

ably attended by suppression of urine. The coma was, doubtless, the result of the presence of urea in the blood. He had also observed that the stage of collapse varied from that of the former visitation; the cramps were not so severe, and though the surface was very blue, it was not so cold. The heat of the body was also easily restored, by the application of warm blankets, &c., to a temperature of 90° , whilst a thermometer placed under the tongue did not rise beyond 78° . In the late cases, perspiration had been entirely absent, whilst in 1831-32 it had been enormous; there was also less shrinking and collapse of the features. The duration of the disease was longer in this than the last. With respect to the treatment, he could add nothing to what had been advanced by Dr. Baly. From his own experience he should say, that in neither epidemics had medicine had any control over the disease; and in no case that he had seen did it appear to him that any good had been done by the means employed. The only exception to this was, that chloroform certainly did, whether applied externally or given internally, have a most beneficial effect in relieving the spasms and cramps, but it had no control over the course of the disease. It had seemed to him that the principal force of the disease had fallen on the kidney, and the restoration of the secretion of this organ would prevent a fatal result, but then, no remedies applied for this purpose had been of any avail. It was observed in his cases, that after the urine had been suppressed, the first water passed afterwards was always albuminous, and accompanied by casts of the tubuli uriniferi, showing that the epithelium lining them had been thrown off. On this occasion he should say that the disease was one of the kidneys as much as of any organ. With respect to the pathology of the disease, the glands of the small intestines had been found, when the patients died in the stage of collapse, to be very much enlarged, with signs of great congestion of the mucous membrane. He had also noticed another symptom in the late epidemic, which when present had invariably proved fatal; this was, a discharge of blood from the intestines. After death, in these cases, considerable patches of ecchymosis were discovered in the large and small intestines, accompanied by coagula. In these cases the mucous membrane, after a time, became gangrenous. He showed some morbid specimens of the disease.

Mr. STREETER had been present in two instances where the injection of saline fluids into the vein of the arm was practised in the St. Giles's Hospital, but in both cases without the slightest alleviation. He had seen the warm bath, the hot-air and spirit-vapour bath, by means of the basket apparatus under the bed-clothes, employed without producing the slightest reaction. He had employed mustard extensively over the skin of the abdomen, chest, and limbs, and although its specific action on the skin followed, it failed to arrest the progress of the collapse; nothing but vomiting, recurring at intervals, retarded or removed that. He had seen phosphorus employed, and in two instances had found the pills in the bodies after death. In one, a rapid case, they had not passed the stomach. In the second, a protracted case, one of the pills was found, curiously enough, in the appendix vermiformis. He had been present at only a few post-mortem examinations, as he always believed cholera communicable, but the only constant appearance that he could attribute to the disease was an increase of vascularity in the mucous membrane of the intestines. The premonitory diarrhoea he had not found to yield to simple and ordinary remedies. During the prevalence of cholera, every bowel complaint should be regarded as premonitory, and receive the most active treatment. His usual remedies were, superacetate of lead, powder of opium, calomel, and capsicum, of each one grain in a pill, to be administered after each action of the bowels, with a dose of acetate of ammonia and nitric ether. In conclusion, he would observe that he felt assured of the infectious nature of cholera, and believed that it was very frequently communicated by the medium of clothes, as well as by personal exposure.

Dr. GARROD, on being called upon by the President to give the Society the results of his researches into the condition of the blood in cholera, stated that in general they accorded very much with those of Dr. O'Shaughnessy, made during the epidemic in 1832. Dr. Garrod had always found that the blood in cholera exhibited characters different from those which presented themselves in any other disease. In the first place, the specific gravity, both of the blood itself, and also of the serum, was always found to be considerably increased; that of the blood from 1065 (the healthy average) to 1075 or 1080, and that of the serum to about 1040, ten or twelve degrees above the normal average; hence its tar-like consistence. This state was exhibited in a patient's blood that he had very recently

had an opportunity of examining, taken just as the stage of collapse was coming on, and he thought it would be a very interesting point to determine whether the collapse in all cases was accompanied by this change, or whether it might arise simply from a poisoned state of blood. Without such an altered physical condition, even in some cases where the evacuations had not been copious—that is, in cases somewhat similar to those named cholera sicca, he had found such an alteration; whereas it had been ascertained, that in ordinary diarrhoea, even when accompanied with profuse evacuations from the stomach and bowels, the serum had been found of a specific gravity not at all exceeding the healthy standard. With regard to the alterations in the fibrine, he thought that it was more one of quality than of amount; for he had found that until it ceased to coagulate, it maintained about the normal average, although when compared with fibrine from healthy blood, its elasticity and strength was much weakened, and, as the disease progressed, it soon lost altogether its power of coagulation. The total amount of solids in the blood was always much increased, especially the organic portion, which arose from the watery part passing off by the intestines, and taking with it some of the salts of the serum, which last, compared with the organic portion, were consequently diminished in amount, but not so much as stated by Dr. O'Shaughnessy. On this point, however, Dr. Garrod would not give a positive opinion, as his analyses were not as yet complete. Dr. Garrod had also examined the blood in several cases of cholera, for the purpose of determining the amount of urea and uric acid, and the results of his experiments might be summed up in a few words—viz., that during the stage of collapse, the urea was usually found in the blood in quantities greater than in health, but its amount was not large; that when partial reaction ensued, the urea was still further increased, but that in the consecutive fever which sometimes succeeds the collapse, its quantity became excessive, and that probably some of the symptoms then exhibited were due to this accumulation. Dr. Garrod considered that in the collapse there was an almost total arrest of formation of urea, as well as of the excreting power of the kidneys, but that in the stage of reaction, the formation of the principle ensued, frequently without the restoration of the excreting function, and hence its accumulation in the blood. The uric acid appeared to follow the same laws as the urea. In one case Dr. Garrod found the serum slightly alkaline in the collapsed stage, but after partial reaction, it exhibited an acid state, which was not altered by the application of heat. The rice-water evacuations were always alkaline in reaction, and contained a large amount of saline matters—viz., phosphate of soda and chloride of sodium, in proportions not unlike those found in the blood, the albumen in small quantities only. The vomited matters exhibited an acid reaction. The bile in one case had a specific gravity, 1044. The late hour of the evening prevented Dr. Garrod from entering into further detail.

"ROSO-GRASS OIL."

To the Editor of THE LANCET.

SIR,—Seeing a request for information in your No. of Feb. 3rd, as to the "Grass Oil," I write to say that I have used the "Roso-grass oil of Ceylon," which I believe to be the proper name of the fluid referred to by your correspondent, at Cheltenham. A sample, the remains of which I send you, was obligingly given to me, at my solicitation, some time since, by the Committee of the Royal Medico-Botanical Society, out of its Museum. I found this highly odoriferous oil to form an excellent embrocation in rheumatism and neuralgia. I have less knowledge of its internal use, having only tried a few drops occasionally as a dose. But I beg to refer you to Earl Stanhope, the president of the Society, who, with his usual philanthropy, will, I am sure, cause a portion of the paper that accompanied the oil, from India, to be forwarded to you as a guide as to dose and effects.

I was formerly senior honorary secretary to the above-named institution, and I assert, from the estimation in which that Society is held abroad, and from the many curious papers and numerous specimens annually sent to it from all parts of the world, that the association has not (in England) received the support and encouragement to which its foreign communications and the numerous scarce articles of *materia medica* it possesses, fully entitle it.

I would on all occasions refer you to the Committee of the Royal Medico-Botanical Society concerning rare vegetable substances, as yet known to but few persons, from being seldom imported, except for experiment and inquiry into their qualities and value.—I am, &c.

W. H. JUDG.