AN ADDRESS ON THE PRESENT KNOWLEDGE OF OUTBREAKS DUE TO MEAT POISONING.

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MR. PRESIDENT AND GENTLEMEN,—Before proceeding to the discussion of the subject of food poisoning I should like to express my thanks to my friend, Dr. Godson, and also to Drs. Bowes, Ashton, Hearn, Clegg, and Wilkinson, for the very material assistance which they have afforded in enabling me to make some investigation into the outbreak of illness caused by eating veal pies in this town last July. I should also like to pay a tribute of gratitude to your fellow townsmen for their share in affording me facilities without the slightest hesitation; in fact, I only met with one "sensible" person who "knew" that at the time of my inquiry it was too late to discover anything about the illness.

HISTORICAL.

The history of our knowledge of the actual nature of meat poisoning dates from the discovery of a very characteristic bacillus by Gärtner in the year 1888. Since then there has been a gradual accumulation of evidence upon the matter by means of careful bacteriological analysis in Belgium, Germany, Holland, and other countries. You will observe—and it is with regret that I have to admit it—that this country does not find a place in the list. The early work which threw light on the nature of the poisons formed by bacteria resulted in the discovery of substances of similar constitution to the vegetable alkaloids. These were called ptomaines and leucomaines. Bodies of this nature were extracted from a great variety of pure and impure cultivations of bacteria in different substances. They were also extracted—but not necessarily present previously—from other poisonous materials, such as snake poison.

During the dozen years which have elapsed since the classical observations of Brieger on ptomaines became known our knowledge has not been allowed to remain at a standstill. And we now know that there are far more important toxic products than these which result from bacterial life. They are different in their chemical constitution from the ptomaines, and are apparently more closely related to substances of an albuminoid or proteid nature. Their poisonous properties are extremely great; in many instances the smallest fatal dose is but a fraction of a milligram (1 milligram = about $\frac{1}{15}$ gr.) when introduced beneath the skin. It is now known that the poisons of venomous snakes, and of some vegetable products—as jequirity and castor-oil seeds—are of a similar nature. But all these kinds of poisonous substances have only a comparatively indirect bearing upon the subject before us, and I have chiefly alluded to their existence on account of the usual practice in this country of calling all illness due to unsound meat as "ptomaine poisoning."

The point which I desire to insist upon is that in outbreaks we have really to deal with actual infections by living bacilli; just as after swallowing water or food infected with properly virulent cholera vibrios or typhoid bacilli, a person may be infected with cholera or typhoid, so after eating meat containing certain bacilli he may become infected with those bacilli, and show symptoms of what in severe cases may be called cholera nostras, for the want of a specific name. According to the dose, the virulence, and the susceptibility of the patient, the result may be anything from a slight passing illness to a severe and rapidly fatal attack. I do not deny that it is possible that some cases may be due to the ingestion of bacterial products solely, but so far we have no evidence that such cases exist; for those in which no satisfactory bacteriological evidence has been obtained must be disregarded. With the exception of Fischer, who asserts that he obtained a poisonous proteid substance from cultures of

Gärtner-like bacilli which withstood $1\frac{1}{2}$ hour's boiling, all authorities are agreed that these toxins are destroyed by a few minutes' exposure to such high temperatures. In the case of the veal pies which caused illness here, it seems evident that those which had been sufficiently cooked contained no poisonous principle (one individual is said to have eaten six of the pies without evil result). It is a well-ascertained fact that comparatively enormous doses of toxins must be given by the mouth in order to produce results which are obtained by tiny doses administered under the skin (for example, snake venom). So far as I have been able to ascertain, the toxic action of ptomaines is based upon experimental injections; it would be interesting to know whether they exhibit poisonous properties when given by the mouth. In every instance where a careful bacteriological examination of fatal cases has been made, an actual infection has been proved to exist; moreover, the precautions which will tend to prevent the occurrence of similar outbreaks of infection will also be of service when mere intoxications have ensued without any actual infection.

BACTERIA CONCERNED.

The essential thing to know is what microbes are concerned with these outbreaks; and I may state at once that we have not to do with the organisms of ordinary putrefaction; the offending meat is usually of good appearance, smell, and consistence. So far as our knowledge goes at present—and as yet we are but on the threshold of the matter—only two kinds of bacilli¹ are definitely related to the causation of outbreaks of meat poisoning; one of these is the bacillus enteritidis first described by Gärtner, the other is the anaërobic bacillus botulinus of van Errenghem. Of these the bacillus of Gärtner must be considered the more important, and to it I shall confine my attention; it has been found in more than a dozen epidemics—for instance, those worked out by van Errenghem, Fischer, Gaffky and Paak, Günther, Holst, John, Kaensche, and Poels and Dhont. My own personal experience is limited to four outbreaks with a total of 256 persons, of whom 9 died. In the first of these, thanks to Dr. Stevenson, I was able to obtain a portion of the liver of a fatal case, and to isolate the bacillus enteritidis from it, as well as to confirm the diagnosis² by serum reactions in some of the patients; in the other three, serum diagnosis has alone been possible, owing to the length of time which had elapsed before my investigation could be undertaken. In all of these the evidence given by this test points directly to the conclusion that the illness was likewise due to a variety of this same bacillus; and I have not the slightest doubt in my own mind that if I had been earlier in the field I should have found that the organs of those who died were swarming with it.

OUTBREAK IN CHADDERTON (OLDHAM).

The Chadderton outbreak is that which interests us at present. Notes upon this outbreak have already been published by Drs. Bowes and Ashton. I obtained blood from 19 persons who had been attacked, and from one who, though he had eaten of the incriminated veal pies, had suffered no ill-effects. The serum obtained from these sources was tested for clumping properties upon seven different varieties of the bacillus enteritidis, two varieties of typhoid bacillus, and another organism (see Appendix); at the same time and previously a number (about 50) of control experiments have shown that the serum of normal persons has not any clumping effect upon these bacilli when properly diluted. In outbreaks like the present, it is essential that a number of different varieties of bacilli should be tested, as, indeed, is also the case when a diagnosis of typhoid fever—and not a mere discovery that a "reaction" is present—is to be made. The reason of this is that varieties can be differentiated by means of their conduct in the presence of immunised sera although they are indistinguishable by ordinary cultural means. Every one of the samples taken from those who were ill gives positive reactions with certain of the varieties of the bacillus enteritidis. At a dilution of 1 per cent. none of them give any reaction with the typhoid bacillus, nor would a reaction be expected, for they react but slightly or not at all with the variety of B. enteritidis (Gärtner) which is most affected by typhoid serum. The sample from the man who had no ill-effects from his meal is perfectly

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negative in reaction, even at 1-20 dilution. The inference is that the epidemic was caused by the presence of the bacillus enteritidis. Have we evidence that this bacillus could have been disseminated in a living condition amongst the pies? There is no such absolute evidence as would have been afforded by the discovery of the bacillus itself.

(1) We have the fact that 4 deaths occurred; this leads to a legitimate conjecture, since, as I have already said, in all fatal cases which have been examined in other epidemics, living bacillus enteritidis has been found.

(2) The fact that two persons became ill after eating other food (cut slices of ham and jam sandwiches) obtained from the same shop whence the fatal pies came. A slight contamination with dead bacterial products is not nearly so likely to have produced such serious effect as an infection of these foods with living bacilli; indeed, one of these persons died. A similar instance is afforded in another outbreak which I have recently been concerned with. One sufferer did not take meals in the house, where he acted as gardener, but he ate a piece of cake which had been placed in a barrow with the infected food from Saturday to Monday. Here, again, though it is possible that only poisonous products were soaked up by the cake, it is far more likely that a bacillary infection, with growth during the following two days, in reality took place. Anyhow, the man was severely ill, and developed a good reaction towards the bacillus enteritidis.

(3) We have the consideration that a positive serum reaction was developed by the sufferers; although by ingestion sterilised cultures of *B. enteritidis* are able to engender the formation of agglutinins, yet it is only when they are given in relatively large doses compared with the amount necessary in the case of living culture; and even then the agglutinins are not so abundantly produced as in the latter case.

Next, let me say a few words concerning the pies themselves. I am indebted to Dr. Wilkinson for information concerning them. It appears that 160 pies were made from veal obtained from the same calf (which was killed on Tuesday, the pies being made on the Thursday, and eaten chiefly on Friday and Saturday), the other ingredients being also the same (see Bowes and Ashton). They were baked "in three to five batches, and not all at the same time." A batch would therefore have consisted of about 53 to 32 pies. The total number of persons who became ill through eating the pies was 54; in some cases single pies were shared by from 2 to 4 persons, so that the number of infected ones is rather less than 54. From these figures it is to be presumed that the whole of one batch was not sufficiently heated, for the fact that in one or two cases pies caused illness, although they were eaten whilst still warm from the baking, suggests strongly that the contamination was not introduced at a subsequent period. All the fatal cases were due to pies which had been kept forty-eight or more hours; this points directly to the conclusion that living bacilli were present. Since the remaining 100 or more pies were eaten with perfect impunity, it is clear that they had been sufficiently heated to destroy living bacilli, and possibly also their products if present. The same may be said of the rest of the carcass, which caused no ill-effects. Two circumstances lead me to suppose that the allowance of time for baking approaches the minimum; the first of these is that only twenty minutes are allowed, the temperature of the oven being 450° to 400° F. (about 230° to 204° C.), whilst, so far as I can gather, in domestic cookery fifty minutes is given as the least time even for a single small raised pie. The second is from an examination of a veal pie kindly sent to me by Dr. Godson; neither the crust nor the meat appeared to have been at all thoroughly cooked. It had been bought on a Monday, and was submitted to bacteriological examination on Wednesday morning. The colonies which resulted from planting minute quantities of the jelly and of the meat were innumerable; those from the jelly and superficial part of the meat were different from those obtained from the deeper portion of the meat; whether the latter were present in a living condition when the pie was taken from the oven I am unable to say, but anyhow the meat did not seem to be completely coagulated. An exposure of one minute to 70° C. is enough to kill the bacillus enteritidis (Basenau).

DISEASE IN ANIMALS.

In many of the known outbreaks the bacillus has not only

been isolated from the organs of fatal cases but also from the suspected meat. It is also a striking fact that in all those cases where the source of the illness could be traced out the animal which supplied the meat was itself in a diseased condition; indeed, cases are not unknown where the sale of the meat was forbidden owing to the condition of the animal, but was surreptitiously put on the market in the form of sausage. The kinds of animal which, so far, have been proved to be at fault, are especially the cow and the calf; the horse, pig, and goat have likewise contributed to fatal infections with Gärtner's bacillus. [Van Ermenghem's bacillus botulinus was associated with a ham; here the clinical symptoms are different (constipation and nervous disorders) from those usually caused by *B. enteritidis*.] Curiously, no case where mutton was concerned has yet been proved. Still more curiously, pork only figures in one (Günther's) of the outbreaks which have been worked out bacteriologically; this is the more remarkable since Dr. Ballard's review in the Local Government Board Reports 1890-91 deals with 12 outbreaks, of which 10 were traceable to some form of pork, and only 1 each to beef and veal. Theobald Smith has insisted on the similarity between the hog cholera bacillus and the *B. enteritidis*. In the epidemics which I have myself worked at two were veal and beef, the other two are not directly traceable to any particular viand with absolute certainty. Rabbit pies may be mentioned, but no bacteriological evidence is as yet available.

Next, I will call your attention to another animal which has been implicated in the dissemination of these infections; I refer to the illness known as "psittacosis" in France. The following is a short review of the circumstances of the first epidemic: a consignment of 500 parrots was despatched from Buenos Ayres, of these only 200 survived to reach Paris on February 3rd, 1892; they were divided into two lots by the owners. Two epidemics localised about the dwellings to which the parrots were sent resulted in a total of 42 cases of illness with 14 deaths. The next winter a fresh but less extended epidemic arose, the origin of which was also ascribed to a diseased parrot; at this time Nocard isolated a bacillus from the bone marrow of a parrot, which in cultural and morphological characteristics is identical with the bacillus enteritidis described by Gärtner five years before. In passing, I may note that the serum samples obtained from your fellow-townsmen show that the bacillus from which they suffered is not widely different from Nocard's variety of the bacillus enteritidis, though they react rather more strongly with certain other varieties with which I have tested them. It is quite certain that in some cases the birds were ill with diarrhoea, but the whole story is not very clear, and I feel by no means sure that in all the recorded outbreaks the birds were the prime cause. If I may make one suggestion in the event of any further outbreak occurring, it would be to call attention to the fact that on board ship birds are put under the tender care of the butcher, and their quarters are in close proximity to the "butcher's shop;" it is possible that the kernel of the mischief lies somewhere here.

The French observers do not hesitate to affirm that the disease may be spread from one person to another by contact; this has also been described by two authors (Gärtner and von Ermenghem) in meat poisoning outbreaks due to the *B. enteritidis* and was thought to be the case in one of the epidemics which has engaged my attention; I must say, however, that the proof did not seem to be quite clear of possible fallacy. In the outbreak which you had in this town I made a good many inquiries to elicit information on this point, and, although in many cases more than one member of a family was affected, I was entirely unable to get any evidence of personal contagion, for those who had eaten veal pies suffered, those who had not escaped; so that if the disease is ever really contagious, it does not appear to be so at all commonly.

Our knowledge of the distribution of the bacillus is still far from complete; in a form of septicæmia of calves, Thomassen has described a bacillus which falls under the name of *B. enteritidis* by its characters. Basenau has also done some most valuable work in examining the flesh and organs of diseased animals brought to the slaughterhouse in Amsterdam, wherefrom he has successfully isolated the *B. enteritidis* (*B. morificans bovis*). The diseased conditions associated with sub-

sequent effect on the eaters of the flesh may be shortly summed up as "puerperal fever," septicæmia, "septicopyæmia," diarrhoea, and localised suppurations; beyond suspicions arising from the insanitary condition in which some cowsheds are kept, we have no knowledge of the mode of infection of the peccant beasts.

Petruschky has described a bacillus under the name of *Bacillus fecalis alkaligenes*, which is identical in its characters with the *B. enteritidis*. The source from which it was isolated was fecal matter from "persons suspected to be suffering from typhoid fever, in which it was frequently found to be present." Probably, then, these patients were not suffering from typhoid fever, but from infection by the bacillus enteritidis. I do not know of a record of the presence of the *B. enteritidis* in normal fæces (or urine).

MILK.

There is another possible means of communication of this specific illness from the cow to man, namely, through the milk. Though so far we have no satisfactory or complete evidence from the bacteriological side, the outbreak described by Gaffky is very suggestive from the clinical symptoms that infection with *B. enteritidis* was communicated by means of the milk; the cow was traced out and was found to have been suffering from diarrhoea and an inflammatory swelling on the shoulder (an ox in the same stall likewise had an inflammatory condition, which, like that on the cow, does not seem to have been investigated).

Basenau has shown that the milk of a guinea-pig which had been infected with the *B. enteritidis* (*var. morboficans bovis*) contained very large numbers of the bacilli. But probably milk is not a common source of infections of this nature, since while the cow is ill the secretion of milk appears to stop. In respect to milk infections, mention must be made of the outbreak at St. Bartholomew's Hospital, in which Dr. Klein found an anaerobic bacillus, which he named *B. enteritidis sporogenes*; the further experiments which have been made with this organism rather tend towards the conception that it is merely a common inhabitant of the fæces and sewage.

TIME OF YEAR.

Next let me point out that although there is a widespread idea that this class of illness is a speciality of the summer months, this is by no means true, for several of the epidemics which have been bacteriologically examined have occurred in the winter or autumn. Naturally, from what has already been said, and the subjoined experiment of Poels and Dhont, it is well shown that summer heat is not necessary to make the meat of infected animals dangerous.

FURTHER EXPERIMENTAL EVIDENCE.

The following experiment made by Poels and Dhont—for which I am indebted to Basenau, the original being in Dutch—may next be of interest to you:

An ox was inoculated with a small quantity of pure culture of a variety of the *B. enteritidis* in the jugular vein; twenty minutes afterwards it was killed, being partially bled out. On bacteriological examination, a few colonies were obtained from the spleen and liver; none could be found in cultures from the flesh; a few bacilli were also present in the blood. However, after keeping a piece of the flesh at 20° C. for seventy-two hours the bacilli had become abundant; the rest of the meat had been kept at a lower temperature—not exceeding 5° C.=41° F.; in this scanty bacilli were detected. "After we had assured the workers in the slaughterhouse that we were convinced, from the smallness of the number of bacilli present in the muscles, that the danger of eating the meat could not be very great, and that it would probably only cause a slight diarrhoea, 53 persons resolved to eat of it. Of these 53, 15 became ill in consequence; headache, gastrointestinal catarrh, and abdominal pain came on in from twelve to eighteen hours; in some there was severe diarrhoea."

Without more details it would not be profitable to discuss some of the points which this important experiment suggests. It shows that an animal experimentally infected with pure culture of the bacillus will give rise to illness when its flesh has been kept. The moral which is especially to be insisted on is the effect of keeping the meat. Experiments of Gärtner

and others show that bacteria are only to be found in the surface layers of sound meat after three days' keeping, and even after ten days they have only penetrated to a depth of 1 cm.; when roasted such meat will be quite sterile in the interior for a long time. Naturally with meat which has been infected before death this will not be the case. It has been said, probably many times, that because a piece of meat was not eaten up immediately no blame could attach to those who were responsible for its sale when persons were taken ill through eating it. Here is a case in point: A piece of beef was delivered and roasted; it happened, however, that the people took several meals with friends and the beef was not eaten for a few days; when it was eaten it gave rise to illness. Inquiry failed to reveal any untoward effects caused by other portions of the same carcass. In the light of the evidence that I have put before you I daresay that you would be inclined to agree with me that that piece of meat was derived from an unsound beast. The keeping was really part of the test of its soundness.

Two points which writers on the subject in this country are fond of referring to may be mentioned. The one is that exposure of meat to the influences of sewer air (Ballard, Poore) is responsible for its subsequent poisonous effects. We have no evidence that the bacillus enteritidis can be as it were "spontaneously generated" from sewer gas. The other is that meats, etc., containing much gelatinous material are especially liable to these contaminations; this is no doubt derived from the fact that media containing gelatine are extensively used for the cultivation of bacteria; the gelatine, however, is not used as a nutrient material but as an addition to other nutrient materials (peptone, etc.) in order to produce a solid transparent medium which can readily be liquefied.

CLINICAL SYMPTOMS.

I do not propose to enter fully into the clinical symptoms of the disease. As in the case of most other bacterial diseases due to a single specific cause, the symptoms and lesions vary to a considerable extent. Diarrhoea is usually present but it may be slight, or especially in "psittacosis" it may be replaced by constipation. Diarrhoea is not a constant feature in experimental observations. Vomiting, another common effect, may be extremely intense or absent. Whether this has any relationship to the portion of the bowel which is chiefly inflamed is not known; though the small intestine is usually affected the colon may be involved. There is also much difference in the extent of lung affection in different cases and outbreaks. Sometimes pneumonic conditions are common, whilst on other occasions they are but little marked or absent. Herpeslabialis, rashes, desquamation (some fourteen days after the attack) on hands and feet, jaundice, and great thirst may be mentioned. The onset is frequently sudden with nausea, and the headache with great pain in the joints, limbs, or back may lead to a diagnosis of influenza. The attack may be ushered in by a rigor: the fever, if any, generally lasts but a few days; however, it may be more protracted, or a relapse may set in. In almost all cases there is very great weakness. Convalescence is slow, lasting for three to six weeks. The more protracted cases may simulate typhoid fever to some extent; according to the descriptions, "psittacosis" is apt to be rather of the chronic type. This review of the clinical side is, of course, by no means exhaustive. In respect to serum diagnosis I have already uttered the warning that many cultures must be used for the test; in some of the "psittacosis" cases serum diagnosis has failed: it is not unlikely that this may have been due to an insufficient number of trials on varieties, though it should be added that the published evidence concerning "psittacosis" appears to me to be very inconclusive.

PREVENTION, PRECAUTIONS, ETC.

Before leaving the subject I will say a few words on preventive measures, and steps which I think should be taken when an outbreak occurs.

From the evidence which is at our disposal, it is clear that the flesh of cows and calves has been accountable for many deaths; it is also certain that a mere naked-eye inspection of the meat will not exclude danger, since meat infected with *B. enteritidis* may have quite a good appearance. The point of importance is to pay especial attention to the animal before it has been slaughtered; if suspicion is thus aroused bacterio-

logical inquiry should be made. Cows, too, are notoriously apt to be tuberculous, and are by no means an ideal source for our beef. Thorough sterilisation of the meat by heat will also destroy much of any formed poisons, which are rather more resistant to heat than the bacilli themselves.

In the event of an outbreak it is impossible to lay too much stress on the fact that promptness and thoroughness are absolutely necessary for getting at the root of the matter; every hour may be of importance, especially as the bacillus enteritidis is easily killed out by *B. coli communis* and proteus. The difficulty of making a satisfactory bacteriological examination with stale materials, improperly gathered, is very great. Suppose an outbreak arises, and is traceable to food obtained from a particular source with a considerable amount of, if not with absolute, certainty. All the remaining goods in the infected shop should at once be put under an official seal of *noli me tangere*; a selection of suspected articles should be made, and at once placed in a freezing apparatus and kept frozen hard until they are delivered for examination. Before anything is again sold from the same quarter thorough cleansing and sterilisation of utensils is necessary, of course with destruction of such perishable articles as would be useless for research. If one of the patients should die, a coroner's order for a bacteriological examination should be in force within an hour or two of death. If materials—such as the outfit of serum tubes for diphtheria examination—are at hand, and someone versed in methods is available, cultures should be made from the liver, spleen, kidney, and heart blood (after searing the surface of the organ with a red-hot poker) within ten or twelve hours of death—earlier if possible. The spleen should be wrapped in a cloth or lint moistened with 1 in 500 corrosive sublimate, preferably without cutting into it; one kidney and a piece of liver cut out with a clean knife treated similarly; all these should be placed at once to freeze (*vide* Appendix). Small portions of the organs—not more than $\frac{1}{4}$ inch thick—should be put into spirit (rectified) for microscopic examination. The loop of intestine, so useful in the case of cholera, is probably of little use here. All these objects should be sent at once for examination, care being taken that sufficient freezing mixture is put in to ensure complete freezing until delivery.

There does not appear to be sufficient evidence of contagion from person to person for isolation of patients to be necessary. When these things have been done, and as much epidemiological evidence as may be possible has been obtained *without bias*, and careful search made amongst the other live stock whence the suspected meat came has been made, the matter rests in legal quarters. In these cases it is usual for the coroner to hold an inquiry. It seems to me that another class of food infection might also be inquired into by this authority with profit, namely, the tuberculous enteritis of children; here the urgency is not so great, for if the tubercle-infected dairy whence the suspected milk was derived is examined even after the weeks which will have elapsed since the actual infection took place, there will probably still be evidence of the presence of tubercle amongst the cows. Personally, in such a case, if it was shown that the owner of the dairy had taken no steps to ascertain the absence of tubercle (by means of tuberculin test, for example) I should consider him to be culpably responsible for the death.

From the point of view of the welfare of his fellow creatures, it would be preferable that compensation should be awarded him in the form of lodging, with suitable employment for awhile, rather than in the form of coin of the realm. It is not improbable that if a few examples were made, people would learn to appreciate that foods more or less loaded with (living) poison could not be sold with impunity.

I hope that I have aroused your interest in this form of infective fever communicable from animals to man, about which there is still much to learn; if I have driven my horse—the bacillus enteritidis—to the death, I can only hope that it may stimulate others to do likewise, not as I have done only in word but in deed also.

APPENDIX I.

Tables showing the serum reactions given by nineteen of the sufferers. The vertical rows each correspond to a certain race of bacilli. The following races are varieties of the bacillus enteritidis:

"Hatt 1" and "Hatt 2" were isolated by the author from the Hatton epidemic.

"Gärtner" isolated by Gärtner in 1888.

"Günther" isolated, and kindly sent to me by Dr. Günther, of Berlin. "Morb." = *B. morificans* bovis, isolated, and kindly given to me by Dr. Basenau, of Amsterdam.

"Psitt." = *B. psittacoseos* isolated by Professor Nocard, and kindly sent to me by Dr. C. Nicolle, of Rouen.

The other cultures used were two races of typhoid bacillus ("HS" and "W") and the bacillus of Dr. Gwyn, most kindly sent to me by Professor Flexner.

I wish to express my indebtedness and gratitude to the above-named gentlemen, who have been so friendly as to send me their cultivations.

The following signs are used to denote: Strong reaction +; well marked but weaker reaction *; trace or doubtful, tr. or ?; no reaction = 0; no observation = —.

The tests here tabulated were made after Professor Wright's plan in capillary tubes, and give the limit dilutions at which recognisable sedimentation takes place. The whole series of samples were first tested on all the cultures at a dilution of 1 to 100. According to the results thus obtained further tests at higher or at lower dilutions were then made. I find this to be the most useful routine when a number of samples are to be examined. One cubic centimetre of a 1 to 50 dilution (= 20 c.mm. of serum) is ample for the preliminary test on nine or ten cultures.

TABLE I.—Chadderton Epidemic, July 30th, et seq., 1898.
Serum taken September 22nd, 1898.

	Culture.								
	Hatt. 1.	Hatt. 2.	Gärtner.	Günther.	Morb.	Psitt.	Typh. H.S.	Typh. W.	Gwyn.
No. 3.									
1-100 ...	+	+	tr.	+	?	—	—	—	—
1-200 ...	tr.	tr.	tr.	+	?	*	—	—	—
1-500 ...	tr.	o	tr.	+	o	*	—	—	—
1-1,000 ...	o	o	o	o	o	o	—	—	—
No. 5.									
1-100 ...	+	+	o	+	?	—	—	—	—
1-200 ...	+	tr.	o	+	?	*	—	—	—
1-500 ...	+	o	o	o	o	o	—	—	—
No. 8.									
1-100 ...	+	+	o	+	?	—	—	—	—
1-200 ...	+	tr.	—	+	o	+	—	—	—
1-500 ...	tr.	o	—	o	o	o	—	—	—
No. 13.									
1-100 ...	+	+	o	+	o	—	—	—	—
1-200 ...	+	+	—	+	o	*	—	—	—
1-500 ...	*	tr.	—	*	o	tr.	—	—	—
1-1,000 ...	tr.	o	—	tr.	o	o	—	—	—
No. 14.									
1-100 ...	+	+	o	+	+	—	—	—	—
1-200 ...	*	*	—	+	o	*	—	—	—
1-500 ...	tr.	o	—	*	o	o	—	—	—
1-1,000 ...	o	o	—	o	o	o	—	—	—
No. 18.									
1-100 ...	+	*	o	+	+	—	—	—	—
1-200 ...	tr.	o	—	tr.	tr.	tr.	—	—	—
No. 19.									
1-100 ...	+	o	o	*	?	*	—	—	—
1-200 ...	?	o	o	?	?	?	—	—	—
No. 20.									
1-100 ...	+	+	o	+	*	—	—	—	—
1-200 ...	+	+	—	*	*	+	—	—	—
1-500 ...	*	*	—	tr.	tr.	+	—	—	—
1-1,000 ...	o	o	—	o	o	o	—	—	—
No. 21.									
1-100 ...	+	+	o	+	+	—	—	—	—
1-200 ...	+	+	—	+	*	+	—	—	—
1-500 ...	tr.	tr.	—	tr.	?	+	—	—	—
1-1,000 ...	o	o	—	o	o	o	—	—	—

Nine samples giving reaction by limit recognisable sedimentation in dilutions above 1-100.

It will be observed that the differentiation is better with those samples which give reactions in dilutions above 1 in 100; for instance, compare Nos. 3 or 13 with Nos. 4 or 15. Speaking generally, the strongest reactions all through are obtained with the cultures Hatton 1 and Günther. The cause of the epidemic was therefore probably more closely allied to these bacilli than to psittacosis and morificans. The reader will notice that the reactions with typhoid and the race Gärtner are in harmony. Experience has shown that serum which affects these races has little or no action upon races like Hatton, especially when considerable dilutions are used. *Vice versa* the serum of typhoid patients acts fairly well upon Gärtner, but not upon Hatton. In my paper in the *Lancet* of January 15th, 1898, tabulated results of the action of typhoid

patients' sera will be found; it may be noted that the culture there referred to as "ös" is very close to Hatton 1 in its serum reactions. The above remarks apply equally to the reactions obtained with the sera of artificially immunised animals upon these races. Serum tests on an epidemic caused by variety Gärtner are urgently wanted.

TABLE II.

No. 1.	Culture.							
	Hatt. 1.	Hatt. 2.	Gärtner.	Günther.	Morb.	Psitt.	Typh. H.S.	Typh. W.
1-20 ...	+	+	tr.	+	+	tr.	o	o
1-50 ...	+	+	o	+	+	o	o	o
1-100 ...	+	+	o	+	+	o	o	o
No. 2.								
1-20 ...	+	+	o	+	+	o	o	o
1-50 ...	+	+	o	+	+	o	o	o
1-100 ...	+	+	o	+	+	o	o	o
No. 4.								
1-20 ...	+	+	+	+	+	+	+	+
1-50 ...	+	+	+	+	+	+	+	+
1-100 ...	+	+	+	+	+	+	+	+
No. 6.								
1-20 ...	+	tr.	+	+	+	+	+	+
1-50 ...	+	tr.	+	+	+	+	+	+
1-100 ...	+	tr.	+	+	+	+	+	+
No. 7.								
1-20 ...	+	+	+	+	+	+	+	+
1-50 ...	+	+	+	+	+	+	+	+
1-100 ...	+	+	+	+	+	+	+	+
No. 9.								
1-20 ...	+	+	+	+	+	+	+	+
1-50 ...	+	+	+	+	+	+	+	+
1-100 ...	+	+	+	+	+	+	+	+
No. 10.								
1-20 ...	+	+	tr.	+	+	+	+	+
1-50 ...	+	+	tr.	+	+	+	+	+
1-100 ...	+	+	tr.	+	+	+	+	+
No. 11.								
1-20 ...	+	+	+	+	+	+	+	+
1-50 ...	+	+	+	+	+	+	+	+
1-100 ...	+	+	+	+	+	+	+	+
No. 15.								
1-20 ...	+	+	+	+	+	+	+	+
1-50 ...	+	+	+	+	+	+	+	+
1-100 ...	+	+	+	+	+	+	+	+
No. 16.								
1-20 ...	+	+	+	+	+	+	+	+
1-50 ...	+	+	+	+	+	+	+	+
1-100 ...	+	+	+	+	+	+	+	+

No. 17 had had no illness in consequence of eating two pies, and gave no reaction with any of the cultures at 1-20, 1-50, or 1-100. Thus 10 samples of the 19 gave weak reactions—that is, below 1-100.

APPENDIX ON AN IMPROMPTU METHOD OF MAKING A SIMPLE FREEZING APPARATUS

Take a moderately large tin, such as a biscuit tin, and a wooden box of sufficient size to give 3 or 4 inches clear space in every direction when the tin is placed within it. Put a layer of sawdust 3 or 4 inches deep at the bottom; stand the tin upon this, and fill the surrounding space with sawdust also. The various organs, after being removed with as much cleanliness as possible, are each wrapped in a clean cloth wetted (not wringing wet) with 1-500 HgCl₂, and further wrapped and sealed each separately in a piece of gutta-percha tissue (most easily done with a trace of chloroform); they are then put into one or more watertight tins (those 1 or 2 lb. tins in which some tobacco is sent out are admirable; they may be more securely closed by means of a paraffin candle and a warm poker). The tobacco tins are placed within the larger tin, together with pounded ice and common salt, in the proportion of 3 lbs. of ice to 1 lb. of salt. The lid is placed on the larger tin, and the wooden outer box filled up with sawdust. An apparatus which holds the above quantities of ice and salt will keep about 1/2 pint hard frozen for more than twenty-four hours. There can be no difficulty in obtaining these materials at a moment's notice. The maintenance of objects at a low temperature when bacteriological examination cannot be carried out completely on the spot is essential for satisfactory results to accrue.

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NOTES, ETC.

¹ I am of course aware that many other microbes have been described in connection with meat poisoning, but further confirmation is necessary before these can be definitely accepted. The importance of the bacillus

enteritidis surpasses that of all others hitherto described on account of the number of times which it has been found as well as the remoteness of the localities. ² *BRITISH MEDICAL JOURNAL*, September, 1898. ³ It is not unlikely that in reality these bacilli proceeded from the urine and not from the faeces; in the case of typhoid fever patients it appears that fewer negative searches for the typhoid bacillus have been recorded where loose than where solid motions were dealt with, and here again it is not improbable that the bacilli in reality were in the urine and not in the faeces. Recently I have examined seven specimens of "pure" urine-free faeces of varying consistence from typhoid patients at different stages of fever or relapse without finding the typhoid bacillus in a single instance, although a very extensive number of plates and subcultures were made. ⁴ The use of bleeding as a means in the slaughter of animals for consumption is probably not without importance from the hygienic point of view. ⁵ Contaminated skewers, knives, etc., may be the means of infecting other articles; an infected wooden skewer might implant the bacilli in the interior of a joint, and make it dangerous if more or less underdone in its centre. On the other hand, a metal one would very likely become sufficiently heated by conduction during the cooking to sterilise its immediate neighbourhood.

ABSTRACT OF THE HARVEIAN LECTURES.

ON

DISEASE AND ITS TREATMENT, AND THE PROFESSION OF MEDICINE IN THE YEAR 1899.

Delivered before the Harveian Society of London, December 1898.

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LECTURE II.

THE MODERN ADVANCES IN TREATMENT AS THEY AFFECT THE PUBLIC AND THE PROFESSION.

SURGERY AND ITS INFLUENCE ON THERAPEUTICS.

The changes which have occurred in the treatment of disease are not inferior to the advances in pathology. One of the greatest is the extension of the clinical domain of surgery. Not many years ago lives were sacrificed daily to the impossibility of establishing the diagnosis of actual suppuration in the abdominal cavity. The idea of laparotomy for the sake of ascertaining that no abscess existed there was as foreign to the professional mind as are to us the thoughts of dwellers in distant planets. The whole anxiety was lest an operation should be attempted whilst pus had not been proved to exist. Most of the cases operated on died, because gangrene and suppuration had proceeded too far before they were revealed by physical signs.

Physicians have a further debt to acknowledge in the influence which surgical methods have exercised over medical therapeutics in the direction of definiteness in diagnosis and of thoroughness in treatment.

THE MEDICO-CHIRURGICAL IDEA IN THERAPEUTICS.

The relation between the work of the physician and that of the surgeon is one of ever-increasing harmony. The spirit of their treatment is becoming more and more identical—to their mutual advantage. Hospital physicians find themselves day by day bestowing more time and thought upon the study of cases which it is given to others to cure, and upon the discussion of methods which the surgeon may carry out; and some of this work and thought may perhaps not be wasted from a surgical standpoint.

MEDICAL QUESTIONS OF TREATMENT: OUR THERAPEUTICS, PAST, PRESENT, AND FUTURE.

Scepticism in therapeutics had existed from antiquity, but it had never prevailed. Early in this century, however, there came a wave of scepticism stronger than any before. Strangely, homœopathy, while it shattered the old belief, made claims upon our credulity greater than any superstition. It failed to build up any lasting medicinal system, though it called attention to some unutilised drugs, and modified our opinion of the mode of action of some of the older ones. But the practical service which it did render was to direct