necessary forms for treatment; for if they do not sign, the less capable will do it in all cases of emergency, and thus the best men will lose their hold on one of the most important of their medical functions. No good will be attained by an attempt at a blockade, for this can never be complete, and the patients will certainly suffer by the delay in treatment.

It appears to me that those who refuse to sign are placed in an awkward position; for they either say "I recommend a treatment which I am not prepared to assist you to get," or else they appear to agree with the general wild outcry against patients being sent to asylums at all. If the patients can as well be treated at home, why has it been so necessary hitherto to send so many to asylums? The doctor is self-condemned, by formerly signing and now refusing to sign.

Another point arises as to who should sign lunacy-certificates. I believe, for many reasons, that the family medical man is one who should take that responsible duty, and I think that, where attainable, an independent physician should be selected for signing the second. I shall be told that this last class is the one most given to decline to sign certificates at present. Many, I know, are refusing, and I believe the reasons are that the majority are not sufficiently versed in the laws of lunacy, and do not think it is worth while to get this knowledge up, and, to avoid bother, they decline to do their duty. In lunacy, there is much difficulty caused by ignorance of the clinical history. In an ordinary case of disease, the general physician is able to piece together the facts observed by himself, and the history of the development of the symptoms given by the patient or his friends. But in insanity this is different; before a certificate can be signed, the medical man must assure himself of facts of insanity present at the time; but there are many slight signs which, to the more expert, are sufficient indications of disease, which, to the less expert, would not suffice to satisfy him who has to sign the certificate. The time is nearing when a more general knowledge of insanity will make the recognition of the symptoms more easy. With the consulting physician, then, I should say ignorance of insanity and ignorance of the lunacy laws at present prevent them from fulfilling their duties. It may be supposed, then, that I should like to see a special set of men appointed to fill up lunacy-certificates; to this I should most emphatically reply "No." If we get an official set of signers, we shall get all the faults of officialism; most of the certificates will be signed, as a part of a duty to be done as easily as possible; the safety of the public will be no more secure in the hands of the official signers, than it has appeared to be in the hands of magistrates or poor-law officers. My own feeling most certainly is, that both the general practitioner and the pure physician should sign certificates for patients who require seclusion, and that they have no right to let the patient be driven from pillar to post on account of their fear, till some catastrophe happens to the patient or his friends.

There is one anomaly out of which it is hard to see any way; it is that those who are most skilled in lunacy, asylum-physicians, are prevented from signing certificates. The Commissioners look most jealously at any certificate which has been signed by an asylum-physician, at least for a fee. There are reasons why there should be no chance of a patient being confined by certificates under the man who signed them, but this has been already arranged for in the Lunacy Laws. The question of the difficulty in getting certificates is not a sentimental one, and I wish I were in a position to place before you in black and white the evidences of the tragedies which have resulted from failure to get certificates signed. I know of both suicides and homicides, which might have been prevented if medical men had not been afraid to do their duty.

To continue this paper to a practical conclusion, I must add a few special details after all this general admonition.

There are some cases in which there ought to be no doubt as to the necessity for certificates. Such are cases in which there is marked and active suicidal tendency, and also where there is direct evidence of homicidal impulses or delusions, such as lead to homicidal attacks. I know it is impossible for even the most careful expert to detect all the conditions of mind which may lead to suicide; but, as a rule, the friends have been able to find out enough to satisfy the medical man as to whether there is real danger or not. For convenience, too, I should say that, if you have made up your mind that a patient is suffering from general paralysis of the insane, there need be little or no fear of legal trouble; for though there may be threats, the mind, slowly becoming weaker, leaves the patient easily led.

There are some dangerous cases in which every precaution and every guarantee should be sought. For, though it has been frequently ruled that a man, suffering from temporary insanity due to drink, may be sent to an asylum, yet I should say that, of all the litigious lunatics, those

from drink are the worst. The acute alcoholic, having experience of his former recoveries, thinks he might just as well have been allowed to get well at home, as to be sent to an asylum, and he is dissatisfied; and the chronic alcoholic is as perverse morally as a man can be, and never, even at his best, perfectly recovers his balance, so that he still harbours vengeance against those who have acted for him in his illness. The more I see of insanity, the more I feel that, of all cases, those due to drink are most unsatisfactory, both from their frequent recurrence, and the want of gratitude of the patients; so beware how you sign for these, but still do not refuse to sign, but guard yourself. A good deal has been said from time to time about getting indemnities from the friends before signing the certificates; and I have heard that such guarantees have proved of service on several occasions, rather by preventing the patient from taking an action which would involve his nearest relation in expense than from any other cause. It is interesting to hear from Dr. Hack Tuke that guarantees were in use twenty-five years ago; and I suppose this points to the recurrence of the scares, and to the recession of panic which I prophesy will follow all this storm. There are many other words of caution which I might give; but the object of this paper is accomplished if both the general practitioner and physician will take their part in allaying panic, and I believe they will do this by doing what is most certainly their duty in signing certificates.

SOME REMARKS ON THE PRESENT STATE OF OUR KNOWLEDGE OF THE COMMA-BACILLI OF KOCH.

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THE reason of my making a few remarks on the above is a twofold one. 1. Certain observations were made by my friend, Mr. Watson Cheyne, at a recent meeting of the Royal Medical and Chirurgical Society (March 24th, 1885), and printed in this JOURNAL (March 28th, p. 655), which appear to me to be based on a complete misunderstanding on the part of this gentleman, as to what Koch has really said and taught as regards the choleraic and other comma-bacilli, and I am, therefore, anxious that the members present at that meeting, and the readers of those observations, should have a correct idea about these matters. 2. I wish to define more fully than I was able to do at that meeting, owing to the lack of time at my disposal, the position I hold in reference to the comma-bacilli.

1. My friend Mr. Watson Cheyne said this: "Koch defined a comma-bacillus as a curved organism, which, when grown in gelatine in tubes, had certain characters; on potatoes, certain other characters; and in plate-cultivations, also special characters. The sum of these characters gave us a definition of the comma-bacillus. The idea seemed to have got abroad that Dr. Koch defined the bacilli found by him in Asiatic cholera by their microscopic appearances, and that they were comma-shaped."

Now, I ask the reader to turn to page 22 of Koch's pamphlet (*Conferenz zur Erörterung der Cholerafrage*), or to the literal translation of it in this JOURNAL, September 6th, 1884, p. 453, and he will find this statement of Koch's: "In Egypt, ten *post mortem* examinations could be turned to account. It is true these were only microscopically examined, for I was not then sufficiently acquainted with the qualities of the comma-bacilli, which they show while growing in food-gelatine, to be able to make use of the gelatine-process for proving the presence of bacilli." (The italics are mine.)

Now, where, may I ask, were the comma-bacilli discovered by Koch? In Egypt. Where has Koch stated that the comma-bacillus is peculiar to cholera, and must, therefore, bear a definite relation to the disease? In Egypt. All the beautiful investigations as to the peculiar behaviour of the choleraic comma-bacilli in gelatine were made by him subsequently in Calcutta. But their discovery, their description, and their specific relation to cholera, were asserted by microscopic examination only. And lest there should be any doubt on this point, Koch says (see the translation of his pamphlet, in this JOURNAL, August 30, 1884, p. 404): "These bacteria, which I have called comma-bacilli, on account of their peculiar shape, are smaller than the tubercle-bacilli."

Mr. Watson Cheyne further said: "Now, Dr. Koch had stated that, having this fact in view, he had searched for these organisms in saliva, in faeces, in all sorts of decomposing materials, and in other materials

and in other diseases, and had failed to find them..... When Dr. Koch said that he had searched for these organisms, he meant that he had not only used the microscope, but had tested the materials by cultivation; and when Dr. Koch said that he had failed in this search, he meant that he had not found an organism which presented the same microscopic appearance, together with the same characteristics of growth, as that discovered by him in Asiatic cholera."

This is what Mr. Watson Cheyne says, but it is not what Koch said. I ask, again, the reader to turn to Koch's pamphlet, or to its translation (BRITISH MEDICAL JOURNAL, September 6th, 1884, p. 453). This is what Koch says: "In accordance with the cholera-material that I have so far examined, I think I can now assert that comma-bacilli are never found absent in cases of cholera; they are something that is specific to cholera. As a test, a considerable number of other corpses, dejecta from patients and persons in good health, and other substances containing bacteria, were examined¹ to see if these bacilli (*i.e.*, comma-bacilli), which were never missing in cases of cholera, might, perhaps, occur elsewhere also. This is a point of the greatest importance in judging the causal connection between comma-bacilli and cholera.".....

"In all these cases, where we had to deal chiefly with diseases of the intestine, no trace of comma-bacilli was to be found. Experience teaches that such affections of the intestine make people especially liable to cholera. So one might have presupposed that comma-bacilli, if they were to be found anywhere else, must be found in these cases. Besides these, dejecta of a large number of dysenteric patients were examined without the comma-bacilli ever being met with.".....

"We examined a considerable number of various dejecta, especially of children's diarrhoea, as well as that of grown-up persons; saliva also, and the mucus that adheres to the teeth and tongue, and which abounds in bacteria, for the purpose of finding comma-bacilli, but always without success.".....

"Nor were they found in the sewage from the drains of the town of Calcutta, in the extremely polluted water of the river Hooghly, in a number of tanks which lie in the villages and between the huts of the natives, and contain very dirty water. Everywhere, where I was able to come across a liquid containing bacteria, I examined it in search of comma-bacilli, but never found them in it.".....

"Besides these observations, I have had a considerable experience in bacteria, but I cannot remember ever having seen bacteria resembling the comma-bacilli." (The italics are mine.)

"I therefore think I may say positively that the comma-bacilli are constant concomitants of the cholera-process, and that they are never found elsewhere."

Is anybody, after reading such statements, unreasonable enough to say that Koch had, at the time he wrote them, *i.e.*, after he had concluded his researches, carried on in Egypt, India, and in France, the slightest inkling that comma-bacilli occur in almost all those conditions where he failed to find them, or that at that time he considered, as he now does, the culture-test as the essential means of distinguishing the choleraic from other comma-bacilli? If Koch had had the remotest idea that there exist other comma-bacilli than those in cholera, surely he would not have spoken in that general way of comma-bacilli. If Koch, who in everything he says is so explicit, had known that there exist all those different comma-bacilli that we now know to exist, he would, one might reasonably expect, have said: "I have found other comma-bacilli in a variety of conditions, but they differ from the choleraic comma-bacilli in their mode of growth in gelatine." But there is nowhere a word of a culture-test, and he expressly says: "that everywhere, where I was able to come across a liquid containing bacteria, I examined it in search of comma-bacilli, but never found them in it."

I challenge anyone to show me in Koch's pamphlet, *i.e.*, when he had concluded his researches in India, Egypt, and France, and laid their results before the Cholera Conference in Berlin, one single word that would indicate that he had the slightest idea that there occur comma-bacilli other than those in cholera.

Mr. Watson Cheyne further stated (JOURNAL, March 28th, 1885, page 655): "The only points of difference between Dr. Klein and other observers in this matter was as to the numbers (that is, of the choleraic comma-bacilli) present, and as to whether they were found in the intestinal wall as well as in the contents. The estimate of the numbers present depended very much on the way in which the examination was made. If the microscope alone were trusted to, a

very much smaller estimate would be made."..... I ask, again, the reader to turn to the translation of Koch's pamphlet in this JOURNAL, September 6th, 1884, page 453. This is what Koch says: "But the investigation has not only proved the existence of the comma-bacilli, but, as I have repeatedly hinted, they always stand in exact proportion to the cholera-process itself; for where the real cholera-process proper caused the greatest modifications in the intestine, they were to be found in greatest numbers; from these upwards, they diminished more and more. In the most complicated cases, they appeared almost like pure cultivations." This is plain enough; the more typical and acute a case, the greater the number of comma-bacilli in the ileum; and it is precisely this point, which, as I shall presently show, is of fundamental importance, but as regards which the English Cholera Commission emphatically differ from Koch; and this difference against Koch is shared by other independent workers (the French Cholera Commission, Dr. T. Lewis, and others), who have had sufficient experience in the matter.

2. The precise position with regard to the comma-bacilli of Koch is this.

a. Koch maintains that, in acute typical cholera, the lower part of the ileum swarms with the comma-bacilli, "almost a pure cultivation." In order to uphold his theory as to the relation of the comma-bacilli to the disease, it was essential for Koch to show that such a condition really obtains; for, the comma-bacilli being absent from the blood and other organs, and they being present in the intestine only, he was necessarily led to assume that they produce a chemical ferment which is absorbed by the system, and which produces the symptoms of the disease. In severe, rapidly fatal cases, the amount of this ferment must be great, and consequently also the number of the bacilli producing it.

But the facts are opposed to these statements, for the number of the comma-bacilli present in the lower part of the ileum stands in no relation whatever to the severity of the disease. Besides, there is this fact, that, contrary to Koch's assertion, the whole of the small intestine in several typical rapidly fatal cases presented an uniform appearance, the alterations extended equally to the whole small intestine. But in these cases, there were no comma-bacilli present except in the lower part of the ileum, and here they were present in very small numbers indeed, the *post mortem* examination having been made very soon after death.

b. The comma-bacilli of Koch do occur occasionally in the tissue of the mucous membrane of the ileum, and they can then be easily demonstrated by the ordinary methods of staining; but in these instances, I maintain, either owing to delayed *post mortem* examination, or owing to the patient having remained in *articulo mortis* for some hours, the comma-bacilli, as well as other bacteria, have immigrated into the tissue from the cavity of the ileum. Such cases will be minutely described in the report of the English Cholera Commission. Several typical rapidly fatal cases have come under observation, in which the most persevering search, the most approved methods of staining—and a good many methods generally used for staining of bacteria in the tissues, as well as new modifications of such staining methods were employed—did not reveal a trace of comma-bacilli or any other bacteria in any part of the wall of the intestine, inclusive of the lower part of the ileum at and around the Peyer's glands, not even in the superficial epithelium, loosened, but not quite detached.

c. From this the conclusion is inevitable, that the comma-bacilli occur only in dead tissues—the fluid and mucus flakes in the cavity of the intestine; and, as we descend towards the lower part of the ileum, their number increases.

d. As far as the observations hitherto go, there is no doubt whatever that the comma-bacilli of Koch are a distinct species, characterised by their behaviour in cultivations in gelatine-material, and that this distinct species occurs in the intestine of human beings suffering from cholera; but whether they occur here in numbers in consequence of the peculiar condition of the intestine in this disease, or whether they are the cause of it—that is to say, whether *post hoc* or *propter hoc*—is, I maintain, an open question, which Koch has not proved, and which he has not attempted to prove. And this question will remain unanswered until the examination of the intestine, under precisely similar but not choleraic conditions (that is, distension of the intestine, and exudation into it of clear fluid and mucus flakes) is made, and until it is found that these same comma-bacilli are always absent. That I should be obliged to prove this (as I am supposed to be by a "Fellow of the Society," see this JOURNAL, March 28th, 1885, page 675), I cannot admit; this is manifestly the business and duty of Koch, who maintains that these comma-bacilli are present only in cholera. But I propose shortly to make such an examination, and Dr. Moreau and Dr. Lauder Brunton have indicated a method of establishing such a condition.

¹ In the German original, there occur here the words: "In the same manner"; but it is quite clear that these words refer to no culture-test, but to a previous paragraph, in which he states that he had examined microscopically nearly 100 cases in all for the presence of "comma-bacilli," and had found them always.

e. I maintain that the salivary comma-bacilli, after acclimatisation through neutral nourishing media, are capable of growing in gelatine nutritive material, and then present the same appearances of growth as the choleraic comma-bacilli. Koch's comma-bacilli themselves, under varying conditions, show similar variations (as will be fully described in the report of the English Cholera Commission). To mention one only: when the choleraic comma-bacilli are mixed with hydrochloric acid (1:1000) for from ten to fifteen minutes, and when, after this, they are sown into alkaline nutritive gelatine, they do not grow at all, or only with great difficulty; but, on sowing them after the treatment with hydrochloric acid in alkaline broth, they grow well, and, then transferred to alkaline nutritive gelatine, they show copious and typical growth.

f. I maintain that the small straight bacilli, which were mentioned by the English Cholera Commission as occurring as constantly as the comma-bacilli in the choleraic intestine, free, in clumps and in the substance of lymph-corpuscles, are a distinct species: their very minute size (one-fourth to one-sixth of that of the comma-bacilli), their permanent shape, and their behaviour in cultivations (they do not liquefy gelatine, and grow chiefly and best on the surface of the nutritive material, prove this conclusively. That the comma-bacilli show variations in length and curvature when grown under various conditions, with this I am perfectly familiar, and these variations will be minutely described in the Report of the English Cholera Commission (see also Koch, Fig. 3, p. 8, of his pamphlet).

g. The experiments performed by Koch on animals in Egypt and in India, as well as other inquiries, led him to say that cholera is not transmissible to the lower animals. Koch had, in several instances, death in animals (rodents) after inoculation with comma-bacilli, and he has ascertained that the comma-bacilli are capable of growing and multiplying in some of these animals; but, nevertheless, he accepted the death of these animals as due to septic poisoning (see his pamphlet). Not only experiments by subcutaneous inoculations and feeding were performed by him in great numbers, but direct injection into the duodenum was practised (see his pamphlet, p. 28), but no result was obtained. Now we are told that he considers, with Nicati and Rietsch, the injection of choleraic comma-bacilli into the duodenum of dogs productive of fatal disease, owing to the pathogenic properties of these bacilli. A large number of such experiments were made by me in Calcutta on rabbits, cats, dogs, and monkeys, with choleraic mucus-flakes full of comma-bacilli, with pure cultivations of choleraic comma-bacilli, and no result was produced. Mr. Horsley, Professor at the Brown Institution in London, has made for Mr. Dowdeswell and myself a considerable number of such injections into rabbits, guinea-pigs, and dogs; no result followed. But admitting, for the sake of argument, that Koch has discovered a method by means of which he can produce death in dogs after injection of the comma-bacilli, is this cholera, and does this prove that the comma-bacilli are the cause of cholera in man? Do we not know that there occurs in normal human fecal matter a species of bacillus, isolated by Bienstock, which produces fatal disease in some rodents? Do we not know that in normal human saliva there occurs a species of bacterium (isolated by Pasteur and Sternberg) which produces fatal disease in animals? Is it not known that rodents, particularly rabbits, die sometimes after slight operations, and with symptoms of diarrhoea, no bacteria of any kind being found in the blood? I have isolated from human pneumonic sputum a micrococcus which produces fatal disease in rabbits, diarrhoea being one of the prominent symptoms.

CLINICAL MEMORANDA.

ACUTE MANIA, FOLLOWING A SURGICAL OPERATION.

J. K., a middle-aged farmer, previously in the enjoyment of good health, on January 11th, 1884, had the anterior part of his foot crushed in the drum of a thrashing machine. On the same day, with the assistance of Dr. Ryott, who was associated with me in the treatment of the case, I amputated part of the foot.

Between the fifth and ninth days, the plantar flap sloughed, and during this time the temperature varied from 100.3° to 101.7° Fahr. The pulse being then very intermittent and irregular, he was ordered three glasses of port wine daily.

On January 20th, the general condition of the patient was improved, and the wound was healthy and granulating.

On January 21st, it was first noted that he had definite delusions, thinking that he was driving horses, and that Dr. Ryott was going to set up a butcher's shop.

During the next five weeks he suffered from mania without delirium. He at first developed a suspicious dread of being poisoned, and

showed a great antipathy towards the persons who, he stated, had been concerned in the act of poisoning. A certain shrewdness, however, was displayed; for instance, he wished to know if the richest of the poisoners would not like to square the matter by giving him a promissory note for £500. He wrote for me to come at once, and put in the postscript, "please bring your stomach-pump." His delusion passed on to have a religious tendency; nothing could be said without an invocation of the Trinity, and his conversation consisted mainly of passages from the Bible, together with references to "the Heavenly Glow." About this time, too, an obscene tendency developed itself in his conversation. He had revelations from people in another world; he had communications with me when I was at a distance; he discovered that he and I were brothers. During the whole of this time, he was quite clear on certain topics. He would direct me the way to a place to which I wished to go. He would remember, and refer to, events occurring on a previous day. On reading a letter, he would notice errors in spelling, and even the omission of stops.

On February 29th, the patient was sitting up, and did not allude to any of his delusions, beyond saying that he had gone through a great deal. After this, the patient gradually recovered his reason.

With regard to family history, I found that a paternal uncle was at one time so despondent, that he had to have a male attendant; but on being assured, by his brother, that he should never want for anything, he became all right, and continued so until his death. With this exception, the family history was negative of insanity.

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PATHOLOGICAL MEMORANDA.

A CASE OF RUPTURE OF THE HEART.

CASES of spontaneous rupture of the heart in young persons are sufficiently rare to deserve recording. In the following case, the patient, a young woman, aged 19, had been apparently healthy when she went to bed on the night before her death. She had had an attack of rheumatic fever when 15 years old, but had not lately complained of feeling ill. She had had no fainting attacks, nor was there any arcus senilis present. A slight loss of memory had been noticed quite recently.

On March 17th, 1885, a little before 7 A.M., I was sent for to see the patient, who was said to be dying. I found her lying in bed on her back, insensible, almost pulseless, her eyes closed, and pupils widely dilated. In a quarter of an hour after my arrival, the heart had ceased to beat.

On March 18th, thirty-two hours after death, the necropsy was made. The body was well nourished, and there were no marks of violence. The membranes of brain were adherent to the skull-cap; the brain-substance was healthy; and there were between one to two teaspoonfuls of serum in each lateral ventricle. On opening the pericardium, there was seen on the anterior surface, near the apex, a slit fully one inch and a half long, which led into the left ventricle, and extended irregularly upwards towards the septum. Slight pressure on the heart caused fluid blood to pour out through this opening into the pericardium, in which there was previously about an ounce of coloured serum. Several of the chordæ tendineæ were also found ruptured, and the aortic valves were incompetent through old adhesions. The lungs were congested, but not diseased; the liver was adherent to the diaphragm; the stomach was healthy, and contained partially digested food; the other organs were healthy, and the uterus unimpregnated.

The special points of interest in the case are the youth of the patient, and the almost total absence of any indication, during life, of fatty degeneration.

H. NELSON HARDY, F.R.C.S. Ed.

OBSTETRIC MEMORANDA.

TREATMENT OF NEGLECTED SHOULDER-PRESENTATION.

I HAVE read with interest Dr. Thornley's treatment of his case of neglected shoulder-presentation, but cannot allow his remark, as to my recommendation of a "dangerous operation" for its relief, to pass unnoticed.

Conceding that, in a very large proportion of such cases, under chloroform, podalic version can be performed, yet, I think, it will be allowed by most obstetricians, there are cases in which it cannot, and evisceration is absolutely called for; also that, in most of such cases, either from prolapsed cord or long continued uterine pressure, the