Weight of the Body.—I stated at the trial that the emaciation was extreme. Part at least is, I think, to be attributed to tubercular disease. With respect to the remainder, the decision must rest upon the circumstantial evidence.

Weight of the Solid Viscera.—Mr. Taylor says the solid viscera of the abdomen were shrunk "to little more than half their usual bulk and weight". The kidneys weighed $7\frac{3}{4}$ ounces. I find the average of healthy kidneys belonging to adult women dying in hospital from all causes is a little under $8\frac{3}{4}$ ounces. This estimate excludes cases of renal disease, but includes many cases of heart-disease. Is this "little more than half"? The spleen weighed $4\frac{1}{2}$ ounces; and an average is given of 6.13 ounces ("little more than half", says Mr. Taylor). I find the average weight of healthy spleens in women, collected as just mentioned, to be about 5 ounces. This includes a range of 2 ounces to 10 ounces. So that I adhere to my statement in court, that from the weight of so variable an organ no conclusion can be drawn. The liver was the smallest organ relatively; its weight is given as $34\frac{3}{4}$ ounces, while the average given is 53.6 ounces. I estimated the average, when asked in court, as 48 to 50 ounces, and was afterwards told this was too high. From the frequency of fatty change, it is very difficult to establish an average. The weight appears to me to be about two-thirds or three-fourths of the average (which Mr. Taylor puts in round numbers as "little more than half"). This no doubt suggests malnutrition, but would not show whether this arose from non-ingestion or from non-assimilation of food. Mr. Taylor does not allude to the heart. The weight is given as $7\frac{3}{4}$ ounces. Dr. Peacock gives the mean weight of the healthy heart in women as 8 ounces 13 drachms, and the ordinary range in acute diseases 8 ounces to 10 ounces; in chronic, 7 ounces to 9 ounces; so that the weight here is really not abnormal.

It seems necessary to repeat the undeniable proposition, that wide deviations from the average are consistent with health; and that extreme deviations, which we cannot explain, are sometimes met with in all organs of the body.

Emptiness of the Intestines.—It must be remembered that there was diarrhoea, said to have been going on for three days, and the evidence showed that the poor woman had been unable to take more than a very little food within about four days? Is any further explanation necessary.

Full Gall-Bladder.—I find weight is attached to this so-called "sign of starvation" by some people, though not by Mr. Taylor. Surely, the gall-bladder is as often found full as empty after death, and its state depends merely on the temporary condition of the organs of digestion.

I must confess that I did not attach any importance to "petechiæ and ecchymoses", nor do I do so now, even if they were all of them *ante mortem*. Such things occur in many chronic diseases.

One really trifling point has been so dealt with by Mr. Taylor that I must just notice it, viz., the redness of the stomach. The medical witnesses for the prosecution—or, at least, some of them—positively asserted that redness or congestion of the stomach was a "sign of starvation".*

At a later period of the trial, the Attorney-General suggested to me the theory favoured by Mr. Taylor, that congestion of the stomach might be caused by overfeeding after privation of food. My answer to this is that, if congestion or vascularity have anything to do with food, it is impossible to distinguish between the effects of use and the effects of overuse. Vascularity of the stomach is certainly *primâ facie* evidence that the stomach was in use immediately before death, and can only be made even to suggest previous disuse of the organ by assuming the very point at issue.†

Mr. Taylor complains of my method of treating the question of starvation. I am certainly not disposed to complain of his, because I find it so far from antagonistic to my position. It consists in stating some purely speculative hypothesis; and assuming that, if such a hypothesis cannot be refuted by special experience, it must be taken as proved. But who can be called upon to disprove that for which no proof is offered? Such questions as, whether overfeeding after abstinence produces gastritis, or might produce such nervous symptoms as I have recounted, or again, whether gastritis produces peritonitis, seem to me hardly fit for discussion till they have been proved to have a foundation in fact.

* Dr. Wilkinson, in answer to the Attorney-General, said: "Inflammation of the stomach is one of the symptoms of poisoning, and also one of the symptoms of starvation." The Attorney-General to Mr. Rodgers, Lecturer on Toxicology at the London Hospita: "Having disproved the existence of poison, what do you now think was the cause of the redness of the stomach, the duodenum, and the rectum?" Mr. Rodgers: "Starvation."—*Daily News*, September 21st, 1877.

† In the sentence, "Special atrophy . . . from disease", the last word is a misprint for *disuse*.

All these questions alike are far too speculative to be introduced into evidence affecting men's lives.*

The only positive information I know as to the danger of overfeeding after starvation comes from Donovan's experiences in the Irish famine.† They are certainly more instructive than the wild conjectures of Mr. Taylor, the Attorney-General, or the witnesses for the prosecution.

"In administering food to the famishing, great caution should be observed; the appetite in such cases is very craving; and, if too much nourishment be given at once, vomiting and exhaustion are induced, which frequently terminate in death; or, if a stimulating diet be continued for any time, reaction, terminating in fever, is very apt to supervene."

Does this accord with the facts of the case?

Donovan also states that, in no case did he see "delirium or mania as a consequence of protracted abstinence, though described as common symptoms of hunger among shipwrecked sailors and others suddenly deprived of food". This statement seems also to put into its proper place a case like that of the Welsh fasting girl, mentioned by Dr. Fowler.

Finally, Mr. Taylor imagines he has shown that there was "fair presumptive evidence of starvation, which completed the chain of proofs against the prisoners". But this opinion is far less than was claimed by the medical witnesses for the prosecution. Their evidence, it should be remembered, was given at the inquest in an unqualified manner, when no direct evidence of privation of food existed (though there were strong suspicions of foul play), since the testimony of Clara Brown on that occasion was directly opposed to what she afterwards gave. It was evidently taken by the coroner's jury and others as implying that the doctors had found starvation in the body, as they might have found poison or cancer, to be the cause of death.

As the legal question is not yet decided, it seems undesirable to enter upon the moral aspects of the case.

I must apologise to readers of the JOURNAL for the length of this communication; but for that I am not wholly responsible. So much space has not been devoted to Mr. Taylor's letter on account of its intrinsic importance. That gentleman does not, so far as I know, speak with any special authority on the subject, and his opinion on the case is opposed to an almost universal *consensus* of our recognised authorities in pathological science. But in a matter of such moment, it is desirable that all objections which can fairly be raised should be fully discussed and left to the arbitrament of those best qualified to judge.

THE USE OF THE OPHTHALMOSCOPE IN DETECTING THE EARLY FORMS OF GRANULAR KIDNEY.

By ROBERT P. OGLESBY, M.R.C.S.,

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So many instances have occurred to me, in which patients have suffered from disease of the kidneys without the disease having been diagnosed by the ordinary methods, and afterwards made apparent by the use of the ophthalmoscope, that I deem it of sufficient interest to pen a few lines on the subject.

It not infrequently happens that, during the early stages of the disease (granular kidney), the patient consults a number of medical men, who fail to detect, after a careful examination of the urine, the slightest trace of albumen; neither does the microscope aid in the diagnosis.

During the examination of cases for the purpose of statistical evidence concerning amaurosis, a patient presented himself complaining of great weakness and lassitude and failing vision. The ophthalmoscope disclosed a condition of retina which led me to believe that the kidneys must be diseased; but a most careful examination of the urine, again and again repeated, failed to give the slightest evidence; the microscope also gave negative evidence. The appearance of the retina in these cases is characteristic, and unlike anything of the kind I have ever seen. It consists in the grouping of small white spots, the outline of each being clearly defined; they are invariably circular, of extremely small dimensions, and present the appearances of a pearl of an intensely bright colour, and stand out from the retina in a marked manner. The grouping of the spots is symmetrical in each eye, and is generally in the form of a crescent. After very careful watching, I at last succeeded in tracing the connection between this peculiar retinal appear-

* Mr. Longrigg very properly described his view of apoplexy produced by overfeeding as a theory, not an explanation.

† *Dublin Medical Press*, February 2nd, 1848.

ance and diseased kidney. The disease usually attacks those who are suffering from an insidious form of wandering gout, associated with torpid liver and bowels, bad digestion, and unhealthy colour of skin, with mental faculties anything but vigorous and business-like, alternating with an unusual flow of spirits lasting but for a short period. After years of anxious watching and much mental speculation as to the cause of these curious retinal spots, I have at last succeeded in arriving at a true and satisfactory diagnosis.

Mr. B., a gentleman who has been much engrossed in a very extensive business, consulted me concerning his vision. He assured me that, for the past three years, he had known no rest, as business anxieties had much depressed him. His medical adviser, in his letter to me, stated that the patient was temperate, a perfect man of business; that he had known him personally for several years; also, that he had been in attendance professionally, with but little intermission, for two years. I found the patient in a state of considerable alarm concerning his eyesight; business letters could but with difficulty be read, and he could barely recognise an acquaintance across the street. He had the appearance of a man much overworked, and his symptoms pointed to a gouty diathesis.

In this case, the retinal appearances above described were present; but, as the medical man assured me that no kidney mischief existed, I did not then examine the urine, and, as I had up to that time failed to detect albumen in any of these cases, I attached but little importance to the statement.

I had hitherto regarded these cases as simply unaccountable, but had never given up the belief that the peculiar retinal appearance was associated with Bright's kidney. During the interval which elapsed between the patient's first and second visit, it occurred to me that I had been somewhat remiss in having neglected to verify the statement that no albumen existed in his urine. I at once communicated with my medical friend, asking him again to go carefully over the ground. The following post brought his reply. "After very careful examination, I believe I can detect albumen; but send you some urine to test." The urine sent undoubtedly contained albumen; and my mind was at once relieved as to the exact nature of these cases.

Out of ten cases carefully watched over lengthened periods, this was the first in which the patient's urine was albuminous. It may be said that, in any ordinary case of Bright's disease, white spots may be seen on or in the retina; that, no doubt, is true, and, at the same time, albumen may invariably be found. But the kind of case I am describing is so essentially different to anything I have ever seen in the retinal appearances, that I never had the slightest hesitation in coming to a diagnosis, viz., that those spots were not associated with the disease recognised as "Bright's kidney".

The spots are so unlike the ordinary albuminoid patches of Bright's disease, are so differently placed, are so regular in outline, and the grouping in the form of a crescent is so perfectly symmetrical in each eye, that I have now no hesitation in affirming that the spots are diagnostic of a very insidious disease of the kidney, that albumen is present in perhaps the minutest quantities, and that any ordinary superficial examination of everyday practice fails. I also believe the disease to be slow granular degeneration of the kidney, as verified by repeated microscopic examinations.

In all the cases watched by me, the retinal patches have been persistent, and the duration of the disease long; a remarkable fact being that no patient has died from the disease whilst under my care.

Of these retinal groupings I have made in several cases accurate drawings; and the notes of the first case I ever saw are as follows.

February 14th, 1868. Matthew Rhodes, aged 64, enjoyed good health till he was sixty years of age, when he suffered from a severe attack of bronchitis; from that period he never was well. He suffered from indigestion, constipation, and gouty pains in the hands and feet, and neuralgic pain about the stomach and liver. He now suffered from vertigo and pain in the head. Eight months previous to his visiting me, his eyesight began to fail. *Examination.*—The lungs and heart were healthy; the urine was clear and pale, of specific gravity 1012, containing no albumen and no sugar. The ophthalmoscope revealed bright spots in the centre of the field.

This case was the first I had seen of the kind, and I was much struck by the fact that albumen was absent. The patient continued to visit me at intervals for upwards of a year. At each visit, the urine was carefully examined, but without result. It is more than probable that, had he continued to attend for another year, the advancing disease would have been detected by the presence of albumen in the urine.

I have been much interested in the fact that the majority of those suffering from this disease are men. I remember but one female who suffered from the disease. Another fact of special interest is that the disease is associated with some deep mental strain—either loss of busi-

ness, loss of money by speculation, or the keeping of family secrets of a nature so depressing and so unfortunate that they must never be known outside the family circle.

A CASE OF ANEURISM OF THE MIDDLE CEREBRAL ARTERY.*

By T. OUTTERSON WOOD, M.R.C.P. & F.R.C.S.

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AS aneurism of the larger cerebral arteries is of somewhat rare occurrence, I venture to lay before the Association the following notes of a case which came under my observation, and which I think will prove of sufficient interest.

N. B., a labourer aged 42, was admitted into the Kent Asylum in the year 1866. His general health was weak, and he was in a condition of mania. This was the first attack, and was of about three years' duration. He was cleanly in his habits, and neither epileptic, suicidal, nor dangerous. The cause of his insanity was stated to be unknown, though his sister was an inmate of the Asylum. The chief feature of the mental symptoms on admission was a constant complaint that persons in the neighbourhood had entered into conspiracies against him. He also stated that his father and mother were both weak in their minds, that his flesh turned of different colours if he ate certain kinds of food, and that he had a dread of being sent to prison. He was very "nervous", and there was marked muscular tremor.

For two years after admission, no material change occurred, either in his general health or mental symptoms. The latter, however, then became more intensified, without any apparent cause. His fear of the conspiracies and dread of being sent to prison, were more frequently mentioned by him. After a time, these symptoms again toned down considerably, and no change in his condition worthy of note occurred until 1872, i.e., six years after admission. He then became very violent and excited, and continued for a period of six months more or less noisy and excited, and had commenced using language of a disgusting character. The exhibition of a cold shower-bath of half a minute's duration every morning seemed to control this excitement; he became somewhat better, and the bath, having been administered every morning for a month, was discontinued. The beneficial results of this treatment were permanent; he continued quieter, and worked daily on the farm, and was looked upon as an useful patient, who, though somewhat abusive and excitable at times, was yet fairly amenable to reason, gave but little trouble to the attendants, and was regular in his habits.

After having been for a period of eight years an inmate of the Asylum, for the last two years of which time there had been no change in his general health or mental symptoms, he died in a very sudden and unexpected manner. There had been nothing to indicate the disease which so suddenly destroyed him; his general health was apparently good; he worked regularly, slept well, and took his food as usual.

The manner of his death was as follows: After working on the farm during the day, he was sitting in the ward, chatting with his fellow patients round the fire, when, without any warning, he fell forward on to the floor; his face became livid, and he appeared to be struggling for breath. The attendant observed several pieces of carrot in his mouth, and not unnaturally imagined he had been eating it, and that a piece "had gone the wrong way". An exploration of the fauces and glottis was made by the finger, and the obstruction not being within reach, tracheotomy was performed and artificial respiration resorted to. The breathing, however, became spasmodic, and death rapidly supervened.

Post Mortem Examination, twenty-five hours after death.—The body was well nourished, the lower extremities slightly oedematous. The calvarium was remarkably dry, the pericranium easily peeling off, and a quantity of serosity was observed issuing from the various foramina. The dura mater was firmly adherent, especially along the course of the longitudinal sinus, where its attachments were marked by a number of large distinct patches of lymph. The pia mater was separated with difficulty from the convolutions, which were abnormally pale. On severing the usual attachments at the base, and turning out the brain, a large blood clot was observed in the fissure of Sylvius on the right side. On commencing the examination of the brain, by removing the hemispheres, the brain substance generally was found to be of firm consistence and pale in colour. The lateral ventricles were

* Read in the Section of Medicine at the Annual Meeting of the British Medical Association in Manchester, August 1877.