I feared I had to deal with a case of masked pericarditis. I, however, thought that the nervous system might be implicated, and on examining the knee reflexes they were found to be exaggerated. Sensation under the usual tests was only very slightly impaired. I could find no indication of paralysis. Up to this point it will be seen that there was no opportunity of examining the urine, even if the symptoms had pointed in the direction of the kidneys. Moreover, I had every reason to believe at the moment that a very complete examination of the urine had recently been made, and at the bedside I remarked, "We may assume then that the water is right." As the patient had suffered severely from sleeplessness it was decided to administer what I considered under the circumstances to be the safest hypnotic—viz., sulphonal. I sent two sulphonal powders containing 25 grains each, addressed to the husband, Mr. F. C. Evans. On the outside of the ordinary square envelope in which they were enclosed I wrote his name and address and "Sulphonal pulv. II. aa. gr. 25." I could not ascertain when they were administered, but the drug should have been given on Friday evening.

On Saturday evening I saw her at 9.45. She was in a semi-comatose condition, and the temperature at this time was 99.4° F. There were stertorous breathing and marked contraction of the pupils. The breathing was fairly regular, though laboured. The patient seemed to me to be under the influence of a preparation containing an opiate, and I treated her on that assumption with the usual means, having remarked that the patient had had an "opiate." She regained consciousness, and I was astonished to hear her say, "Don't let the children know I have done it." Considering the urgency over I did not press for an explanation. A message was sent from my surgery asking me to return. I then left the patient, the husband suggesting that he could "manage." On Sunday morning I received no message, but I sent to inquire. In response I received the following letter:—

"DEAR BUTLER,—My wife rallied after you left and spoke quite sensibly to me. She sat up in bed at 1 A.M. and said she was much better. I lay down at her request then and woke several times during the night, when she seemed quite herself again and expressed herself sorry for having been the cause of so much trouble. When I awoke at 9 A.M. she had again become unconscious and remains so now. I have injected ether twice, one drachm each time, but she does not even feel the needle. I could not give her any coffee, though I made some, as she could not swallow. Bathing the face has no effect. She is almost pulseless. I can hardly count the number, but think it is about 130 per minute. Do not trouble to come if busy; I am doing all I can.

"Thanks for inquiry,

"F. C. E."

Being somewhat astonished at the condition—and especially as a pulse of 130 was totally inconsistent with sulphonal—I visited the patient in spite of the letter, and she died in the afternoon at 1.30. I did not feel justified in writing a certificate without further consultation. The husband and I drew up a statement of facts and laid them before Dr. George Robertson, whose judgment on this bare statement was that the certificate might be granted, stating that death was due to—anaemia, six months; influenza, four days. To this at the moment I felt bound to submit, but insisted to the husband in a private interview (there and then) that I should have to explain my seeming dissent more fully to Dr. Robertson. I subsequently did so, and he advised to let the matter rest. Next day I wrote the following letter, which I personally delivered at his house:—

38, Carlton-vale, N.W., Dec. 24th, 1895.
DEAR DR. ROBERTSON,—I am decided. In the event of a voluntary examination, do you agree to be present? The result might prove my fears to be groundless. No narcotic; no publicity. To avoid anything of that kind I will go any length consistent with honour, and I shall be thankful for any suggestion towards sparing poor Evans's feeling as much as may be.
Yours sincerely,

G. R. BUTLER.

That evening I communicated with the coroner, and on the following day he decided to hold an inquest. This he did on Saturday last. The death was there described in highly scientific terms as due to "stoppage of the heart's action," and I was ordered to be "severely censured." Dr. Gordon Hogg accordingly severely admonished me "for prescribing a dose of sulphonal without first making a proper examination" and for "carelessness." These are the circumstances, and I have no doubt they will be helpful to many of my professional brethren. I may, however, add that the patient had had for a long period of time a fear of impending death and other morbid ideas. Regarding the dose of sulphonal, I scarcely need say that twenty-five grains would do very little harm under any

circumstances when properly administered. In the *Archives für Experimentelle Pathologie und Pharmacie*, Leipzig, vol. xxxi., No. 1, Kast gives the symptoms of poisoning by sulphonal as vomiting, diarrhoea, nervous and urinary phenomena, and states that it is generally admitted that doses of thirty-one to thirty-six grains may be safely administered. Given within those limits he describes it as a "harmless medicament." The cases of poisoning by sulphonal have always occurred after prolonged use of the drug in enormous doses. Schaumann insists that it is preferred to chloral hydrate, because it has no injurious effect upon the heart and vessels. Schedtler has published forty-one cases of female patients to whom doses of thirty-one grains have been given for prolonged periods without any untoward symptoms, and forty-six grains have been very often necessary especially in "melancholia with anxiety." It is not enlightening to hear a jury of tradesmen discussing the therapeutics of sulphonal; but it seemed to me deplorable that the police surgeon, who officiated in place of an expert, should betray so much unfamiliarity with so common a drug as to insist that five grains might be a dangerous dose. Such a statement is perfectly absurd under any circumstances, and a medical man has a right to expect that a medical coroner should be able of his own knowledge to defend a brother practitioner from such assertions. I challenge his accuracy. Dr. Hogg knows perfectly well (if he knows the literature) that twenty-five grains of sulphonal is a non-toxic dose. The symptoms were not those produced by sulphonal, and he did not take pains to inform the jury of the nature of the contents of the stomach before he accepted their verdict. The verdict of "Died through the visitation of God," which the old legal coroners gave, does not excite more contempt in the minds of practitioners than that of Dr. Hogg's jury of "Death from stoppage of the heart."

I have to complain that I was not permitted to be present to hear Mr. Evans's evidence. One would have thought that the coroner would have been pleased to have my assistance. How could he expect it if I did not hear the whole of the evidence? The circumstances appear to me to indicate that medical men are not safe from "public censure" or even public insult, although using every endeavour to exercise their calling with the honour which is its due. In this case I have been compelled to go into the forum and attempt the impossible task of explaining the matter *coram populo*.

I am, Sirs, yours faithfully,

Carlton-vale, N.W., Jan. 1st, 1896.

GEORGE R. BUTLER.

* * Assuming that the above letter fully represents the facts of the case, we are of opinion that the sulphonal (gr. xxv.) had nothing to do with causing the death. Considering that our correspondent assumed that an opiate had been taken, and especially after the alleged statement of the deceased, we think he would have acted wisely in declining to entertain the question of giving a death certificate without first having informed the coroner. As regards the circumstances related as having occurred at the inquest, we think Mr. Butler was not dealt with too generously.—ED. L.

"PARACENTESIS OF THE THECA SPINALIS."

To the Editors of THE LANCET.

SIRS,—*Apropos* of Professor Quinke's paper, read before the Association of German Naturalists and Medical Men, on "Paracentesis of the Spinal Theca," in which he says that failure to secure a flow of fluid is to be attributed to imperfect performance of the puncture—e.g., the passage of the needle between the nerve roots outside the sheath or the presence of thick pus which will not flow through a too fine needle—Professor Fürbinger writes, in an article in the *Deutsche Medicinische Wochenschrift* of Nov. 7th, 1895, in reply to a remark of Professor Ewald, who had several times observed that the puncture produced no fluid although it had penetrated into the spinal theca, the following:—

"I observed in the concluding remarks of my paper, read before the Berlin Medical Society, on 'Clinical Importance of Spinal Puncture,' that I frequently had the same experience as Ewald. I had succeeded in drawing hardly any liquid, without the needle having passed the surface of the dura. I consider these facts as pathological, because the average corresponds to a considerable quantity of cerebrospinal fluid in the lumbar sac. These statements have not,

however, prevented Quincke, in the meeting of naturalists and medical men, from observing: 'The failure regarding the negative result of the puncture, such as Fürbringer describes, may be attributed to technical errors.' I do not know whether the speaker entirely meant this, as I observe that the sentence is missing in the original publication, which has just appeared. It remains, however, uncontradicted in various medical papers, which fact justifies my going into the matter thoroughly, and trying to find out with what right such a reproach is made. That can hardly be done by the way of theoretic reasoning, but a hard matter-of-fact basis in the form of a post-mortem discovery is necessary in such 'negative' cases, an expression, perhaps, more concise than absolutely correct. Only direct inspection of the cerebro-spinal sac can give a certain answer to the question whether technical errors have been avoided or whether the needle really 'slipped to the side' or between the roots of the 'nerves,' or whether it was 'too short' or 'too thin.' I am able to answer this question, as I—quite contrary to my usual custom—in April last performed lumbar puncture fourteen times on a child with negative results, and the pathological anatomist showed me, *ad oculos*, why I had produced no fluid during life. The case (although by no means unique in my own experience, and for experienced pathological anatomists not especially rare) is of great interest and of importance in the question we are discussing; I therefore give a short *résumé* of it.

"The patient, a child aged five months and a half, had, according to the statement made by the parents, five weeks before he was brought to the Friedrichshain Hospital on April 8th, 1895, received a violent blow on his head. Five days before admission to the hospital vomiting had occurred, and since then fever and convulsions. The last two days the child had taken no nourishment. The parents were healthy. On examination the child was found to be in a good state of nourishment, but suffering from fever and torpor. There was decided opisthotonos. The face bore an expression of pain when the head was moved. The arms were held forward. When touched the child screamed. The body was much swollen. The large fontanelle was decidedly prominent and much stretched. Partly because of the last-mentioned fact, and partly because the diagnosis wavered between tuberculosis and purulent meningitis, I at once applied spinal puncture, and in spite of the hopeless nature of the case performed the operation eight times at different parts of the lumbar spine. Although I had not the remotest doubt that the needle was free in the lumbar sac I did not succeed in procuring one drop of cerebro-spinal fluid, not even after I had (against my principle) applied aspiration. The temperature became almost normal, otherwise the patient remained in the same state. In the night the temperature rose again. Next morning I again tried lumbar puncture. In spite of six injections only a drop of blood was discharged, but no other fluid. On April 9th the patient died.

"The result of the necropsy made the following day by Dr. Hansemann was as follows. The lymphatic gland, as in the root of the lungs as far as the jugulum, had become caseous; the lungs, spleen, liver, and mesenteric glands were permeated with numerous, mostly fresh, tubercles. Regarding the state of the brain and spine I quote Hansemann's *dictat* word for word: 'The front fontanelle was much extended. On opening the brain a large quantity of clear fluid was discharged, which proceeded solely from the ventricles, while the gyri were much flattened on the surface and the pia firmly attached. The whole of the base is converted into a gelatinous yellowish-white oedematous mass permeated with numerous tubercles. This gelatinous mass extends along the spine, on the posterior as well as the anterior surface, so that the dura is much stretched, not from the liquid but from this oedematous, spongy mass, which is here, as at the base of the brain, permeated with submiliary tubercles. Loose, flat blood coagula lie on this mass between it and the dura, chiefly at the cauda equina, and extend over the lumbar swelling, where they are all confluent; higher up, however, they again become scanty and decrease in size as they approach the cervical spinal cord.' I add to the above statement that, as far as recognisable, all the punctures in the lumbar region—I naturally only punctured in this region—perforated the dura mater, and that there was no sign of any damage to the nerve roots of the cauda. The lumbar punctures did not, therefore, as this precise anatomical inquiry proved, fail because the needle slipped to the side, also not because it was too long or

too short; finally, also not because it penetrated into the nerve roots or the spine, but simply because, by reason of the morbid alterations, no fluid existed. A 'technical error' had never been suggested. It seems to be worthy of notice in Lenhartz's above-mentioned statement in the discussion of Quincke's paper that out of fifteen cases where he had performed puncture the majority were without result, although the needle was doubtless in the vertebral canal, and, further, Von Ziemssen's observation that it is surprising to find with uræmia only a few drops of fluid in the cerebro-spinal sac in spite of the high pressure. I must observe that out of my three or four 'quite negative' cases one patient suffered from uræmia. Anatomical investigation was unfortunately not possible in any of these cases. In two of the cases, where basal meningitis existed, I suspected similar complications as in the above-described case. Finally, I must not omit to mention that the post-mortem examination of a uræmic patient, in whom lumbar puncture had only produced a few drops of cerebro-spinal fluid, revealed the same in 'vanishing quantity' in the region of the spinal sac. Thus in this case also the illness, and not the physician, was responsible for the failure of the puncture. I shall, therefore, continue in further 'negative' cases—naturally not without careful examination in spite of my ever-increasing practice—to exclude the possibility of technical errors on my part, but my next step will be to consider the opinion I expressed seven months ago as corresponding to the pathological state. It is possible that lumbar puncture may be considered in this direction from a new point of view, rather scientific or diagnostic than practical or therapeutic. With regard to the latter, I refer to later reports of my experiences. I recognise with pleasure the unexpectedly favourable opinion which Quincke and Von Ziemssen have given on the therapeutic value of the method, and which spurs me on to bring lumbar puncture (latterly somewhat neglected) in my wards to a state of perfection."

I am, Sirs, yours truly,

Baden-Baden, Dec. 1895.

W. H. GILBERT, M.D.

"WORD-BLINDNESS AND VISUAL MEMORY."

To the Editors of THE LANCET.

SIRS,—The article by Dr. James Hinshelwood in THE LANCET of Dec. 21st, 1895, is one of the most, if I may be allowed to say so, interesting and instructive papers which has appeared in THE LANCET for many a long day—aye, and for many a short one too. The article has a special interest for me because Dr. Hinshelwood mentions, besides word-blindness—(1) "visual memory for places"; (2) and (3) "visual memory for form and colour"; and (4) "visual memory for figures," each having localised cortical centres in the brain; for, as Dr. Hinshelwood states, "there exist partial, special, or local memories, each of which has its own special domain." Now phrenology, which has been a pet subject with me for many years, and on which I have written a small book, has conditions showing all these four memories. 1. "Visual memory for places" is locality in phrenology. Dr. Gall, the originator of the study, noticed, when he was a boy at school, that one of his schoolfellows, although of moderate abilities, could always find the trees in which they had set snares for birds without the slightest hesitation, whereas he had the greatest difficulty in discovering those they had marked. Later in life Dr. Gall, putting a plaster cast of this boy's head side by side with casts of men who had the gift of finding without any difficulty places they had once seen, was able to satisfy himself that locality was marked by an enlargement above the eyes on each side of the root of the nose. Dr. Gall termed the instinct *sens des localités, sens des rapports de l'espace*. (2) "Visual memory for form and (3) colour" Dr. Gall termed *mémoire des personnes, sens des personnes, sens des rapports des couleurs*. He placed the former enlargements on the inner sides of the superciliary ridges, and No. 3 in the outer parts of the ridges, in close approximation to No. 4. In connexion with (4)—"visual memory for figures" (*sens des rapports des nombres*)—Dr. Hinshelwood mentions the "calculating" men of the present day, MM. Inaudi and Diamandi. In the early part of this year I interviewed M. Inaudi in Paris at the Hôtel St. Pétersbourg, at the time he was exhibiting at the music-hall Olympia. He told me that his extraordinary memory for figures, is, as Dr. Hinshelwood states is in the