Caroline Holloway, *ætat* 19, a serofulous looking girl, after suffering a few days from pain in the upper part of the left side of her chest, but not so much as to oblige her to desist from her employment as a still-room maid, was suddenly attacked with an alarming sense of suffocation, which obliged her to jump up in bed, when she brought up, in a stream,

so large a quantity of fetid pus, that it was received in a wash-hand basin. The symptoms recurred in paroxysms frequently afterwards, and were always relieved after the discharge of fetid matter; the quantity brought up the first day measured more than three pints, that which came up daily for a week afterwards, exceeded a pint. Her left side was everywhere very dull on percussion, no sound of respiration could be discovered in it any where except at the root of the lung; the voice did not produce the ordinary vibration, but agophony was well marked under the posterior and inferior angle of the scapula. These signs were due to the presence of an empyema, and he accounted for the purulent vomiting by supposing that a communication existed between a bronchial tube and the cavity of the pleura filled with pus, and yet the symptoms indicative of the presence of air were wanting.

The granular matter referred to in Case VI. was, doubtless, of tubercular origin, and its existence so far enhances the interest attaching to this case, inasmuch as that Cruveilhier, as quoted by the editor of *Ranking's Abstract*, never saw an example of tubercle in the brain at the age in which it occurred in the above named patient. Jones appears to have met with one instance only out of 117 cases; and Abercrombie was, apparently, not much more successful. Recent researches, however, coupled with some personal experience, would dispose me to regard its occurrence in the brain, especially in its meningeal portion, as by no means so rare as the investigations of these able pathologists would lead one to suppose, and I can, in confirmation of this statement, refer the reader to a paper on Diseases of the Pons Varolii, and incidentally on the phenomena of this complication recently contributed by Dr. Herman Weber to the Medico-Chirurgical Society of London. It must be confessed, however, I think, that its symptoms during life are often so variable and uncertain—though, in the main, compatible with those described above—as to admit of being easily confounded with other less dubious or equivocal physical conditions, and hence, possibly, one reason for the comparative rarity of its recognition after death and of the conspicuous absence of those details which aid us so materially in other more inviting and accessible fields of diagnosis and pathology.

CONGENITAL.

Case VII.—A. A. F., *Æt.* 11.—An extremely ill-conditioned, cachectic, and rather repulsive-looking child, of strumous habit; languid, wan expression, and phthisical tendency. Had the ordinary diseases of infancy, including scarlatina, which was followed by swollen feet, puffy adenomatous face, scanty high colored urine, and the usual concomitants of scarlatinal dropsy. Is very liable to catch cold from any exposure, and has been troubled with winter cough for the last four years, occasional spasmodic catchings of the breath; rambling and sensorial disturbance during sleep, irregular appetite, hectic and faintness. Has had no hæmoptysis, only occasional trifling diarrhoea; no sweats or vaginal discharge, and save the above-mentioned aspect, appears in fair health.

Outline of chest rather irregular, receding laterally, and showing undue depression towards upper-third and some prominence and bulging on right side, which exceeds the other by at least three-quarters of an inch. The breathing is very harsh at left apex, where also the voice is shrill and bronchophonic and percussion note is hard and wooden. Heart completely displaced to right of sternum, beating audibly between second and third ribs, rather above and to inside of mamma, where it tilts forward against the chest, and emits an equable, uniform sound, except when flurried or set a-going by any excitement. This was most probably a case of congenital displacement, but lacked the transposition of other viscera, which usually accompany or follow that freak of nature. She left the hospital in good health, and was afterwards reported as able to undertake a voyage to Australia.

Case VIII.—S. H., *Æt.* 19.—Clerk; a tall, thin, spare, rather delicate-looking young man, of steady, active habits, and liberal culture. Is disposed to phthisis by the mother's side, and has suffered, of late, from sweats, diarrhoea, and indescribable feelings about the throat, the glands of which are swollen on either side as if from bronchocele. Is subject to palpitation, nervous headaches, and dreadful dreams; but had no hæmoptysis or expectoration, and is apparently otherwise in good health, though he looks pallid and anæmic; is rather anxious as to the future. On examination, the heart is found pulsating feebly on right side, the first sound slightly prolonged, distant, and booming as if from anæmia, but free from

any valvular complication or other structural change, and at no time specially attracting his notice on the score of danger or position. In fact, he was almost unconscious of the alteration, and made very light of the matter, so far resembling the case of an engineer, who some time since presented himself for the inspection of Dr. Brinton, to whom I am indebted for the particulars, and never knew until told by that gentleman of the altered condition of his heart. In both cases, the pulmonary symptoms were trifling, and likely to be improved by change of scene or ferruginous treatment.

The following note from the second edition of Dr. Walshe's able work on the Heart and Lungs, may be here appropriately introduced, if only by way of commentary or sequel to the above, and as an evidence, however vague and imperfect, of the comparative infrequency of some of the cases of this paper:

"I have twice, at the Consumption and University College Hospitals, seen the heart permanently beating in the right thorax, where no pleuritic effusion on either side had ever existed, as far as could be made out by present signs or past history; where the liver and spleen lay in their natural positions, and where great tuberculous excavation and destruction on the right side seemed to have drawn the heart in that direction, aided by, perhaps, the *detruding* influence of hypertrophy of the left lung.

CONGENITAL DISPLACEMENT OF HEART AND OTHER VISCERA.

Case IX.—L. M., *Æt.* 14.—School girl; born and residing in a healthy neighbourhood, and always well provided as regards food and clothing; of phlegmatic temperament, dark complexion, and healthy parentage. States that she has always had delicate health, and has never been free from cough, even from her birth, except, perhaps, for a short time, in the summer season, or when under the influence of expectorant or anodyne medicines. The breathing is at times very short and oppressive, the voice thick and husky, and she expectorates, rather freely, a thickish green matter of offensive odour and adhesive consistency. Has an irregular failing appetite, costive bowels, but no night sweats, hectic or wasting. On admission, however, she complained of increasing dyspnoea, persistent tight cough, hoarseness, debility, languor,

great inaptitude for exercise or work, and on examination, the heart was found considerably to the right of the mesial line, beating in the fifth interspace, both sounds healthy, but dull, muffled, and toneless, audible over large space, but not obscuring chest sounds in that side. The liver is situated on the left side, and can be clearly isolated in left hypochondrium; the spleen also appears to have altered its situs, and the other viscera, as far as can be ascertained, seem to have undergone a similar transposition.

EMPYEMA.

Case X.—J. B., *Æt.* 23.—Needlewoman, residing in London. Admitted under the care of Dr. Cotton. A small, thin, spare, cachectic looking creature, of anxious pallid aspect, nails clubbed, gums marked, no hereditary tendency.

History.—Has worked very hard for years, frequently spending nights together at her needle, faring indifferently meanwhile, but generally enjoying good health, and being at no time, to her recollection, the subject of any serious ailment. About thirteen weeks since, first began to suffer from cough, night sweats, and copious purulent stringy phlegm, which latter is now diminished in quantity and apparently influenced by the discharge from what was regarded as an abscess, connected with diseased bone and situated below left mamma. This, having pointed between the fourth and fifth ribs, was opened about eight months ago, and gave escape, at the time, to more than three quarts of matter; it has continued to discharge ever since, and still gives exit to three or five ounces of pus daily. Had trifling streaky hæmoptysis, and suffers now from retching, flatus, and dragging pains in the abdomen; is besides hysterical and complains a good deal of nausea, bad taste, thirst, and sinking at the præcordia. Pulse 96; respiration normal; right side at base 13½ inches, left side at base 14 inches; outline of the chest moderately symmetrical; heart entirely transferred to right side, and beating feebly under same nipple. As may be expected, the breathing was almost completely suppressed on the left side, and quite inaudible at base. Dulness was absolute both in front and behind, and the intercostal spaces obliterated.

On right side percussion note morbidly resonant, especially so over cardiac region, which is, probably, overlapped by margin of lung. Breathing in same is exaggerated, puerile

and blowing, and became, subsequently, of amphoric type in upper third of left, but without any corresponding elevation of the voice or perceptible alteration as regards measurement. The subsequent history of this case was rather a chequered one, and would only involve uninteresting details of hopeless suffering and pain. Suffice it to say that she lingered on for some time, gradually losing ground, complaining of great irritability of the stomach, accompanied by frequent vomiting, griping, and tormina in the neighbourhood of the umbilicus, frontal headache, insomnia and general malaise. These were aggravated by the miserable consciousness she retained to the last, and only relieved by her death, which occurred some two months after her admission.

On post mortem.—The chief appearances, as regards the chest, may be included in the following summary:

The heart, which was large, flabby, and friable, was so closely adherent to the pericardium that separation could not be effected and, together with mediastinum, was completely displaced to right of spine, its orifices, which were abnormally dilated and pervious, were free from deposit or other structural change. This may, perhaps, so far account for the absence of any valvular murmur as noticed during life.

The right lung was crepitant throughout, exhibiting some superficial marginal emphysema and basic congestion, but free from every trace of tubercular or cretaceous deposit, small, and weighing only fourteen ounces.

The left may be regarded as entirely obliterated, *quoad* its function, being reduced to the size and consistence of a small elongated and closely adherent cake of hard fibro-cartilaginous substance, which was situated at the upper angle of the chest, and was so firmly bound down by adhesion, as to require considerable force in removal. A small irregular opening was, with difficulty, discovered between first and second ribs, and seemed to communicate by a tortuous winding fistulous tract with the external aperture, which was situated below and somewhat to outward side of mamma, and between the fourth and fifth ribs. There were several small circular patches of ulceration on left costal pleura, the sac of which contained about sixty-one ounces of stringy greenish pus, and was besides so disintegrated, or rather riddled throughout, as to admit of the matter easily escaping and burrowing in the superjacent muscular structure from which it might

have discharged externally. A larger quantity of opaque, slimy, inodorous fluid was found in the cavity of the abdomen, and a similar, but more cheesy, material lined the anus and genital fissure.

Considering the physical signs, the absence of any well marked pleuritic seizure, of hydrothorax, tubercle or other exciting cause, this purulent fluid must have, gradually, supervened on some old-standing mischief, and, by its pressure on left lung, have caused collapse thereof, and, consequently, entailed on the opposite the function of both for several months back.*

The late Dr. Theophilus Thompson published in the Transactions of the Medico-Chirurgical Society of London for 1844, the particulars of a successful case of paracentesis in a boy five years of age, from whose right side—in addition to the loss sustained by a continual oozing and the presence of a fistulous opening—upwards of seventy ounces of pus were removed by four successive punctures. Dr. Archer has seen eleven pints of serum removed, at once, by operation, and Dr. Roe testifies to the withdrawal of one hundred and five ounces of the same kind of fluid under similar circumstances. The quantity of purulent fluid and amount of cardiac displacement above referred to assimilate this case to one which was kindly described to me by Assistant Surgeon White, of the Artillery Battalion at Sangor, and I cannot, I conceive, elsewhere more appropriately introduce the record of a case of a similar type, which was apparently regarded as a great novelty by the author of the *Curiosities of War*, who places it under the heading of “remarkable wounds and hair-breadth escapes,” and describes it in the following words:—

“One of the most singular wounds, perhaps, ever received, and not to cause immediate death is the following: the account is extracted from the report of the Medical Board on the officer in question. Lieutenant French, of the 38th Regiment, received a gun-shot wound on the 18th of June 1855, before Sebastopol, in the upper portion of the left shoulder, which penetrated the chest, and resulted in a most copious suppuration from the left side with compression of

* Dr. Lowe, with apparently some surprise, speaks of the case of a boy, fourteen years old, who survived the occurrence of pleural fistula nine weeks, and in whose chest more than two pints of greenish purulent fluid had, meanwhile, accumulated.

the lung, and removal of the heart from the left to the right side. The left arm is powerless, and his general health very delicate; the suppuration from the left lung, though considerably diminished, not having yet subsided.

"He died on the 9th of December 1857."

HYDROTHORAX.

Case XI.—S. G., *Æt.* 20.—Single, gardener, born and residing in the country; of strumous delicate aspect and phthisical parentage. States that some months ago, while endeavouring to obtain some water from a deep pond, he accidentally slipped, was drenched all over, and remained in his wet clothes for several hours. A few days after, he complained of some tightness and of pain at the chest, sore throat, dysphagia, and malaise; had pains in his legs, arms, and all over him, and was so generally ill as to be obliged to give up and take to his bed. For these he applied at an hospital, and was under treatment for six weeks; but, experiencing no positive relief and becoming weaker from abstinence and other causes, applied to me, and was after some short attendance, as an out-patient, admitted, representing himself as losing flesh and strength; feeling as if he had some water in his chest, and complaining of cough of paroxysmal type and variable severity, but liable to recur on any movement, and invariably induced by right recumbency; sweats, insomnia, hectic and occasional pain in left side, which, indeed, appears more uniform, round, and bulging than natural, and exceeds the right somewhat on measurement. There is very marked dulness on percussion on the same side, extending about two inches above the nipple, and continued in a horizontal line to the back of the chest. This can be removed from the anterior surface by placing the patient on his back and *vice versa*, agophony and deficient breathing also exist and leave no doubt as to the presence of fluid. Pulse 106 sitting; 112 standing; respiration, 24; heart's action healthy; but its position is reversed, being entirely displaced to right of sternum, and beating rather feebly between third and sixth ribs on that side. He was treated, for some time, with cod oil, fly-p^o blisters, jalap purges and such diuretics as blue pill, digitalis, and squills, with only temporary relief; and, as his strength began to fail, the chest symptoms became more urgent; tapping was practised and two pints of a *serous* looking fluid obtained thereby. The wound was ordered to be kept open,

or rather the outward pressure of matter, which escaped rather freely, kept it so; and, though every precaution was taken to prevent inflammatory action and every means employed to support the constitution and obviate the tendency to death, yet his appetite began to flag, his cough and expectoration became more troublesome, persistent, and wasting, amphoric breathing and metallic tinkling were alone heard in the left chest, diarrhoea supervened and death closed the scene.

On examination, two pints of thick tenacious muco-purulent matter were found in left pleura, which was much thickened and closely adherent towards apex and along the spine for nearly three inches. The lung itself was reduced to a mere flattened carnified mass, exhibiting some tubercle scattered through its substance and a suppurating vomica at either end. The heart was pale, soft, and flabby, its foramen ovale was quite pervious, and its position was so completely reversed that it could not be replaced without force, and immediately returned to the right side when the force was withdrawn. This was an ordinary case of hydrothorax, degenerating into an empyema and consequent on inflammatory pleuritic effusion which latter caused the cardiac displacement, without, however, influencing its rhythm or interfering with its tone, otherwise, than as described above.

PNEUMOTHORAX.

Case XII.—Susannah Merton, *Æt.* 22.—Single, admitted under the care of Dr. Thompson, complaining of troublesome cough, night sweats, occasional hectic flushing, debility, and general emaciation. Has beautifully silken hair, dark blue eyes, rather prominent cheek and nasal bones; is, otherwise, symmetrical, of more than average stature and commanding altitude, and looks in every respect the very ideal of attractive comeliness and grace. Says that, some few months before her admission, she caught a cold and suffered from some sore-throat, thick pellety expectoration, tightness, headache, and slight dysphagia; has, occasionally, noticed some streaky hæmoptysis; not menstruated for the last five months, is hypochondriacal, fancying various things, very anxious about her health or rather despondent as to the future, and otherwise depressed and low-spirited.

There is considerable depression under either clavicle, hard wooden percussion note, diffuse bronchial breathing, and dry cavernous crackle in both; but especially so on the left side,

where a very moderate tap elicits the characteristic *Bruit de Pot fêlé*, and the voice is grating and pectoriloquous. To these are superadded, though to a comparatively moderate extent only, the usual concomitant appearances of progressive tubercular cachexia, notwithstanding which, she rapidly improved and, wishing to revisit her friends, but dissuaded from doing so by her fellow-patients, who justly regarded her improvement as temporary, she, as it were in spite of them, and to show her immunity from danger, ran hurriedly upstairs, whence she returned with difficulty, exhausted and faint. Some hours after she was suddenly seized with sharp pain at left apex, which, soon extending to diaphragm and thence laterally to spine, involved the entire chest on that side, and gave rise to severe and urgent symptoms of dyspnoea, exhaustion, and collapse. These were slightly relieved by mustard applications and some anodyne draughts, but the side meanwhile increasing in bulk, soon reached to 15½ inches, while the right measured only 14 or a little more, was prominent, bulging, and tumid, abnormally resonant, emitting a clear drum-like tympanitic sound on percussion and appearing free from any irregularity or intercostal depression. The pulse averaged 120, while the breathing rarely fell below 40; she could only lie on the affected side, and looked very anxious and imploring.

The breathing subsequently became of amphoric character, accompanied with occasional metallic tinkling, but the voice was at no time agophonic, though basic dulness clearly indicated the presence of fluid. Apex pulsation of heart, between fourth and fifth ribs somewhat above and to outer side of right mamma, heard over a considerable space of that region, and also posteriorly at inferior angle of scapula. This poor girl lingered on some days, alternately feeling better and worse as one application or another afforded its meed of relief; but, diarrhoea supervening, soon put an end to her sufferings; and on postmortem examination, the following prominent appearances were noted as referrible to the chest:

The heart, as in the previous cases, was found quite displaced to right side, but otherwise presented no appearance deserving of record. The right lung contained a large secreting cavity, protected by a thickened false membrane, and not communicating with body of lung, which was freely infiltrated with crude tubercle throughout: the left had entirely

collapsed and fallen in, being only adherent at apex by a fibrous band, splenified pancake-like, and devoid of any trace of vesicular structure. Could not be inflated through bronchus, but allowed of the opening in pleura being discovered, and this was found to correspond, by inference, with the lower margin of third rib, where the pain was first complained of.

On puncturing left pleura with a trochar, a continuous and whiffing current of air escaped with such force and volume as to extinguish a lighted taper, its sac contained over 70 oz. of sanious watery pus, and was all over covered with patches of ulceration and softening, the result of an equally grave and apparently simultaneous ~~abscess~~ and development of empyema and pneumothorax.

PARTIAL DISPLACEMENT WITH PROCIDENTIA FROM HYPERTROPHOUS EMPHYSEMA OF THE LEFT LUNG.

Case XIII.—G. H., *Æt.* 43—Printer, a pallid, worn-looking man, of anxious, suffering, and livid expression of countenance, with prominent eye-balls and occasionally flushed and hectic aspect. Was admitted under the care of Dr. Brinton at the Royal Free Hospital, and gave the following account of himself:

Had very good health up to about twelve months ago, when he began to suffer from difficulty of breathing, hard persistent cough, frothy inodorous sputum, gasping on any exertion, and great tightness and oppression about the precordia—had, besides, considerable pain under left mamma, occasional palpitation, vertigo, and "exhausting dreams."

For these he was confined to bed, blistered, cupped, and treated otherwise, as if for an inflammatory affection of the left chest, and was so far relieved, for the time, as to be able to sit up and go about. He has remained, however, in a weakly condition up to about two months back, since which he has declined apace, and is now suffering from an accumulation of the above symptoms in a very aggravated form. On admission, the breathing was very harsh, wheezing, and bronchial all over the chest, dry sibilous and snoring ronchi, copious at either base, where they obscure all other sounds, almost simulate rubbing. Percussion note clear-sounding and tympanitic over entire left aspect of chest, its cardiac space abnormally resonant, and heart's action suppressed and distant therein, being clear in opposite space of right, where it tilts visibly

against parietes, and beats equally below and to inner side of mamma on that side; there is also some slight displacement downwards, but no murmur or other indication of valvular mischief. The respiratory murmur is generally more feeble and uniform on right side or only partially obscured—not arrested—by superincumbent heart at lower third. This man was so much benefited by small and repeated doses of morphia, followed by quinine, support, and flying blisters, as to be able to leave the hospital comfortably and resume his work with the prospect of being able to continue it.

OLD EFFUSION.

The following case I saw among the out-patients of the Royal Free Hospital:

Case XIV.—E. K., *Æt.* 44.—A weakly, pallid-looking woman, of waxy aspect and feeble gait, has, for years, suffered from pain and uneasiness in left side and at angle of scapula posteriorly. Had pleurisy in November 1856, and was cupped and blistered for same at an hospital. Has been declining ever since, complaining of pain in left side, cough, occasional hæmoptysis, sweats, hectic and great debility. On examination, the heart sounds are almost inaudible in left cardiac space, but quite clear and sounding in opposite region, the apex pulsation being slightly above and to outer side of mamma, which latter is quite flabby, pendulous, and displaced downwards. The breathing is almost suppressed on left side, no ægophony, but complete and absolute dulness prevails all over lower third of that side, and occasional splashing on succussion or postural change. There is also a very foul odour from the breath and rather copious puriform sputa, indicating, doubtless, some circumscribed empyema and progressive lung disintegration. There was no murmur with the heart's sounds, and I subsequently lost sight of her; possibly she was obliged to discontinue her attendance from illness, or, mayhap, died suddenly from exhaustion.

EMPHYSEMA.

Case XV.—I made the following hurried notes on a case that came under my notice while busily engaged in disposing of a large number of out-patients at a public charity, and am satisfied of their general accuracy, having had them subsequently confirmed by repeated examination.

J. S. is 21 years of age, rather above the medium sta-

ture, of pasty pallid aspect, and strumous tendency. Is disposed to phthisis on the mother's side; has not menstruated regularly of late; is weakly and worn-looking, and is very anxious to get rid of the troublesome night sweats, cough, and insomnia with which she has been afflicted for the last eighteen months. Had an attack of hæmoptysis, which threatened her life as she says, and confined her to the house for a fortnight. On uncovering the upper portions of chest, the latter is flat, receding, and slightly sunken towards the clavicles, especially so on the left side, where also the breathing is found to be harsh, tubular, and noisy, the expiratory murmur being, at the same time, prolonged, and the voice and tone shrill and bronchophonic. Diffuse humid crackle at apex, and copious liquid rales towards base mark the respiratory sounds on the right side, but allow the heart's action to be clearly seen, and its pulsatory thrill to be distinctly heard outside and below right mamma. It is, however, only partially displaced, being also heard in left cardiac space though to a much less extent, and possibly the emphysematous condition of right lung may serve to account for its more audible rhythm, and defined resonance, though it will not suffice to remove the other difficulties of its altered position, or explain by what process it was removed from its normal site, besides a partially consolidated and shrunken lung in face of another, whose bulging sponginess and elasticity ought to have been the very means of replacing or retaining it at a distance.

HYPERTROPHY.

Case XVI.—C. E., *Æt.* 18.—A labourer by trade, "has not been particularly strong nor yet otherwise," as he says; but feels a creeping sense of uneasiness and discomfort for which he cannot account, but which he timidly ascribes to disability of the chest, the result of hereditary disease, and thinks both were aggravated by cold, dissipation, and exposure. The breathing all over front of chest is harsh, bronchial, and wavy; the expiratory murmur is markedly prolonged in same; and the humid crackle, accompanying deep inspirations in right cardiac space, leaves no doubt as to the existence of progressive disease and excavation in that region. The heart's sounds are noisily diffused over chest, emitting a bellows whiffing murmur with the basic systole, and exhibiting a much larger area of surface dulness in mid-sternal region

than natural. The organ itself is thrust downwards, and to the right, being obviously enlarged and hypertrophous, its position may be regarded as a good measure of the extent to which mere adventitious deposit, or in plain English, weight, may—in conjunction with advancing solidification and contraction of opposite lung—give rise to procidentia or lateral tilting, and cause diffusion and resonance of sound over greater part of chest. A heart measuring $15\frac{1}{4}$ inches in circumference, 6 inches in length, and weighing forty-six and a half ounces, was recently exhibited at the Pathological Society of London, by Dr. Gibb; but, beyond the fact that pleurisy and pericarditis preceded death, no further details are given as to its situation or condition during life.

ANEURISM.

Case XVII.—A. B., a middle aged man, of temperate habits, came under observation, on or about the ninth of October 1858. He then complained of persistent pain at the epigastrium, lumbar dragging, and uneasiness along whole of dorsal region. These sensations were, occasionally, accompanied with feelings of stifling and fulness about the head and chest, sinking at the præcordia, and, latterly, they had so far declined in severity as to admit of his enjoying some rest at night and the conversation of his friends. The pain, however, was wont to recur on the slightest provocation, and soon becoming more erratic and liable to subsidence or intensification according to variation of posture or weather, kept him continually in a state of anxiety and suspense. There was a slight beat with the apex systole, which was only audible to right of sternum, where, indeed, cardiac pulsation could alone be heard, and to which also the heart itself had been substantially transferred. He had a peculiarly anxious, worn and somewhat malignant expression of countenance, without, however, any attendant emaciation or constitutional sympathy, saving that noticed above. The diagnosis arrived at during life assumed the existence of an aneurismal dilatation of aortic arch, with mitral valvular disease, but the extent of the former was not even surmised. He died suddenly after an unusually severe and protracted attack of paroxysmal dyspnoea, and, on opening the chest, the heart was found, as just stated, entirely displaced to right of sternum, occupying a position somewhat above that usually assigned to it, on the left side, between the 3rd and 5th ribs,

lying superficially over the lung, which it had somewhat compressed inwards, with its apex towards the right nipple, and otherwise so located as to twist the aorta partly on itself, and so possibly give rise to the murmur alluded to above. The aneurismal dilatation commencing apparently at, and almost including a portion of, the heart itself, extended from top of third dorsal to corresponding portion of 3rd or 4th lumbar vertebra, involving not only their surfaces, but actually eating into and destroying them in some parts, as far as to laying bare the vessels and coverings of the spinal cord, leaving, however, the inter-vertebral fibro cartilages whole and intact.

It was found to be hour-glass-shaped from diaphragmatic constriction, the lower part being, of course, more baggy and elastic than the upper, and having burst into the left pleural sac, so filled that cavity as to compress that lung and deprive it of its ordinary breathing capacity, whereby, doubtless, fatal dyspnoea was induced.

I have also had, more recently, another case of aortic aneurism under my care, in the person of a man aged about 25, of whose postmortem the following brief notice must suffice for the purpose of our illustrations. An unusually large and sacculated aneurism of the aortic arch was found occupying the upper portion of the chest, displacing the left lung upwards and backwards, and the heart outwards and downwards, and, by its pressure on the trachea and bronchus, giving rise to aphonia, and reducing the lung to considerably less than a third of its average normal bulk.

ENCEPHALOID CANCER.

Case XVIII.—A thickset muscular man, of lax flabby habit; a private of the 2nd Battalion of H. M.'s 15th Regiment, was admitted (under the care of a colleague, who soon, however, transferred him to me) into the Military Hospital, Pembroke Dock. He had a sad, sullen, inexpressive look; a pale, bilious, phlegmatic cast of countenance, and his general appearance bespoke central implication, long-standing gastro-hepatic disorder or malignant cachexia. On examining his chest, and stripping him for that purpose, the glands on the left side of neck were found to be distinctly enlarged, and infiltrated with softened semi-fluid matter of greasy pul-taceous consistence; the intercostal spaces on the same side soon became obliterated; the face assumed a puffy œdema-

tous and somewhat repulsive outline, and percussion elicited absolute dullness all over left side of chest. The urine was tested in vain for albumen or other indication of renal derangement, and, meanwhile, the brain giving no evidence of sympathy or decay, and healthy breathing being entirely absent or suppressed on the left side, we were left to the option of choosing between empyema or cancer. The occasional supervention of amphoric gurgling, with its accompanying metallic tinkling, somewhat complicated the case, but, taking into account the condition of the cervical glands, the man's malignant expression, the absence of any previous pleuritic attack, the immobility of the area of chest dullness, and its being unaffected by any amount of postural change or succussion, soon led us to infer the existence of encephaloid disease of the lung. This, indeed, was suspected all along, and, without entering into unnecessary details, it will be sufficient to state that, after lingering on for some weeks in a state of painful suspense and suffering, he died rather suddenly; and, on further inquiry, afforded us an opportunity of verifying the diagnosis above referred to. It was found that the lung had not only filled the left cavity, but had forced itself downwards towards the stomach, and upwards under the clavicle, displacing not only the heart and its tributaries, but also the mediastinum, and so compressing the large efferent venous trunks as to cause a stagnation and plethora of blood in the sinuses of the brain and all their ramifications.

The heart, as just stated, was completely tilted towards the right; its apex beat corresponding with a point about an inch below the nipple of that side, and its structure had evidently undergone fatty degeneration. Portions of lung tissue, subjected to microscopic examination, were found to present the features and contain the elements usually recognised as belonging to encephaloid cancer, and the case itself is only interesting in so far as it tends to illustrate the pathology of our subject, merely confirms the experience of years, and offers a melancholy contrast to the results that usually follow the treatment of other thoracic maladies.

HYPERTROPHY OF THE THYMUS GLAND.

Case XIX.—As I was preparing on one occasion for the usual morning visit at the Royal Free Hospital, to which I was then attached in the capacity of its Resident Sur-

geon, my attendance was urgently requested on behalf of a child, who was, subsequently, described as weakly, pallid, and failing habit, and who, whilst eating slowly of meat and potatoes, and apparently in the enjoyment of his usually feeble and variable health, was suddenly seized with symptoms of such urgent dyspnoea and suffocation as necessitated immediate reference to a medical man and removal to the hospital. When I saw him, respiration had ceased, and life was extinct. Notwithstanding the clearest evidence of this being so, and that I repeatedly assured the parents of the absolute inutility of further interference, they seemed indisposed to believe me; and, more out of consideration to their feelings than from any expectation of a favourable result, I was induced, after having searched the fauces with my finger, and passed a soft elastic tube into the stomach, to try other measures, including the Marshal Hall plan. Bleeding from the arm, galvanism, friction, and nasal stimuli having been resorted to in vain, and there being no further doubts on the minds of his parents as to the supervention of death, his body was removed to the dead house, and a post-mortem examination having been duly instituted at the instance of the coroner, the following interesting appearances were obtained therefrom:

Body thin, spare, and diminutive; some discoloration and lividity, not amounting to actual decomposition, over surface; face pinched and contracted, and cadaveric rigidity entirely gone. The lungs were, throughout, crepitant, of average weight, floated freely in water, and except some slight marginal emphysema, exhibited no other trace of disease than a trifling basic vascularity, which was produced most probably by gravitation or the spasmodic efforts of expiring life. On, however, removing the sternum, which was closely adherent at its upper third, a large elongated and somewhat pyriform mass was seen to occupy a considerable space of its under surface, firmly attached in front to the anterior mediastinum, behind enclosing the trachea on which it seemed to lie without causing any obstruction, extending up as far as the opening in the neck, and downwards so as to overlap and displace the heart towards the right—to the extent of at least 2 inches from midsternum, it was irregular in shape, rather flattened and triangular, of hard cartilaginous consistence, cutting like gristle, and containing in the centre a circumscribed patch of opaque glistening substance which,

under the microscope, showed some granular matter of tuberculous character, insoluble in acetic acid, and imbedded in a loose fibrous stroma of variable size, but apparently analogous, in point of minute structure, to the product of the glandular enlargement generally found accompanying, and usually described under the name of bronchial phthisis. It would weigh from ten to fourteen ounces, or perhaps more; was found to correspond in size and shape with the structure known, in hibernating animals, as the thymus, and was regarded by several competent to judge as nothing less than a highly exaggerated example of chronic and persistent hypertrophy of that organ. There was also considerable scrofulous enlargement of adjacent bronchial glands, and of one especially, which was situated at the bifurcation of the bronchi and which, doubtless, aided in producing the cardiac transposition just referred to. That this was, obviously, due to the bulk and pressure of the above-named substance, and not to any postural change or postmortem condition, will appear from the fact of its remaining permanently stationary, and not returning to its abnormal position when left to itself. It could not, besides, be in anywise influenced by elevating or depressing the chest, by blows, movement, or lateral tilting, and the abiding outline and impression of the enlarged gland structure removed all further doubt as to its causation and pathology. Whether this displacement contributed in any way to the sudden suspension of life, could not, as stated at the time, be clearly ascertained. That reflex nervous irritation of the vagus, by its spasmodic effect on the bronchi and pulmonary parenchyma, potently influenced it, cannot, I think, be reasonably doubted, and possibly the enlarged and flatulent stomach by diminishing the capacity of the chest and impairing the contractile power of the diaphragm, may have also enhanced the difficulties of escape. However this may be, the literature of the subject was, up to a recent period, miserably meagre and imperfect; it was reserved for Mr. Simon to revise its physiological relations and natural history from the reproach of its barrenness, and though Mr. Hood, of Kilbowen, as stated by Copland, first called attention to its bearing on infantile death and disease upwards of thirty years ago, and though Baillie, Burns, and others passingly attended to it in connection with the same topic, it is only within the last few years, that it has been illustrated by several notices from the pens of Messrs. Brent and Pret-

ty; and of course more ably and systematically handled by Dr. Stokes, of Dublin, in his elaborate work on the heart. It is quite possible, however, that others may have turned their attention in this direction, and equally so that further reading may enable me to speak more hopefully for it. At present I must content myself with the above, and plead my ignorance in justification of silence.

CONGENITAL MALPOSITION OF STOMACH.

Case XX.—I published in the index number of the *Lancet* for December 1860, some particulars respecting the cause and circumstances attending the sudden death of a lad of nineteen years of age, who was recently received into Her Majesty's Service; of whose antecedents we knew almost nothing at the time. He was progressing favourably under the treatment employed, for a boil on the back of the neck, and was apparently quite free from any uneasiness up to the forenoon of his decease, when he first complained to the orderly of headache, thirst, and sense of tightness and throbbing about the forehead and temples. Having, subsequently, exhibited some signs of mental disturbance, coupled with fever, restlessness, and general excitement, I was requested to see him, and finding him very hot, confused, and irritable, I ordered him an emetic of ipecacuanha, one scruple to a quarter of a grain of tartarized antimony. He could not be induced to vomit, notwithstanding the efforts that were made to produce sickness, and he complained to the ward-master—in deprecation I presume of his further efforts—that he never remembered having been sick in his life, and on subsequent inquiry, it was ascertained from his mother that this was so. That he was wont to suffer from uneasiness and oppression about the chest after food or exertion, and that palpitation and dyspnoea, invariably, followed any rapid motion or excitement. Having to perform some duty elsewhere, I visited him again on my return and, finding no particular change for the worse, contented myself with satisfying his immediate wants, and ordering a spirituous lotion to the head. Making only a short stay on my way through the dispensary, I had scarcely arrived in my room before my attendance was again pressingly desired, as the patient was represented as struggling convulsively in a fit, and, on returning in all haste to the ward, I was much surprised to find that he had ceased to breathe, and was, to all appear-

ance, quite dead. The usual measures having failed to produce any beneficial effect, he was removed to the dead house, and, on subsequent examination, was found to present the following strange appearances.

As the symptoms immediately preceding death were almost exclusively referable to irritation of the brain, the cavity of that organ was first examined; and, on removing the calvaria, a quantity—estimated at two or three drachms—of dark fluid blood was found pressing on the dura mater, lying loosely about midway between the frontal sinus and the parietal suture, not dependant, apparently, on rupture or injury of any large vessel, but probably due to pressure or oozing from the longitudinal suture. On percussing the walls of the left chest, I was much surprised to find my tapping elicit a clear ringing tympanic sound, like that resulting from the pressure of accumulated air—pneumothorax—and, unable to account for the resonance on other grounds, and unwilling to acknowledge so grave an error of diagnosis, was gratified to find my conjecture disproved by the following strange deviation of Nature on the part of the stomach. This viscus, in addition to being itself completely displaced upwards through a congenital opening in the left tendon of the diaphragm, and occupying a position in front of the left lung—which it had so compressed against the spine as to reduce its weight to six ounces, while the right weighed upwards of twenty-two—had so entirely filled the left cavity of the chest as to encroach upon and push the heart and its appendages wholly to the right, and that organ would have pulsated during life considerably below the nipple, and at least $2\frac{1}{2}$ or 3 inches to right of sternum. That this latter displacement owed its origin to that of the stomach, and was, probably, of equal standing in point of time, cannot, I think, well be doubted, and the bed it had burrowed for itself, as well as the numerous adhesions it had contracted to surrounding parts, coupled further with the fact, that its own weight was more than sufficient to counterbalance the effects of pressure or postural change, leave, I believe, no reasonable doubt on that score. In connection with this case, my then colleague, Staff Surgeon Bacot, gave me some particulars, of a very similar character, touching the sudden death of a gentleman residing in Wellington, New Zealand, who had been attacked with faintness and vomiting after retiring early to bed the previous evening, with an overloaded stomach, and much

exhausted by the bustle and fatigue of the day. When seen by Mr. Bacot, the symptoms were obscure and puzzling; his skin was cold and clammy, his pulse rapid and small, his countenance fearfully anxious; the features drawn and haggard, but the intellect perfectly clear. He suffered from occasional hiccup, subsultus and unavailing spasmodic attempts at vomiting, there was little or no pain or tenderness of the abdomen, no unusual tympanitis or distension, and the bowels had acted freely up to the period of his seizure. The diagnosis was strangulation by band, twisting, or intussusception, but they had some difficulty in reconciling the absence of pain and other local signs with the assumption of mortification, which could scarcely have supervened so rapidly, but with which, however, these appearances were alone compatible. He died a few hours after my friend's visit, and, on opening the chest in the presence of several practitioners, there appeared what the bystanders at first mistook for an uncommonly distended pericardium, an unusually large tympanic and bulging membranous bladder-like mass, which, on being punctured, allowed a free escape of air, and thereby led to its recognition as the stomach displaced upwards in front of the lung, through a recent rupture of the diaphragm. As in the previous case, the heart was entirely tilted upwards and to the right of sternum, its apex occupying a position parallel with the nipple, but it easily returned on removing the stomach, and was obviously due to the strange and violent transposition and straining which that organ had so recently undergone.

In both cases some portions of the abdominal contents had escaped into the chest, and though I cannot at present, for want of reference and material, call to mind any instances in point, I believe there are a few cases on record wherein diaphragmatic hernia (the result of gunshot wound or other injury) gave rise to, or was coincident with, unmistakable evidence of cardiac transposition.

As will be seen from a perusal of the remarks preceding this collection, Graves and Bouilland are quoted as having recorded examples of cardiac detrusion from diaphragmatic hernia. I believe some similar conditions are given by the older writers on Military Surgery.

HEPATIC.

Case XXI.—A delicate, thin, spare woman, of about five

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and thirty, was brought to hospital having an appearance of protracted exhaustion and suffering, but stating that she felt no particular pain, and complained only of a sense of weight and dullness in the region of the liver, occasional tendency to vomit, and sometimes tenderness with a feeling of twisting at the epigastrium. She had a peculiarly sallow cachectic look, an air of melancholy resignation, which seem to imply more than she expressed, and her general aspect bespoke malignancy. On examination, it was found that the liver had extensively undergone cancerous transformation, its outline being so much increased, as not only to affect neighbouring structures, but also, by pushing up the diaphragm, to displace the heart sensibly upwards, and to the right where its apex beat was frequently recognised. She lingered on several weeks in a state of hopeless but not altogether unmitigated suffering, and finally ulceration having destroyed the abdominal coverings, and exposed to view the nodulated sprouting mass, the drain of its numerous outlets, coupled with that of its previous growth and increase, soon told on her already shrunken frame, and she yielded apace and died peaceably notwithstanding.

Case XXII.—A tall, lanky looking individual, of apparently intemperate habits, was brought to hospital in a state of such utter prostration and helplessness as precluded all hope of obtaining any information from him. It was, however, gleaned from the scanty, rambling, and incoherent statements of his female friends, that he had been ailing and delicate for a long time; complaining of uneasy sensations in the stomach and loins, and always worse after exertion, or a bout of drink. On examining him, it was found that the stomach was enormously distended, drumlike and tympanitic to the touch, seemingly very susceptible of pain on pressure (judging from his half-stifled groans and more expressive gestures,) and, in the absence of more positive information, the diagnosis arrived at by the physician in attendance—an authority on such subjects—was that the duodenum had undergone some slight laceration or rupture; that the gradual escape of its contents had set up peritonitis, and that the man was now dying of the combined effects of that malady and the accumulation of foreign matter in the cavity of the abdomen.

This being a stretch of diagnostic refinement for which we were not prepared, a good deal of curiosity was excited

as to the result, and many conflicting suppositions were hazarded, all, however, tending to the assumption that the duodenum was more sinned against than sinning. Meanwhile having satisfied myself, by physical and auricular examination, that the heart, instead of its own, occupied a position considerably to left of sternum, with its apex beat somewhat below and to outer side of left nipple—unduly so to the extent of at least two inches, as I satisfied myself on subsequent measurement—I took up a neutral position, and assuming a tone of silence and suspicion of both sides, but siding with neither, left it to be understood that I reserved my judgment.

Taking my stand on this vantage ground, throwing out occasional dark and suspicious hints as to my own views, and mysteriously qualifying those of others, I was able to announce, after the post mortem, that my impressions were confirmed by its disclosures. These latter showed, convincingly, that the duodenum was quite intact; that the cause of death and displacement arose from the bursting of an enormous hepatic cyst, whose contents, filling the abdomen and giving rise to intense peritonitis, had induced speedy collapse, and otherwise contributed to the symptoms so imperfectly described above.

Case XXIII.—While attending, in her confinement, the wife of a low pot house-keeper, in London, she induced her husband—a blustering, ill-favored fellow of gigantic proportions—to consult me with reference to a swelling of long standing, and slow but progressive growth, which, filling the region of the liver, and extending right across the stomach, had completely deranged the ordinary relations of these parts, and had given the man a conical air of bulging rotundity and corpulence which made him the butt of all his rude associates. Finding some obscure evidence of deep seated fluctuation; that the man had scarcely ever been ten miles from his counter; that he did not ascribe the enlargement to any blow, accident, or injury, and having been perhaps at the time reading up the subject, I came to the conclusion that he was suffering from hydatid disease of the liver, and, having assured him that I did not consider his condition immediately dangerous, left with the assurance on his part that he would not subject his person to any rough usage, or indulge too freely his craving for beer. Happening to fall in with

his taste for strong working physic, his opinion of me increased apace; and, though I saw him but rarely, I understood he rigidly adhered to my directions, notwithstanding which however his contour in no wise diminished, the breathing became wheezy and embarrassed; he could with difficulty get up-stairs or into bed, and having contracted a cold, the lungs became engorged, and he died, in a few days, of apnoea.

On hastily examining the body—a privilege which I obtained with difficulty—a hydatid cyst occupying apparently the whole substance, and so firmly bound by adhesion to neighbouring structures as to be almost inseparable from them, had, by its bulk and position, so encroached upon the chest as to diminish its healthy capacity, and tilt the heart perceptibly over towards the left, where it was found pulsating during life, and whence it could not be easily removed by any ordinary effort at replacement after death.

Case XXIV.—While recently attached to Her Majesty's 80th Regiment, a poor woman, just convalescing from the effects of parturition, became the subject of dysentery, the irritation of which, coupled with her previous exhaustion, so reduced her powers of life as to leave but faint hopes of ultimate recovery, and prepare me at any time for intelligence of her approaching dissolution. In this condition she came under the temporary charge of another gentleman, and I was not surprised to hear that, whilst attempting to get out of bed, she fell heavily on the floor, was returned quite senseless, and soon after expired. It became my duty to perform the post mortem; and, finding an unusual amount of circumscribed globular bulging and prominence at the epigastrium, with distinct evidence of cardiac malposition as manifested by palpation and subsequent measurement, I was at some loss to account for both, but surmised at least hepatic cyst or abscess. The post mortem removed my doubts by revealing the existence of a communication, through the diaphragm, between the liver and pericardium, whereby the purulent contents of the former found access to those of the latter, and gave rise to the anomalous appearances just referred to, and furnished material for comment in connection with the subject of this paper. Though I have seen myself and heard of several instances of death, more or less sudden, and with or without peritoneal implication supervening on the occurrence of capsular rupture, abscess, or other intu-

mescence of the spleen, I have not been able to connect any alteration in the position of the heart with either of the above conditions; and, in the absence of any further personal knowledge of the subject, I would merely direct attention to the following note from Walshe, in proof of the possibility of such a change:—"I have known the heart permanently raised an intercostal space by the splenic enlargement of leucocythæmia."

Whether hepatic abscess, of itself, can, by its bulk or locality, so derange neighbouring structures as to influence the position of the heart, I am unable to say, neither do I pretend to such familiarity with the other diseases of this erring organ as to venture on a display of my ignorance in presence of Indian readers; and, assuming simply that they are all acquainted with the classical writings of Abercrombie, Saunders, Frerichs, and Budd, I would respectfully recommend a reference to Kuchenmeister and Siebold on the subject of its parasitic liabilities, and though, doubtless, they are few who will, in this climate, consent to wade through or unravel the prolix and elaborate physiology of Bernard, I am satisfied all will profit by a perusal of the comprehensive and masterly deductions of Rokitansky.

NOTE ON THE USE OF BINIODIDE OF MERCURY.

BY N. C. MACNAMARA.

DR. NEVINS, in his translation of the *London Pharmacopœia*, states that the Biniodide of Mercury has lately been expunged from among the official list of medicines, as it has been found almost inert in small doses, and could not, of course, be used in large ones; he does not, however, refer to its peculiar properties when applied externally, and it is on account of the remarkable influence it exercises in causing the absorption of Goitres and such like adventitious growths, that I am desirous of directing the attention of the medical profession to the subject. I was first induced to try the effects of this drug as an external application, from having read an account of it, written by Dr. Mouat, in the *Indian Annals*, in which he describes the result of the late Major Holmes' practice, and I believe it is to this gentleman we are indebted for having discovered the beneficial results which followed its use in cases of Goitre. I am lead to believe, however, that this interesting communication has not attracted that amount of attention which it deserved, and as Dr. Mouat had had no practical experience in the use of the Biniodide, I think it may be useful to add my testimony to the accuracy of Major Holmes' experiments, which must, I think, carry some weight, when I state that no less than twenty-three thousand Goitre cases have been treated at the dispensary under my charge, within the past three years.

In hospital practice, the Biniodide of Mercury will be found to be particularly useful, as it is easily made, is very cheap, and a little of it goes a long way: all important points for consideration by the medical man in charge of a large dispensary.

It may be advisable to give a few directions as to the best mode of preparing this drug, and if the following points are attended to, there can be no difficulty about it. Great care must be taken to weigh out the iodine and mercury accurately, and not to allow the former to escape in the form of fumes, which it is very apt to do, when the iodide of mercury, a comparatively inert compound, will be formed.

The mercury can be purchased in the lazzar, but as it is

often mixed up with dirt and small lumps of common salt, I usually boil it in dilute sulphuric acid, and, after drying, allow it to pass through a funnel-shaped piece of filtering paper with a small hole, pricked in its side, through which the mercury will run, leaving any dirt or such like impurities behind. Take an ounce of this mercury and ten drachms of iodine, triturate them together in a stone mortar for a minute or two, until the fumes of iodine are given off; the moment these appear, pour about a drachm of spirits of wine into the mortar, this will check the fumes, but in a few seconds they will again make their appearance, add a little more spirit, and continue to drop in a few minims more if the iodine fumes are again given off; the contents of the mortar must be rubbed up during this time, and in about five minutes a reddish black powder will have formed; empty this into a bottle, and in the course of twenty-four hours the colour will have changed to the beautiful scarlet of the Biniodide of Mercury. There is no necessity to boil the powders in a solution of muriate of soda; the above manipulation is all that is required, and if it be strictly adhered to cannot fail to answer. The ointment I am in the habit of using, contains fifteen grains of the biniodide to an ounce of simple cerate; if a stronger ointment is used, a great deal of unnecessary pain is caused, and a nasty troublesome sore may be the result, which is very difficult to heal.

The direct rays of the sun seem to assist the action of the drug, and I am in the habit of leaving a thin coating of the ointment spread over the Goitre, and I then make the patient sit down in the sun; if this is not possible, I contrive so that the light may have free access to the part. The ointment must be smeared all over the Goitre, and should not be wiped off. Half an hour after its application, the skin will commence to smart and burn, and in another hour a blister will have formed, which can be treated in the usual way with a little simple ointment.

The effects of the Biniodide when applied over a Goitre continue long after the blister, which it has caused, has healed up; the tumour will continue to decrease day by day for a month or six weeks, and even for the cure of the largest Goitre the ointment should not be used more frequently than once in every two months. I have often seen tumours of this kind, extending from the chin to a line drawn be-

tween the mammae, disappear after two or three applications of this drug. A common blister exercises no such effect; I have tried them on several occasions, but never saw any good result ensue from their use.

Up to within the last three years I was in the habit of employing Ung. Iodine Co. for the cure of Goitre, but I found it took a very considerable time to effect any material change in a large tumour, and this, combined with the expense of the preparation, was so serious a drawback to its use, that I found it impossible to continue prescribing it; and the poorer classes have not the inclination, if they had it in their power, to be constantly going backwards and forwards to the dispensary for a fresh supply of medicine; as it is many of them come from a great distance, but notwithstanding this, the number of patients has rapidly increased under the present plan of treatment, and though this may be thought a doubtful proof of its efficacy, it must be remembered that in some parts of Tirhoot it is almost the exception to meet a man not having a Goitre; and the district contains some three millions of inhabitants.

The Biniodide of Mercury will be found of great use in hastening the cure of "spleen;" it should be applied over the enlarged organ as described for the cure of Goitre. I have often seen the worst and most intractable instances of this disease cured by this means, though it will be well to administer the spleen powders at the same time.

A grave objection has been raised to the use of this drug on account of its causing salivation; if such were the case, there can be no doubt it would act most injuriously on the system, especially if the spleen were affected. I have never, however, seen an instance in which the Biniodide has caused salivation, though my Sub-Assistant Surgeon tells me he has met with a few cases of the kind; there can be no doubt therefore that this mishap may occur, still the cases are so few and far between, that we need never be deterred on this account from using the ointment. I employ it also in glandular enlargements about the neck and groin, and in cases of nodes, and if the ointment is made in the proportion of ten grains to the ounce, it will be found a convenient form of counter irritant in rheumatism and allied affections; when thus used, it can, of course, have no peculiar influence over the disease, but as a vesicant may save the expense of the more costly Iodine Paint and such like preparations.



THE CASE

OF THE

MEDICAL OFFICERS OF THE ARMY

FAIRLY STATED,

IN A LETTER TO THE

RIGHT HON. EARL DE GREY AND RIPON,

SECRETARY OF STATE FOR WAR.

BY

A RETIRED DEPUTY INSPECTOR-GENERAL OF HOSPITALS.

*Inveni portum; spes et fortuna valens,
Nil mihi vobiscum, habito unco alio.*

LONDON:

CHARLES GRIFFIN AND COMPANY,

10 STATIONERS' HALL COURT.

1864.

LETTER.

MY LORD,

The motto prefixed to this Letter is intended to indicate, at the very outset, that I have no personal interest in the question to which I am about respectfully to invite your Lordship's attention. My "haven" is indeed "found." I have nothing to hope or fear from any measures affecting the present state or future prospects of the service that may be determined on. In thus venturing to obtrude my opinions on a statesman charged with the high responsibilities of office I am actuated by no motive of personal ambition. I desire to see an indispensable branch of the public service, at present rapidly becoming disorganized, restored to a state of efficiency. I desire to see the breach that now unhappily exists between the Medical Officers of the army and their combatant brother-officers finally closed. I desire to see such an arrangement made as will once more restore confidence in the good faith of the civil and military authorities of the army in quarters where it has been altogether destroyed—viz, the Schools of Medicine in the three kingdoms—so that the public service may once more offer such reasonable attractions to young men of superior parts and acquirements as to make them seek eagerly for a career in its ranks. Finally, I desire that our soldiers, in peace and war, at home and abroad, may have, what it is the intention of the country they should have, the best possible advice and assistance when disabled by sickness or wounds; and it is because I honestly believe that a dispassionate statement of facts may contribute in some degree to bring about such desirable ends, that I venture to address your Lordship.

I need not take up your Lordship's time in laying evidence before you of the existence of discontent in the Medical Department of the army, because it everywhere abounds. The opinions

of the Professors and Teachers in Medical Schools and Colleges, the debates in the Medical Council, the leading articles and correspondence in the Medical Journals and Reviews, and, above all, the impossibility, in the present state of feeling in the profession, of recruiting the ranks of the service with properly qualified young Medical Officers, have combined, only perhaps too forcibly, to make this discontent known to you.

There is a general rumour that dissatisfaction has arisen in official quarters at the expression that has been given to this discontent in the current professional and other publications by the Medical Officers of the army. Because they have printed their grievances, they are said to be "insubordinate." Because complaints have poured in from many quarters, a "combination" is said to exist among them. It may be quite true—indeed, it is true—that it is contrary to the letter of the Queen's Regulations for officers, soldiers, and non-commissioned officers, on full pay, of Her Majesty's army, to make known their grievances through the press; but no one knows better than your Lordship that practically this is done every day in the year. I do not know how many newspapers and journals there are in this country devoted almost exclusively to the discussion of military and naval matters,—there are at least three or four; but this I know, nearly two-thirds of the space in these papers is taken up by letters from men of all ranks, or comments upon them by the editors: and so far from this practice being confined to "Doctors" and other non-combatants, their complaints make up but a small proportion of the whole. I do not defend—on the contrary, I reprobate—unjustifiable attacks on those who are charged with the administration of military and naval affairs: in a country like this, and with a press such as ours, attacks of an unjustifiable nature would not be tolerated; but nothing short of a press law as stringent as that of France will suffice to prevent Englishmen from making known their grievances, and temperately seeking redress in the way most in accordance with the habits and traditions of a free country. As for Medical Officers, take away this resource, and they must be helpless indeed. Not a single concession has ever been made to them in the army or navy except under the pressure of enlightened public opinion, called forth by temperate discussion in the press. Let combatant officers be touched, or even threatened, on any point affecting their

interests, privileges, or even their prejudices, and a dozen powerful voices are raised in their defence in both Houses of Parliament. Your Lordship's parliamentary experience will remind you how seldom the authorities are troubled in this manner when only the grievances of "Doctors" are in question. As to "combination," in the offensive sense, attributed to the Medical Officers of the army, nothing, I feel assured, having even the semblance of this exists, or ever has existed among them. The nature of the service makes combination all but impossible. Medical Officers are scattered, broken up, and dispersed in small numbers all over the world. The effect of the regimental system has ever been to centre the *esprit de corps* of the army surgeon more in his regiment than in his department. The discontent which has at last found vent in the professional press is a proof, therefore, not of combination, but of dissatisfaction widely spread and deeply felt.

In a journal which (rightly or wrongly, I cannot say) is regarded by the public as the semi-official organ of the present Government, it has been said that the discontent of Military Surgeons dates only from the recall of certain privileges which were conceded to them "by mistake," and that this discontent has its root in a desire for military command and a puerile craving for such military baubles as "spurs," "brass scabbards," and such like trivialities, which ought to be beneath the notice of professional men.

It is not difficult to give a satisfactory reply to these so-called explanations. And, first, to show that the discontent of Medical Officers with their position was antecedent to the publication of the Warrant of 1858, and, indeed, led to the granting of that now famous document, it is only necessary to glance at the then condition of the department. For this purpose I ask your Lordship to take up the Minute by the Most Noble the Governor-General of India on the Indian Medical Service—a Minute the writing of which was almost the last official act of the late Lord Dalhousie, and which was concurred in by His Excellency the Honourable G. Anson, Commander-in-Chief in India. I take this Minute, which refers more particularly to the Indian Medical Service, because that Service was, in almost all particulars, better off than the Medical Department of Her Majesty's Service. Indian Medical Officers were better paid; and they had numerous staff

appointments open to them, the emoluments of which were considerable. The climate of India made their services so essential to all classes that men, whatever their rank and station might be, were glad to be on good terms with men to whom they, and those most dear to them, could alone look for assistance in the hour of utmost need. The feudal prejudice against those who profess the healing art did not press on Indian Medical Officers with the galling force so much complained of in the Royal service, for the simple reason that all the branches of the Indian service were, as a rule, drawn from the upper ranks of the middle classes; and civil and military Medical Officers being for the most part the sons, brothers, or near relations of members of other branches of the public service, held a social position in India very much higher than that accorded to their brethren of the home service in England. Add to the above, the various "Funds" established in the three presidencies, which secured a comfortable provision for their widows and children in the event of death, and a considerable addition to their pensions in the way of a life annuity on retirement. Such being the conditions of service, it cannot be wondered at that, in the words of Lord Dalhousie, the Indian medical service never wanted "many men of the highest capacity and value, who elsewhere would have won an European reputation." So much was admission into this service at one time sought, that Lord Dalhousie mentions in the same minute that, "twenty years ago, I (Lord Dalhousie) asked for a medical appointment from one of the directors. I was told that "it was as difficult to obtain an appointment in the medical service as in the civil service; and in point of fact it was only "obtained for me on that occasion by an exchange." Lord Dalhousie then goes on to state the grievances under which the medical service suffered—grievances which he deemed real, and which, in his opinion, "ought to be removed." "I refer," wrote his Lordship, "to the inequality which now prevails between the position of a medical officer and that of his brother-officers, in respect of pension, honours, and rank.

"I respectfully submit that such inequalities are founded on no sound grounds of justice, expediency, or policy: no valid reason ever has been or can be alleged for maintaining them. Their effect is to depress the spirit of the medical officer, to depreciate a profession and class of service which ought to be held in the

"utmost respect, and supported equally from motives of prudence and gratitude.

"All such inequalities should be at once removed, and the medical officer, in respect of real rank, dress, honours, and promotion, should be placed on a footing with his brother-officers who hold commissions like himself." And a little farther on his Lordship, the Governor-General of India, urges the Court of Directors to represent "strongly and perseveringly to Her Majesty's Ministers the claims of their medical officers to share reasonably, and in a far larger proportion than they do at present, in the military honours and decorations which are granted for service in the field."

On the question of rank Lord Dalhousie wrote—"But the most galling, the most unmeaning and purposeless regulation, by which a sense of inferiority is imposed upon medical officers, is by the refusal to them of substantive rank. The surgeon and assistant-surgeon rank invariably with the captain and lieutenant; but the rank is only nominal wherever medical officers and others are brought together on public duty. The former has no rank at all, and the oldest surgeon on the list must, in such case, range himself below the youngest ensign last posted to a regiment.

"It is impossible to conceive how such a system as this can have been maintained so long, on the strength of no better argument than that 'it has been,' therefore 'it ought to be.' It is impossible to imagine what serious justification can be offered for a system which, in respect of external position, postpones service to inexperience, cunning to ignorance, age to youth—a system which gives a subaltern who is hardly free from his drill, precedence over his elder, who perhaps has served through every campaign for thirty years—a system which treats a member of a learned profession, a man of ability, skill, and experience, as inferior to a cornet of cavalry—a system, in fine, which thrusts down gray-headed veterans below beardless boys."

I have taken this graphic description of medical grievances as they were in India when Lord Dalhousie wrote this Minute, because it exactly describes the condition of the Royal Medical Service prior to the issue of the Warrant of 1858; and because this very Minute formed part of the evidence laid before the

Royal Commission on the sanitary state of the army: and if this be not enough to satisfy the writer in the semi-official newspaper, I would refer him to that part of the Report of the Royal Commission in which they embodied the result of their inquiries into existing grievances. The result was the Warrant of October, 1858. So far from that document, or any part of it, having conveyed privileges to Medical Officers "by mistake," as alleged, the Royal Commission grappled with the whole question, laid down the principles on which the Warrant was based,—and none knows better than your Lordship that every clause of the Warrant was made the subject of anxious deliberation by men the most competent this kingdom could furnish to advise Her Majesty on the subject. How far in its letter and spirit this famous Warrant was obeyed is another question, on which I shall have something to say presently.

The next assertion put forward by the semi-official journal is that Medical Officers desire, under cover of the rank given by the Warrant of October, 1858, military command. Now, my Lord, this has been repeated so much and so often that perhaps those who first put it forward to prejudice the public against army surgeons have at last persuaded themselves of its truth; and it is consistent with my knowledge that, until better informed, your Lordship's predecessor in office, the lamented Sir George Cornwall Lewis, believed that Medical Officers did really aspire to military command. I have heard that out of somewhere about two thousand Medical Officers of the General and Indian staff, one or two may have been silly enough to put forward certain pretensions of this kind; but it is equally well known that by no class of officers were such pretensions more bitterly ridiculed than by the whole body of the department. The Queen's Regulations—nay, the Warrant itself is so precise on this point, that none but a very silly man, probably under some momentary irritation, could think of putting forward any such claim. What Medical Officers do claim is the observance of Clause 17 of Her Majesty's Warrant of October, 1858—a clause which has practically, in most of its provisions, been set aside or explained away.

What has perhaps given rise to this, I would charitably hope, misapprehension about command, is the dissatisfaction that has arisen of late, since the publication of the Warrant, from the practice of designating Medical Officers of the army as "Civil

"Officers." Now the absurdity of this must be evident to all who take the trouble to reflect on the matter for a moment. Military surgeons are non-combatant officers,—it is not their business or duty to fight, or, more properly speaking, to direct those who are fighting: but for all that they are not civilians; they wear military uniform all the time that they are on full pay; they are subject to the Mutiny Act; they can sit as members of courts-martial, instances of which have occurred in my own time; and, to quote once more from Lord Dalhousie's Minute,—"The medical officer comes constantly under fire like other men. Every campaign which is fought exhibits the names of Medical Officers in the lists of killed and wounded; and the returns invariably show that they still more often fall victims to their own exertions on behalf of their suffering comrades.

"Proof can hardly be required of such well-known facts. If it be, the fatal record of the service which our countrymen have been performing during the last year and a half in the Crimea will more than bear out the statement I have made."

One medical officer from each regiment mounted the heights of the Alma with the attacking columns; an assistant-surgeon saved the life of the present Commander-in-Chief in the midst of the fight at Inkerman; and the surgeons of the army took their tour of duty in the trenches before Sevastopol as regularly as their combatant brother-officers. The late Inspector-General of Hospitals, Dr. Macleod, when attending to the wounded in the trenches before Badajoz, was for the moment blinded by his brother's brains, a captain in the regiment in which he served. It is well known all over India that at the battle of Corriegaum, one of the most brilliant feats of arms in the annals of British India, nearly all the artillery officers were killed, and at the most critical period of the action Assistant-surgeon Wylie directed the fire of the guns against the enemy in such a manner as materially to contribute to the success of the day. For this he was in after years rewarded with the Companionship of the Bath, and to the day of his death was known in the Madras army, to which he belonged, as the "Hero of Corriegaum." Surgeon A. Home rescued the wounded under his charge at Lucknow from falling into the hands of the enemy, barricaded the houses into which he had them conveyed, defended the post for two days, and with his own hand killed many of the assailants. For this gallant act he was

rewarded with the Victoria Cross. In the late operations on the frontier in India, Assistant-surgeon Pile, on duty with a picket in an exposed position, when the combatant officer in command sought safety by deserting his post, remained with a gallant young ensign, and fell nobly doing his duty to the wounded around him. In a late despatch from New Zealand, General Cameron mentions the gallant conduct of Assistant-surgeon Temple in the discharge of his duty, in such terms that it is well known this medical officer has been set down for the Victoria Cross. The number of Medical Officers of both services who lost their lives during the Mutiny in Bengal, and the operations for its suppression, was greater in proportion than among any other class of officers in the service.

My own experience in war, as compared with that of some of my old brother-officers of my own standing, has not been great; yet I have often been out at night on picket duty immediately in front of the enemy; I have landed more than once with troops under fire; I have served with the advanced guard under fire; the commanding officer of my regiment was killed within a few feet of me; the captain of the light company was slain while in the act of conversing with me; and a few minutes after, while attending to a wounded officer, a shot struck the parapet so closely above my head as to cover us both with the debris of the crushed bricks and mortar. The *proportion* of Medical Officers at this day wearing the Victoria Cross is greater than among any other equal body of officers in Her Majesty's service.

I could fill pages with similar examples, but enough, it is hoped, has been said to explain why Medical Officers object to be classed as civilian hangers-on to the rear of an army, and to be treated as a superior class of "camp followers." I have given these examples, not to show that army surgeons have anything to do with fighting, but in proof of the fact that they cannot discharge their own peculiar duties without sharing largely in the honourable dangers of the field. Marshal Radetzky—no mean authority on such a point—declared, when commander-in-chief in Italy, "that the difference between officers as combatants and surgeons as non-combatants must cease. I see everywhere military officers and surgeons equally exposed to the fire; and therefore the surgeons shall enjoy advantages and distinctions in every respect equal to those of the combatant officers." Paymasters, chaplains, commis-

sariat officers, except under the rarest possible circumstances, are not exposed to the risks of war,—their duties are in the strictest sense civil; and if it be the wish of the authorities that military surgeons are to be treated as civilians, they (the Medical Officers) have a right to expect that their duties and risks shall be regulated accordingly.

As regards the matter of dress, about which there have been so many sneers, it need only be said that dress in the army is the outward and visible sign of rank, and so long as it is the pleasure of the authorities that army surgeons shall wear military uniform, they have a right to expect, not only that they shall not be made to wear a dress which shall give rise to invidious distinctions in the eyes of their comrades, but that their uniform, while it sufficiently marks the class to which they belong, shall at the same time both indicate unequivocally their relative rank, and be suitable to the professional duties which they have to perform.

I need not detain your Lordship by going into the terms of the Warrant of October, 1858. I have already said that document was not issued, as some pretend, in haste and without due consideration. On the contrary, every part of it had been carefully considered; it was a large and liberal measure of justice graciously conceded to those who had laboured well and waited long. The effect on the department was magical. Discontent ceased, men thankfully accepted the status in the army they felt to be their due, and turned from the hateful necessity of urging their grievances on the unwilling ears of those in power to the more congenial duties of their profession.

In the civil ranks of the profession the Warrant gave equal satisfaction; and as its healing words passed from mouth to mouth in the Schools of Medicine, the rising generation of well-educated students saw at last an honourable career open to them in the army. Candidates with high qualifications came forward in sufficient numbers to compete for commissions; professors and teachers in the Schools of Medicine encouraged their best students to prepare for the examination; and when the late lamented Lord Herbert opened the first session of the Army Medical School, he saw on the benches before him the first-fruits of a wise and generous policy.

I have now arrived at the most painful and distasteful part of

my subject. I have now to relate how this gleam of sunshine proved to be only

"The uncertain glory of an April day."

To the astonishment of the Medical Officers of the army, they soon found that the Warrant was a source of extreme dissatisfaction to the military authorities, and, I am sorry to add, to many of their combatant brother-officers also. I say they learned this with surprise, because they naturally expected that, as no class of public servants were so dependent on the skill and professional ability of army surgeons as combatant officers, it might naturally have been expected that they would rejoice over concessions certain to attract into the service a superior and more highly educated class of men than had, as a general rule, been found in it before. Medical Officers expected this all the more because the Warrant, while it improved their own position, deprived combatant officers of no advantage or privilege enjoyed before.

The Queen's Warrant of October, 1858, bearing the signature of General Peel, was hardly promulgated ere that long series of attacks commenced, some open, some covert, which never ceased until the very name of it became offensive to the ears of those for whose benefit it was intended.

There are somewhere about 75,000 British troops serving in India, with a medical staff proportioned to such a large number serving in an unhealthy climate; and yet, from the time it was published until now, Medical Officers serving there, whether belonging to the Home or Indian services, remain, in point of pay and substantial privileges, exactly as they did before the Warrant was published.* The assistant-surgeon of both services, of six years' standing, is treated as if he ranked with a lieutenant, as when Lord Dalhousie's Minute was written; and a surgeon-major of the Indian army, with the nominal rank of lieutenant-colonel, when he comes to England receives the pay of a captain. Memorials, remonstrances, petitions, questions in the House of

* While these sheets are passing through the press, a document purporting to be the India Office scheme for the re-organization of the Indian Medical Service is going the round of the press. Whether this is published by authority or not, I have no means of judging, but as it refers mainly to matters of pay, and is silent as to the constitution of the new Indian Medical Staff Corps, it cannot be said to describe the re-organization of the service. Under any circumstances the text correctly describes the state of matters in India up to the present time.

Commons, have alike failed to obtain redress. The Warrant of 1858 remains in 1864 a dead letter in a part of the world where nearly one-half of the British army is constantly serving.

Need I weary your Lordship with the pitiful details of breaches of faith nearer home? Need I relate the thrice-told tale, and tell how all that was valuable in Clause 17 of the Warrant was either shamelessly abrogated or disingenuously explained away, either by circulars issued in the name of the highest military authority, or by orders in answer to references by generals commanding in all parts of the world. I regret to say that the civil administration of the army was not a whit behind the military in putting unfavourable interpretations and strained meanings on very plain passages. I have no patience to dwell on the small slights and petty affronts to which military surgeons were, as it would appear, in the mere wantonness of military authority, subjected;—how surgeons coming mounted to parade, as they were entitled according to their rank to do, if that rank was anything but a delusion and a snare, were ordered, in the face of whole brigades, to dismount;—how the right of surgeons to contribute to mess and band funds as field officers was graciously conceded, while their right to the place due to the same rank on social occasions of ceremony was by the decision of a general officer politely denied.

When the clamour raised by certain combatant officers was at its height, in an evil hour the then Secretary for War, by a new Warrant, deprived regimental and staff surgeons of the rank conferred on them by the Warrant of October, 1858—that is to say, making them, while still retaining the rank of major, to remain permanently *junior* to all majors, regimental and brevet.

Your Lordship's predecessor in office was pleased to recall this second Warrant, from seeing that its issue was a fatal mistake; but, as your Lordship knows, the effect of the concession has not been sufficient to restore confidence to the department, or to the profession from which alone its ranks can be recruited.

The steps taken by the War Office, the India Office, and the Horse Guards have thus undone the work of Her Majesty's Sanitary Commissioners, broken faith with those who entered the service under the Warrant of 1858, disorganized the department, and sowed such discontent and distrust in the schools and colleges, that a commission in the medical branch of the British army is at this moment at such

a discount in the intellectual labour market, that 200 vacancies exist in the general and Indian services together as these sheets are passing through the press. The scarcity of Medical Officers in India is such that the service, even in this time of profound peace, can hardly be carried on. And what, my Lord, suffer me respectfully to ask, are the measures taken to restore confidence? The profession knows of but one. An advertisement signed by the Director-General has appeared in the newspapers, inviting medical men to come forward *without examination*, to act as army assistant-surgeons for home service on ten shillings a day. In the face of all that has occurred, qualified candidates, willing to enter the service through the gate of the examination at Chelsea and the course of instruction at the Military Medical School, are not to be had, and an attempt is being made to gather from the highways and byways of the profession a sufficient number of men to whom ten shillings a day is an object, to carry on the service.

Gentlemen are invited to enter the service by one door,—let me for distinction's sake call it the front door. The conditions are a strict examination, followed by a course of hard study and strict discipline at the Military Medical School of Netley; the reward, when all this is done, being ten shillings a day, *unlimited foreign* service in every climate, with the good faith and treatment I have ventured to describe; while the same pay, with *home* service, are to be had without examination or disagreeable questions asked,—entry by the back door in Whitehall Yard.

Is it expected, my Lord, that such a measure as this will allay discontent and restore confidence?

But what of the Queen's soldiers? How are sick officers and men to fare at the hands of these "Acting Assistant-Surgeons?"

About officers I have not much to say: at home, at all events, they can provide for themselves. But as regards the men the case is different.

Perhaps your Lordship has not had time to read the debates in the Medical Council on the subject of medical education. In the course of the debate some strictures were made on the examination for admission into the Medical Department of the army. Some of the speakers were of opinion that such an examination was not necessary, seeing that all who were received as candidates at the Chelsea examination had already been examined by one or more of the licensing bodies. This brought up Dr.

Parkes, a member of the council, and well-known to your Lordship as Professor of Hygiene in the Military Medical School, and one of the most experienced medical examiners in London. The disclosures made in the course of his speech were startling. Dr. Parkes had in his hand a series of the written examination papers of rejected candidates for medical commissions in the army, from which the learned Professor, without, of course, mentioning names, or the schools whence the men came, quoted largely, and conclusively established the fact that unless an entrance examination was exacted before admission to the army, the most frightful consequences would ensue. Men offered themselves for examination so ignorant of anatomy and surgery that, if they had performed the operations they were called on to do on the dead body, before their examiners, in the same way on the living body, the instant death of their patients must have been the necessary result. Others did not know the commonest doses of the commonest medicines, nay even the drugs themselves when presented to them. In answer to the question, What becomes of men of this kind in civil life? Dr. Parkes replied by quoting a passage from an Address of one of his colleagues, Dr. Maclean. I give the passage below.*

* "If we are correctly informed, objection has been taken to the principle of competition in relation to medical commissions in the army; not only is it by some desired that we should return to the old system of nomination, but it is proclaimed that an examination test of any kind prior to admission is an injustice to candidates or nominees, a work of supererogation, and an insult to the licensing bodies and universities of the kingdom. It is argued that as nothing but a degree or a diploma is required of a civil practitioner, nothing more should be demanded of those who are to follow the same profession in the army; that the life of a soldier is not more valuable than that of a civilian. It is curious that those who so argue do not see that the two cases, put in this way, will not bear comparison. Is it true that civil practitioners enter at once into the confidence of the public, and the rewards resulting therefrom? Is it not rather the case that there is for them a trial, a competitive examination, if you so choose to call it, so stringent, so chilling, so long-continued, that in comparison with it that which stands at the threshold of the public service, and bars the way to incompetence, sinks into insignificance. Into the cold and rapid river of public life those who seek public confidence must adventurously plunge; in that swift stream the strong swimmers only live; the idle, the dissolute, the incompetent, sink in its waters, or are swept away, and heard of no more. To drop metaphors—the public can protect themselves. With the soldier it is different: he has no choice, no freedom of selection, and the State must protect him. If the authorities could so far forget their duty to the sick or wounded soldier as to throw open the public service without a preliminary test, the Medical Department would soon become the refuge of the intellectually destitute, and the

Now, my Lord, these are the men who may, and who probably will, enter the service by the backdoor now opened for them. There is no reason why the very men who exhibited such frightful ignorance as Dr. Parkes disclosed should not enter the service and maltreat the Queen's soldiers.

And now, my Lord, what, it may be asked, are the remedies for these evils?

They are few and simple. If your Lordship has followed me so far, you cannot but see how little mere money has entered as an element into these painful discussions. The whole profession, in the service and out of it, including the most eminent Professors and Teachers in the Schools, Universities, and Colleges of the kingdom, declare that, constituted as the Army Medical Service is, it is not a service in which honourable and highly educated men can serve without losing what such men will not sacrifice for any consideration, pecuniary or other—their self-respect.

Let, then, the Warrant of October, 1858, and Clause 17 in particular, be restored in all its integrity; let the Government give a simple assurance that in future it will repress firmly all attempts, covert or open, to explain away by circulars or alterations in the Queen's Regulations its plain, honest, and obvious

hope of the professional lounge, to whom the struggles of private practice offered nothing but starvation. In a brief time a department so constituted would become a national reproach; public indignation would be kindled against it and consume it away.

"Not only has objection been taken to an examination test, but the one now in existence has been objected to on the score, not only of severity, but of partiality. It has been said that the Examining Board display hostility to the graduates of a particular University. A more reckless and unfounded charge was never brought against honourable men. Far be it from me to offend these distinguished men by defending them from a charge that refutes itself, least of all is any defence necessary here, in the presence of gentlemen who know from personal experience how unfounded it is.

"The simple truth is, that at a time when the service is, for reasons into which I need not enter here, under what I hope and believe will prove only a temporary unpopularity, certain men, with slender qualifications, attempted to run this wholesome blockade, and as the examiners had the firmness not to lower their standard to meet a temporary difficulty, these contrabanders of medicine were unsuccessful, and like others of whom we have read and heard, they forthwith raised a clamour at the partiality of their judges. I trust the Examining Board will excuse me for noticing a charge that might safely have been left to fall to the ground by its own weight. I would not have done so, but for the reason I have given." (*Address by Professor Maclean at the opening of the Military Medical School at Netley, on the 1st October, 1863.*)

meaning and intention. If any Medical Officer, presuming on this clause, should be silly enough to put forward any the smallest claim to military command, your Lordship may rely with confidence on the Queen's Regulations and the Mutiny Act in the hands of military commanders to repress such pretensions should they arise, without having recourse to exceptional regulations injurious and affronting to the whole department.

Let such pressure be put on the India Office by Her Majesty's Government as shall insure the extension of the Warrant in its integrity to India.

Make retirement optional on a fair pension after twenty years. The nature of the military medical service is so trying to health that many after that time, for the most part spent in unhealthy climates, are incapable, without much risk and suffering, of further duty.

Make retirement at twenty-five years also a matter of right, at the full rate of pension for this period of service.

Let a few simple alterations in the dress of Medical Officers be made, so that, while there will be no difficulty in recognizing them as members of the Medical Staff, what is offensive in the details of the present dress may be done away with.

Let the pay of Medical Officers, beginning say at *12s. 6d. per diem*, increase in some regular progression at stated periods until the rank of surgeon-major is attained, with the present pay of *25s.* a day.

Finally, let assistant-surgeons be promoted at the end of ten years' full pay service.

These measures will keep hope and energy alive in the service, banish discontent, restore confidence in the Schools, and promote the highest interests of the public service.

I have the honour to be,

My LORD,

Your Lordship's most obedient humble Servant,

A RETIRED DEPUTY INSPECTOR-GENERAL OF HOSPITALS.

Dr Inspector General Longmore
with the Author's best wishes

THE

CONCLUSIONS

ARRIVED AT BY

THE MEDICAL COMMITTEE,

APPOINTED BY THE ADMIRALTY AND THE WAR OFFICE,

TO INQUIRE

IF THERE IS A SYPHILITIC VIRUS,

ARE

NOT BASED ON PATHOLOGICAL FACTS.

BY

DAVID MACLOUGHLIN, M.D.,

MEMBER OF THE LEGION OF HONOUR.

LONDON:

CHURCHILL AND SONS, NEW BURLINGTON STREET.

1866.

To Mr Inspector General Langmore

London, 12th September, 1866.
36, Bruton Street, Berkeley Square, W.

SIR,

Inclosed is the copy of my protest against the conclusions to which the Medical Committee appointed, two years ago, to inquire if there is such a thing as a syphilitic virus have arrived at; which I take the liberty to forward, in the hope that it may be brought under the notice of the Lords of the Admiralty.

As it will be seen that this Committee refused to examine me, scientifically, to ascertain the grounds on which I asserted that there is no such thing as a syphilitic virus; and that no medical practitioner, in this country or in France, can point out one symptom pathognomonic of a syphilitic virus; as they suppressed important parts of my testimony to suit their preconceived opinions; as they did not examine any witness they called before them, to ascertain if there is a syphilitic virus—I submit, that either this Committee did not know their profession, or that they were disingenuous.

Whatever may have been the cause, the conclusions to which they have arrived are not based on pathological facts. This report is an imposition on the Admiralty, the War Office, and the Public.

Therefore, in the interest of the navy, in the interest of the army, and in the interest of the public, as the Admiralty and the War Office accepted my suggestions, two years ago, to have this so-called syphilitic disease scientifically studied, and as their confidence has been abused, permit me to hope that another and a more carefully selected Medical Committee be appointed to carry out this inquiry.

I have the honour to be, Sir,

Your obedient Servant,

DAVID MACLOUGHLIN, M.D.,
Member of the Legion of Honour.

The Secretary to the Admiralty.

P.S.—That the Committee may have an opportunity to controvert anything I have said in the enclosed pamphlet and in this letter, I have sent the President and each of the Members a copy of the enclosed pamphlet and a copy of this letter.

A copy of this letter has been sent to the War Office.

PREFACE.

It is now above eighteen months since the Admiralty and the War Office, on my suggestion, appointed a Medical Committee to study and to report if a syphilitic virus exists; and if it does exist, what are its pathognomonic symptoms on the reproductive organs, or on any other parts of the body; what is its method of cure; and what prophylactic means are to be employed against this virus.

This Committee have published their report, and they have concluded "*that there is a syphilitic virus.*" But they have given no proofs—as it was their duty to do—either that they themselves had gone to the bedside, and there scientifically interrogated nature; or that by the scientific pathological researches of others, whom they called before them, they had discovered the existence of a syphilitic virus.

Still worse, they have gone so far as actually to mutilate and to suppress the testimony of witnesses opposed to their preconceived views. Consequently this report is an imposition practised on the Admiralty, the War Office, and on the public.

It is, moreover, a libel on the professional acquirements possessed by the Medical profession of this country.

May 24, 1866.

TO THE ADMIRALTY AND TO THE WAR OFFICE.

MY LORDS,

As the Admiralty and the War Office were pleased, on my suggestion, to appoint a Medical Committee to study scientifically, and to report if there is such a thing as a syphilitic virus; and if so, what are its pathognomonic symptoms; and as this Committee have now published their report, I owe it to myself to place before your lordships the proofs that I have not misled you—that this Committee was not competent to perform the duty they had undertaken to perform; and that they have imposed on your Lordships a so-called Medical Report. Therefore I will place before your Lordships the proofs.

1. That no Medical practitioner in this country or in France can point out one symptom pathognomonic of a syphilitic virus.

2. That this Committee did not know how to proceed to inquire, scientifically, into the pathology, the etiology, and the Medical treatment of this so-called syphilitic disease.

3. That this Committee have not attempted to ascertain, as it was their duty to do, if there are any symptoms on the genitals, or on any other part of the body, pathognomonic of a syphilitic virus.

4. That this Committee have not attempted to ascertain, as it was their duty to do, if the witnesses whom they called before them had personally made any researches as to what are the pathognomonic symptoms of this so-called syphilitic disease, or if they spoke according to the opinions of others.

5. That this Committee have endorsed as facts, the errors of their predecessors, and the dreams of the empirics and the impostors of the sixteenth and seventeenth centuries.

6. And that this Committee have acted throughout this inquiry disingenuously.

To the facts:—

1. That no Medical practitioner in this country or in France can point out one symptom pathognomonic of a syphilitic virus.

Of the syphilidographers of the past or the present time there are only two, whose opinions are considered as authorities by the whole profession on the existence of a syphilitic virus, viz., the late Mr. John Hunter and the present Dr. Ricord of Paris.

Mr. Hunter tells us "that a chancre, like other inflammations which terminate in ulcer, begins first with an itching on the parts. If it is the glans-penis that is inflamed, generally a small pimple, painful, full of matter, without much hardness at the base, or seeming inflammation; and with very little tumefaction, the glans not being so readily tumefied from inflammation as many parts of the body are, especially the prepuce; nor are the chancres attended with so much pain and inconvenience as those on the prepuce. But if upon the frenum, and more especially the prepuce, an inflammation more considerable than the former follows—or at least the effects of inflammation are more extensive and visible, these parts being composed of very loose cellular membrane, afford a readier passage for the extravasated fluid, continued sympathy, also, more readily takes place in them. The itching is gradually changed to pain; the surface of the prepuce, in some cases, is excoriated, and afterwards ulcerated. In others a small pimple or abscess appears, as in the glans-penis, which forms an ulcer. A thickening of the part comes on, which, at first, and while of the true venereal kind, is very circumscribed, not diffusing itself gradually and imperceptibly into the surrounding parts, but terminating rather abruptly; "its base is hard and its edges are a little prominent."

Therefore the hardness at the base of an ulcer on the prepuce is, according to Mr. Hunter, the true pathognomonic symptom of the existence of a syphilic virus.

Let us now pass to Dr. Ricord, and let us see what he considers the pathognomonic symptom of this pretended syphilitic virus.

Four and twenty years ago, at a public consultation on this

question, "Is there a syphilitic virus, and if so, what are its pathognomonic symptoms?" the writer of this letter stood single-handed before Dr. Ricord and some dozens of his friends and admirers, and brought the doctor to admit that neither by the use of his eyes, nor by the use of his fingers, could he point out the existence of a syphilitic virus on the genitals, or on any other part of the body. But Dr. Ricord maintained that he could prove the fact by inoculation. To this he still adheres.

In his eighth letter on syphilis, lately published, he says:—"Le diagnostic absolu-univoque, irréfragable, ne peut être obtenu que par l'inoculation artificielle." Dr. Ricord and Mr. Hunter are here completely in antagonism.

According to Dr. Ricord, the hardness at the base of an ulcer on the prepuce is not a pathognomonic symptom proving the existence of a syphilitic virus.

But what right has Dr. Ricord to conclude that inoculation proves the existence of a syphilitic virus? He tells us that all ulcers on the genitals or on any other parts of the body which secrete inoculable pus are syphilitic, and this is his proof of the existence of a syphilitic virus.

Dr. Ricord is not aware that the ulcers of herpes prepuccialis, a disease so named when it occurs in men, and herpes pudendalis when it occurs in women, and both arising spontaneously; that is, without "coitus", when in a state of active inflammation, secrete an inoculable pus. (See Dr. Eeans, on *Ulcerations of the Genitals*, page 20.) And further, Dr. Ricord is not aware that this inoculability of pus depends on the state of active inflammation, in which the ulcer is at the time, and not on a specific virus.

Thus Mr. Henry Lee, Surgeon to St. George's Hospital, attempted to inoculate with the pus, taken from a hard based ulcer on the prepuce, in a state of chronic inflammation, but failed. He then irritated this ulcer, and when it was brought into a state of active inflammation, the pus then became inoculable. (See Mr. Henry Lee's report of his experiment published in the *Lancet*, 31st March, 1866.) (See also Professor Wilhelm Boeck, of Christiania, statement to the same effect as Mr. Lee's experiment, reported in page 348 of the Medical Committee's Report.)

Therefore Dr. Ricord's inoculation, as a proof of the existence of a syphilitic virus, is a myth, placed in the room of Mr. John Hunter's myth; for no medical practitioner, in this country or in France, can point out one symptom pathognomonic of a syphilitic virus.

2. That this Committee did not know how to proceed and inquire scientifically into the pathology, the etiology, and the medical treatment of this so-called syphilitic disease, I now proceed to show.

Everyone knows that when a scientific medical practitioner is called to the bedside of his patient, his first duty is to inquire what were the first symptoms which marked the first deviation from perfect health—then to trace their effects on the body or mind, or on both—then to endeavour to ascertain the cause or causes of these symptoms, and then to prescribe.

When the impostor, on the contrary, is called to the bedside, he neither inquires as to the symptoms, nor the cause or causes of these symptoms. Simply, he assumes that his patient is labouring under such or such a disease, and he prescribes his nostrum.

This Medical Committee began their inquiry by assuming that there is a syphilitic virus—they assumed also that this virus resides in the hard base of an ulcer on the prepuce—and they further assumed that this syphilitic virus can be absorbed into the body of the individual; there remain dormant for years; and then manifest its presence on his offspring by symptoms which they call secondary symptoms of syphilis!!!

The writer of this letter was the first witness this Committee called before them. Having denied the existence of a syphilitic virus, and knowing that this Committee knew that they were assembled at his suggestion, to study and to endeavour to ascertain if there is a syphilitic virus, he naturally expected that this Committee—who were said to be a most scientific Medical Committee—would, as scientific medical practitioners, begin this inquiry by ascertaining the symptoms which marked the first deviation from perfect health in this so-called syphilitic disease. But they did no such thing.

On the contrary, the first question they asked him was relative to the so-called secondary symptoms of this so-called

syphilitic disease. Thus beginning their examination at the "Omega," instead of the "Alpha," of the question before them. (See Dr. Macloughlin's examination before this Committee, page 1 of this Report.)

The first question they put to their second witness they called before them was this:—"Do you concur in Dr. Macloughlin's views on the subject of syphilis?" His answer was:—"Certainly not, nor have I any confidence in that statement."

Since Dr. Macloughlin's name and opinions were thought worthy to be mentioned, this Committee ought to have known that it was their duty to ask this witness on what pathological facts he based his opinion that there is a syphilitic virus, and how he arrived at the conclusion that he ought not to have any confidence in Dr. Macloughlin's statement.

The Committee pursued the same line of examination with the third witness. They did not ask him a question about the first symptoms of a so-called primary syphilitic ulcer on the genitals, or on any other part of the body. They assumed that this third witness, like the witness who preceded him, could at once point out the diagnosis between a so-called syphilitic and a non-syphilitic ulcer on the genitals, or on any other part of the body. Their first question to him was: "Have you seen the pamphlet of Dr. Macloughlin on the subject of syphilis?" "I have," was the answer; "Dr. Macloughlin sent me his pamphlets successively."

"You are familiar with his opinions on syphilis, and I may ask you, do you approve them and adopt them?" "Certainly not", was the answer. Why or wherefore he disapproved of Dr. Macloughlin's opinions, they did not think it necessary to ask.

Their next question was this: "Do you approve the division of sores into infecting and non-infecting?" "Yes; I approve of the division of sores into infecting and non-infecting," was the answer. Did they ask him why? They did not.

Now, as neither this Committee nor this witness had given any pathological proofs that there exists a syphilitic virus, what right had this Committee or this witness to assume that there is a syphilitic virus, and that one kind of ulcer on the genitals is infecting, and another kind is non-infecting?

"Do you limit the infecting sores to such as are accompanied by thickening or induration?" was the next question. "As a rule I do," was the answer.

With every respect for this witness, I submit that, as he had neither proved by his pathological researches that there is such a thing as a syphilitic virus, nor that a syphilitic virus resides in the thickened or indurated base of an ulcer on the prepuce. The answer he gave was the mere echo of the preconceived opinion of his interrogators.*

Is it necessary to demonstrate further that this Committee had accepted a duty they did not know how to perform; that, being unacquainted with the law of evidence, they did not know how to examine the witnesses they called before them. Their so-called Medical Report is before the medical world; and every one who knows his profession, and who has a right to have a medical opinion, can judge the value of this Report.

3. That this Committee have not attempted to ascertain, as they were bound to do, if there are any symptoms on the

* I beg to call attention to Dr. Balfour's questions, from 94 to 105. He took exceptions to what I had stated in my pamphlet, relative to the abuse of mercury for the cure of syphilis in the army. A great part of what was said by Dr. Balfour and myself is suppressed; and thus, at 104, he promised to show me the proofs that the administration of mercury for the cure of syphilis was, as a rule, given up in the army; and it was agreed that I should call on him the next day at his office, to see his proofs. I did call. He refused to see me; and I was requested to write what I had to say to him. I insisted on a personal interview; that he was to name the day and the hour. He named three days after, at half-past 3 P.M. I recalled to him that I was come, according to agreement with him before the Committee, to see his proofs that mercury for the cure of syphilis, as a rule, was given up in the army. His reply was that, as I was in no official employ, he must decline to give me the proof; that he would do so to the Committee. And, at No. 105, I am made to answer, "My retraction is sufficient"; whereas my answer ought to have been reported thus: "My retraction *will be* sufficient." And I leave the reader to decide if Dr. Balfour has redeemed his promise to bring before the Committee the proofs that, as a rule, the use of mercury is given up in the army.

But the question at issue, it must not be forgotten, was, "*Is there a syphilitic virus?*" and not "What is the treatment of syphilis?" And Dr. Balfour had no more proofs than the other members of that Committee, that such a virus exists. He, like his associates, wanders from the question before him.

genitals, or on any other parts of the body, pathognomonic of a so-called syphilitic virus, and, if so, what are its pathognomonic symptoms?

Had this Committee known anything of the literature of their profession, they must have been aware that they were the first medical committee appointed in any country to inquire scientifically if there is a so-called syphilitic virus; and, if there is, to determine its pathognomonic symptoms. We have seen how they have acquitted themselves of the duty intrusted to them.

We look in vain in this Report for any scientific researches, either on the part of this Committee or on the part of the witnesses they examined, as to the existence or non-existence of a syphilitic virus, its pathognomonic symptoms, or its method of cure. Like the empirics, the impostors, and the Parliament of Paris of the sixteenth and seventeenth centuries, they have, without a particle of proof, assumed that there is a syphilitic virus!!!

4. That this Committee have not attempted to ascertain, as it was their duty to do, if the witnesses whom they called before them had personally made any researches as to the pathology, &c., of this so-called syphilitic disease, or if they spoke according to the researches of others.

I have shown how this Committee examined the three first witnesses. Here are specimens how they proceeded to examine other witnesses:—"Have you any doubt of the existence of a specific disease known as syphilis?" "I have no doubt about it," was the answer. With this answer the Committee were perfectly satisfied. It never entered their heads to ask, What do you consider are the pathognomonic symptoms of a so-called syphilitic ulcer on the genitals, or on any other part of the body? But, no; he is asked, "Do you include under that term all venereal sores caused by promiscuous sexual intercourse, or do you divide the sores into a simple one, and a specific one?" "According to my experience," the witness replied, "there are several distinguishable sores resulting from infection obtained in sexual intercourse. I do not consider syphilitic sores in the same light as a small-pox or cow-pox pustule; they have not the regular character and progress which these have; there are several sorts." "I wish," says

the Chairman, "at present to confine my inquiry entirely to every sore that is called a syphilitic sore." "I term," says the witness, "all sores syphilitic that come from infection in sexual intercourse, the appellation is just as applicable to one sore as to another."!!!

The examination of another distinguished surgeon commenced thus:—"You have taken a great deal of interest in the subject of syphilis?" "Yes." "You have made it a study for many years of your professional life?" "Yes." "Have you had any opportunities of treating the primary stages of syphilis?" "A great many." But they did not ask him, What are the pathognomonic symptoms of a so-called syphilitic ulcer on the genitals or on any other part of the body?!!

Knowing by inspiration that there is a syphilitic virus, the Committee would have thought it discourteous to ask this eminent surgeon if he knew the A, B, C, of this so-called syphilitic disease. Their next question was: "What proportion of the cases which you at first deemed to be soft sores have been followed by secondary disease, and is that a common occurrence?" Answer: "It is not a very infrequent one; I should *guess* that about once in thirty times a sore, which I had supposed to be a soft sore, was followed by secondary symptoms; I give that just as a *guess*, and not as the result of any calculation."

Of a third distinguished surgeon they asked, "Do you apply the term 'syphilis' indiscriminately to all forms of venereal disease?" "No." "To what do you restrict its application?" "Setting aside gonorrhœa, to all forms of venereal disease." "All sores?" "Yes, all other sores." Not a question is asked if all sores on the genitals have, or have not, the same appearances.

The above three surgeons, of whose examination I have given a specimen, are amongst the most distinguished members of the medical profession; and their testimony proves the want of knowledge of the pathology of this so-called syphilitic disease by the medical profession in this country.*

* To those readers of this pamphlet, who have not read the Report published by the Committee, I must inform them that I have stated what the witnesses considered the strongest facts in support of the opinion they gave.

5. That this Committee have endorsed as facts the errors of their predecessors, and the dreams of the empirics and impostors of the sixteenth and of the seventeenth centuries as to the pathology of this disease. It is seen that throughout their Report this Committee consider Mr. Hunter's opinion, that a hardness at the base of an ulcer on the prepuce is the pathognomonic symptom of the existence of a syphilitic virus. Are they not told by the greatest syphilographer of the present age, Dr. Ricord, that the hardness at the base of an ulcer on the prepuce is no proof of the existence of a syphilitic? And since they had been appointed to inquire scientifically if there is, or not, a syphilitic virus, was it not, I repeat, their duty to inquire if Mr. Hunter or Dr. Ricord is right?

But is it not on record that other pathologists have informed them that if caustic is applied to a healthy prepuce, that the ulcer which follows this application of caustic has a hard base. Thus demonstrating Mr. Hunter's error. Has not the ulcer caused by herpes prepuccialis a hard base, especially when any irritating application is placed on the ulcer? (See *Bateman on Cutaneous Diseases*.) And is it not known to every scientific medical practitioner, that this hardness at the base of an ulcer on the prepuce depends on the degree of active inflammation which has existed, and on the loose cellular texture on which it is placed? But is it not also on record that Mr. Hunter described the symptoms of herpes prepuccialis as the true symptoms of syphilitic disease, and thereby misled the medical profession?

I repeat, as they had these facts before them, had they known their profession, and had they known their duty, they ought to have inquired into them, and ascertained if they were justified to place any confidence in Mr. Hunter's statement. But, not content with assuming as facts the errors of Mr. Hunter as to the pathology of this so-called syphilitic disease, they assume the dreams of the empirics and impostors, that the syphilitic virus can lay dormant for years in the body of the parent, and then is conveyed, with the semen, into the body of the offspring, and manifest itself in the offspring *even "thirty years after his birth"!!!* And, to prove this, they called before them specialists, and they get from each the stereotyped answer, of nothing like leather.

They called before them a great authority on the question of hereditary syphilis, and he assures the Committee "that he finds no difficulty in discovering hereditary syphilis in a person thirty years after birth, for the bridge of the nose is sunken or flat, and the upper central incisor teeth present vertical notches." Did they ask him how he could trace the connection between a so-called syphilitic ulcer on the genitals and the sunken and depressed bridge of the nose, and the vertical notches on the upper central incisor teeth? They did not.

The zoologists inform us that there is a species of animal which where one goes all follow. Is it the same with the specialists? Another distinguished practitioner whom the Committee call before them also affirms "that he knows that syphilis is hereditary in consequence of the offspring having these vertical notches on the upper central incisor teeth." But the Committee forgot to ask him how he arrived at that knowledge, and if he knew what are the pathognomonic symptoms of a so-called syphilitic ulcer on the genitals!!!

They also called before them a dentist: he knows that syphilis is hereditary, "because there is a great amount of irregularity in the form of the teeth individually, especially in the front teeth." Did they ask this gentleman if he knew what are the pathognomonic symptoms of a primary so-called syphilitic ulcer on the genitals? They did not.

They called another witness before them who affirms that syphilis is a hereditary disease "because he has seen necrosis of the upper jaw bone," the consequence of hereditary syphilis. Did they ask this gentleman if he knew what are the pathognomonic symptoms of a so-called primary syphilitic ulcer on the genitals, or on any other part of the body, and how he could trace the connection between a primary syphilitic ulcer and this necrosis of the bone of the upper jaw? They did not. They, of course, knew as well as this witness that necrosis of the upper jaw bone can be caused only by hereditary syphilis!!!

They call before them a distinguished dermatologist [*Anglicè*, skin doctor], and they begin to examine him, in their usual way, by an assumption. Thus, they ask him: "You are very familiar with diseases of the skin, and amongst other varieties

of disease have you had opportunities to see varieties of syphilitic diseases, whether in children or in adults?" "Yes." "In the form of eruption chiefly?" "Yes; eruptions and affection of the mucous membrane of the mouth." Did they ask him for evidence of facts to account for this belief? They did not.

It is possible that this gentleman entertains the same opinions on cutaneous eruptions as a late celebrated dermatologist, Dr. Alibert, who believed that any eruption on the skin, and every ulcer on the body which could be cured by mercury, was syphilitic. When told that his patient never having had "*coitus*," could not have contracted the disease, Alibert would reply, "His father or mother, or his grandfather or grandmother, or some of his ancestors, must have contracted the disease, and it now appears in the individual before me. Consequently, according to Dr. Alibert, any virtuous descendant of Adam might have contracted this disease from any virtuous descendant of Eve, for anything he, she, or they, might know to the contrary!!!

This is the dogma of the French, and it is the dogma accepted by the English dermatologist as a demonstrated fact, that syphilis is hereditary, and accepted by this Committee as a pathological fact that cannot be doubted!!! And as this Committee, throughout their inquiry, allowed the witnesses before them to consider that the symptoms of scrofula and those of hereditary syphilis are the same, it follows that, according to this Committee, and according to the testimony of the witnesses that came before them, that almost the whole population of the world are labouring under hereditary syphilis. And, as a proof of the correctness of what I am stating, I refer to the case of Mr. M., who appeared before the Committee. (See p. 365 in the Report of this Committee; see also pp. 467, 468; see also pp. 475, 476.)

Apparently knocking at every door for information as to this so-called syphilitic virus being transmitted from the parent to the offspring, this Committee called before them a distinguished accoucheur. What was their first question to this gentleman? They would not have been true to themselves if they had not begun by their usual assumption, thus: "You are familiar with the various aspects presented by children at

their birth, and you are, doubtless, familiar with the appearance presented by infants born of syphilitic parents?" "I am quite familiar with them." "Will you be so good as to describe the appearance presented by a syphilitic infant?" "If I were to point to local symptoms that mark the existence of hereditary syphilis, I would say that the *alæ nasi* are depressed in almost every syphilitic child, and it is a very marked feature," &c. But they forgot to ask him this most important question, How do you know that syphilis is the cause of this depression of the *alæ nasi*?

They called before them another distinguished accoucheur, and they began as usual by asking him, "You are familiar with the appearances which are presented by new-born children who are afflicted with syphilis?" "Yes." "Will you be so good as to describe, as briefly as you please, the appearance which a child presents at birth, and which induces you to say that the child is syphilitic?" "The children are very much emaciated; they have a sort of monkey face in appearance; and they commonly have a rash either at the time, or which breaks out afterwards, extending over the nates, the thighs, and the genital organs, of a light copper colour; very often the skin easily peels off the feet and exfoliates in that way."

Where is the medical practitioner, acquainted with his profession, who does not see that that gentleman describes a case of strumous disease, and assumes it to be a case of hereditary syphilis? He himself admits this, for he says a little farther, "If the child survives it is apt to go into struma." He also tells us, "That there is no difficulty to establish the diagnosis between hereditary syphilis and struma"!!! But the Committee forgot to ask him what were his proofs of the existence of a syphilitic virus? and how he could trace the eruption on the child to the ulcer on the parents' genitals? and what are the diagnosis between syphilis and struma? Therefore this gentleman's evidence can afford us no proofs of the existence of a hereditary syphilis.

That all the specialists might be represented in their Report, the Committee call before them also a distinguished oculist. He affirms that he knows that syphilis is hereditary by the inflammation of the cornea and by iritis. Of course the Com-

mittee were satisfied with this information, as this gentleman has affirmed it; and of course they are well aware that inflammation of the cornea and iritis can only occur from hereditary syphilis!!!

It must not be lost sight of, that the above specialists are esteemed amongst the best informed medical practitioners (as specialists) in this country; that they are believed on their oaths, on medical questions, in courts of law. But we must not altogether blame these specialists for their shortcomings. Did not this Committee begin, I repeat, their inquiry by assuming the existence of a so-called syphilitic virus? while they had been appointed to study and to ascertain if such a virus existed. And have they not, during this inquiry, done all in their power to bring us back to the days when the empirics and impostors alarmed the Parliament of Paris, by assuming the contagious nature of syphilis—that it was communicated from an infected to a healthy person by breathing the same air, by sitting on a seat where an infected person had sat, etc., which caused the Parliament of Paris to decree that any foreigner, who laboured under this disease, and did not leave France in twenty-four hours, should be hung; that every Frenchman who laboured under this disease, if out of his house and in the street, should be hung?

Although this Committee have had before them some of the best medical practitioners in this country, have they examined one of these scientifically? Their Report is there, to answer—certainly not.

6. That this Committee have acted, throughout this inquiry, disingenuously.

When I appeared before this Committee as a witness, on the 6th of October, 1864, and when I found that, instead of beginning, as it was their duty to do, to ask me on what grounds I doubted the existence of a syphilitic virus, and when I found that they had assumed that there is a syphilitic virus, I immediately protested against this unscientific manner of carrying out this inquiry. I sat on the left of the President; the shorthand writer sat on my left. The President, while I formulated my protest, told the shorthand writer not to take down what I was saying; as it was merely conversation, that

he would tell him when to take notes. This protest does not appear in their report of my examination before them. They have suppressed it.

But there is a more important part of my evidence suppressed. It is seen above that Dr. Ricord maintains that inoculation is the only proof of the existence of a syphilitic virus. Dr. Donnet, while examining me, said that inoculation was a proof of the existence of a syphilitic virus. Upon which I turned to Dr. Evans's work on Venereal Disease, and I requested the President to read to the Committee an account which Dr. Evans gives of an experiment practised at Valenciennes in 1816 by my late friend, Inspector-General Dr. Murray. Dr. Murray inoculated a man with the pus from an herpetic ulcer on the prepuce in a state of active inflammation, and a pustule was the result, showing at once that Dr. Ricord is in error when he asserts that only syphilitic ulcers secrete inoculable pus; therefore that Dr. Ricord has no proofs of the existence of a syphilitic virus; and that, therefore, I repeat, no medical practitioner in this country or in France can point out one symptom pathognomonic of a syphilitic virus.

This quotation from Dr. Evans was an important one, and especially as I was at Valenciennes at the time, and that I was cognisant of the fact; consequently this quotation from Dr. Evans, which the President read out to the Committee, and what I had stated after the President had read the quotation, I submit ought to have appeared in the Report of the Committee, as it demonstrates, I repeat, that there is no proof of the existence of a syphilitic virus.

I say I sat on the left of the President, and the short-hand writer sat on my left. He was going on taking down Dr. Donnet's questions and my answers. But when I had put Dr. Evans's work into the President's hand, and after I had requested him to read the above quotation relative to Dr. Murray's experiment, the President told the short-hand writer that he would give him the quotation to copy. Whether the President accidentally or intentionally omitted to give the above quotation and my protest to the short-hand writer to be inserted in his notes of my examination I leave others to decide. But it is not only in my evidence that important

passages have been omitted or mutilated to meet the views of this Committee.

I have before me a pamphlet published by Dr. Dickson. It appears that this gentleman formerly served in the army in India. Having had great opportunities to see and to study diseases of the genital organs, he satisfied himself that the pathology of syphilis was not understood. On retiring from the army he published his doubts. As I was not in this country when Dr. Dickson published his opinion relative to the non-existence of a syphilitic virus, I was not aware of his publication on the subject.

My pamphlet fell into the hands of the Secretary to the Admiralty, who, struck with the facts which I brought forward, Mr. Romaine, although personally unacquainted with Dr. Dickson, and knowing him only by his writings, sent him my pamphlet, requested his opinion on it, and begged his advice whether it would be useful or not to appoint a Medical Committee, as I recommended, to study the pathology of this so-called syphilitic disease, and requested to see him at the Admiralty.

After Dr. Dickson had carefully read my pamphlet, he gave it as his advice that a Medical Committee ought to be appointed to carry out my suggestion; and the above Medical Committee was appointed. The Admiralty desired this Committee to examine Dr. Dickson.

It appears by Dr. Dickson's pamphlet, that he is the only medical practitioner that came before that Committee who had the moral courage to give, and to support his opinions by facts, which this Committee could not controvert; and that this Committee suppressed and mutilated part of his testimony to suit their views. Dr. Dickson remonstrated against this suppression and this mutilation of a part of his evidence; a correspondence, it also appears, took place between him and the Secretary to the Admiralty, in the course of which Dr. Dickson threatened to apply for an injunction against the publication of his evidence in the Report of the Committee in its mutilated state. I now see that Dr. Dickson's evidence does not appear in the above Report. The hiatus is filled up with these words, "the evidence, comprising the questions from

Nos. 2255 to 2340, inclusive, is omitted here, on the recommendation of the Lords of the Admiralty." Therefore, since this Committee did not hesitate to mutilate, and to suppress parts of Dr. Dickson's evidence to serve their views, I say that they have done the same with my evidence; that they are not, therefore, trustworthy; and that their Report is based on assumptions and not on facts, and that it is an idle romance; and therefore, I beg leave to say, that I have demonstrated,

1. That no medical practitioner in England or in France can point out one symptom pathognomonic of a syphilitic virus.

2. That this Committee did not know how to proceed to inquire scientifically into the pathology, etc., etc., of this so-called syphilitic disease.

3. That this Committee have not attempted to ascertain, as it was their duty to do, if there are any symptoms on the genitals, or on any other part of the body, pathognomonic of a syphilitic virus.

4. That this Committee have not attempted to ascertain, as it was their duty to do, if the witnesses, whom they called before them, had personally made any researches as to what are the pathognomonic symptoms of this so-called syphilitic disease, or if they spoke according to the researches of others.

5. That this Committee have endorsed, as facts, the errors of their predecessors, and the dreams of the empirics and impostors of the sixteenth and of the seventeenth centuries.

6. And that this Committee have acted throughout this inquiry disingenuously.

Finally, it must be said, that from the above it is evident that this Committee did not attempt to ascertain, as it was their duty to do, if there is a syphilitic virus, and if so, what are the pathognomonic symptoms of a primary so-called syphilitic ulcer on the genitals, or on any other part of the body? It is evident that they have assumed the existence of a syphilitic virus, and it is evident that they have no pathological facts to authorise them to conclude as they have done,

"That this Committee cannot neglect the opportunity of calling attention to the evidence of the many distinguished authorities, so strongly confirming the opinion which has of late years been increasing in strength, amongst the profession,

as to the fatal effects of syphilis on the human offspring. They testify to its prevalence amongst all classes of society; its insidious nature; the frequent failure of all but men of great experience to recognise it; and, moreover, to this most important fact, that the poisoned *fetus in utero* is no infrequent cause of miscarriage."

Consequently, we are authorised to say, that the conclusions which this Committee have arrived at are based on assumptions instead of being based on well established pathological facts; that they are the productions of untutored imaginations; that they are a deception practised on the Admiralty, the War Office, and the public; and that they are repugnant to the present medical acquirements of the medical profession in this country.

I have the honour to be, my Lords,

Your Lordships' obedient servant,

DAVID MACLOUGHLIN, M.D.,
Member of the Legion of Honour.

London, 24th May, 1866.

36, Bruton Street, Berkeley Square, W.

P.S. Since the above was written, I have received the copy of the Instructions, given by the Admiralty and the War Office to the Medical Committee.

The Admiralty and the War Office will permit me to say, that every sailor is not a scientific nautical man; consequently that when a scientific nautical question is sought to be solved, the question ought to be put in scientific nautical terms, and then referred for an answer to scientific nautical men. The same thing holds good in the medical profession. That every medical man is not a scientific medical man; that when a scientific medical question is sought to be solved, the question ought to be put in precise scientific medical terms, and then referred for an answer to scientific medical men.

The instructions given to the above Medical Committee have been, it is evident, drawn up by persons unacquainted with medical science; who did not know what information

they ought to ask for, and who sought for information on a medical question from persons, like themselves, unacquainted with medical science.

No person need be told that to study any question scientifically we must begin at what is known, and thereby endeavour to arrive at what we do not know; consequently, that we must not begin by *assuming* as a demonstrated fact, that which we have not demonstrated to be a fact.

The above instructions begin by assuming "that there is a syphilitic virus," hence a so-called syphilitic disease; and requires the Medical Committee "to consider the pathology of this disease, only to such an extent as they may deem absolutely necessary, to enable them to deal with the main object of the inquiry, namely, the attainment of a sound rule of treatment."

Had such instructions been given to a committee composed of scientific medical men, they would have seen the error which had crept into their instructions; they would have seen that they were called on to act the tragedy of "Hamlet" with the part of Hamlet left out; they would have corrected this error; they would have gone to the bedside and there interrogated nature; they would then have sought for information from persons who they believed could give them information, and they would have ascertained, by a careful inquiry, if these persons were acquainted with medical science, and entitled to be heard as witnesses; and having done all that medical science dictates to arrive at a knowledge of the disease, they would have pronounced their views as to the pathology, etc., etc., of this so-called syphilitic disease.

But unfortunately this Committee, to which the above instructions were given, was composed of persons unacquainted with medical science, and unacquainted with the duty they were called on to perform. They did not go to the bedside and there interrogate nature; and when they had before them scientific medical men well acquainted with their profession, and competent to give, and to be heard when they give, an opinion on a medical question, they did not examine them in such a manner as to give them an opportunity to show that the testimony they gave was based on well ascertained patho-

logical facts. They refused to listen to pathological facts; they suppressed pathological facts; they mutilated and suppressed evidence to suit their preconceived opinions; "they assumed the existence of a syphilitic virus."

They have put before the Admiralty, the War Office and the public, the creation of their untutored brains, as a demonstrated pathological fact.

But this Committee have not completed their romance; they have not published the medical treatment they recommend for their bantling. Out of respect for the medical profession it is hoped that it will never be brought to light. Based as their report is on the creation of their untutored brains, the medical treatment they will recommend, for this so-called syphilitic disease, will rest on imagination. It cannot be useful to the public. It will entail scorn and contempt on the medical profession of this country.

From the above facts it is evident, that the Admiralty and that the War Office have fallen into incompetent and unfaithful hands; that their confidence has been abused; that they have been imposed upon; and that the pathology, the etiology, and the medical treatment of this so-called syphilitic disease, is where it was, still requiring to be scientifically studied.

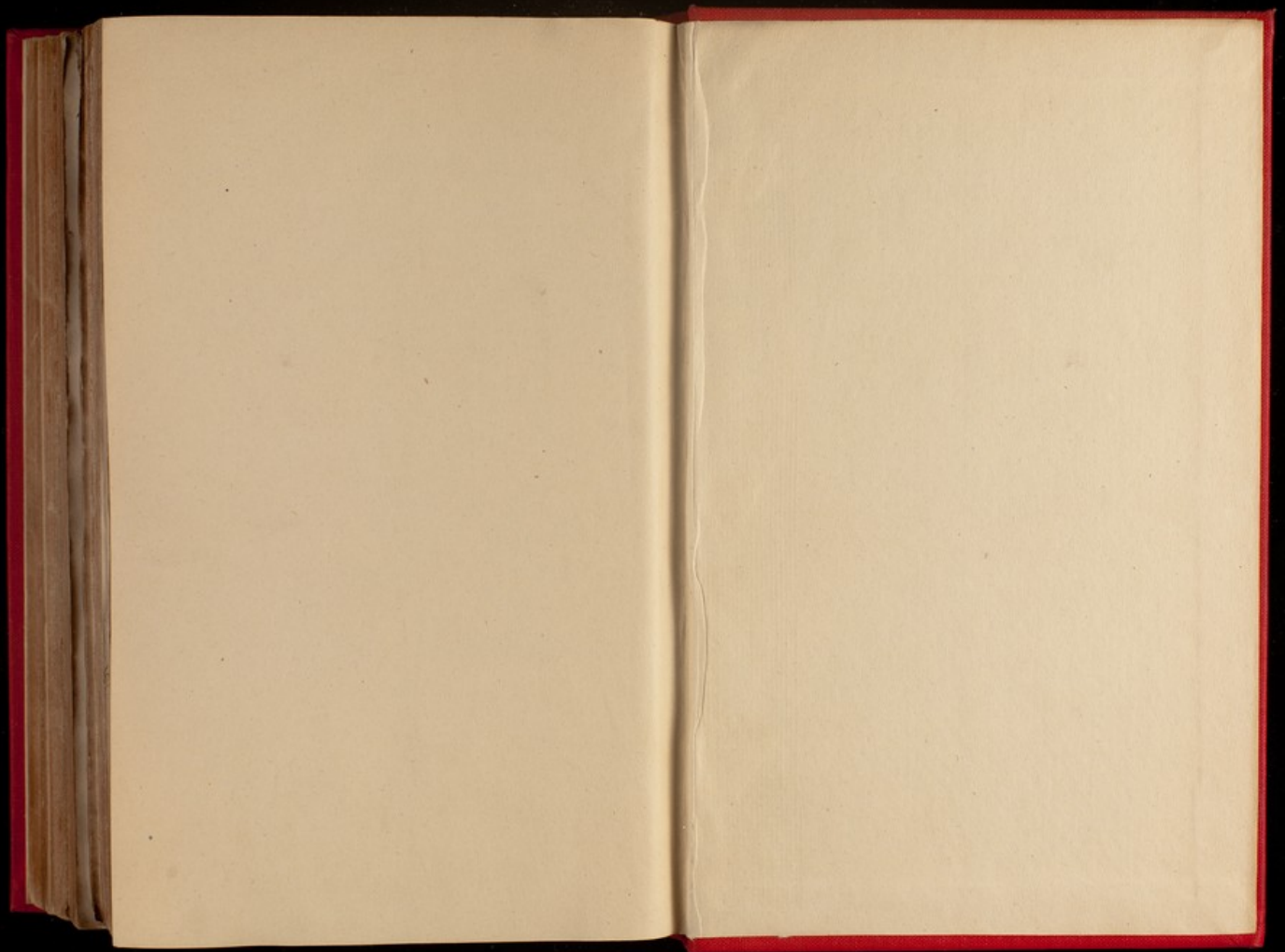
As the Admiralty and the War Office adopted my suggestions two years ago, and appointed this Medical Committee to have this disease scientifically studied, with every sentiment of respect, and in the interest of the Navy, in the interest of the Army, and in the interest of the public, it may be permitted to hope, that the above two great departments of state will not be deterred from further inquiry into the pathology, etc., of this disease, by the failure of this first attempt.

They will please remember that the art of gunnery has not arrived at its present state of improvement but by slow and repeated investigations, that therefore they cannot expect that a medical question, which has heretofore been in all countries chiefly in the hands of quacks and impostors; which has, heretofore, never been scientifically studied, in any country; by scientific medical practitioners, or by any scientific medical authorities; and which, heretofore, every government, in

every country, have deemed it derogatory to themselves to take cognisance of such a disease and to have it studied.

I say, with sentiments of respect, that it may be hoped that, from motives of duty towards their subordinates, from motives of duty towards the public, and from motives of humanity towards the human race, that the Admiralty and the War Office will give the medical profession the same opportunity to be useful that they have given the gunners, and that they will appoint another medical committee to study scientifically if there is a syphilitic virus? if so, what are its pathognomonic symptoms on the genitals and on other parts of the body, and its medical treatment, and the hygienic means they recommend to prevent the occurrence of this disease in the Navy and Army.

June 18th, 1866



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PAMPHLETS

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