TRANSCRIPTION OF HANDWRITTEN TEXT BY SIR WILLIAM WATSON CHEYNE, CONTAINED IN TWO VOLUMES OF HARDBACK NOTEBOOK. ANY NOTES OF MY OWN ARE MARKED THROUGHOUT IN […].
JANE COUTTS, FETLAR MUSEUM TRUST, JULY-DECEMBER 2003.

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This handwritten original contains Cheyne's original notes, and the text is transcribed here with his notes, corrections and amendments.

The following is a transcript of Volume 2.

To Dr. Storer
182 Boyston St.
Boston
Mass. U.S.

Vol. II

Antiseptic Treatment. What are its essential details? How are they best carried out in practical form?

By

Truth, our guide

Proposed method of solution is fallacious

Experiment to determine the first point that the putrescible substance does not contain germs

To show that water does not contain them

By artificial water

[This paragraph crossed out in pencil]. At the end of volume I, I quoted Pouchet's fallious method of stating the method of arriving at the truth of heterogeny. Having made the statement referred to he then proceeds to test it in the following manner.

With regard to the first point he says that it is evident that the putrescible material does not contain the germs of the Proto organisms since, when they are charred previously to their employment, one sees the water in which they are placed become rapidly filled with microzoaires and cryptogams.

Experiment. 10 grammes of any of the following seeds.
Maize, peas, beans or lentils are placed in an iron spoon and completely charred; then the product is placed into a glass vessel containing 500 grammes of distilled water & covered with a belljar. In 20 days the fluids were found to contain micro organisms & cryptogams. He next points out that it is not the water which contains the germs because, if one places again substances in artificial water animalculæ & cryptogams still develope [sic].

To show this an artificial water is obtained in the following manner.
"Dans un grand flaçon à deux tubulures on met de l'eau et des fragments de zinc. L'une de ces tubulures recevait un tube terminé en entonnoir[?] à l'aide duquel on versait de l'acide sulphurique dans le flaçon; l'autre était annexée à un gros tube horizontal rempli …

[in original script, translation written above each line of the French]: Into a large flask with 2 orifices water & fragments of zinc are placed. One of the orifices transmits a tube terminating in a funnel by means of which one introduces H_2SO_4 into the flask. The other orifice is attached to a large horizontal tube filled with …

d'amiante et d'où sortait un petit tube affilé à la lampe à son extremité et se terminant près de l'extérieur d'une vase de métal rempli [sic – should be remplie?] d'eau froide. L'acide sulphurique ayant été versé il se dégagea du gaz hydrogène que l'on allume à sa sortie du tube et dont la flamme rapprochée du vase de métal en ?ant toute la vapeur d'eau resultant de la combinaison de l'oxygène de l'air avec l'hydrogène de l'appareil; et cette vapeur, après s'être condensée trouvait dans un [sic] vase de platine. Cet appareil ayant été entretenu en combustion pendant trois jours, on obtenait[?] 200 grammes d'eau qui furent employés dans deux expériences comparatives.

English translation continued: … asbestos and which leads to a small tube drawn out at its extremity and terminating close to the outside of a metal vessel filled with cold H_2O. The H_2SO_4, having been introduced[,] hydrogen gas is disengaged and is lit at its exit from the tube. The flame being close to the metal vessel moistens its walls with watery vapour, the result of the combination of the oxygen of the air into the hydrogen from the apparatus; & this vapour being condensed is caught in a platinum vessel. The apparatus having been maintained in this state for 3 days[,] 200 grs. [grammes] of H_2O were obtained which were employed in 2 compv. exts [comparative experiments].

Half of this water was boiled for a quarter of an hour in order to kill the germs which might have fallen into it, although he does not consider that to be necessary. This water is then placed in a vessel with 5 grammes of hay which had been raised to a temperature of 200°. This vessel was then placed in a basin containing a little water and was covered with a small shade. In 4 days there was a granular pellicle and two species of Paramecia. The other portion of distilled water was not boiled & to it was added hay which had not been heated. The result was absolutely identical.

Lastly Pouchet states that it is evidently not the atmosphere which disseminates the germs since

To show that air does not contain germs

he has seen organisms appear in flasks containing only artificial air or in flasks containing air which had been heated or which had passed through sulphuric acid.

Repitition [sic] of Schulze's experiment

He next takes up Schulze's experiment and presents the following as a counter experiment and as complete disproof of the former: - a vessel of the capacity of a litre was half filled with water to which 5 grammes of hay were added. The cork of the vessel was traversed by 2 tubes bent at right angles 5 centimetres above this place of exit. One[,] the afferent[,] did not descend lower than the neck of the vessel, the other, the efferent[,] reached to within one centimetre of the liquid in order the better to remove the heavy stagnant gases. To each of the tubes was attached Liebigs bulbs filled with sulphuric acid. To the efferent was further attached a water aspirator. The cork &c were all luted with copal. The fluid was then boiled for an hour. In 26 days spirilla, vibriones and penicillium were present in the fluid.

Pouchet's "more rigorous" experiment

In order, as he supposes, to have more rigorous conditions than those of Schulze he introduces the following method of procedure. A flask is used having the capacity of a litre & having 3 necks. This is quite filled with concentrated sulphuric acid. The first neck contains a bent tube which by one extremity communicates with an air pump while the other extremity passes down to the bottom of the acid. Through the middle neck passes a siphon which commences in the flask at the level of union

of the upper and middle thirds while its other end passes into an empty vessel. The third orifice contains a tube passing down to the bottom of a second vessel but only originating in the first at the very top. This second vessel, of the same capacity as the other, had been filled with boiling water; its first orifice allows the passage of the tube from the first vessel. The central orifice allows the passage of a siphon similar to that in the first vessel while from the third opening a tube passes into a third vessel. This vessel had the same capacity as the others but had only two openings – the first receiving the tube from the second flask which however does not pass down to the bottom of the third flask as in the other two cases & the second giving exit to a siphon arranged as before.

A strong decoction of boiling hay having been introduced into the third vessel so as to fill it exactly the whole apparatus was luted with the greatest care and thus the apparatus contained only Sulphuric acid and water previously boiled. Air was then forced slowly into the Sulphuric acid & traversing the acid it collected at the top forcing the Sulphuric acid through the siphon. This air passed on through the second passing through the water & forcing the water through the siphon & from the second into the 3^{d} vessel forcing some of the decoction through the siphon & then the apparatus was abandoned to itself. A quantity of the same decoction was placed outside

Results.

for comparison. In 20 days both in the hay infusion apparatus & in the vessel outside contained mucor & bacteria [sic].

Second Experiment

A second experiment was done by introducing into the 3^{d} flask 5 grammes of hay which had been exposed to the action of steam for 5 minutes. The vessel was then filled with boiling water. A similar result was obtained.

3^{d} experiment.

Again he introduced into the 3^{d} flask filtered water & 10 grammes of hay exposed for 30 minutes to a temperature of 200°C. Nevertheless monads & vibriones were found.

Repetition ("more rigorous") of Schwann's Heated Air.

A similar experiment to Schwann's was also performed – the air being here passed over a heated tube instead of through Sulphuric acid. Nevertheless organisms (penicillium & vibriones) were found. Two flasks (instead of 3) were used, the first filled with boiling water (this flask having the 3 necks[,] one receiving the heated air, one for the siphon & the 3^{d} leading into the second flask) & the second with 2 necks containing boiling decoction of hay. Same result. He succeeded better in obtaining organisms, when filtered. Unboiled water was substituted for the boiling water & as he has already proved, as he thinks, that water does not contain germs of organisms, he considers it to be a matter of indifference whether the water be boiling or not!

Artificial air

Similar experiments with artificial air yielded the same results.

Objection that if air contains germs the greater the mass of air the more numerous the organisms.

A new objection is then brought forward viz. that if the atmosphere be the universal disseminator of germs, the greater the mass of air in contact with a fluid[,] so much the greater the number of organisms which ought to grow in that fluid. This he has not found to be the case & further in flasks containing the same fluid, placed close to each other at the same time, different organisms develope [sic], a fact which he does not consider capable of explanation on the theory of Panspermism.

Examination of air dust

Pouchet further examined air dust microscopically & he admits that he finds spores of fungi &c present in it but he does not think that they are ever present in sufficient quantities to explain the numbers of organisms found in an infusion, for he states that the bacteria themselves do not increase in number by fission &c (or but very slowly) but that on the contrary each is spontaneously generated.

Proof that organisms do not come from dust.

To prove this he takes 5 grammes of dust from the roof of the Rouen Cathedral & places it in 100 grammes of distilled water. Then into another similar vessel he places another 100 grammes of distilled water containing 5 grammes of the dry stalks of China Aster, previously exposed for 2 hours to a temp. of 200°C. These 2 vessels were placed under the same glass shade. Eight days later the first contained vibriones, monads in small numbers, & a few Kolpodes. The second was full of monads and Kolpodes.

[in pencil at side] Dust now in lungs – see Neudörfer's paper.

Examination of Pouchet's Experiments

Absurdity of the method of solution proposed

Criticism of the Experiments

And not only does Pouchet support the doctrine of the spontaneous generation of bacteria, & the lowest forms of animal & vegetable life[,] he is also prepared to accept the spontaneous origin of fleas, aceri & cysticerci.

By how we carefully examine these experiments we shall see that they are full of the grossest blunders. Take, for instance, his experiments to show that organisms are not present in the putrescible substance in the water or in the air. The first experiment is simply absurd. Some seeds are charred & introduced into a vessel containing distilled water. & covered with a bell jar; organisms develope [sic]. What does that prove? Does it prove that organisms were not present & may not constantly be present in putrescible substances exposed to the air? No. It merely proves that this cannot be their only source for here the water & the air were not heated or otherwise purified & therefore the organisms might be easily communicated through them.

Again[,] to prove that water doesn't contain them an artificial water is prepared, boiled for a quarter of an hour & placed with some hay which had been previously heated in an unheated vessel & exposed to ordinary air. What does this show? Certainly not that ordinary water does not contain organisms. It may be admitted that this specimen of water did not contain them but there was ample explanation of their presence from the fact that the fluid was placed in an impure vessel & that the air had access to it.

Nor is Pouchet more successful in his

attempt to show that the air is not the vehicle.

As to observation of the atmosphere from the number of organisms.

In considering the question as to the presence of organisms or their spores in the air Pouchet puts forward the idea that if organisms were present in the atmosphere in sufficient numbers the atmosphere would be totally obscured. But it has been asserted & shown by Pasteur & others (as we shall see later on) that organisms are by no means so numerous in the atmosphere as was formerly supposed being generally derived from dust which had settled or from water. Further Tyndall has demonstrated by means of the beam of light how multitudinous the minute particles are which fill the air around us.

Repetition of Schulze's Experiment.

In his repetition of Schulze's experiment it must be admitted that he has a stronger case but even here the flask & the tubes were not purified & there is always the possibility of a flaw in the cork or in the joinings of the various tubes, which latter I should think the most probable explanation. And further this experiment loses its force when Pouchet admits that he does not always get organisms & states that, on the contrary, when a simpler apparatus is employed a negative result is obtained. Thus to quote his own words:-

He in reality confirms Schulze's results

"Dans un appareil à simple rentrée d'air" (this consists of a flask having only one tube passing through its cork, to which tube Liebig's bulbs are attached) "et dont les boules de Liebig contenaient de l'eau, on remplit le tiers

Further confirmation of Schulze.

du ballon de colle de farine légère; que l'on y tint quinze minutes en ébullition à l'aide d'une lampe. Celle-ci éteinte, l'air rentra dans l'appareil en traversant l'eau peu-à-peu. L'appareil fut abandonné deux mois à une température moyenne de 14 degrés, et pas la moindre moisissure ne se déclara à la surface de la colle durant tout ce temps.

"Au contraire, un criterium, placé à côté et en contact avec l'atmosphère avait au bout de cinq jours toute sa surface envahie par des champignons.

"Une expérience enterprise le même jour et dans le mêmes conditions, mais dans laquelle l'air est introduit dans l'appareil en traversant des boules de Liebig remplies d'acide sulfurique donna absolument les mêmes résultants."

Thus Pouchet showed that not only was Schulze's Experiment correct when performed with a simple apparatus but he further demonstrated that it was not necessary that the air should pass through sulphuric acid; if it were merely washed in water it was sufficient.

Again "dans notre appareil à simple rentrée d'air, et dont les boules étaient remplies d'eau, on mit 175 grammes d'urine humaine et on l'y tint en ébullition pendant un quart d'heure. Ensuite l'air rentra en

traversant l'eau, et l'appareil fut abandonné sous l'influence d'une température moyenne de douze degrés. Deux mois après, l'urine était encore parfaitement limpide et pas la moindre moisissure ne s'était déclarée à sa surface. Un critérium placée à côté, au bout de huit jours était envahi par une abondante végétation cryptogamique.

Une expérience est faite le même jour et absolument dans les mêmes circonstances, seulement l'air ne rentre dans l'appareil qu'en traversant les boules remplies d'acide sulphurique. Le résultat est absolument le même que dans l'expérience précédente, l'urine est intacte."

His more complicated arrangement

Such are examples of Pouchet's general results with this simple apparatus & when he states that with the more complicated arrangement even with much more prolonged boiling, he has generally cryptogamic vegetation, I do think that I am only drawing a fair inference when I suppose that there was something defective about his apparatus.

Contradictory assertions

Nor is he more fortunate with Schwann's Experiment. Referring to Pasteur's results that when he (Pasteur) performed Schwann's Experiment of having in contact with fermentescible substances only air previously heated to redness he obtained neither fermentation not yeast nor infusoria [-] he says "l'air

Pouchet's "more rigorous" conditions.

calcinè a ici encore arrêtée [sic – should be arrêté] la fermentation et les produits organiques qui en dérivent; cet air est donc également impropre au développement de phénomènes chimiques, comme il l'est à celui des phénomènes vitaux. L'expérience de Schwann, et celles qui ont étés calquées sur elle, sont donc absolument insignifiantes." Truly a strange interpretation of the facts[,] more especially as he had previously attempted to demonstrate that calcined air & air which had previously passed through sulphuric acid were equally incapable of preventing the developement [sic] of organisms, and as in the "more rigorous" conditions under which he performed the experiments he found organisms always develope! [sic].

Looking more closely at these so-called more rigorous conditions we find that they contain a sufficient number of loopholes to explain the result.

What are we to think of the third experiment to show that the air is not the vehicle of these organisms? An impure vessel is taken, containing dust &c. Unboiled filtered water is introduced into it together with some heated hay. Is it any matter of surprise that organisms should be found here however pure the air? The dust in the vessel contained plenty of these spores, the various tubes passing into it contained dust and impure air while the water itself was teeming with

Pouchet's strongest experiment.

organisms or their spores. And similar objections may be waged against the first & most rigorous experiment. Boiling hay infusion is introduced into a vessel containing dust; no attempt is made to destroy this dust or to purify the air & the dust in the tubes leading to & from it. Air is now forced into this infusion carrying along with it the organisms which Pouchet ought to have seen were theoretically present in the air in the last tube as well as on the walls of the tube. This is the rigorous experiment which is sufficient "renverser les deux expériences de Schwann et de Schulze." [above this is written in pencil: *to overturn the exps. of S. & S.*]. It is needless to pursue the criticism of such experiments.

The following experiment is however stronger & he considers it also sufficient to upset those of Schwann & Schulze. A flask was introduced to the bottom of a vessel containing a decoction of barley sprouts [last word added in pencil] which had been kept boiling for 6 hours. The flask being completely filled with this fluid was brought to the surface & corked & then the circumference of this cork was surrounded by varnish. On the 6th day a deposit of yeast was seen & the flask burst on the 7th. Here an impure vessel[,] cork &c were used & the heat was not applied to them for a sufficient length of time. Further the fluid cannot have been boiling when the vessel was introduced & corked otherwise the operator would have scalded his fingers. It is noteworthy that Pouchet only got organisms in one experiment of this kind.

Error in supposing that sufficient organisms do not fall from the air to account for the numbers found in infusions

In his “Micrographie Atmosphérique” Pouchet falls into the great error of assuming that organisms do not subdivide quickly enough to account for their rapid appearance in infusions. Now it has since been made out by several observers that organisms do divide with sufficient rapidity and of this I may mention two instances from Mr. Lister’s work. In the course of observations on a form of micrococcus which he terms “granuligera” he found that in little more than 1½ hours they had trebled their numbers – a fact which he made out by observing the subdivision of a little group. And with regard to the Bacterium lactis he found that it doubled by fission in an hour so that given one bacterium lactis there would be in 24 hours no less than 8,388,608 bacteria[,] & other forms of organisms develope [sic] even more quickly.

Objection that with free access of air a great variety of forms ought to occur, discussed.

Then again Pouchet objects that with free access of air a greater variety of forms ought to be found, but it must be remembered that what may nourish one form may not be food for another & that the products of the growth of the form which in the first instance was most vigorous & most numerous may interfere seriously with the growth of other varieties. And in the experiment which he mentions it is quite evident that in the vessel to which the dust was added there is little or no nutriment compared with that containing the stalks of the China aster.

His experiments really go against Heterogeny

And lastly when he places similar infusions similarly treated in flasks of the same size under the same glass shades & in similar conditions & fluids that the organisms which appear differ in form in the two flasks. He does not obtain as he supposes a proof of spontaneous generation but the contrary. For the same infusion, in the same conditions ought to give rise to the same species; & the occurrence of different forms can only be explained by supposing that different spores gained access to the various infusions – a view quite in accordance with the theory of Panspermism.

Pasteur's researches

Appearing shortly after Pouchet's work & leading to diametrically opposite conclusions were the researches of M. Pasteur which have by many been considered to administer the death blow to the theory of Heterogeny. [Crossed out: We must therefore examine them very carefully.]

As to existence of spores in air.
Not satisfactory; not necessary.

Pasteur (Annales des Sciences Naturelles série IV T. XVI 1861 begins by attempting to demonstrate the existence of spores in air [-] a fact which Pouchet had previously admitted. Such attempts are however very unsatisfactory partly from the difficulty in recognising what are & what are not spores & also from the fact that if spores do exist they must be so excessively minute as to be in many cases invisible under the microscope. And Prof. Tyndall has demonstrated by the beam of light the existence of innumerable solid particles in fluids in which but few could be detected by the microscope. Pasteur certainly demonstrated, & this is

Tyndall has shown presence of innumerable invisible particles.

Pasteur succeeded in avoiding the growth of organisms in heated air.

Method.

It has been remarked by several authors that Pasteur was unable to succeed in many cases, in repeating Schwann's experiment with calcined air. This is true; but it is that experiment where the flask is inverted over mercury & the calcined air then introduced which has failed & Pasteur has pointed out that it is the mercury itself which infects the liquid.

generally admitted, that the spores of fungi do occur in the atmosphere. It is not necessary however to have this demonstration of the existence of spores, for the matter can be set at rest by experiment alone, & it is these experiments & not the demonstration of the existence of spores in the atmosphere which give the value to Pasteur's work.

Operating with an albuminous saccharine fluid in the following manner Pasteur always succeeded in preventing the growth of organisms in the presence of heated air. "J'ai certainement en l'occasion de répéter plus de cinquante fois l'expérience, et, dans aucun cas, cette liqueur, si alterable, n'a donné vestige de productions organisées … en présence de l'air calciné."

Into a flask with a capacity of 250-300c.c. were introduced 100-150c.c. of this saccharine albuminous fluid. The neck of this flask, which had been drawn out, was then connected with a tube in which the air could be heated. The fluid was boiled for 2 or 3 minutes & then the calcined air allowed to enter. Although the experiment succeeded in the case of this & some other fluids, it did not succeed with milk. Leaving milk out of consideration for a moment it is shown that the other fluids which, when exposed to ordinary air, become rapidly the seat of developement [sic] of organisms[,] remain barren when exposed to air previously

But dust caused developement [sic] in heated air.

heated. It remained to enquire further what happened when in this same air dust which had not been heated was introduced into the fluid. This was done: the air coming in contact with the boiled fluid was, as in the former case, previously heated & the fluid had remained barren for one or two months; then[,] precautions being taken that no unheated air be introduced, unheated dust was put into the flask & as a consequence of this, developement [sic] of organisms rapidly occurred & these organisms were of exactly the same kind as in fluid freely exposed to air. Thus was Pouchet's objection to Schwann's experiment completely upset.

Fresh urine behaves like other fluids.

Fresh urine behaves in exactly the same manner when treated in the two ways described.

Milk requires longer heating or a higher temperature

In the case of milk Pasteur was however unsuccessful when the boiling temperature was continued only for 2 or 3 minutes (a length of time quite sufficient in the former cases) & this want of success he believes to be due to the fact that in this fluid organisms can resist the boiling temperature for a longer period. Two to three minutes of a temperature of 110°C were however sufficient to sterilize milk & in the same manner by prolonging the temperature of 100° he was able to render it barren [word *keep* crossed out and replaced by *maintain*, and word *sterile* crossed out and replaced by *barren*]. This resisting power he considers to be due to the alkalinity of fluid, for he finds the same difficulty if he renders his sugared yeast water alkaline by the

Difficulty of succeeding in an alkaline fluid first shown.

addition of Ca CO_3. In milk & in the latter fluid, obtained pure & preserved for some weeks, the addition of the atmospheric dust produces the same results as in the former fluids.

Pasteur's Experiment with flasks with bent necks.

But it is not necessary to use calcined air, ordinary air will be equally inert if only it has had time to deposit its dust before being admitted into the flask. This he showed to be the case by his well known experiments with flasks with bent necks. Into a flask a suitable quantity of the fluid to be experimented on is introduced. The neck is then drawn out long & bent in various directions (Pasteur only figures a long curve) so as to present obstructions to the entrance of solid particles along with the air. The fluid is then boiled for the requisite length of time &, the lamp being removed, ordinary air is allowed to enter. Fluids may be prepared in this way & preserved for any length of time desired. In the case of milk the boiling must be done under pressure.

The cause of the growth of organisms is not continuous as it would be were it a force.

That the cause of the growth of these organisms is not continuous as would be the case were it brought about by some known or unknown force[,] but that it is the advent to the fluids of solid particles floating in the atmosphere, is shown likewise by the following experiment. Flasks were prepared containing the saccharine yeast solution & sealed during boiling. These flasks being opened in different places, air rushed in carrying with it of course any dust present

It was thus found that the air in some places & in some conditions contained none of these particles which give rise to organisms, while where they were present the organisms were of every known variety.

One of the situations in which such flasks could be opened without any developement [sic] of organisms was in some cellars which were never entered & in which the dust had therefore settled. Precautions were of course taken that the operator neither introduced the particles himself nor stirred up the dust of the cellar.

“Le 14 août 1860, j’ai ouvert et renfermé dans les caves de l’Observatoire dix ballons contenant de l’eau de levûre de bière, et onze autres ballons de la même préparation dans la cour de l’établissement, à 50 centimètres du sol, par un vent léger. Tous ont été rapportés le même jour dans l’étuve de mon laboratoire, dont la température est de 25 à 30 degrés. J’ai conservé jusqu’à ce jour tous ces ballons. Un seul de ceux ouverts dans les caves renferme une production végétale. Les onze ballons ouverts dans la cour ont tous fourni des Infusoires ou des végétaux du genre de ceux que j’ai déjà décrits”.

Albuminoid materials are not necessary for developement [sic] of organisms

But the greatest blow was given to the views of the Heterogenists at that time when Pasteur demonstrated that albuminoid materials are not necessary for the developement [sic] of bacteria & fungi, but that they can be

Pasteur's fluid

What does Pasteur's work show?

replaced by crystalline salts such as phosphates & salts of ammonia.

His formula is	Eau pure	100 grammes
	Sucre candi	10 grammes
	Tartrate d'ammoniaque	.2 à .5 grammes
	Cendres fondues de levûre de bière	.4 grammes

"Si l'on sème dans cette liqueur, en présence de l'air calciné, les poussières qui existent en suspension dans l'air on y voit nâitre les Bactériums, les Vibrions, les Mucédinées" &c.

(It may be mentioned here that, since Pasteur published, other similar fluids have been introduced in which organisms can grow.
Thus Cohn uses the following:

distilled water	20c.cm.
Tartrate of ammonia	.2 grammes
Phosphate of potash	.1 gramme
Crystallised sulphate of magnesia	.1 gramme
Tribasic phosphate of lime	.01 gr.

[added in pencil] Here there is no question of albuminous particles combining to form an organism. If developed here spontaneously they must be built up from mineral salts.[end of addition] Such is Pasteur's first work – the work which is considered by many to have struck the final blow at Heterogeny. Let us see what it really does prove.

It shows that a certain number of boiled fluids can be preserved without any growth of organisms when in the presence of calcined air or of ordinary air, the dust of which has been allowed to settle. That the introduction of dust into these flasks is the only condition requisite for the developement [sic] of organisms; that the source of organisms is something discontinuous & particulate; that organisms are not

Pouchet's reply.

He disputes the facts

the result of changes in albuminous materials for they grow rigorously in artificial mineral fluid. Further Pasteur has shown that among the particles present in the dust of the atmosphere there are spores of fungi & bodies which may be bacteria or their spores.

This is all that is proved by these experiments & how does Pasteur answer them? By asserting that these solid particles are not bacteria or their spores but lifeless particles which, under certain conditions, become virified & appear as various forms of organisms? The only possible theory one would think! No. Pouchet disputes the facts. He does not, it is true, take the trouble to repeat Pasteur's striking experiment of the flask with bent neck. He simply says: "C'était une erreur". Further "Nous avons refusé de répéter les expériences de M. Pasteur, parce que, logiquement, rationellement, pour des physiologistes, du moment où il est reconnu que celles de Schwann" (I previously pointed out that Schwann's experiment which was occasionally unsuccessful in Pasteur's hands was not that to which M. Pouchet alludes, but that performed over mercury; & Pasteur has abundantly demonstrated the fallacy here & how it can be overcome.) "sont absolument erronées, et je pense que pas un seul de ceux-ci ne voudrait aujourd'hui le contester, les expériences du chimiste de Paris sont conséquemment frappées de la même nullité."

Pouchet again returns to the simple experiment

Contradictory statements

See Nouvelles Expériences sur la génération spontanée by Pouchet.

A “beaucoup plus sévère “ experiment than Schulze’s!!

in repetition of Schulze’s, which we have previously quoted but now his statement is directly opposed to that formerly made. He here says: “En employant, dans cet appareil, (à simple rentré e d’air) de la colle de farine etrêmement légère, de l’albumine, de l’urine, de la bière, du foin, ou de la noix de galle, constamment on voit apparaître des microphytes ou des microzoaires après un temps fort court.” How is this contradiction to be reconciled?
[Added in pencil: Is not the former experiment most likely to be correct? – end of addition]
He then goes on to describe a “much more rigorous” [in pencil, replacing “beaucoup plus sévère”] experiment than that of M. Schulze. “Dans un ballon dont le col placé horizontalement supporte un robinet, je mets une certaine quantité d’eau ordinaire. Un corps fermentescible, renfermé dans un gros tube de verre, et qui a été prèalablement chauffé à 150°C. pendant cinq heures, est place à l’intérieur du col de ce ballon: ce tube est fermé par un opercule rôdé à l’émeri et scellé hermétiquement avec lui par une substance facilement soluble dans l’eau. Le ballon communique avec trois tubes en U et des boules de Liebig. L’un de ces tubes contient de la ponce sulfurique, un autre de la potasse caustique, et le troisième du coton cardé.
“Les boules de Liebig sont remplies d’acide sulfurique concentré.
“A l’aide d’une lampe, on met l’eau du ballon en ébullition pendant dix minutes, et ce n’est qu’ensuite qu’on articule les tubes en U à l’extrémité du tube de Liebig. Enfin

Errors in this experiment

on éteint la lampe, et, tandis que l'appareil se refroidit, l'air extérieur y rentre en traversant l'acide sulfurique, le coton, la ponce sulphurique et la potasse. Quand l'appareil est parvenu à la température ambiente, on fait tomber le tube dans le liquide, et lorsque celui-ci a dissous la substance soluble qui lute l'opercule ce dernier s'ouvre, et l'eau envahit l'intérieur de ce tube.

Peu de temps après, constamment, on voit la liqueur du ballon se peupler de végétaux et d'animaux microscopiques selon les substances que l'on a confinées dans le tube. Jamais, en suivant ce procédé, l'expérience ne manque."

Such is the much more rigorous - certainly much more complicated – experiment than Schulze's. Nevertheless there are numerous loopholes in it. Thus:-

This little tube containing the fermentescible material is cold & must be covered with dust when introduced into the neck of the flask & further there is no guarantee that the soluble substance used to hermetically seal it is pure. It is certainly strange that, in order to be certain that organisms should develope [sic] it is necessary to heat the great bulk of the water used, & the putrescible material in a small quantity of water, separately & then to unite them in the manner described. For Pouchet himself admits that he does not always get organisms with Schwann's simple method.

How can Pouchet's statements be looked at side by side with Pasteur's?

Then again, it is no easy task to cork thoroughly a vessel heated to the boiling point of water; & the cork now was in no way purified. It is true that steam passed over it for a short time but then steam is dry heat & it is generally admitted that dry heat at the boiling point of water is not sufficient to destroy all organisms. And then in tilting the vessel to introduce the tube into the water the imperfect joints may open or living dust may be shaken into the fluid from the neck or cork, leaving out of consideration the fact that it is in all probability carried in with the small tube.

Pouchet looks on this experiment as completely disproving those of Shulze, Schwann, Schroeder & Dusch, & Pasteur; & why? Because with this apparatus he constantly obtains organisms. How then does he explain Pasteur's results? Pasteur boils his fluids for 2 or 3 minutes & finds that they remain barren. This experiment can only be answered by Pouchet (so long as he adheres to this line of argument) by supposing that Pasteur is making a false statement or that by boiling his fluid he has destroyed its power of producing organisms spontaneously. As regards the first, a committee composed of the first scientific men in France confirm the truth of what Pasteur asserts, while the second can have no force when looked at in the light of Pouchet's own experiments for we find that the latter obtains

Since that time other French writers such as Joly & Musset have supported Pouchet but as their experiments furnish little or no additional evidence nor new arguments I think it is unnecessary to detail their experiments. They will be found in the Comptes Rendus during the first years of the 6th decimal[?] period of this writing[?]

Jeffries Wyman

His results must be carefully considered

organisms after keeping his fermentescible material at a temp. of 110° for several hours & boiling the water for from 30 to 60 min.

Such a method of reasoning & of experimentation would not have deserved so much notice were it not for the great influence which Pouchet's work has exercised & does still exercise on those who have not read it. And I felt it the more necessary to consider his work in detail as he is one of the last observers who maintained a more or less complete heterogeny & as I am not aware of any work in which his experiments have been subjected to a searching criticism.

[The following is crossed out: *Since this time most scientific men have adopted the panspermic theory but there still remain a few who in attempting to satisfy themselves on this subject have met with facts which they have been unable to explain without recourse to the view of spontaneous generation]*

Of these Jeffries Wyman of Cambridge U.S. is the most prominent. I say most prominent not because he has written most nor because he has shown himself an ardent supporter of Heterogeny but because he has evidently approached the subject with a perfectly unbiased mind & has therefore simply recorded his facts without attempting to force any undue conclusion from them. The following are the facts which

Experiments
First method of preparing flasks

Experiments

Results

have been adduced by supporting spon. Gen. as favouring their views. [Crossed out below this is an earlier version in pencil: "…*which seemed to him inexplicable in any other manner than by supposing a spontaneous generation*"] (See also [?] Microscopical Journal 1863 &c) It may be well to quote his own description.

There are 3 methods in which flasks were prepared.
1. "The materials of the infusion were put into a flask (the general relation between the quantity of fluid & the capacity of the flask was, that about 20 c.cm. of fluid were introduced into flasks of about 500c.cm. of capacity.) & a cork[,] through which was passed a glass tube drawn out to a neck[,] was pushed deeply into the mouth of it. The space above the cork was filled with an adhesive cement composed of resin, wax & varnish. The glass tube was bent at a right angle & inserted into an iron tube & cemented there with plaster of Paris. The iron tube was filled with wires, leaving only very narrow passage ways between them."

Into these flasks such fluids as – sugar, gelatine & hay infusion – cheese, sugar & gelatine – flesh, sugar & gelatine &c. – were introduced & boiled for various periods from 15 minutes to 2 hours, while, at the same time, the iron tube, filled with wires, was heated to redness. On withdrawal of the lamp from the flask, the air which entered passed over these heated iron wires. When cold the flasks were sealed with the blow pipe. 14 experiments of this kind are mentioned &, of these, 10 showed, when opened after the lapse of various periods of time, the existence

Second method of preparing flasks

Results

Third method of preparing flasks

Results

What is the explanation of his results?

of organisms, generally vibriones & bacteria. The other 4 remained barren.

2. In others the cork in the neck of the flask is avoided, the neck itself being drawn out & bent at right angles & into the orifice of this tube the iron tube is cemented. The other conditions were the same as in No.1. Similar fluids were used here as in the former case such as gelatin & sugar with a few drops of urine & milk, [b]eef infusion &c. 13 flasks were heated in this way & in all organisms appeared.

3. In others the flask was sealed at the ordinary temperature of the room after the fluid to be treated had been introduced & then it was submerged for a variable period in boiling water. This was a repetition of the experiments of Needham & Spallanzani. Some of these flasks were boiled in Papin's digester. In all the flasks so treated organisms developed.

Such facts coming from an accurate & totally unprejudiced observer cannot be dismissed lightly. It is quite evident in reading Wyman's paper that the facts are accurately narrated & we must therefore see whether any flaw can be detected in the method of experimentation & we must attempt to find some explanation of results so diametrically opposed to those obtained by Pasteur which are, it must be remembered, equally indisputable.

Now if we compare this method with that adopted

Look at size of flask as compared with amount of fluid.

by Pasteur we shall see that, with one exception, the essential details are the same. This exception is however an extremely important one & is probably the explanation of the diverse results obtained by several honest workers & even by the same worker at different times. Pasteur takes a flask having a capacity of 250 to 300 c.cm. & into this 100 to 150 c.cm. of the liquid are introduced. Wyman uses flasks varying from 500 to 800 c.cm. in capacity. & into these 12 to 14 c.cm. of the liquid are introduced. (In neither case was any attempt made at preliminary purification of the walls of the flask nor of the air in its interior.) In Pasteur's case the fluid occupies ½ or more of the capacity of the vessel. In Wyman's it only occupies $1/20^{th}$ to $1/30^{th}$.

Probable explanation.

Such is the only important difference between their methods & this difference affords, I believe, sufficient explanation of the opposite results. For in Pasteur's flask only a proportionally small part of the wall of the flask has to be purified by the steam & the extent of this unpurified part is of course much diminished by the ebullition of the fluid during boiling. There is also in Pasteur's flask only a very small quantity of air, with its dust, to be acted on. It is thus not to be wondered at that a barren result was obtained. But in Wyman's experiment by far the greater part of the flask & of its contents is impure & can only be purified by the steam.

Bastian

His views.

* Bechamp[?] & Ester[?] 1865

Now steam, as heat, must be looked on as dry heat, & it is stated by Wyman in a later publication (American Journal of Science & Arts Vol 44. 1867) that certain forms of organisms may resist the prolonged application of even higher dry temperatures than 212°F. And he at the same time points out that the temperature of the air even ½ inch above the surface of boiling water is many degrees below the boiling point. How much lower then will this temperature be at the orifice of this large flask! But even admitting that steam is moist heat (what [sic] I am by no means disposed to allow) several remarkable instances of vegetable growth at higher temperatures are produced by Wyman – in one case even at a T. of 208°F.

Such is the explanation I would give of Wyman's results & that this is a true explanation will be very evident when I come to the consideration of the method of experimentation adopted by Mr. Lister. This explanation accords in every way with my own experience in which I could point to several similar instances.

*The last defence of Heterogeny which it is necessary to consider is that of Dr. Bastian (The beginnings of Life 1872 &c.) He gives up the theory of organic molecules derived from previously living molecules and attempts to demonstrate that vital force & living matter may

arise de novo under the action of the ordinary physical forces, heat, light, electricity &c. This change of face on the part of the Heterogenists is clearly brought about by the overwhelming evidence produced against Pouchet's views & more especially by Pasteur's success in cultivating organisms from dust in fluids containing no organic matter. A further admission is made which somewhat simplifies the question viz. that organisms have the power of multiplication. The limitation of cases of spontaneous generation which has been gradually taking place is exceedingly instructive. Beginning with the higher animals it became gradually more limited[,] frogs, flies &c being by degrees excluded[,] till now it is only in the case of the lowest forms of life that the doctrine is asserted & even there only in certain instances.

There are 3 classes of cases given by Bastian as instances of heterogeny

The cases which are yet doubtful are given by Bastian in the work quoted & may be grouped into 3 classes. The first class relates to the developement [sic] of organisms in various fluids in vacuo – a condition which Pouchet looked on as inimical to life!

Developement [sic] in vacuo

Into flasks portions of various infusions were introduced. The latter were then boiled for from 10 to 20 minutes & hermetically sealed while still boiling. The fluids used were turnip & hay infusions & also solutions of certain salts, chiefly citrate of iron & ammonia, containing portions of wood &c. In looking at these experiments we are

!!

My results

Again

Also contrary to Bastian himself.

at once struck with the conditions of the first one mentioned. “A closed flask containing a very strong infusion of hay (boiled for 5 minutes) to which had been added 1/20th part of carbolic acid was opened 12 days after it had been hermetically sealed.” Bastian states that this flask contained organisms of a peculiar form . Such a statement as this – that a saturated solution of carbolic acid (for a fluid containing 1/20th part of carbolic acid is saturated) can permit the growth of organisms, - is absolutely opposed to all experience & experiment. In experimenting with turnip infusion, cucumber infusion &c. I have never been able to grow any sort of organism in these fluids when they contained a larger proportion of carbolic acid than 1/275 even although several drops of fluids swarming with bacteria were introduced. Further I have lately performed the following experiment. In Jan. last I introduced into flasks containing strong hay infusion (not boiled) carbolic acid so as to have a strength of the acid present varying from 1 in 20 to 1 in 200 parts & these were covered with cotton caps. When examined 6 weeks later there had not yet appeared in any one of them any sort of organism. And lastly this statement that organisms can resist the boiling of or can develope [sic] in acid fluids is contrary to the whole tenor of Dr. Bastian’s later remarks. His strong point is the developement [sic] of organisms in alkaline – not in acid – fluids after prolonged boiling.

I have repeated Dr. Bastian's experiments with negative results.

*The physical forces & whatever else it may be were however not suitably disposed for spont. gent. [spontaneous generation] at the time & place where I performed these exp[eriments] for to my surprise …

Deposit probably mistaken for organisms

** No doubt other exp.[eriments] have produced evidence supporting Dr. Bastian by showing that long periods are sometimes required for sterilization, but many of these results depend I believe on the same causes as Wyman's while the fact that in some instances such resistance was met with implies the presence of some form of encysted organism or resisting spore rather than some form of organic molecule or physical force. [following this is an illegible sentence]

Bastian further employs turnip & hay infusions (without carbolic acid) & also solutions of such salts as citrate of Iron & Ammonia & he finds that there is generally a slight sediment containing organisms. He himself has to introduce such things as deal wood, cheese &c in order to get these appearances.

With regard to experiments on such fluids as Hay, turnip infusion &c. I may state that I have quite lately repeated them with exactly opposite results. At first I proceeded to repeat them following closely Dr. Bastian's directions in the expectation of getting organisms & looking out for some explanation of their occurrence. *To my surprise I was unable to obtain any developement [sic] of organisms. I tried several modifications in the hopes of finding the cause of their absence but whichever of these vegetable fluids I used I was able to preserve them with the greatest ease [added in pencil: with proper precautions]. Some specimens were very difficult to filter & of course the deposit might be looked on as organisms or not according to the desire of the observer. But I have filtered the fluids under pressure (I could not in this way remove any of Dr. Bastian's supposed physical forces) & having thus obtained a perfectly clear liquid I treated it like the others. There was now no deposit & nothing which could be mistaken for organisms.

** The second series of facts on which Bastian bases

Second series of cases.

Superheated infusions.

Third series of cases

Alkaline fluids.

Reply to the first series.

his arguments are that certain solutions may be exposed in airless & hermetically sealed flasks to a temperature of 270° - 275°F for 20 min. & yet that organisms may subsequently develope [sic] in these flasks. Such fluids were strong infusions of turnip rendered alkaline by liquor potassee. In only one or two cases were numerous and distinct bacteria found. In one of these which is made use of by Dr. Bastian (Nature July 7th 1870) Dr. Frankland found immediately after the boiling that the vacuum, which had previously existed, was unpaired, & yet this flask is, when examined & found to contain bacteria, still brought in proof of spontaneous generation. In other flasks various forms of organisms were found & fungi were present more especially when tartrate of ammonia was used. In some cases he mixed deal wood with the fluid & found bodies like vegetable cells which are undoubtedly portions of the wood.

The 3d & indeed the only series of experiments which apparently form a support to Heterogeny [replacing the crossed-out phrase: *which are worthy of consideration*] are those with alkaline fluids which, as is well known, are more difficult to sterilize than the acid or neutral fluids & Bastian states that superheating does not prevent the developement [sic] of organisms in these.

To the 1st of these arguments I have already replied & my reply is that they cannot in most instances be repeated & that no doubt the deposit which occurs after ordinary

methods of filtration has contained the forms described – these forms not having developed since the introduction of the fluid into the flask. For I would ask, if they had developed anew why was the fluid not full of them? Why was there only a slight deposit[?]. When organisms are really present (as in some of Bastian's specimens) they fill the fluid & render it turbid often with a scum on the surface. [From here struck through in pencil]: The manner in which Dr. Bastian contradicts himself & other Heterogenists is indeed strange. For {in pencil: he had already stated in Nature Vol.I p.9.)] he states that turnip, urine & sometimes hay may remain for an indefinite time in Pasteur's flasks with bent necks without any developement [sic] of organisms occurring in them. Is this because a vacuum is better suited for the occurrence of spontaneous generation than the presence of air? Such is what we are led to suppose to be Dr. Bastian's view & yet M. Pouchet, whose experiments Dr. Bastian accepts as correct, states expressly that spontaneous developement [sic] of organisms is impossible in a vacuum & employs vacuum for repeating Pasteur's experiments, trying to show that, as soon as air is admitted, spontaneous generations occurs.

Huxley's reply.

The whole of Bastian's experiments are completely answered by Prof. Huxley in his letter in reply to Dr. Bastian's attack on his speech at the Liverpool meeting of the British Medical Association. [End of striking through] Prof. Huxley (Nature Oct^{r} 13th 1871) having[?] seen Dr. Bastian's experiments & preparation[,] examines

his belief that the organisms which Dr. Bastian got out of his tubes were exactly those which he put into them, that in fact he had used impure materials & that what he imagined to be the gradual developement [sic] of life & organization was the simple result of the settling of these solid impurities. (Bastian himself had pointed out that crystals of tartrate of ammonia, when old, generally contain fungi in their interior (Nature Vol.II.p.220)). [Struck through with pencil]: As an example of Bastian's imperfect experimentation he points out that [end of striking out] [Addition in pencil]: Prof. Huxley goes further & accuses Dr. Bastian of imperfect observation for he points out that Dr. Bastian imagines that water can boil at a temp. of 230° to 250°F in flasks with holes in them! He further relates how Dr. Bastian showed him one specimen as a fungus developed spontaneously which turned out to be nothing but a fragment of the leaf of a sphagnum!

[Addition in pencil]: It is however but fair to Dr. Bastian to quote Dr. Burdon Sanderson. [The whole of the following paragraph has been struck out and replaced in pencil with: *I shall return to Dr. Bastian presently*.] With such an experimenter I should not have dwelt so long had it not been that his writings have been received by many as possessed of great weight. Nevertheless his statements are now contrary to the great majority of observations & are so contradictory both with each other & with those made by the party whom he tries to support that there is evidently an error somewhere.

Experiments with alkaline fluids

The only experiments which require consideration are those with alkaline fluids because a difficulty has been found by various observers notably Pasteur in preserving these fluids. This difficulty in purifying these alkaline fluids is attributed by Pasteur

Roberts disproved Heterogeny in this case

* [Addition in pencil] My own results with alkaline fluids.

These facts show that the alkali added to the fluid do not ? any tendency[?] of the molecules of that fluid to ? ? but they show that the alkali does give to particles the power of resisting heat.

His method.

[to the] imperfect wetting of the organisms. However that be Dr. Roberts (Phil. trans. 1874) has conclusively proved that it is not a case of spontaneous generation for he has shown that, while on the one hand an alkaline fluid is very difficult to sterilize yet the same fluid without the caustic potash is very easily rendered barren – the caustic potash is pure & if each be sterilized separately & then brought together, without any fresh access of dust, the fluid still remains pure; in other words the caustic potash does not determine spontaneous generation. On the contrary he shows that the potash acts by increasing the resistance to heat of the particles – which are the forerunner of organisms - not the abiogenic aptitude of the infusion. “Ten flasks were charged with unneutralized hay infusion. Five of these were simply plugged with cotton wool & boiled over the flame of a lamp for 5 minutes. The other 5 were also plugged with cotton wool, but through the centre of each plug there passed an hermetically sealed glass tube, bent obliquely & constraining the quantity of liquor potassee requisite to neutralize the fluid in the flask. These tubes had been previously heated (after being charged with liquor potassee & sealed in oil up to 121°C. in order to destroy any organisms they might contain. The flasks thus prepared were then boiled over the flame for 5 minutes. At the end of a fortnight their contents were unchanged. The tube was now broken & the liquor potassee mixed with the fluid. Not one flask germinated – at the end of 2

I can quite confirm Dr. Roberts' statements for I have used his method of boiling & separating as an easy mode of obtaining any required degree of alkalinity & I have never got any results which in the least support the view that the addition of liquor potassæ to a fluid will make organisms develop in that fluid.

Mr. Lister's Experiments.

*Thus avoiding Spallanzani's error

**[Addition in pencil]: Mr. Lister's method has removed a great source of error in all these experiments & I am confident that if his instructions be thoroughly followed out these instances of difficulty in purifying fluids will become fewer & fewer. During 4 years more or less continuous work at such experiments I have only once met with an instance of difficulty in purifying fluids.

Dr. Roberts' Experiments.

months they were still barren. But although these flasks had not acquired the power to germinate they had acquired the property of enabling freshly produced germs to survive a boiling heat, for when the flasks were unplugged & infected with ordinary air or water & then replugged & boiled 5 minutes, their contents in every instance germinated in a few days."

I have already mentioned Mr. Lister's method of procedure in preserving fluids. I have mentioned how successful this was & how all the difficulties as to tall & small flasks &c. were overcome simply by purifying the walls & the air in the flasks,* & by taking care, in the introduction of the impure fluids, to avoid their contact with the neck & walls of the flask so purified (above the level of the liquid) and by avoiding spurting & frothing. By Mr. Lister's method all sorts of fluids may be preserved & transferred from one vessel to another without the developement [sic] of any organism – in the same way as, as mentioned previously, without the occurrence of any fermentative change. **As to the absence of fermentative changes in purified fluids exactly the same remark applies to the experiments of Dr. Roberts & Prof. Tyndall. For their experiments were undertaken with a view to the determination of the question of spontaneous generation.

And in my own experience, in order to test various

My own experience

*[Addition in pencil] The case in which I found difficulty in purifying milk has been mentioned & will be discussed presently.

Thus Heterogeny has been gradually driven from one point to another

materials, as to whether they contained organisms or not, I have prepared many hundred flasks of cucumber & turnip infusion & also many of milk, meat &c without in any single instance obtaining the slightest evidence of abiogenesis.* The rapid souring of milk during a thunder storm is looked on as a change due to the electricity. With this rapid souring there is a rapid increase of bacteria. I have kept flasks of pure milk for a year, through several virulent thunder storms without any change taking place in it & without the appearance of any organisms. And at the end of the year the milk was quite fluid, & of normal character. Yet in a few days after these flasks were opened (they were covered with cotton wool caps) the milk had coagulated, was putrid & contained numerous bacteria.

[Whole of next paragraph struck through]
Thus we have seen how the first assertions of the Heterogenists with regard to the developement [sic]of the more highly organised animals have one & all broken down, while we have traced how they have been gradually driven from their strongholds till at last the only support in favour of that view rests on a few isolated experiments [from here further struck through] by one whose evidence is not accepted by those who have seen his work & who are most competent to judge while his experiments are evidently in great part incorrect.

With the great mass of evidence existing against the

Unboiled fluids & tissues.

Lister's attempts to obtain pure milk. What they prove.

view of spontaneous generation I should have no hesitation in casting it aside. But much stronger facts remain to be alluded to viz. those with regard to unboiled fluids & tissues.

I have clearly referred to these in a former part of this essay & I may now state that where I have mentioned that putrefaction was absent I might equally well have said that organisms were absent. I have mentioned that blood, milk & urine could be preserved, unboiled & unacted on by chemical reagents for an indefinite period without putrefaction & I may now add, without the developement [sic] of organisms. Of these I may revert for one moment to Mr. Lister's experiments on milk [next phrase struck through] & to the experiments by Cazeneuve & Livon on urine [end of striking through]. Mr. Lister in milking into pure flasks found that in none did he obtain the lactic ferment but in all but two (out of some 50 or 60 flasks) organisms of some form or other occurred, these organisms being in many flasks of several wholly distinct forms. Now these facts prove a great deal against the view of spontaneous generation. Two flasks had no organisms at all, therefore milk does not spontaneously generate organisms, does not naturally contain them, has no inherent tendency to undergo fermentation. Then milk which has never been in the dairy does not undergo lactic fermentation hence the cause of this fermentation was something which came from the dairy, not any molecules present in the milk. Then, not only the fact that in two flasks no organisms

Cazeneuve & Livon's experiments on urine. (These are important)

*[Addition in pencil] My own exp. on milk referred to before bear out Mr. Lister's results in every particular – distinct evidence is shown that a much larger proportion of flasks may be preserved if attempts, however, incomplete, be made to render the particles in the air inert by the use of carbolic acid.

**[Addition in pencil] Use under head of Fermentation of Urine

Cazeneuve/Livon's exp. already mentioned & further discussed immediately add great additional evidence.

developed, but also the great variety which appeared in the different flasks, prove that they could not have appeared spontaneously. For if we have 12 tubes under the same shade each containing the same specimen of milk taken at the same time under the same conditions, any change occurring in one due to something in this milk would have occurred equally in all. But when we see one remaining without organisms, another having a pigment micrococcus, another having bacteria, another fungi &c we must conclude that the appearance of these forms cannot be due to anything inherent in the milk, [struck through: *otherwise under exactly the same conditions each might have been the same*] but that it is due to something which has entered the milk from without.

* **In Cazeneuve & Livon's experiments the same statement may be made, that the absence of change was concomitant with the absence of organisms.

The method employed has been previously described & in the experiments formerly mentioned as being successful no organisms were found while if the somewhat concentrated urine were removed & heated with water it became alkaline in 24 hours & was filled with "torulacée". Similar results to those mentioned were obtained when the urine had been previously rendered alkaline by the administration of soda & Potash. Simple puncture of the bladder was soon followed by alkalinity & developement [sic] of organisms in the urine hence it is not the absence of oxygen from the bladder

which was the cause of its non putrefaction. And the following experiment shows that the merest trace of oxygen is all that is required if indeed it be at all necessary.

Prevent the evaporation through the walls of the bladder by immersing it, immediately on its removal from the body, in melted paraffin at the temperature of 45°C. This temperature is insufficient to destroy the germs which fell on the wall of the bladder during its transit from the abdomen to the paraffin. (The reason why the organisms do not develope [sic] in the bladder hung up to the air is that the fluid dries as soon as it transudes & therefore the organisms have no fluid in which to develope [sic].)

“Thus a layer of paraffin covers the outside of the bladder, preventing the rapid evaporation of the fluid which transudes. In 24 hours remove the paraffin case. It is then found to contain an alkaline turbid fluid with organisms. These organisms have not however had time to penetrate into the interior of the bladder & therefore the urine in it is found to be devoid of living organisms & acid. The same is the case with alkaline urine – the fluid outside containing organisms, that inside being free.

But let the bladder be first dipped in paraffin at 100°C so as to let no living organisms be in contact with the wall. It is then removed at the end of 1 minute & plunged into paraffin at 45°C. so as to get a thicker coat (this paraffin having previously been heated to 110°C & cooled, protected from dust).

My own experiments on unboiled Tissues.

Beakers were tested.

Test experiments.

After 3 days the fluid outside the bladder - in the paraffin cup is still found to be clear, acid & devoid of organisms.

This same bladder is now left exposed to the air for 5 hours, then it gets a second coating of paraffin at 45°C. & is left surrounded by this for 3 days. Now the fluid outside the bladder is found to be ammoniacal & to contain organisms. Hence the walls & the fluids were not modified by the heated paraffin in the first part of the experiment.

I also mentioned the experiment on grape juice by Van der Broeck, Pasteur &c. & on vegetable tissues by Roberts. And then to my own experiments on animal tissues.

I described the method I had employed, how the tissues were introduced into & preserved in flasks containing cucumber fluid & how they remained unchanged, & I may now add without any developement [sic] of organisms. That no organisms did develope [sic] I further ascertained by testing the beakers, by transferring by means of a pipe syringe a drop of their contents into a flask containing pure cucumber or meat infusion & I have found that no organisms developed in the latter.. That organisms would have been found by this method if they had been present in the tissues is shown by the fact that, if bacteria were injected into the animal immediately before death, they were found in the beakers & in the flasks. I shall presently

*[Addition in pen] That the question has no answer itself to this, that it is now no longer a question of spont. gen. of organisms in fluids wh[ich] have been severely boiled, but that it is a question as to whether some of the particles wh[ich] get into fluids & which resist heat may not be organic molecules[,] not necessarily derived from previous bacteria[,] or whether they are bact. or their germs[,] is evident from what has gone before & at last Dr. B[astian]. has formulated similar views in his paper in 19th Cent. &c. This corresponds very closely to the position wh[ich] Liebig was ultimately driven to take up on the subject of germs.

Conclusion

What are the particles which originate organisms

Probably spores

The question of sp. generation then narrows itself to the following: what are these particles & have we any ground for considering it [?] probable that some organic molecule resists heat longer than organisms or their spores. Have we any grounds for believing that organic molecules ever develope [sic] spores [added here: quote Bast. p.195 19th Cent. Exp. show that they do not whether boiled or unboiled[,] why should a few floating about do it (under 2).

Analogy with plants

Spores of fungi have been found in dust

Bacteria propogate in a fluid

Dr. Bastian has at last formulated the same views in his paper in the 19th century.

Under 2

Take for example the albuminous discharges from wounds. I shall subsequently prove that these may be kept in wounds & cavities (& in wounds & cavities then no carbolic acid can have access as in psoas abscess) & that any developement [sic] of organisms whether [… note incomplete]

[in ink] Under 2 Bacteria develope [sic] minute spores … & sporules.

Bastian does quote the views of [those who] also hold that living matter has come into being independently but these are only views & there is no shadow of proof in them.

See Proceedings of Royal Society 1878

3. Then take up the death point of org.

Under 2 or refer to Cazeneuve or Livon which will be quoted later or to Paul Bert (ditto)

return to this subject.

It may therefore be regarded as certain that organisms do not originate spontaneously [added in pencil] in the sense formerly held [end of addition] under any circumstances but that they appear in animal or vegetable fluids as the result of the entrance of solid particles into the fluids after their exit from the living organism. What are these particles?* [addition in pen] Firstly, there is ? reasonable[?] in ???. [end of addition]. That we cannot answer with certainty but in all human probability they are organisms or their spores. We know that the seeds of plants are often wafted by the air & this is more likely to be the case the lighter the seed is & therefore it is generally admitted that the seeds of fungi – large or small – are carried by the air. Indeed both Pouchet & Pasteur found & recognized the spores of fungi in air dust. Further bacteria propogate by fission in fluids [-] that is to say one bacterium developes [sic] from another & this process has been often observed. Then again some forms of bacteria have been found to produce spores notably bacilli. Thus the spores of bacillus anthracis have been demonstrated by Koch & still more recently the whole cycle of events – notably the occurrence of small sporules of excessive minuteness has been traced by Ewart. Ewart has also shown that other forms of bacteria, & of Spirillum produce spores in like manner. If then in fluids bacteria always

Wyman's algi
I have stated then that I had only met with one instance of difficulty in putrefying fluid. This was milk got from a special shop near the Strand. [?] fresh milk could be easily purified but I have immersed a flask containing this milk for as long as 3 hours in boiling H_2O without sterilization – identical results developed in all cases – as far as could be judged fr. mic. observations long bacillus & also four fermentative spores from the milk presented the same changes. Here we had some particle which got into the milk & caused a special change & a constant developement [sic] of a particular & I may say easily recognizable organism. Is it more reasonable to suppose this a germ or an organic molecule.

Where do the organic molecules come from. They are not present in unboiled organic fluids for they do not cause change or organisms. They must be specially manufactured in the air.
Bastian in 19th Century [-] question as to whether these particles are derived from [?] or not is made to depend on [… unfinished note]

2nd No grounds for supposing that organic molecules can give rise to bacteria. See previous page.
3rd No evidence that organic molecules can resist heat more than [?]
Bast. exp. on heat p.74 are no doubt valuable but they do not touch the question that there may not be spores which resist much higher temp. For here neither spores nor organic molecules developed.

Relation of organisms to Fermentation

Wind up with Yeo's views as to spermatozoa

No.2 I have inoculated numerous fluids with pus, serum, blood &c. – all sorts of organic mol. living or dead without germs or bacteria.

grow from bacteria, if bacteria like fungi produce spores why refuse to believe that the particles in the air which give rise to bacteria are bacteria or their spores as the particles which give rise to fungi are shown to be their spores?

The excessive minuteness of the bacillus spores as described by Ewart renders them difficult of discernment in fluids under the microscope. When dried & shrunk we should expect them to be hardly visible. And if the spores of such large organisms as bacillus anthracis are hardly visible how much less likely are we to find the spores of organisms which themselves can be seen only with difficulty. It seems to me that it would be a very extraordinary thing if in the case of these minute organisms alone the particles which gave rise to them were not derived from preexisting forms. But when we find that under certain circumstances the same form of organism originates from these particles, as we should expect were the latter spores, the case is made still stronger. This will be more evident as we consider the relation of these bodies to fermentation [addition in pencil] & the following experience of Mr. Lister's confirms this view. Granuligera [-] results of exp. I have twice only tested the main points in this question . Numerous [?] were found but their result does not …[remaining few words illegible]

I must now pass on to the relation of these bodies to Fermentations. And first, I may say

Organisms are always present

According to L. it is decaying matter wh[ich] causes fer[mentation][?] but decaying pus[?] which he introduced into milk - mention p. - & no ferment. Contrast with the introduction of pus containing zinc[?]

Particles cause fermentation
Particles cause organisms

Hence ? organisms cause fermentations

Discussion of the process of fermentation between vital fermentations & chemical ferments

it is now admitted that organisms are present in all fermenting fluids, though formerly this was denied on account of the imperfection of the methods of examination.

We have already seen, in the first part of this essay that the cause of fermentation in organic fluids was the entrance into them of solid particles held in suspension in air. We have also seen that the cause of the developement [sic] of organisms in fluid is the entrance into them of solid particles suspended in the air. We also know that in all fermenting fluids organisms are present, & that in the absence of organisms no fermentation occurs. What more likely then than that the particles which cause fermentation & the particles which originate organisms are one & the same? That in fact the fermentation of a fluid is the result of the growth of organisms in it? It may be that the immediate cause of the chemical change is some chemical substance resembling ptyalin, pepsin &c. No doubt there is this difference between ordinary fermentations & those due to a chemical substance[:] that in the former instance the ferment itself multiplies. This however is easily explicable if we suppose each organism to be a former of the ferment. The process in these “vital” fermentations of which we may take the alcoholic as an example may be made quite analogous to fermentation[?] by unformed ferments on the following suppositions

Both fermentations produced by cells

Cells of one free hence ferment apparently self multiplied. Cells of other fixed.

German views on production of ferment by yeast [note in pencil]

the same as in the so called chemical fermentation. Thus, to speak of the salivary action, ptyalin is not a chemical compound formed spontaneously or the result of any sort of double decomposition, it is a substance formed as the result of the vital action of certain cells. The vital fermentation would be brought into the same category. Supposing that, for instance, the alcoholic ferment is likewise the result of the vital action of certain cells, the yeast cells. The ptyalin itself has not the power of self multiplication but the cells which form it produce it continuously; the ferment of alcohol would not have the power of self multiplication, the cells which form it produce it continuously (as a proof of this contrast the effects of ptyalin a starch with those of yeast on sugar. The ptyalin is more or less immediate in its effect. It is a very actively formed ferment.The yeast acts slowly because the ferment is only formed in proportion as the yeast grows. Hence the explanation of the rapid action of the one & of the progressive action of the other). The apparent self multiplication of the ferment in the alcoholic case is due to the fact that the producers of it are free, float freely in the fluid, & hence ultimately no drop of it can be taken which will contain sufficient ferment to act without the ferment producer being also present.

Emulsin & alcoholic ferment compared

*Or it may be that the organisms in living feed on the organic substances & that the result of the fermentation is the portion of the food which has been rejected or excreted by the organisms [addition in pencil].
Pasteur's view.

In the case of the saliva the ferment producer is fixed & the ferment is obtained separated from its originator hence it does not multiply. The same causes which cause the production of ptyalin to cease, cause also the alcoholic fermentation to cease for they destroy the tiny cells which separate the ferment.

The case of the emulsin is exactly the same as that of the yeast. The emulsin itself does not multiply but the seed – the producer of it [-] does. Sow a seed of the bitter almond & there springs up a plant bearing numerous seeds, numerous producers of emulsin. Sow a yeast cell & there follows the growth of numerous yeast cells each producing a quantity of the ferment. {Struck through] The analogy is absolute – the same analogy runs through all the cases & I consider this a most probable view. [end of striking through].*

Again, it may be that, as Pasteur holds, the cause of the fermentation is not the production of a ferment but the breaking up of the chemical compounds by the growing plants in the search for nutriment. It is quite clear that there must be such a breaking up. It may be that in this breaking up some of the molecules may rearrange themselves & form a ferment & the presence of a ferment of this kind I consider [struck through] to be absolutely necessary [end of striking through]

the best explanation of special fermentations though I prefer to hold that the ferment is excreted from the cell itself. But following out the other view it is quite clear that a breaking up & a rearrangement of the molecules of an organic liquid must occur during the growth of organisms in that liquid & it is just possible that this is the whole process. In any case there is nothing unreasonable in associating these changes with the growth of living cells, in fact all analogy points to such a relation.

Only other theory now tenable.

The only other theory which seems to me to be tenable in presence of the facts stated would be that the particles which cause the fermentation are not the same as those which give rise to the growth of organisms but that they are bodies which have the power of causing fermentation[,] possibly decomposing bodies or special ferments. But then is it conceivable that fermentation & the developement [sic] of organisms should always be associated or that organisms of the same form, e.g. yeast cell, should always be present in the same fermentation? Or supposing this [is] conceivable how are we to account for lactic fermentation in one specimen of milk, putrid fermentation in another &c?

Inconceivable.

Liebig's views have been modified.

*We have seen how intimately Liebig's view was associated by Dr. B. with his own & how he quote …. [Cheyne's ellipsis] [addition in pencil]

[In pen] That Liebig's theory of decaying matter is incorrect will be seen by the exp. mentioned on pp ….. are there considered [sic].

Liebig originally propounded the view that fermentation was a change in organic fluids & tissues originated by the presence of oxygen or of bodies in a state of decomposition. The organisms he at that time regarded as quite accidental. He supposed that when organic matter was exposed to the air it underwent a slow process of oxidation which he termed érémacausis & that this change, communicating itself to other molecules[,] caused them to break up or putrefy. In the presence of the facts stated as to the relation of dust to fermentation of boiled & unboiled fluids Liebig has lately altered his views & in his last publication he admitted that yeast was in some way or other connected with the alcoholic fermentation but he considered it most probable that the relation between them consisted in this, that when the yeast cells died they decomposed & that the chemical change thus set agoing propagated itself to the sugar & caused it to break up into alcohol & carbonic acid.* [Struck through] There are however certain other definite facts known which show that organisms do take part in certain fermentations & these cases together with the constant presence of these bodies & the other parts already stated render it to my mind certain that these organisms are in one of the ways indicated the causes of these chemical changes. [end of striking through].

The first case of fermentation which was studied &

Alcoholic fermentation & the yeast plant

the one about which the most is known is the alcoholic fermentation. The facts & experiments on this subject are now so universally known that it would be superfluous for me to do more than summarize them here.

Cells always present

The yeast plant (Torula Cerevisiæ) is always present in a state of vitality when sugar undergoes the change into alcohol.

When destroyed – no fermentation

If yeast be raised to the temperature of 60°C., at which temperature the cells die, the fermentation of sugar no longer occurs. Here the cells are dead & if Liebig be right that dead cells not living ones are the cause of fermentation, that process ought still to go on.

Juice of grape has no spontaneous tendency to ferment

Van der Broeck's exp.
Pasteur's.

The juice of the grape has no spontaneous tendency to undergo fermentation as shown by the experiments of Van der Broeck &c formerly mentioned & by the following experiment narrated by Pasteur. In a flask the neck of which was drawn out to a fine point Pasteur boiled water which had been used to wash the outside of a grape which therefore according to other experiments contained abundant causes of the alcoholic fermentation. The point having been heated was now plunged through the heated skin of a grape, was broken in its interior & a drop or two of the juice was thus introduced into the boiled water. Here then we had unboiled grape juice, oxygen, water & dead organisms

…Other parts obtained by Pasteur.

& organic molecules & yet no alcoholic fermentation occurred. This & other experiments (Etudes sur la bière) led Pasteur to the following conclusions; “le moût de raisin cuit ne fermente jamais au contact de l’air priver des germes qui s’y trouvent en suspension.
Le moût de raisin cuit fermente quand on y introduit une très petite quantité d’eau de lavage de la surface des grains de raisin ou de la surface du bois de la grappe.
Le moût raisin ne fermente pas après qu’on y a introduit cette eau de lavage, portée à la température de l’ébullition, puis refroidie.
Le moût de raisin ne fermente pas quand on y introduit une très-petite quantité du suc interieur d’un grain de raisin.”

Pasteur further shows that the apparent spontaneous commencement of fermentation in these fluids is due to the presence, in the air, of spores of the Torulæ though as a rule they are not present in great abundance. Pasteur further brings forward experiments to show how other fungi as Mycoderma aceti can, when there is too little oxygen present, cause the splitting up of sugar in alcohol.

Butyric fermentation. Similar proof.

Butryric [sic – Butyric] fermentation. Pasteur has also brought forward evidence of a similar nature as to the butyric fermentation. The organisms which produce butyric fermentation are bacilli

Formation of Pigment.

which live without free oxygen, in fact are killed by it, & which may be inoculated from milk into Pasteur's fluid & even there continue to cause butyric fermentation.

Pigment Bacteria.

Striking facts as to the association of a definite chemical change with the presence of organisms of a definite form are brought forward by Cohn & Schroeter with regard to "pigment bacteria". (Beiträge zur Biologie der Pflanzen). They showed that while many forms of organisms could grow on such soil as boiled potatoes yet with the constant presence of one form only was a definite pigment produced. These were small round organisms called "Micrococcus prodigiosus." These pigments were sometimes scarlet, sometimes blue &c. & when once obtained they could be propagated from one soil to another, the same colour & the same organism always recurring after inoculation from a specimen. Not only might these pigment bacteria grow on boiled potatoes, they could flourish also on cheese, meat, white of egg, bread, starch &c. the same pigment being invariably produced. Cohn further found that these same organisms developed readily & produced the same pigment in artificial cultivating fluids containing ammonia & a carbonate but no trace of albumen. Once obtained in this fluid they could be propagated in many specimens indefinitely, the same

These cells resemble pigment cells of plants & animals only the former are free.

*These facts therefore absolutely prove that the pigment fermentation occurred during the life of the organism. The pigment must have been built up by it & could not have originated as the results of chemical changes propagated to the fluid.

pigment being constantly produced. Lastly, the conditions under which this pigment appeared were exactly those which were most favourable to the life of the organisms while those in which the pigment was absent were those in which the organism could not develope [sic].

Here then we have a definite chemical change undoubtedly brought about by a definite variety of organism. No dead tissue, no spontaneous change ever occurs in Cohn's artificial fluids resulting in the formation of these pigments & yet as soon as these organisms which are associated with definite pigments when growing on albuminous soil are introduced into these mineral solutions, these same pigments appear. That, in the latter case, the pigment is formed by the bacteria is evident, for it is a complex organic substance – closely related to the aniline colours – though formed from a few inorganic salts. And as before said[,] Lister refers to other facts. There is nothing remarkable in this production of pigment by living cells. It is only what is constantly occurring in the many vegetable & animal cells & in the former they are formed from inorganic compounds. Indeed it would be against all chemical experience that by any simple chemical process a complex organic compound would be so readily built up from a few inorganic salts of the most simple kind.*
Burtin

Lactic Fermentation

Bacterium lactis always present

Bacterium lactis

Lactic Fermentation.

The instance, in which the relation of organisms to a fermentative process is most absolutely proved, is that of the lactic fermentation of milk investigated by Mr. Lister. (Pathological Transact. of London 1878).

As already pointed out Mr. Lister found that milk had no inherent tendency to undergo lactic fermentation, in fact unless it were brought into a dairy or in contact with dairy vessels or workers all sorts of fermentations would occur rather than the lactic. In examining milk undergoing lactic fermentation he found an organism of a definite & early recognisable form constantly present, while in milk which was not brought into the dairy & which did not undergo lactic fermentation this form of organism was absent.

"This organism is a motionless bacterium … occurring most commonly in pairs but frequently in chains of 3, 4 or more individuals, each segment being of somewhat rounded form, more or less oval, with the long diameter in the direction of the length of the chain & often showing, on careful focusing, a line across their central part indicating transverse segmentation. They vary in diameter …full sized specimens measuring about 1/20,000 inch."

The lactic ferment is as just remarked rare in the air & it is also equally rare in water.

Lister's experiment (very conclusive)

Now was this organism so constantly present in milk undergoing the lactic fermentation, the cause of that change or not? Was some other bacterium the active agent or was it some hypothetical organic molecule? Mr. Lister solved this question in the following beautiful manner. He prepared a series of flasks containing fresh boiled milk & these flasks were kept until it was certain that they would remain permanently sterile. Having calculated how many organisms were present in a given quantity of fermenting milk he so diluted this milk that there was only one bacterium present in each drop of the fluid, considering them to be equally divided. This was done in the following manner. "By means of the syringe already described" (one graduated to the 1/100 of a minim) "one or more hundredths of a minim could be measured with precise accuracy; & I found that 1/50 min. exactly occupied a circular plate of thin covering glass, half an inch in diameter, so that when such a drop was placed on a glass slide, & a cover glass of the size mentioned & quite flat was put down upon it, all air was expelled from under the latter & the rim of fluid that formed round about its margin was so narrow as not to measure a quarter of the diameter of the field of the microscope even when the highest magnifying power was used. In other words 1/50 min. was disposed in a thin uniform layer of the

exact size of the cover glass. Hence the number of bacteria under the glass slip, that is to say in 1/50 minim was equal to the number of the bacteria in a field of the microscope multiplied by the number of times the area of that field went into the area of the covering glass. The micrometer gave the diameter of the field in thousandths of an inch; & the cover glass measured 500 thousandths of an inch across: & the areas of the circles were of course proportioned to the squares of those diameters. All that was needful, therefore, in order to enable me to calculate the number of bacteria in 1/50 min. was to form a fair estimate of the number of bacteria per field, & this was done by counting the organisms in a considerable number of fields & taking the average."

"As the result of the estimate which I made of the number of bacteria present in every 1/50 minim, I found it necessary to dilute the milk with no less than a million parts of boiled water in order that every 1/100 minim should contain on the average a single bacterium.

"This having been done 1/100 min. of the infected water was added by means of the syringe to each of five glasses of pure boiled milk. The result of this inoculation was that only one of the 5 glasses was affected at all." The others remained unchanged without fermentation

Conclusive proof that bacteria & not a hypothetical organic molecule was the cause of the fermentation.

& without bacteric developement [sic]. The other underwent lactic fermentation & in it was the bacterium lactis (described before) alone. No other form of organism was present. This bacterium was inoculated into urine & developed there. After 4 days milk was inoculated with this urine. The milk underwent lactic fermentation & these bacteria were present. Drops of urine diluted so as to contain 3 bacteria per drop caused lactic fermentation in all the flasks to which they were added.

"The following experiments afford absolute proof that this bacterium was the cause of the fermentation.

"On the 30th of August last, having provided sixteen pure glasses of boiled milk, & having estimated in the manner already described, the number of bacteria present in every 1/50 minim of a glass of boiled milk which had been inoculated the day before by touching it with a heated needle dipped in milk curdled under the influence of the pure ferment, I diluted a drop of this milk with boiled water to the requisite degree, & introduced into each of ten of the 16 uncontaminated glasses a drop calculated to contain on the average a single bacterium, while 5 of the rest received each a drop supposed to contain 2 of the organisms, & the remaining glass was inoculated with a quantity in which, according to the estimate, there would be 4 bacteria. The result was

Putrefactive fermentation.
[in pencil] Detail some of Pasteur's & Cohn's experiments

that within 3½ days the glass into which 4 bacteria were supposed to have been introduced contained a curdled mass, & the 5 which had received the drops arranged for 2 bacteria each, had all undergone a similar change. Of the 10 inoculated with drops averaging one bacterium each the majority were at this period still fluid, but some assumed the solid condition in the course of the next 24 hours, though at different times. But of this series of 10 exactly 5, as it happened, remained permanently fluid."

Every glass which had curdled contained the bacterium lactis, the 5 glasses which were unaffected contained no organisms.

Hence it seems clear that where this organism is present lactic fermentation occurs, where it is absent this change does not take place. We could hardly suppose an organic molecule or ferment to occur exactly in the same cases as the organisms appeared without there being some intimate relation between them. If that were the case some flasks ought to undergo lactic fermentation without the presence of any organisms, & thus ought to show developement [sic] of these organisms but no lactic fermentation.

Putrefactive fermentation

Numerous facts have of late been brought forward with regard to this by Pasteur & others. None of them

*[addition to passage below in pencil] It is an investigation of this sort which has been demanded by many writers as for instance by Mr. Holmes. At least that is what I take him to mean by foll[owing] pass[age]

Pasteur has produced facts.

[in pencil] Cazeneuve & Livon.
Lemaire's[?] Experiments
Bèchamp & Estor's theory . See under Caury[?]
Paul Bert's Experiments

Results of Aseptic Surgery very important

[in pencil] All these experiments which have gone before relate to fluids & tissues removed from the body & preserved in flasks. It now remains before quitting the whole subject to see if any conclusions hold good for fluids & tissues retained in connection with the living body. *The mode in which I propose to ascertain this is by examination of such fluids & tissues in various ways & under varying circumstances. We have before seen that the method which we have agreed to term the Aseptic method was nothing more or less than a constant series of exp. to render atmospheric dust inert before reaching wounds. We must therefore enquire how far this fulfils its objects. 1. does it prevent putrefact[ion]? 2. If it prevents put[refaction] does it exclude organisms from the wounds. 3. If under any circumstance organisms enter the wounds what are their peculiarities, do they do any harm & if not why not? 4. Are organ[isms]. present or do ferment. occur in fluids or tissue in the living body wh[ich] have never been exposed to the atmospheric dust. 5. If they do occur what happens [&] how is this to be explained. Can these facts be reconciled with the conclusions wh[ich] have gone before. Is there any other mode of explaining them. 6. If organisms do enter aseptic wounds 1. Does this method prevent put[refaction]. Undoubtedly it does. Whence do they come.

however afford absolute proof. I shall therefore proceed to produce a piece of evidence obtained by myself which is of immense interest & which furnishes, I venture to think, very strong evidence of the connection between bacteria & putrefaction more especially when taken together with the facts produced by Pasteur &c.

[Struck through] The ground on which Listerian aseptic surgery is based has been already stated & we may now go further & call these particles which he attempts to exclude, bacteria or their spores. Mr. Lister's treatment is a constant series of experiments on the germ theory of putrefaction. Open an abscess connected with disease [sic] bone, keep it open without any antiseptic means & the pus in the abscess cavity will certainly [addition in pencil] ferment, & most probably [end of addition] putrefy. The cause of this putrefaction we know to be certainly due to particles arriving in the fluid from the atmosphere, particles which we know may be excluded by filtration or destroyed by heat or various chemical substances. The latter is the method employed by Mr. Lister to prevent their access to this abscess cavity & this method has been fully described in the first volume. What now is the result of Mr. Lister's experiment? [Struck out] Does it prevent putrefaction? [end of striking out – replaced in pencil with:] Compare the result with a case treated by the strict List. method. I may say after a very long experience of Mr. Lister's own practice, [addition in pencil] & after practicing it for a long time myself [end of addition]

Does it prevent organisms.

Investigation by myself.

that it does prevent putrefaction entirely. In the case of the supposed abscess or any wound in a situation where antiseptic dressings can be applied & treated in the manner previously described, one who has had some experience in these experiments may reckon with certainty on avoiding putrefactive or other fermentation of the discharges. This result I reiterate is constant in careful & experienced hands, & anyone taking the trouble to go round Mr. Lister's wards would see that it is so. [Struck through] I have under my care, happily now cured, a patient of Mr. Lister's who 3½ years ago came under my care with 2 psoas & a lumbar abscess. These have been opened & dressed at varying intervals – at first every day, latterly once a week – during these 3½ years & putrefaction has never occurred.

Such being the facts with regard to the absence of putrefaction is it equally the case that organisms are absent? If so then we have another strong proof of the relation of organisms to fermentative changes. We saw the constant relation of the lactic fermentation to the bacterium lactis [-] can similar facts be found with regard to septic & aseptic wounds. [end of striking through]

The first communication on this subject was made

Ranke's results from his microscopical exam.

*He does not carry his investigations farther but on this evidence at once rejects the germ theory as explaining the etiology of septic diseases.

Klebs objected
Ranke replied
[1]Archiv für Experimentelle Pathologie Bd.iii p.315
[2]Deutsche Zeitschrift für Chirurgie Bd.7 p.63
.

6 years ago by Dr. Ranke of Halle (Chirurgische Centralblatt 1874). He published a note of some 300 examinations of the discharge from 15 wounds treated aseptically & following an aseptic course in which he states that on only one occasion did he fail to find organisms. The organisms present were for the most part micrococci in pairs, also streptococcus, rarely small or middlesized bacteria. [Struck through] He therefore does not accept the germ theory as applied to putrefaction taking place in wounds [end of striking through & replaced by *]

While by some these observations have been received as accurate & as confirming their previously formed views, by many they have been looked on as erroneous, either from having been made on cases in which the aseptic treatment had been imperfectly carried out, or in themselves faulty. In answer to objections of the former nature, urged by Professor Klebs of Prague[1], Dr. Ranke published another paper in July 1876[2], quoting cases to show that the treatment had been in reality properly carried out. He instances especially cases of hydrocele, treated by making a small incision into the sac, stitching it to the skin, & treating aseptically, where cure followed without any inflammation or constitutional disturbance, but where, nevertheless, organisms were present in the discharges. From those cases, as well as from the various published

reports of the results of Prof. Volkmann's practice, there seems no reason for doubting that the observations were made on wounds treated with all due precautions & following a course similar to that which Mr. Lister himself would expect.

Demarquay
[1]Comptes Rendus 1874

About the same time Demarquay[1] published the results of 8 cases treated "aseptically" in all of which organisms were found. The general course of the wounds so treated, as described by the author, & the fact that one of the 8 cases died of pyæmia, show that the aseptic treatment had not been carried out with sufficient care.

Fischer used chemical reagents
[2]Deutsche Zeitschrift Bd.vi p.320

Two years later there appeared in the "Deutsche Zeitschrift für Chirurgie", a paper by Dr. Fischer[2], giving the result of investigations carried on in Professor Lücke's wards in Strasburg. He employed chemical tests, as recommended by Von Recklinghausen & found organisms in all his cases. He, however, states that bacteria were not infrequently present, his results in this respect differing from those of Dr. Ranke. Now, it so happens, I spent the summer of 1876 in Strasburg, & thus had frequent opportunities of seeing the "aseptic practice" in that hospital & I can only say that I was not surprised to hear that bacteria were present in the wounds there treated.

Schüller used cultivation experiments
.

The last paper on this subject was published by Dr. Schüller

[1]Deutsche Zeitschrift Bd.vii

Objections to Schüller.

in the spring of 1877[1]. In his investigations cultivation experiments were employed. He found that in many cases organisms were absent both from the discharge & from the cultivating fluid, whilst in others their presence was shown by the increasing turbidity of that liquid.

His results, although much more definite than those previously published, are nevertheless somewhat contradictory, & are open to several objections.

Thus, in the first place, the cultivating fluid used was what is known as Bergmann's fluid, a liquid by no means the most favourable for the developement [sic] of organisms. Indeed, many forms refuse to grow in it at all.

Further, the antiseptic employed in the treatment of his cases was salicylic acid, which has not been found to yield such good results as carbolic acid.

Lastly, the method of inoculation employed was faulty. In order to make the inoculation the edge of the dressing was lifted so as to expose the drainage tube, no spray being used. Now as the result of this, in an hospital atmosphere, error might arise in two ways; either organisms might gain access to the wound, develope [sic] there, & be obtained at the next inoculation; or dust might fall into the flask during the momentary exposure. [Addition in pencil] I believe that this method of exp. explains his somewhat contradictory results. Schüller does not make any statements as to the forms of organisms present in the [?].

My own experiments

[In pencil] As long ago as 1876 I began the investigation of this subject. My first observations were microscopical observations on the discharges of the wound. These disch. were …[?] I need not now detail them as they were not sufficiently precise to settle the question but from such &c…. [Cheyne's ellipsis]

Results from microscopical Exam. combined with chemical tests.

*This will be evident on examining some of the specimens sent in.

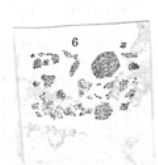


Fig. 6 Discharge from a wound treated aspetically x 920

[Struck through] When I became Mr. Lister's house surgeon in 1876 I resolved to take advantage of my opportunities to investigate this matter carefully. My wish was heartily seconded by Mr. Lister, & since that time he has placed all his cases unreservedly at my disposal for examination.

I began by making microscopical examinations of the discharge from wounds, using at the same time reagents, more especially acetic acid & glycerine, as recommended by Prof. Von Recklinghausen. From such observations, continued daily for several months, I arrived at the conclusion that bacteria are not present in the discharge from wounds treated aseptically. To determine the presence or absence of micrococci was, however, much more difficult. The discharge from a case treated aseptically differs very markedly from that obtained where other treatment has been adopted. Leucocytes are generally absent, or, if present, are very granular, apparently undergoing degeneration. There is also a very large quantity of granular matter, probably derived from degenerated pus cells, or portions of fibrine rubbed off from the surface of organising blood clots.* To affirm or deny the presence of micrococci in such a fluid is a matter of extreme difficulty, & even now, I would not be bold enough without having employed other methods of investigation, to express an opinion in many cases (fig.6)

Accordingly, in January 1877 I commenced a series

Cultivation Experiments

Fluids used

of inoculation experiments, transferring the discharge from wounds into flasks containing fluid highly nutritive to the various forms of organisms. The fluid which I first employed, & which I have since used at times, was milk. This, however, from its tendency to froth when boiled, is troublesome to prepare pure, & it does not possess many of the advantages of clear fluids, I looked about for equally good transparent cultivating liquid. I tried Pasteur's & Cohn's fluids, & after reading Schüller's paper, Bergmann's, but I found them too insensitive to be of any value for my purpose. I then used vegetable infusions, more especially turnip, which I found to answer very well; but I ultimately adopted infusion of cucumber, which seems to be as sensitive as milk, & in which I have never yet found any organism refuse to grow. I also, in many cases, employ, as additional test solutions, milk & infusion of meat.

Preparation of fluid

To prepare the infusion, a cucumber is sliced, placed in water, & infused for about 4 hours, till a specific gravity of about 1003 is attained. This fluid is then filtered, introduced by siphon into flasks previously purified by Mr. Lister's method, boiled for twenty minutes, kept for some days (at least two) in an incubator, & then decanted under a spray of carbolic acid into smaller purified flasks, which are likewise placed in the incubator for several days before being used. These

flasks either have cotton caps, [addition in pencil] purified by heat or carbolic acid, [end of addition] or stand on a glass plate & are covered by a glass cap & a glass shade as described before in the case of Mr. Lister's liqueur glasses.

Method of inoculation

For the purpose of inoculation small capillary tubes such as those used for vaccination, are employed. These possess the advantage over needles in that, while they take up a larger quantity of the discharge, they protect it from the carbolic acid of the spray during the transit from the wound to the flask. The tubes are dropped into the flask containing the cucumber, & this is then placed in an incubator kept at the temperature of the human body.

Procedure described.

The procedure may be shortly described as follows. The outer portion of the dressing having been removed under the carbolic spray, a tube which has been previously purified in carbolic lotion, is heated in the flame of a spirit lamp in the spray, so as to drive off all the carbolic lotion & to render it dry. This tube is now rapidly introduced into the drainage tube, & from thence immediately into the flask, which is opened in the spray close to the wound. The flask is then placed in an incubator kept constantly at a temperature of 98°F. Where flasks with cotton caps are used it is well to wet the margin of the cap with carbolic lotion before lifting it, so as to

prevent any dust from falling from the cap into the fluid.

Test experiments

Before proceeding to the experiments I tested this method & found it perfectly trustworthy. Thus whenever I inoculated flasks in this manner from a fluid containing bacteria, I got bacteria; when, on the other hand, I inoculated from a pure fluid, the fluid in the flask remained pure, this fluid at a later period rapidly becoming the seat of developement [sic] of organisms when these were introduced into it.

Flasks kept in an incubator

In performing these experiments I always inoculated two flasks, & often another was taken & the whole process gone through in the same place, with this difference, that the tube in the latter case, when heated, was put directly into the flask without touching the wound. These latter flasks remained, without exception, clear.

When developement [sic] occurs in the flasks inoculated they generally become muddy in 30 to 50 hours, but, where the fluids remained clear, I have kept them in the incubator for weeks, & then tested them by the addition of fluid containing bacteria.

Results.

As a result, I find that in cases treated aseptically where, of course, there was an unbroken skin to start with, one of two things may happen – either the fluid

Two kinds of results

No organisms
or micrococci

Fig.2 Micrococci from wounds treated aseptically x 920

Explanation of occasional occurrence of irritation, probably the German "Carbol Eczem" or the "Paraffin Eczem"

remains perfectly clear, without the developement [sic] of organisms, showing that none were present in the wound, or the fluid becomes turbid from the presence in it of organisms of the form shown in fig.2. In both cases the wound follows an aseptic course i.e. no local or constitutional disturbance results from the operation, &, from the appearance of the wound, one could not tell in many cases whether these organisms were present or absent. These organisms are minute spherical bodies, arranged in pairs, short chains or groups of smaller or larger size; in fact, they belong to the division of schizomytes termed micrococci.

I have said that in many cases one could not tell from the course of the wound whether these organisms are present or absent but sometimes their presence can be suspected. Those who have worked long at aseptic surgery will have met with cases, where when a dressing is left on for six or seven days, or when a deep dressing is left on for some weeks, the discharge has a sour odour & the skin around the wound is somewhat excoriated. As the wound in other respects follows an aseptic course, Mr. Lister concluded that this was probably a chemical change taking place between the discharge & the [added in pencil] materials in the [end of addition] gauze dressing. Knowing the peculiar property possessed by

Contrast these results with those of a septic wound.
The bacteria are almost always present.

Fig.7 Some of the common forms of organisms found in wounds not treated aseptically x 920

salicylic acid of preventing chemical fermentations. Mr. Lister uses it in such cases with the effect of diminishing or preventing this change.

In these instances I have always found micrococci. If micrococci be grown in drachms of cucumber fluid, after 3 days they seem to die; at any rate, they will not grow in any liquid. But yet if the fluid be kept for some weeks it will gradually become red, till it ultimately is of a dark vermilion tint. Thus chemical changes continue after the activity of the organism has ceased. May not something of the kind occur in these cases? Chemical changes are primarily set agoing by these organisms, but continue of themselves, & thus salicylic acid acts by preventing these changes, as Mr. Lister supposed, though, according to this view, the organisms are necessary for their commencement.

If now we contrast these results with those obtained in wounds not treated strictly aseptically, we find this marked difference – that in none of the latter were organisms absent, while in almost all bacteria as well as micrococci were present. Fig.7 represents some of the most common forms of organisms found. It is to be observed that in many of these cases antiseptics were employed, both externally & injected into the wound, but no precautions were taken, either to

Bacteria can grow under an antiseptic dressing.

*[addition in pencil] It was thus satisfactorily established that there was a very marked difference between the discharges of aseptic wounds & those not treated aspetically. From the former organisms were generally absent till about the end of the case when the dressings were left on for several days while in the latter organisms were present in the first 24 hrs.
Again in the former when organisms did appear they constantly belonged to the class of micrococci while in the latter rod shaped organisms were always present & generally in large quantities especially if there was any putridity.

penetrate all the recesses of the wound with the antiseptic so injected, or to prevent the access of organisms during or after the dressings.

I may mention that in four cases which were originally treated aseptically bacteria were formed, but, in all, their presence was indicated by disagreeable smell or by symptoms of local or constitutional disturbance. One of these patients was a man with psoas abscess, who was in a very weak state on admission. He suffered so much from absorption of carbolic acid that the dressings were made smaller than usual, & were left on much longer than would otherwise have been the case, on some occasions even for 36 hours after the discharge had appeared at the edge. Thus it is not surprising that putrefaction should have occurred. Here the admission of bacteria was followed by foul smell of the discharge, profuse suppuration, hectic fever, & death. It is thus evident that bacteria as well as micrococci can flourish under an antiseptic dressing. The explanation of their absence must therefore be that the circumstances which permit of the entrance of micrococci are not such as to allow the advent of bacteria.

*But, it may be said, there is no specific difference between micrococci & bacteria; the micrococci found

Is there any difference between bacteria & micrococci?

During the past year I have renewed the study of this subject in a different manner. I have adopted Koch's method of obtaining pus & I employed it in all Mr. Lister's cases from the beginning of March till the end of June (4 months) & my results confirm absolutely & in every respect the results I had got by the method of cultivation. I found that in the first few days after an aseptic operation no organisms appear & that ultimately when they do appear they are mic. and not bact. I have to submit a number of these for examination. They represent a small number of the cases examined but they are, I believe, sufficient to show the accuracy of my statements.
Method: These must be examined by lenses[?], stains Bact. & mic. & if staining is too much[,] granular matter
If those taken from wound not treated aseptically be first looked at it will be seen that in all there are both bacteria & mic. Take any wound not treated aseptically which has not united by first intent & which has been somewhat recently made & this result will be obtained. Case I. Thus No.1 is taken from a case of compound dislocation of the thumb which had not been treated asept. & in which the discharges were very foul smelling. The patient by the way died of tetanus. Here bacteria of many different forms – small but long bacilli, micrococci.
Case II &c. No.2 is taken from a wound of the scrotum in which a small slough was lying. The discharge here was also very foul & this is full of minute bacteria & mic. Case III Nos. 3 & 4 were taken on 2 occasions from a case in the outpat[ient's] room not treated asept. in which there was very little discharge & in which there was no putrefactive odour, rather a slightly rancid smell. Here well defined bacteria are seen.

Definition of "Micrococcus".

[1]Coccobacteria septica
[2] Parasitologische Untersuchungen 1878 p.67
[3] Archiv für Experim[entelle] Pathol[ogie] u[nd] Pharm[acie]. Bd. I. 1873
[4] Beiträge zur Biologie d[er] Pflanzen.
[5] Virchows Archiv

in aseptic cases are simply bacteria altered in form by the new conditions in which they are placed. Professor Billroth, indeed, has gone so far as to assert that there is only one species of organism - coccos - in the group of schizomycetes, that this may under varying circumstances assume the form of bacterium or coccos, these two being transmutable into each other[1]. That micrococcos is an organism distinct from bacterium is also denied by Hallier[2] & doubted by Klebs[3], while it is strongly affirmed by Cohn[4], Rindfleisch[5] & others.

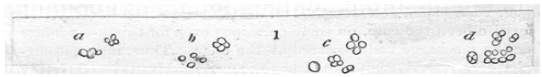
During the long time that I have observed these organisms I have never met with any instance in which a micrococcus has become a bacterium or vice versa; indeed my work has furnished me with decided evidence to the contrary. It will be more convenient, however, if the summing up of this evidence be left till the end of this research. In the meantime, in order that it may be clearly understood what I mean by micrococci, I will describe them, following for the most part Cohn's definition, as follows:-
Colourless or coloured round cells, very small, generally under one mikrometer in size, with or without movement, growing from pairs into short chains or groups of smaller or larger size, not derived from bacteria nor developing into them. Other living spherical bodies may be found

[1] Beiträge zur Biologie d[er] Pflanzen Bd ii 1876
[2] Microscopical Journal Vol XVIII.

Method of grouping & mode of growth.

[3] loc.cit.

[4] Transactions of the Royal Society of Edin[burgh]. Vol 27, 1875



Developement [sic] of micrococci after Mr. Lister.

in cultivating fluids, such as spores of fungi or indeed of some forms of bacteria as pointed out by Koch[1] & Ewart; these however, when fresh nutriment is added, develope [sic] again into fungi on the one hand & into bacteria on the other. The life history of micrococcus seems only to consist in developement [sic] from pairs of short chains or groups of larger or smaller size, this cycle being repeated on the addition of fresh pabulum.

The characteristic mode of grouping is shown in figure 2 (page [Cheyne's omission]) & better in fig.3. The irregularity of the chains & the rapid formation of groups is attributed by Cohn[3] to softness of the intercellular substance allowing the individual bodies to be displaced, their division, according to him, taking place only in one direction (transversely to the long axis of a chain). Another & more probable view has been put forward by Mr. Lister[4]. In performing some experiments on the germ theory of putrefaction his room became infested with a minute organism which he termed granuligera, which in reality belong to the group "micrococcus" as above defined. He studied the developement [sic] of these organisms & a copy of his drawings is here shown (fig.1.) According to his observations, the irregularity of the chains & the formation of groups is due to the fact that the cells divide not only transversely

but longitudinally; indeed in one instance he observed both modes of division going on in the same cell. I have made several attempts to observe the growth of these bodies on the hot stage, but their dancing motion is so great that I have found it impossible to do so. By examining them, however, at various periods of growth, their method of grouping seems to me strongly confirmatory of Mr. Lister's view, & more especially is this apparent when they are grown in fluid containing carbolic acid. [Addition in pencil] I have never, however, either by mic. observ. or by staining growing mic. found any such appearance of longitud. as Mr. L. describes. [end of addition]

Natural History of Micrococci

[Struck through] Micrococci grow luxuriantly in cucumber & turnip infusions. In meat infusion they attain a larger size than in other fluids. In milk they produce but little change as regards smell & taste; the milk, however, separates into two parts, an upper clear fluid & a lower white granular deposit.

Suitable Papulum

Relation to temperature

Micrococci which have grown at a high temperature will not develope [sic] when inoculated into a flask kept at a much lower temperature. Thus, micrococci from a wound, the temperature of which is 98°F will not grow if placed in an incubator kept below 80°F. [End of striking through]

No chemical reagents

Tiegel has pointed out that if a fluid containing organisms be made strongly alkaline with carbonate of soda bacteria quickly disappear, only a few micrococci remaining which also ultimately vanish[1]. This indicates a chemical difference between the two forms.

[1] Virchows Archiv. Bd 60

[2] Untersuchungen über die Otiologie der Wundinfectionskrankheiten 1878

Relation to gastric juice

How is it that these organisms produce no bad effects?

Changes produced in fluids by them.

Koch finds by the use of his new method of investigation that, while micrococci stain with hæmatoxilin, bacteria do not.

Micrococci when acted on by a strongly peptic solution at the temperature of the human body remain unaffected, in contrast to ordinary albuminous granular matter which soon disappears. Many forms of bacteria similarly treated become aggregated into clumps or may, for the most part, disappear, only a few irregular rods & granules remaining.

We must now approach the consideration of the questions which necessarily arise in connection with the presence of these organisms under aseptic dressings, & the first which presents itself is – How is it, if organisms are the causes of many of the evils consequent on wounds, that the cases in which these micrococci are present do not apparently differ from those from which all organisms are absent?

In answer to this question it is to be observed that we have here to deal with a well marked & distinct group of organisms. When grown in cultivating liquids, be it milk, meat or cucumber, but little alteration is produced. There is slight increase of acidity, very faint, sour odour, but only slight alteration in taste. And so when they grow in wounds they produce no smell in the discharge nor symptoms of local irritation.

Contrast with bacteria

State of literature on this subject.

[1] Deutsche Archiv für klinische Medizin Bd. viii

showing that the products of their growth are not acrid, for it will be admitted that any chemical irritant present in a wound would give rise to more or less inflammatory symptoms. Nor are their products, absorbed into the circulation, poisonous. In these points they present a marked contrast to the ordinary forms of bacteria. These, when cultivated in a suitable fluid, produce more or less alteration in it; the fluid becomes acrid, disagreeable to the taste, & it may be foul smelling; & thus, when they gain admission to a wound, their presence is announced by the developement [sic] of disagreeable odour or by symptoms of irritation about the wound, or of constitutional disturbance showing that chemical substances of a more or less acrid or poisonous nature are present in the discharges.

But when we come to look into the literature on this subject, we find that, by many, micrococci are regarded as the causes of important, more especially septic diseases. Thus Oertel[1], in his paper on diphtheria designates as micrococci the organisms to which he ascribes the production of this disease. He, however, adopts Hallier's views & accordingly, in the figures which he gives, well marked bacteria of various forms are grouped together with micrococci proper under the one comprehensive term of "micrococcos" & I can see

[1]Medizinische Centralblatt No.12. 1868.

[2]Allgemeine Chirurgie 1873
[3]Virchows Archiv Bd.60 p.418

[4]Ibid. Bd 50
[5]Zur Kenntnis der Bakterische [sic] Mycosen 1872

[6]Virchows Archiv Bd.58 p.437

[7]Archiv der Heilkunde XVI - 1873

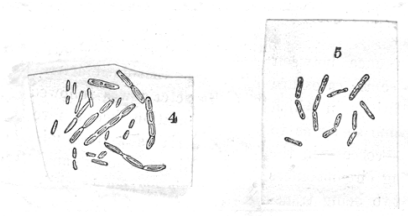
[8]Virchows Archiv Bd.56 – p.407
[9]Ibid Bd 57 p.228

[10]Schmidts Jahrbuch vol.166 p.190

nothing in his paper to lead one to suppose that the round bodies rather than the rods are the cause of the disease. In the diphtheria of wounds Hueter[1] had previously discovered organisms in the tissues & in the blood, & he likewise calls them micrococci (monads). Again by Hueter[2], Recklinghausen[3], Lukomsky[3] & others the margin of the erysipelatus blush is said to contain micrococci which are regarded by them as the cause of the disease. Similar views are entertained by Recklinghausen[4], Nassiloff[4], Eberth[5] &c as to the so-called diphtheritic inflammation of the cornea, produced by the inoculation of putrid fluids on it.

Orth[?][6] has also found micrococci in the peritoneal fluid in puerperal peritonitis, &, in connection with the subject of septic diseases Birch Hirschfeld[7] thinks that, though bacteria may be important factors in these affections, yet the essential role is played by micrococci. Further, a form of endocarditis has been described by several observers as Heiberg[8] Eberth[9] &c in which micrococci are said to be present in the ulcers on the valves, & this disease has therefore received the name of micrococcal endocarditis. And, lastly, may be mentioned the views of Birch Hirschfeld[10] who finds, as a result of experience, that where micrococci are present in wounds, these generally assume an unhealthy appearance. In none of these cases, however, is the evidence sufficient

My own experiments of animals



Figs. 4 & 5 forms of bacteria which, injected into the circulation of rabbits, caused death. x 920

to show that micrococci rather than bacteria are the cause of these diseases, nor to show that the micrococci might have been accidental, as indeed, as I shall presently point out, is not unlikely to have been the case.

Now in order to test this matter with the micrococci at my disposal I performed some experiments on rabbits. In the first experiment 7 cubic centimeters of cucumber infusion in which micrococci were present & calculated to be at least as numerous as 2 millions per minim, were injected into the jugular vein of a rabbit, while, at the same time, into another rabbit 6 c.c.of cucumber fluid of the form shown in fig.4 were introduced. The first rabbit was quite unaffected by the injection, it took its food as usual, its temperature never varied, & it did not lose flesh. The second rabbit was very ill on the following day, it would not take its food, & its temperature was only 100° (the temperature before the injection was 102°.) On the second day its temperature was 98°. It had diarrhæa, & was rapidly losing flesh. On the evening of the 4th day it died, its temperature during the forenoon having been 92°. Similar experiments have been performed 6 times with micrococci from aseptic wounds & for comparison, with different forms of bacteria. In each case 2½ c.c. of cucumber infusion containing respectively micrococci & bacteria of the same age were used. In none of the animals into which micrococci were injected were any effects pro-

duced, while all of those which were treated with bacteria died.

Source of the micrococci used in these exp.

But not only are micrococci obtained from aseptic wounds harmless, micrococci got from the air, from tap water, from unopened abscesses, indeed from rabbits themselves by methods to be later explained, have proved in like manner innocuous. Nor do they, when inculated on the cornea, or injected into the subcutaneous cellular tissue of a healthy animal, produce any of the inflammatory processes which so frequently follow the use of many forms of bacteria.

Answer to the question asked.

We must, therefore, conclude that the ordinary forms of micrococci are harmless, & that the reason why no symptoms are produced in the cases in which they occur is, that the products of their growth are not chemically irritating nor poisonous. But I must guard against making too absolute a statement. There are no doubt many forms of micrococci. This is evident from the fact that some forms have the special property of producing pigment, & of these there are different kinds producing different pigments. Koch[1] also has in his recent beautiful work brought forward strong evidence to show that pyæmia in the rabbit is due to the developement [sic] of micrococci in the blood. Certain it is that, if forms of micrococci are hurtful, these are rare in the surrounding world, & that the conditions which allow of the admission of the ordinary forms to wounds treated aseptically are

[1]loc.cit. p.54

Why do micrococci occur in wounds?

Explanations

Do organisms occur in the blood occur in the blood or tissues of the healthy living body?

Literature

[1]British Medical Journal Jan[y]. 26[th] 1878
[2]Virchows Archiv Bd. 69.
[3]Coccobacteria Septica

not such as permit the entrance of the hurtful kinds. But why do micrococci & not bacteria gain access to wounds treated aseptically? How is it that organisms of any kind are found in the discharge from aseptic cases?

These two questions must be considered together, & in answer to the latter three possible hypotheses may be put forward, & must be individually dealt with

1. The organisms gain access to the wound through the body, being carried to it in the circulating blood.
2. They enter the wound through some loophole in the aseptic method, or through faulty application of it.
3. They arise in the wound through a process of spontaneous generation.

In considering the first view we are at once met with the question – Do organisms occur normally in the blood or tissues of the healthy living body?

I have already touched on this subject twice & have described the method of investigation employed but at the risk of some repetition I must for the sake of completeness refer to it again. Dr. Burdon Sanderson in his lectures at the University of London in Dec[r] 1877[1] referred to experiments lately performed by Tiegel[2] & Billroth[3]. These observers found that, on rapidly removing portions of various organs from a newly killed animal, & dropping them into melted

paraffin, bacteria developed in these organs in a few days. They therefore concluded that organisms capable of developement [sic] are normally present in many of the tissues of healthy living beings. Dr. Sanderson further stated that he had repeated these experiments more carefully & had obtained similar results.

My method.

In investigating this subject I have made use of a totally different method. A number of beakers are prepared, each containing a quantity of pure cucumber infusion. The hair of the abdomen of the animal, rabbit or cat, having been well soaked with 1-29 carbolic lotion, the creature is killed by a blow on the back of the neck & its abdominal cavity quickly opened under a distant spray of carbolic acid. Portions of various organs – liver, spleen, kidney, pancreas, muscle, heart with its contained blood, are rapidly cut out with pure instruments, & transferred to the beakers containing the cucumber. The latter are then placed in an incubator.

Rationale of the method.

The rationale of the method is as follows: The organs are removed without any possibility of external contamination. They are put into flasks carefully purified, as is further certified by the fact that the cucumber infusion which they contain has remained pure, although they have been kept for some time in an incubator. This cucumber fluid keeps the tissue moist

& prevents the drying which would otherwise take place. Further, it is itself an extremely favorable medium for the growth of organisms, & therefore these, if present, would in all probability develope [sic] in it. This fluid in which the organs lie, is not however merely pure cucumber infusion, it is also an infusion of meat, thus consisting of a combination in the highest degree putrescible. The extremely minute quantity of carbolic acid which gains access to the organs is not sufficient to have any effect in retarding developement [sic]. This will be quite evident presently.

To test the beakers.

In order to test these beakers, & thus be absolutely certain as to the presence or absence of organisms, I inoculate from them at various periods flasks containing cucumber infusion. If organisms are present in the beakers they will develope [sic] in these flasks; if they are not, the fluid will remain unaltered.

Results.

As the result of this method of investigation now carried out in many cases, I find that organisms do not occur in the blood or tissues of the healthy living animal.

Objections to the method.

Answers to these.

But, it may be said, the method is not correct; organisms may be present & yet not be able to develope [sic]. The following facts will however sufficiently answer this objection. If in removing the liver I wound the gall bladder bacteria develope [sic] in the beakers. If the

Conclusion

*[Added in pencil} A remarkable series of exp[eriments]. were perf[ormed] in 1870[?] which do not seem to have attracted the attention in regard to this point which they deserved.

Confirmed by other observers.

[1]Loc. Cit.

2Quarterly Journal of Mic. Science Ap.1878

[3]Ibid vol II. 1873

manipulations be imperfectly carried out organisms appear. Thus, in one of the first experiments, performed in Dec[r] 1877 I allowed one of the organs to touch the outside of the beaker. In this vessel bacteria developed. If micrococci in sufficient quantity be injected into a rabbit, & the animal killed, say in half an hour, or even in 24 hours, micrococci & these alone, will develope [sic] in the beakers & in the flasks. In the same manner if bacteria in sufficient quantity be injected into the animal they will be obtained from the organs, and apparently of the same kind as those introduced. Here no micrococci are found. Thus where micrococci are introduced they are obtained; where bacteria are injected they in like manner develope [sic]. Where the organs of an animal into which neither micrococci nor bacteria have been artificially introduced, & which is in a healthy condition, are tested in the manner described, no organisms are found. Therefore organisms are not present in the blood or tissues of the healthy living animal. Further evidence in favour of the utility of this method will be immediately forthcoming.

*This assertion does not rest on my own unsupported testimony. That the fluids of the body do not contain organisms has been shown by various observers. Their absence from the blood has been proved by the experiments of Klebs[1], Lister[2], Burdon Sanderson[3], &c.

[1]Transactions of the Royal Society of Edin. 1875

[in pencil - a number of illegible names of researchers]

[2]Journal of Anatomy & Rhynology Ap. 1878.

Objection to Tiegel's Method.

[3]Klebs, loc.cit.

That the urine is sterile is evident from the researches of Mr. Lister[1], Mr. Pasteur [added in pencil:] Cazeneuve & Livon. The same thing has been demonstrated, as regards milk by Roberts & Lister. The existence of organisms in the healthy living animal is also denied by Koch in his recent work: & finally by removing the organs of rabbits aseptically, & wrapping them in gauze or placing them in calcined beakers, Chiene & Ewart[2] have come to a similar conclusion. And if we examine Dr. Tiegel's method more carefully we find many objections to it. The most important of these has already been answered by Tiegel. It is this – when the knife cuts soft tissues such as the liver the vessels are emptied of their blood by its pressure &, after it has passed through, they must become filled with air[3]. Tiegel acknowledges the force of this objection, & says that the cases to which it would not apply are the kidneys & salivary glands. (In removing the kidney, for example, Dr. Tiegel places a ligature pedicle close to the hilus, & the vessels are then divided beyond this constriction. The substance of the organ is thus not cut into.) But it is just in these organs that he gets negative results: "was indessen durch ganz andere Umstände bedingt sein kann." What the other circumstances are he does not state. Indeed, the only case in which he found organisms in all the organs of the same animal was in a dog, where, 24 hours before death, the abdomen had been opened, and the

[in pencil] Dr. Ogston foot of p.7

Organisms in diseased animals

in acute inflammations

[1]Medico Chirurgical Transactions. London 1873

In Bad Nutrition

not due to destruction of intestinal epithelium

Healthy blood & tissues destroy organisms

intestine ligatured in two places. But surely Dr. Tiegel does not mean to say that this was a healthy animal? Here indeed I think it not improbable that the organisms thus formed were present in the organs before death.

When, however, we come to examine animals suffering from disease we find that in certain cases organisms capable of development [sic] may be present in the blood & tissues. The determination of the conditions in which this occurs is a matter of the greatest importance, & therefore I do not intend to do more just now than to mention one or two points which are necessary to complete this subject.

I have found that, if an acute inflammatory process be induced in an animal, say by the injection of ammonia, as pointed out some years ago by Dr. Burdon Sanderson[1], the organs examined by the method I have described may be found to contain organisms; [struck through] I say "may" for organisms are not always present. [end of striking through]

Or again I find that if the nutrition of an animal be profoundly interfered with, as in slow poisoning by phosphorus, organisms will after some time be found in the blood & tissues. Here, it may be said, inflammation of the intestinal tract has been caused & the epithelial barrier against the entrance of organisms has been removed. But the blood & tissues, when in a healthy state, have the power of themselves of

destroying organisms when these are introduced into the body. Thus into the veins of 4 medium sized rabbits I introduced ¼. ½, ¾, & 1c.cm. respectively of ordinary bacterial fluid. I kept the animals alive for 24 hours, & then killed them & preserved their organs. In the first 3 no organisms were found, while in the last they may or may not be present. Where larger quantities of the fluid have been introduced they will certainly be found. Thus, even though the organisms could gain access to the blood, yet, so long as the nutrition of the animal is fairly well carried out, they would be destroyed. The reason that they are found where large quantities of bacterial fluid are injected seems to be that along with the bacteria their products are introduced, that these act in the same way as phosphorus, as poisons, & that thus the resisting power of the animal is diminished. [Struck through] In like manner, if sepsin be so prepared & introduced as to avoid the introduction of organisms along with it, organisms will nevertheless be found in the internal organs. [end of striking through]

Conclusion

In other words, in severe inflammatory processes, or in great interference with the nutrition of an animal, organisms may pass into the blood without losing their vitality. The organisms usually found are micrococci.

Are the facts the same in man?

In investigating this point in man, I have confined myself to the examination of abscesses

Examination of chronic abscesses

[1]loc.cit.

…. of acute abscesses

Billroth's results differ somewhat.

Evidence of other abscesses. In acute osteomyelitis

when opened. [Addition in pencil] At first I used only cultivation exp[eriments]. & this par[agraph] gives the results of these [end of addition]. In chronic abscesses I have not as yet found any organisms, & in this, my results agree with those obtained by Billroth[1], by microscopical examination alone. Of acute abscesses I have inoculated from 32 cases. In 25 of these no organisms were present, while in 7 micrococci were found. In none were bacteria present. I omit here one abscess in the ischiorectal fossa, where I found both bacteria & micrococci, & one with fæcal odour in the lumbar region, from which I did not inoculate when opened, but in which undoubted bacteria were seen on microscopical examination. Billroth has paid special attention to this subject, & he finds micrococci in a larger proportion of acute abscesses than I have done. It must be mentioned, however, that the abscesses which I have examined were not as a rule very acute. Thus, some were buboes[?] &, indeed, of 3 of the cases where micrococci were found two were buboes[?] after soft chancre, & one was a suppuration in the groin, following a sore on the foot. Prof. Billroth's cases seem to have been more severe, & thus the difference in results may be explained. He likewise only mentions the occurrence of micrococci. In acute osteomyelitis, where a communication had not yet been established with the external air, organisms have been found in the medullary

[1]Deutsche Zeitschrift Bd.iv p.239
[2] Ueber Schusswunden [*On Gunshot Wounds*] 1871
[3]Virchows Archiv Bd.65 p.341
[4]Lancet Novr 21st 1874

[5]It is probable that the presence of micrococci in ulcerative endocarditis may be similarly accounted for.

These micrococci are not infective

[In pencil] Here discuss my own results with staining; my own results with inoc.

Application of these facts to the previous subject

canal or in the pus surrounding the bones by Von Recklinghausen[1], Klebs[2], Eberth[3] & Godlee[?][4]. These organisms were as a rule micrococci. Bacteria have but very rarely been present, & that only in the most severe cases. [Struck through in pencil] That these micrococci are simply accidental & not essential to the inflammatory process[5] is evident, because they do not occur in all cases, & because, when introduced into animals subcutaneously, or into the circulation, or inoculated on the cornea, they produce no effect. (Billroth inoculated the cornea of a rabbit without effect, with the pus from a newly opened acute abscess of the thigh containing micrococci, while the discharge from a healing wound not treated aseptically produced the so-called diphtheric inflammation.) Further, I have injected into my own arm, on 2 occasions, one & 5 minims respectively of cucumber fluid containing micrococci at least as numerous as two millions in each minim, without the production of abscess. [end of striking through]

If now, we apply these facts to the subject under consideration I think it must be concluded that it is only rarely that the micrococci can have reached the wound through the body. To do so implies such an inflammatory disturbance or depression of the nutritive processes as is not usually present after operations conducted aseptically. A person on whom an operation has been performed aseptically is, after the effects of

the chloroform have passed off, provided there has been no great loss of blood, practically as well as before the operation. There is no inflammation & no febrile disturbance. The patient, as far as one can judge, ought to be as able to resist the entrance of organisms into his blood as before the operation.

Micrococci not found during the first few days

If, however, these micrococci did enter the wound from within they would do so during the first few days after the operation; but, on looking at my notes, I find that for a week at least the wound has generally remained free from them.) [Struck through with pencil]

Illustrative case

The following case is very illustrative:- a patient was admitted on March 8th 1878, with 7 chronic abscesses, one in the neighbourhood of each knee joint, one close to the left elbow joint, & 4 about the right wrist joint. They were all opened on March 9th & they followed an aseptic course. On March 11th inoculations were made from all without result. On March 22nd micrococci were found in the abscess in the left leg, but none had as yet been obtained from the others. On March 24th they were present in the elbow. On Ap. 28th micrococci were found in the right leg, the wrist still remaining free. Here, if the organisms came from the blood, why did not all the wounds contain them at the same time? And why were none present in the abscesses on March 11th? Another case bearing on this point may be mentioned. A boy was admitted in the beginning of April 1878 with caries of the thumb. There was an

Another case.

Are the antiseptic arrangements at fault

Carbolic acid is sufficient to destroy them.

open wound in which were bacteria. An abscess formed in the axilla of the same side & was opened on Ap. 11th. No organisms were present in the abscess when opened, but on Ap. 13th micrococci were found. On Ap. 29th a second acute abscess was opened in the upper arm but contained no organisms. If the micrococci came from the blood, why were they not present in the second abscess? If they passed along the lymphatic vessels from the wound to the axilla why were none found in the first abscess when opened? Other similar cases might be narrated. [end of striking through]

Accordingly, with regard to the first suggestion, we must say that, while in some cases the micrococci may have entered the wound from the body, yet such does not seem to be the ordinary course of events.

In looking for a loophole in the aseptic [Cheyne has crossed out *antiseptic*] dressings, we may divide them into 8 parts – a lotion in which the various substances [the word *substances* replacing the word *objects*] are soaked before being brought into contact with the wound, - a spray to purify the surrounding atmosphere, - and a dressing so constituted as to give off carbolic acid to the discharge as it passes under it.

That the lotion is sufficiently potent to destroy any organisms which come in contact with it will be very evident to any one who chooses to examine the subject. If one places a preparation of actively moving bacteria under the microscope, & allows a little carbolic lotion 1-20, to flow under the cover glass, the movements will be seen

Spray also sufficient
(Stimson's paper).
An attempt has lately been made by Dr. Lewis Stimson, Nov^{r} 25th 1879, to show that the spray does not act as a germicide. His conclusions are thus directly opposed to mine. He exposed flasks containing pure boiled urine in a cloud of carbolic spray. Three tubes were heated & allowed to cool in the spray & then exposed under it for 1½ hours. They were then closed with plugs of cotton wool wet with alcohol. Particles of dust were seen to be caught on the edge of the tube & therefore a purified glass rod was used to push them into the fluid. One tube in which this was done developed bacteria. In another case the tube was tilted so as to bring the fluid in contact with the neck & here also developement [sic] occurred. The 3^{d} was undisturbed & remained pure. In another set of experiments wide mouthed beakers were used & were exposed for 3 quarters of an hour. In all these organisms developed. Test experiments showed that these organisms had entered during the exposure.

These experiments do not however seem to be satisfactory. We are told in the first place that the spray was placed a foot above & 3½ feet distant from the tubes & that 15oz. of carbolic lotion 1-20 were used. During half the time a board was placed so as to throw the spray back over the flasks. Now if the flasks were only [continued opposite p.180]

instantly to cease. This, in fact, is the method which I use when I wish to draw moving bacteria with the aid of the camera lucida. And, further, it is by soaking the cotton in carbolic lotion that I purify the cotton caps covering the flasks.

[Stuck through] But if carbolic lotion can destroy organisms so must the spray. For the spray is simply carbolic lotion, the particles of which are in a state of extremely minute division. Any speck of septic dust falling into the spray must come in contact with the carbolic lotion, & be instantly deprived of vitality. [Addition in pencil] That the spray is sufficient has been shown on pp. … [Cheyne's ellipsis] See also empyema case, quote Stimson]

Further, the performance of the experiments under consideration would have been quite impossible without the aid of the spray. The room in which the flasks were prepared & many of the experiments performed is a very small one, & in it I keep pathological preparations & putrefying fluids & tissues of all kinds. The most momentary exposure of a flask without its cap in this atmosphere is certain to be followed by the developement [sic] of bacteria, while any attempt at transferring organisms from one flask to another without the access of fresh forms of bacteria is quite impossible. But flasks may be exposed without their caps for any length of time in the spray without the subsequent developement [sic] of organisms, though the latter will grow readily if added intentionally. And organisms may be transferred from one flask to another in the spray with the certainty that only the variety

[continued from opposite p.178] …under a spray, a foot distant from it, during half the period of exposure, the result cannot be wondered at for eddies would be produced by the spray which would drive unpurified dust into the vessels. This must be so for if the spray passed directly over the mouths of the flasks, - if these were enveloped by it, - it is difficult to conceive that the spray could have played so long at such a close distance & that so much lotion could be used without the entrance into the urine of a quantity of carbolic acid more than sufficient to render it sterile.

But granting that the method is correct in these respects, Stimson sweeps the floor[,] raises large masses of dust which he can see & expects that as they fall through the spray they will be soaked through & through & any organisms in their interior be destroyed!!! A more extraordinary assertion I have seldom heard. It was never asserted that a mass of filth falling in a moment through a spray can be soaked completely by the acid! We do not sweep the floors while performing a surgical operation.

Additional proof of the value of the spray may be obtained clinically by considering the case of empyema, in which air is constantly sucked in at every dressing & yet in which if treated rigidly aseptically, putrefaction does not occur.

Dressings probably at fault. Cases which support this view

of organisms thus introduced will develope [sic]. Indeed, I believe that the aid of the spray is invaluable in such investigations, & that its use will clear up many of the contradictory results obtained by different observers. Other facts in relation to the usefulness of the spray are mentioned in Vol.I.

Our suspicion, if a loophole do exist in the method, must therefore fall on the dressing & an examination of the cases will confirm this. For, as I have said, it is at a late period in the treatment of a case that these organisms appear, generally where the dressings have been left on for several days, & where the discharge has reached the edge some hours before changing, or where the discharge has been considerable, even though the dressings have been changed more frequently. And thus I have been able in some cases to prevent the entrance of these organisms by changing the dressings daily, & conversely, they are easily enough obtained in any given case simply by dressing it less frequently. [Addition in pencil] This may be easily seen from the cases recounted. [end of addition] To mention one instance. A psoas abscess was opened on Jan^{y} 16th 1878. I determined to see if in this case these organisms could be excluded. The dressings were accordingly changed daily till Feb^{y} 16th. Fluids of different kinds inoculated during this period remained barren. During March it was dressed every 3 days. Still no organisms. On Ap. 24th when it was dressed for the second time after an interval of 4 days, the discharge

having on both occasions appeared at the edge of the dressing some hours before it was changed, micrococci developed in the fluid inoculated. There had been no alteration in the condition of the patient. [end of striking through]

Relation of carbolic acid to albuminous fluids.

Milk.

If such a fluid as milk be tested with the view of determining the amount of carbolic acid which must be added in order to prevent the developement [sic] of organisms, it will be found that a large quantity is necessary. Thus they readily develope [sic] in a proportion of carbolic acid & milk 1-60 & I have grown them in as large a proportion as 1-54. On examining the milk while the carbolic acid is being added a granular precipitate will be seen to take place.

White of egg

In the same way, if carbolic acid is added to serum or white of egg a white precipitate occurs, & here likewise a large proportion of carbolic acid is necessary to prevent developement [sic]. In artificial cultivating liquids such as Pasteur's fluid, no precipitate takes place, & a much smaller quantity of carbolic acid is required to hinder the growth of organisms. In other words, where carbolic acid is added to an albuminous fluid a compound is formed, which is but little antiseptic. So in the case of the antiseptic [changed from *aseptic*] dressings; where there is profuse discharge, though the first which comes through may not be putrescible, yet very soon the carbolic acid is not present in sufficient quantity to prevent the developement [sic] of organisms. And, as carbolic acid is very volatile, when a dressing

Contrast with effects in other liquids

Application of facts to the dressings

is left on for several days a considerable amount of carbolic acid will have escaped by evaporation, & thus the meaning of the following rules of treatment derived from experience becomes apparent:- “The dressing must not be too small.” “The dressing must be left on longer than 24 hours after the discharge has appeared at the edge.” “In no case is it safe to leave a dressing unchanged for more than 8 days.” Experience has shown that any marked disregard of these rules will in all probability be followed by putrefaction in the wound.

Why do micrococci alone occur

But this still leaves unexplained why it is that micrococci only are found in wounds treated aseptically. At first the only hypothesis which I could think of was that micrococci can grow in fluids containing carbolic acid in larger quantity than those in which bacteria can develope [sic]. I have accordingly performed numerous & elaborate experiments to test this view, but I have been quite unable to find any such difference in the first instance. An observation which I made in these experiments seems, however, to furnish the clue to this mystery. I had previously observed that where one flask was inoculated with bacteria, & another with micrococci in like amount, that which contained the bacteria was, as a rule, muddy in from 12 to 20 hours (the quantity of fluid in each flask being ziij to ziv while 30 to 50 hours elapsed before the fluid in the flask into which the micrococci had been

Probable explanation.

Micrococci grow more rapidly in a fluid containing carbolic acid than bacteria do.

Proofs of this

Additional proofs

[Struck out in pencil] In performing such experiments one must remember that to mix bacterial & micrococcal fluids together will likely result in the death of the micrococci, & therefore the bacteria & the micrococci must be separately introduced into the cultivating fluid. It is best to use fluids in which micrococci & bacteria are growing together, &, further, they ought not to be more than 24 hours old, for micrococci become incapable of further developement [sic] in 2 to 3 days & as the bacteria develope [sic] their products may exercise a noxious [continued opposite p.188]

introduced became opaque. But if to this fluid carbolic acid, say in the proportion of 1 to 500 be previously added the result is just the reverse; the flask containing the micrococci becomes opaque in 24 hours while that containing the bacteria does not become muddy until a later period. Following out this line of investigation, I have found that if micrococci & bacteria be introduced together into a cultivating liquid containing carbolic acid, the micrococci will develope [sic] rapidly, often to the complete exclusion of the bacteria. Where no carbolic acid is present exactly the reverse is the case; most forms of bacteria grow quickly, the micrococci being apparently prevented from developing. But, it may be said, in the former case the bacteria became transformed into micrococci. But if the same bacteria be introduced into a flask containing no micrococci bacteria alone develope [sic].

So in the room in which I work I have never been able, without the aid of the spray to transfer micrococci from one flask to another. For in the latter flask bacteria almost invariably developed. But if carbolic acid be previously present in the fluid the operation may be done in the most leisurely manner with a certainty of containing micrococci alone in the second flask. If a drop of tap water be added to an infusion bacteria develope [sic]. To a flask of meat infusion containing carbolic acid I added a drop of tap water & micrococci were the result.

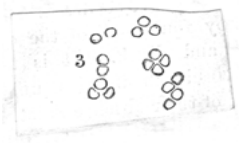
[continued from opposite p.186] … effect on the micrococci. The bacteria may be conveniently separated from their products by Hiller's method.

Summary

Micrococci also increase in vigour in a carbolised fluid

It is thus apparent that though bacteria & micrococci can grow in fluids containing like amounts of carbolic acid, yet the micrococci find these liquids more suitable for their growth than do the bacteria; indeed they may grow more rapidly in them than in fluids containing no carbolic acid at all. And therefore when bacteria & micrococci fall together into discharge containing carbolic acid the latter develope [sic] with much greater rapidity than the former, & may thus reach the wound long before them. If, however, sufficient time be allowed to elapse before the changing of the dressing bacteria also may enter the wound.

One other observation completes this subject. The largest proportion of carbolic acid in cucumber infusion in which organisms develope [sic] is from 1-450 to 1-500. In one of the last experiments performed with the view of seeing whether micrococci could grow in a larger proportion of carbolic acid than that sufficient to prevent the developement [sic] of bacteria, I used micrococci which were growing in a fluid in which a small quantity of carbolic acid was already present. This was done April 14th. On examining the flasks on April 15th I found one containing carbolic acid in the proportion of 1-400 quite opaque from the developement [sic] in it of micrococci, while those in which a larger proportion of carbolic acid was present remained clear. On the same day I inoculated from flask 1-400 a new series containing carbolic acid in the following proportions:- C 1-400, C 1-350, C 1-300, C 1-250. On Ap. 16th



Conclusions

Micrococci growing in cucumber fluid containing carbolic acid.
x 920

Hypothesis of Spontaneous Generation not tenable

*Here insert Ogston's results

[addition in pencil] that this is so will be seen from an exam[ination] of cases …. [Cheyne's ellipsis] in which organisms, & in one of the cases indeed bacteria as well as micrococci penetrated under the dressing for a certain distance but the dressing was changed before they reached the wound. Had the dressing been left on longer micrococci first & then bacteria would have spread to the wound. Ogston.

C 1-350 & C 1-400 were quite muddy from the presence of micrococci, while C 1-300 & C 1-250 were clear. That afternoon a fresh series C^1 1-350, C^1 1-300, C^1 1-250, C^1 1-200 was inoculated from C 1-350. On Ap. 17th C^1 1-300 & C^1 1-350 were muddy. Here the limit seems to have been reached. For though I have obtained slight developement [sic] in carbolic acid & cucumber, 1-275 & 1-250, this is not vigorous. As the micrococci grow in larger proportions of carbolic acid they become much larger, & the grouping & mode of growth described by Mr. Lister is more evident – (See fig. 3.[])]

The facts, then, seem to be that the discharge when profuse or when it arrives at the edge of a dressing which has been left on for some days, does not contain sufficient carbolic acid to prevent the developement [sic] of organisms in it; that micrococci, which I find to be more abundant in the ward atmosphere than bacteria, find this a particularly favourable medium for growth; & that as they grow they increase in vigour, & become more able to live in fluids containing a larger proportion of carbolic acid, & thus, if time be given them, they will ultimately reach the wound.

*The hypothesis of spontaneous generation however convenient when no other explanation is forthcoming, has been so disproved of late & has been already so fully discussed that I shall just allude to one or two points. Were spontaneous generation at all frequent, such a method of investigation as I have

General summary of the results obtained with regard to organisms in aseptic wounds.

employed would be impossible, for in various conditions of the atmosphere, temperature &c. organisms would spontaneously develope[sic] in the cultivating fluid; & yet I have kept the same flasks of cucumber or milk for months until they have dried up, without in any instance organisms developing unless they had previously been introduced from without. But if these micrococci arise spontaneously in a wound why do not bacteria likewise appear? (The supporters of the spontaneous generation theory speak for the most part of the latter as originating in this manner.) And, further, if their origin be due to physical conditions external to the patient, why do not these micrococci appear in all the wounds in the same ward at the same time? Or, if their advent be owing to the same change in the nutritive or other conditions of the patient, why do they not arise in all the wounds in the same patient at the same time? (Compare the case of the man with 7 chronic abscesses).

To sum up we find: that where the aseptic treatment is properly carried out, organisms are either completely absent from the wounds or, if present, they belong to the class of micrococci while in wounds not treated aseptically organisms are always present & generally consist of various forms of bacteria: that bacteria can live in wounds under aseptic dressings when they gain admission: that while micrococci give but little indication of their presence, the entrance of bacteria is generally accompanied by the developement [sic] of smell or by symptoms of local or constitutional disturbance: that micrococci

form a group of organisms quite distinct from bacteria, as shown by their mode of growth, their relation to reagents & to carbolic acid, their effects on fluids & on the living body, & further by the fact that through they have been under observation for 2 years under the most varied conditions & in different kinds of fluids, no instance has occurred of a transformation of the former into the latter or vice versa: that the ordinary forms of micrococci wheresoever derived are harmless, whether they be introduced into the veins, under the skin or inoculated on the cornea, thus contrasting markedly with the ordinary forms of bacteria which are more or less hurtful: that organisms do not occur in the blood or tissues of a healthy living animal, though they may be present in states of disease as in acute inflammatory processes: that they are not essential for the inflammatory process however much it may be confirmed by their presence: that the explanation of their presence in wounds is that the discharge flowing from underneath an aseptic dressing is not too strongly antiseptic to prevent the developement [sic] of organisms in it: that the organisms which find it the most suitable patulum are micrococci, & these as they continue to grow in it become stronger & able to grow in fluids containing more carbolic acid: that this seems to be their ordinary mode of entrance, though they may possibly in some cases come from the blood: that the admission of bacteria into a wound is the result of carelessness, while

We have learned from Dr. Bastian & other experiments that the introduction of cheese into vessels is almost essential for the occurrence of spontaneous generation. We need not therefore fear the spontaneous origin of organisms in wounds nor the spontaneous occurrence of ferment, so long as we do not introduce cheese into them. Cheese à la Bastian therefore is a bad dressing.

Conclusion

*So long as we do not introduce cheese à la Bastian

Consideration of the various principles which may be employed in interfering with the occurrence of putrefaction

Exclusion of organisms

their entrance from the body can only happen in the most grave disturbances of the vital functions, in such conditions that only operations of the utmost necessity will be performed.

This investigation though at first sight tending to upset the germ theory of putrefaction in reality affords it strong support. For numerous facts have been brought forward by previous observers which tend to associate putrefactive changes with certain forms of rod shaped organisms – bacteria [-] & in this research we find the same association – absence of bacteria, absence of putrefaction – presence of bacteria, presence of putrefaction.

Such are the chief facts known as to the causes of putrefaction; & taking everything into consideration, I for one have no hesitation in accepting the germ theory of putrefaction & in looking on the true principle of antiseptic surgery – surgery directed against the causes of putrefaction – as a battle against the entrance into & growth of organisms in the discharges of wounds*.

How surgical means may best be directed against the entrance of organisms into wounds has been already described, we have now to consider the various methods by which the growth of organisms on wounds & the production of their poisonous products may be best hindered.

Add antiseptics to fluid

Drain off the fluid rapidly

Wash it away

Allow it to become too concentrated & supply plenty of oxygen

*Guérin's treatment seems to act on the first part of this principle

Make use of the fact that healthy living tissues can destroy organisms

These antiseptic methods, which merely interfere with the developement [sic] of organisms in the fluids & which do not aim at their total exclusion, may act on various principles.

1. By the addition of various antiseptics to the discharge either in the wound or after it flows out this discharge may be rendered an unfit soil for the developement [sic] of organisms. – use of antiseptics
2. The discharge may be allowed to flow away so rapidly as not to have time to undergo fermentation to any extent in the wound itself – free drainage
3. This removal of the discharge may be facilitated by washing it away constantly with water alone or with water containing antiseptics – treatment by irrigation or by water bath
4. By freely exposing the discharge to air evaporation takes place & the fluid becomes too concentrated to permit the growth of bacteria. [Addition in pencil: *Guérin's method*] At the same time by supplying these organisms with plenty of oxygen there is no necessity for them to break up the albuminous compounds in their search for oxygen &, as shown by Pasteur, their fermenting power is diminished. Open treatment*
5. By keeping the part at perfect rest & by operating only when the patient is in good health the tissues & the blood are in such a state as to resist the developement [sic] of bacteria in the thin layer of lymph between the cut surfaces

Several of these principles are generally combined.

As these methods are so very numerous & as they are generally modified by every surgeon who uses them, few indeed using them on a true principle, I think it will be best merely to make a few remarks under each head & when we come to the [?] part we shall be able to fill up blanks in the following description.

Addition of various antiseptics

What is the best antiseptic?

Not carbolic acid.

& union by first intention thus occurs.This is best carried out by perfect rest & accurate approximation of the cut surfaces

Healing by scabbing acts on the last two principles. Other minor points will be alluded to at the end.

Although these various methods may [be] described as acting on the different principles mentioned yet there is no hard & fast line between one & the other. Indeed at the present day advantage is gained, & is now constantly employed, from the use of the various principles combined as in open treatment, employing also drainage, antiseptic irrigation &c. [Struck through] I have already answered the questions of the committee stating the principles on which antiseptic surgery is based & I have described at full length the best means of carrying it out. On that account & in view of the difficulty of separating these various methods of treatment from each other I shall content myself with making a few remarks under each head. [end of striking through]

1. The addition of various antiseptics to the discharge so as to hinder the developement [sic] of organisms in it. What are the best antiseptics to use for this purpose? Carbolic acid is the one most frequently employed but in my opinion it is by no means the best. We have already seen that in vegetable infusions in the proportion of 1-300 or 1-250 all further growth of organisms is prevented but that

Cabot's experiments

Objections to them

Objections to carbolic acid

in organic fluids such as serum, milk, pus &c. the acid forms a compound with the albumen & a much larger proportion of the acid is needed. Thus in milk carbolic acid in the proportion of 1-54 is just enough to prevent developement [sic]. [Struck through] Then it was found by Cabot (Boston Medical & Surgical Journal Nov^{r} 27. 1879) in some experiments as to the rapidity with which various antiseptics act that 1-40 carbolic lotion required 4 minutes to ensure the complete destruction of bacteria. This method however hardly allows a correct comparison between carbolic acid & other substances as to their germicidal properties, for as mentioned before carbolic acid coagulates albumen & as I have observed it causes bacteria to aggregate in masses. But independently of the last consideration there are always masses of bacteria & supposing some to be present the coagulation of the albumen on the outside of the mass may interfere with the penetration of the acid into its interior. Thus the carbolic acid may appear to be weaker as a germicide than it really is. However Cabot found that 1-20 carbolic acid acted more rapidly than any other antiseptic which he tried. [end of striking through]

There are however several objections to carbolic acid. My own experience of it as a disinfectant in putrid cases in the form of watery solution is very unfavourable to it.

Further 1-20 is very irritating & interferes with healing. Injected once or twice a day it destroys the superficial granulation cells & produces a thin slough over them in which bacteria develope [sic] & from which it is very difficult to dislodge them. Then its poisonous qualities are objectionable & are of course much more evident when the acid is injected into wounds or abscess cavities than when used in the manner described under antiseptic surgery.

Further Dr. Wilhelm Hack in a paper on the power of absorption by granulations demonstrated that granulations treated by carbolic acid possess many of the qualities of a recent wound as regards absorption. Apomorphia which was only absorbed by wounds treated with water dressing during the first 12 hours was readily absorbed at any time by granulating wounds treated with carbolic acid & therefore, in the absence of information to the contrary, I should fear that some of the poisonous products of putrefaction might be absorbed. Hence I do not like carbolic acid unless used aseptically.

Chloride of zinc

Chloride of zinc applied to the cut surface has been already alluded to. A single application has the remarkable property of preventing putrefaction in a wound for some time after an operation, sometimes indeed till in fact granulation is far advanced.

Boracic acid

Boracic acid as a lotion is too weak an antiseptic

Sulphurous acid

Chlorinated soda

Alcohol

[Addition in pencil] JWH [Jonathan Hutchinson?] especially has remarkably good results from the use of alcohol. His method is as follows: … [Cheyne's ellipsis] No doubt as he himself states he selects cases for operation carefully, a thing wh[ich] is not necessary & which is not done in aseptic surgery & thus part of his good results are no doubt due to principle 5. Nevertheless for the reasons I have stated I consider alcohol one of the best antiseptics. In many[?] of Mr. H.'s cases he has no doubt operated aseptically for to swab …

A method suggested

… a wound with strong alcohol & then stitch it up & apply antiseptic dressings outside very nearly approaches the regularity of aseptic surg[ery] though it is contantly liable to failure in that respect. Mr. H. also used free drainage so that [?] the [?] aseptic selection of healthy cases. The use of drainage & the use of alcohol all combine to give the good result. [end of addition]

to be of much service as an injection but the boracic ointment & the boracic lint act beautifully as dressings. One of the best antiseptics is sulphurous acid. This is a powerful germicide. It was found by Buchholtz to destroy bacteria when present in a proportion of 1-666. It is further non irritating & perfectly free from any poisonous qualities.

The chlorinated soda is mentioned by Cabot as standing next to 1-20 carbolic acid lotion in rapidity of action on bacteria.

Alcohol is not a bad antiseptic but it requires to be used pretty strong. It has a further advantage. Prof[r]. Hack has shown that granulations treated with alcohol do not absorb at all or only very slightly & thus to some extent may be attributed to the remarkably favourable course of the cases in which wounds are simply washed out with an alcoholic solution & a rag dipped in the same liquid applied outside. [Addition in pencil: *Mr. H.*]

Along with the use of antiseptics free drainage cannot be neglected.

The method I should recommend as the best acting on this principle is the following & I may say that I have tested it in practice & have found it to work splendidly.

After the wound has been made & before any

Method continued

stitches are inserted the raw surface ought to be thoroughly sponged over with chloride of zinc solution (40grs to the oz.) In the case of operations on the extremities this is best done before the tourniquet is relaxed so as to ensure its thorough application for otherwise the blood would wash it away before it has time to act. Then more stitches are inserted[,] special care being taken to ensure free drainage – large drainage tubes being employed. As a dressing in the first instance till the bleeding is stopped several layers of wet boracic lint (wet in boracic lotion) are applied.

As after treatment on the day following the operation the lint is removed[,] the surface of the wound is thoroughly cleaned with sulphurous acid or chlorinated soda lotions [addition in pencil: *or Hutchinson's lotion*] & the drainage tubes are washed out with the same though not removed. The dressing is now a strip of the full strength boracic or salicylic ointment thinly but equally spread on cotton & outside this one or more layers of boracic lint.

On the 2nd or 3rd day the drainage tube is removed & is washed in 1-20 carbolic lotion[,] the wound being then syringed out with the sulphurous acid [addition in pencil: *or other*] lotion. After a day or two the ointment over the line of incision is changed to the half strength but that over the drainage tube is maintained at full strength. These dressings are changed daily.

Terebene & sanitas are a remarkably good application where

the smell is very bad.

The results of this treatment are of course not so perfect as those of the aseptic method for however carefully one washes out the wound there are pockets in it into which the fluid does not enter, & any small piece of slough cannot of course be disinfected. Thus prolonged suppurations occur, caries continues without tendency to cure & even accidental wound diseases (py æmia &c.) attack the patient.

Case in use of chloride of zinc

With regard to the use of chloride of zinc I ought to say that it must not be applied to wounds which must if possible be got to heal by first intention as in incisions about the lips or face.

Lemaire's method.

It was on this principle as we shall see that Lemaire employed carbolic acid & coal tar & his results though very good by no means correspond to those obtained by strict aseptic treatment, [struck through] & from everything which has gone before it will be quite evident that the attacks which were made on Mr. Lister & the priority claimed for Lemaire had no foundation but in the ignorance of the writers [end of striking through].

Use of balsams acts similarly

It is also on this principle that the good results obtained by the use of balsams of various kinds &c. were obtained &[,] of the success of such attempts at rendering the wound secretions incapable of putrefaction[,] that of Bilguer's in the last century is the most remarkable. No doubt where the wound is shallow & possesses few recesses

& where the balsam or other antiseptic employed fills these up we really have an aseptic result & an aseptic treatment.

Neudörfer's method

By sprinkling powdered salicylic acid on wounds till no more fluid passes out Neudörfer manufactures a paste under which he says that healing occurs without suppuration.

Drainage

Action & methods

2. I have already touched on the method of drainage in a former part of this essay. It is quite clear that if discharge flows away as fast as it is formed there can be no marked development of bacteria or of their products. The free drainage of a wound, from which organisms are not from the first excluded, is therefore of the utmost importance. I have already described the use of indiarubber tubes & have referred to catgut & horse hair. Now as when a wound is not treated aseptically fermentation followed by suppuration will follow in the track of the drain[,] we must provide such a drain as will permit the escape of pus. Now neither horse hair nor catgut can drain pus. A tube of some kind may be used. This tube may be an indiarubber one or it may be rigid, made of various kinds of metal perforated at its sides & cut flush with the surface. This tube of whatever material must be removed from the wound daily & washed with a strong antiseptic, say 1-20 carbolic lotion. If this be not done portions of decomposing tissue &c remain inside the wound,

As to dependent opening

& become more & more putrid till very soon they become quite caustic. Where the wound is not treated aseptically the principle of having the most dependent opening possible must be carried out to the full.

Irrigation

3. The principle of free drainage is never of course used alone – other principles act along with it. Of these one of the most satisfactory is that of not only allowing the discharges to flow away but of washing them away & the further addition to this principle of adding an antiseptic to the water used for the washing & of thus having the wound kept constantly bathed in an antiseptic fluid.

With an antiseptic solution

The latter is the form in which irrigation & the water bath are now always employed viz. irrigation with an antiseptic.

Where practicable

Irrigation is as a rule only practicable on the extremities though it may be carried out on the trunk. For the latter however the continuous water bath is the most convenient. The wounded part having been arranged at perfect rest over an apparatus to catch the liquid[,] the vessel for irrigation is fixed at a considerably higher level than the patient. The form of irrigator most generally used is Esmarch's. This consists of a cylindrical lead or zinc vessel from the side of which, close to its bottom, a tube passes. In the

Method.

*In employing irrigation – about ?ing & greasing the skin.

Water bath.

end of this tube a long piece of indiarubber tubing is fastened with a nozzle at the end. This nozzle is so arranged so as to direct the fluid into the deeper parts of the wound. The fluid used is generally some weak antiseptic as chlorinated soda solution or sulphurous acid or boracic acid &c. (It should have been mentioned that the vessel for containing the fluid has a ring at its upper end by which it is hung up to a nail on the wall.)

These vessels may be varied in shape & size. A very good apparatus can be made in an emergency (Thiersch) by knocking the bottom out of a champagne bottle & having a tube passing through the cork to convey away the fluid. The bottle is inverted[,] filled with the solution & fastened to the wall.

The fluid used may be tepid or cold as we shall see. There is no special advantage in using it very cold as recommended by some.

*The continuous bath consists of a bath in which the patient [addition in pencil: *or in wh[ich] the wounded portion*] lies immersed, from which the foul water is constantly flowing out while fresh water is as constantly coming in. It is kept at the temperature of the human body. The bath is provided with a table on which the patient can place his books, food &c. As a rule he does not sleep in this bath but is taken out, wrapped in a flannel & placed in bed. This method is used much by Hebra[?] in the treatment of buboes[?] &c.

More details of these methods will be found under the history.

Advantages of irrigation

The advantages of the treatment of irrigation are that the discharges are removed as fast as they form & at the same time where an antiseptic is employed the part is kept constantly sweet. Thus where the cavity is small & uncomplicated there may be a truly aseptic state of affairs.

At the same time where tepid water is used granulation is favoured while pain & nervous irritation are very much diminished.

Further the parts are kept at absolute rest, it not being necessary to move them in order to change dressings, clean them &c.

Contrasted with water bath.

The continuous water bath is considered the most efficient method by those who have tried both. See Amussat &c.

{Struck through] The continuous water bath contrasts unfavourably with constant irrigation for there is less change of the fluid. There is not the same washing away of the discharge. At the same time it is the most convenient method as regards the trunk & where an antiseptic solution is employed it acts fairly well. [end of striking through]

The favourable results of the constant irrigation in preventing septic diseases are very remarkable & in this point it probably stands next to strict aseptic treatment. [Struck through] It certainly is far before the continuous water bath. [end of striking through]

Disadvantages

The disadvantages of these methods are for the most part the sodden state of the wound & the consequent oedematous condition of the granulations. For this reason they are dis-

-continued where granulation is perfect & has filled up the deeper parts of the wound & where the time for the developement [sic] of blood poisoning has passed.

Good results not due to heat

That the good results of irrigation & immersion are not due to the heat is shown by the results of M. Guyot in his incubation method. Here the part was enclosed in an incubation apparatus & kept permanently at a high temperature. This method, though much lauded recently, seems to have favoured considerably the developement [sic] of septic diseases.

Open method

The method of treatment which has been the greatest stumbling block in the way of the acceptance of the antiseptic principle is the open method[,] for surgeons have been unable to see how the success of this method could be reconciled with the germ theory of putrefaction. They have looked on it as the antithesis of aseptic treatment[,] as acting on the very opposite principle to that on which the aseptic method is based. And yet when we come to consider the matter in the light of the true principles of antiseptic surgery we find that in the open method there is a considerable advance in antiseptic treatment while of course other principles such as perfect rest, free drainage &c. [unfinished sentence?]

Principles

Now I have stated that this open method acts antiseptically in two ways & these I must briefly consider.

a. It acts antiseptically in that the discharges become dried

Liquid becomes too concentrated

up – more concentrated in fact - & thus become unfit soil for the growth of bacteria.

Pasteur's experiments on concentrated liquid

That concentrated fluids are not suited for rapid developement [sic] of organisms is well known. Thus Pasteur pointed out that sugary solutions which had become concentrated could not form organisms. This fact is used in the arts in the preservation of prints. Sugar is added in large quantities & then the prints can be kept an indefinite length of time. It is not that the sugar is an antiseptic, killing the organisms. It is merely that by its presence in large amount the fluid is rendered unsuitable for developement [sic].

Examples from the arts

The same principle is made use of in preserving milk. The milk is evaporated to 1/3^{d} its original volume & a considerable quantity of sugar is added to it. Without the addition of sugar the condensed milk may be kept for a considerable length of time without the appearance of organisms. With sugar it may be kept indefinitely.

The same is the case with other albuminous fluids such as the discharge from wounds. Evaporate pus[,] & organisms cannot develope [sic] nearly so quickly in it.

[Struck through] As at the present time antiseptic irrigation once daily is generally combined with the open method[,] additional advantage is gained in this respect. [end of striking through]

Oxygen is freely admitted

b. In the open method a further antiseptic advantage is gained by the free admission of oxygen to the discharge.

Effects of oxygen in retarding fermentation

Comptes Rendus Tome 56.

Vincenz von Kern's method

Some very remarkable effects of oxygen in retarding putrefaction & fermentation were published long ago by Pasteur. He pointed out that if a sugary solution were freely exposed to air in a thin layer, the yeast though it grew luxuriantly caused very little fermentation. On the contrary if oxygen were excluded[,] only a small developement [sic] of the yeast cells was necessary for fermentation.

With regard to putrefaction he has brought forward similar evidence. The organisms which cause putrefaction are according to him incapable of living in the presence of oxygen. If a putrescible fluid be freely exposed to the air in a thin layer putrefaction does not occur[,] at least not for a very considerable time. In the same way as in the butyric fermentation[,] oxygen not only interferes with the fermentative process but actually destroys the bacteria which cause it. Hence the free exposure of a putrescible fluid to the air results in comparative freedom from putrefaction partly because the oxygen interferes with the developement [sic] of fermentative processes & partly because the oxygen directly kills the putrefactive bacteria.

[Struck through] Vincenz[?] von Kern is generally looked on as the introducer of the open method. This however is not the case when the term "open method" is used as indicated above for Kern really introduced treatment by water dressing[,] a method which I do not include under "antiseptic" methods but

which on the contrary I regard as a form of "septic" surgery. [end of striking through]

Bartscher & Vezin's method

There are however two main methods acting on the principles above alluded to. The first is that introduced by Bartscher & Vezin & was carried out in the following manner. "After every bleeding vessel had been tied & after the stump had been cleansed from blood clots by means of a sponge & cold water, the patient, for whom 2 beds are provided close to each other, is placed in bed & the stump is laid on a soft pillow, & over it a piece of gauze or linen is loosely placed to keep out the flies, the whole stump being freely exposed to the air." At the morning visit the surgeon pushes his hand under the stump[,] raises it[,] removes the pillow & applies a new one without any further cleansing of the wound.

By this method all attempts at union by first intention are given up.

Burow's method

Burow of Königsberg published his method in 1859. He attempted to obtain primary union as far as possible. This he did by bringing the surfaces of the wound in contact after a few hours by means of strips of plaster applied over part of the surface[,] all other dressings being avoided.

Rose adheres to the first but adds antiseptic irrigation

Rose[,] whose results of open treatment are best known adheres to Bartscher & Vezin's method with the exception that he daily washes out the wound with some

antiseptic lotion. Crusts are removed as they are never complete enough to prevent putrefaction & only cause tension by their presence & indeed protect the discharge underneath from the action of the air or of the antiseptic lotion.

Rose further uses very free ventilation so as to have a plentiful supply of air to the stump. He employs this method for all wounds except those on the face where union by 1st intention is desirable.

The open method pure & simple is that of Bartscher & Vezin's just described for here while the discharges are allowed to flow away freely they are left to the unaided action of the air.

The antiseptic effect of this is undoubtedly increased by its combination with intermittent antiseptic irrigation.

Burow's method cannot act nearly so perfectly for in it discharges must accumulate in various parts of the wound & they are therefore not nearly so conveniently placed for thorough action of the air.

Objections to method

The objections to these methods are that in all except Burow's[,] union by 1st intention cannot possibly occur; indeed no attempt is made to obtain it. As a consequence of this the time in healing is very much prolonged while a much larger scar is obtained.

[Note added in pencil:] other objections

Healing by scabbing

Closely allied to this open method of treatment stands healing by scabbing. This may be brought about in

two chief ways. The crust may either be allowed to form naturally or its formation may be aided by artificial means.

Mode of action

This healing by scabbing acts in 2 ways. In the first place the first principle of the open method of treatment comes into play; the discharge dries up & becomes an unfit soil for the developement [sic] of organisms. In the second place the fluid underneath it is present in such a thin layer that the living tissues in the neighbourhood prevent the developement [sic] of organisms in it. Of course in many cases no living organisms would be there to develope [sic] for the scab would form an absolute protection against their entrance.

Natural formation of crust

The natural formation of the crust can only take place efficiently in small wounds & only exceptionally where there are cavities & recesses present. For in larger wounds as we have seen there is too much discharge for a sufficiently rapid formation of a crust & if this is not perfect & quickly formed putrefaction takes place underneath it while at the same time by confirming the discharges it causes tension & ulceration in place of healing.

Artificial formation by powders

But though this natural formation of a crust cannot be trusted to in most cases yet by artificial means a very perfect one may be obtained. This may be done by the application of various powders

best powder

such as starch[,] alum, flour &c. to the discharge so as to form a paste. The best substance which can be employed in this manner is however powdered salicylic acid as recommended by Neudörfer & mentioned before under “treatment by antiseptics”. This is powdered on till no more exudation occurs & it combines the advantages of a thorough crust with those of an antiseptic dressing.

By application of caustics

Other attempts have been made to produce a scab by the application of various caustic substances to the surface of the wound. The crust so formed, containing as it does a strong caustic, is unable to undergo putrefaction & where the inflammation caused by the application is not too great & does not lead to accumulation of fluid under the scab, an excellent result is obtained.

In this case as no organisms are present under the crust (they were destroyed by the caustic) & as the firmly adhering crust prevents their entrance we have the typical aseptic result – healing of an open wound without suppuration or granulation.

Objections to this

There are various objections against this method of crust formation[,] the chief of which is the loss of substance involved in the process while the same disadvantages are present as in the open method proper viz. slowness of healing & a large scar.

[Addition in pencil: *Others form a crust by the cautery*]

These methods of crust formation are seldom suitable

A. Guérin's method

Original method

The details of this method will be found in the history insert – these were from Blanchard's paper – objections see D[?]. at F. Academy.

Principles

[Note in pencil] A point wh[ich] G[uérin] regards as of first importance is that the packet of wool should not be opened till the surg[eon] is ready to use it & the dressings should not be changed in the ward.

unless in the case of superficial wounds without recesses or cavities for there an opportunity is given for the occurrence of tension under a crust. It is a method by no means easy or universal of application.

Where it is employed I should recommend the use of Neudörfer's method – the formation of a crust by the aid of salicylic acid powder.

A. Guérin's cotton wool treatment seems to act on the first part of the principle of the open method but it hardly comes into the category of antiseptic methods.

As originally used the wound was simply washed with water & a large mass of cotton wool applied around it & firmly bandaged on. At present the wound is washed with some antiseptic lotion[,] & layers of cotton wool containing camphor powder sprinkled in it [addition in pencil: *or the deeper portions moistened with carbolic lotion*] are applied

This method can hardly be called antiseptic in the true sense of the word for the antiseptic principles on which it acts are not very powerful. As the result of these dressings the discharge becomes thick & concentrated & not a very good medium for developement [sic] of organisms. Nevertheless this concentration of the discharge cannot occur to anything like the extent which takes place when it is left freely open to the air.

The second principle on which it acts is that it ensures

*I have before described the antiseptic application of cotton wool

Improvements in the method

Lister's original experiment

Barker's method.

Value of Guérin's method

Principle of Perfect Rest

How this acts antiseptically

absolute rest of the wound [struck through] & thus aids the tissues to resist the developement [sic] of bacteria in the manner to be presently described. [and replaced with:] though bacteria & their products are present the gran. wall is complete & there is no means by wh[ich] they can penetrate into the body.

[Struck through] Nevertheless the method is very imperfect. Attempts have been made to improve the method by purifying the cotton wool. Before Guérin introduced it Mr. Lister had tried a similar dressing of purified cotton wool applied aseptically & found it to work admirably but the purification of the wool was expensive & troublesome & as it did not hold the antiseptic[;] it was found that when the discharge arrived near the edge of the wool putrefaction at once spread inwards.

Barker has likewise in a recent number of the British Medical Journal published a similar attempt but it has not yet been brought to a practical form. [end of striking through]

Returning however to Guérin's method it cannot be recommended except in some exceptional cases of disease of joints with sinuses & even here the retention of the putrefying discharges & the consequent irritation render it of very doubtful value.

*5. The last principle of antiseptic surgery which I will touch on is that of Perfect Mechanical Rest. By this means when the tissues are in perfect health & the blood clot is undisturbed the tissues & clot may be kept in

Union by 1st intention without aseptic treatment

Principle on which it occurs

In connection with this quote John Hunter on the living principle of the blood

such a state as to resist the developement [sic] of organisms. This method, though without recognition of the antiseptic principle has been long practiced & has of late been written up by Sampson Gamgee. (Treatment of Wounds).

It is of course a well known fact that without any antiseptic appliances whatever[,] wounds, more especially those about the face[,] heal frequently by 1st intention. How can this take place if blood exposed to the air putrefies as the result of the developement [sic] of living organisms? For during the operation organisms enter the wound both as dust from the air & surrounding objects & also from the water in which the sponges are soaked. There is also between the cut surfaces a layer of blood or lymph (which however must be in very small amount otherwise healing by first intention does not occur) which if it were exposed between two plates of glass would to a certainty be putrid in a few hours. How is it then that it does not putrefy in this case? How is it that union by first intention can occur under these circumstances?

As Mr. Lister has pointed out, the fact is that a thin layer of blood although containing numerous causes of putrefaction & although rapidly putrefying in a glass vessel[,] does not putrefy if it be placed between two healthy living cut surfaces.

Or to state the fact in another way these organisms cannot develope [sic] in a thin layer of blood or lymph placed

Principle continued

My own exzperiments

between two healthy living freshly cut surfaces.

Or to state the same fact differently (taking into consideration the different result when the same layer of blood or lymph is placed between two plates of glass) the living tissues when in a healthy state have the power of preventing the developement [sic] of organisms in their immediate vicinity.

I have already had occasion in a former part of this essay to point out that if into a healthy living animal a small quantity of ordinary bacterial fluid be injected, these bacteria lose their vitality & disappear. I have shown how organisms cannot be found in the healthy living body (I except here specific pathogenic organisms as bacillus anthracis) unless a considerable amount of their products be introduced along with them. On the other hand I demonstrated how if the animal were out of health organisms could live in their blood & tissues much more easily. And the same is the case in union by 1st intention. If the part be of high vital power & in a healthy state & if there be an extremely small amount of blood or lymph between the cut surfaces union by 1st intention will almost certainly occur. If the part become inflamed or if the patient be in a weak bad state of health union by 1st intention, without antiseptic means, becomes a matter of great uncertainty.

Traube & Gescheidlen's experiments

Lister's experiment on urine

Traube & Gscheidlen have likewise found that blood taken with precautions from a healthy living rabbit into which 24 or 48 hours before 1½ cub. cent. of bacterial fluid had been injected, could be kept for months without undergoing putrefaction.

And, further, the facts which I have already mentioned that the blood & tissues of healthy living animals do not contain living organisms shows sufficiently that they have the power of destroying them for otherwise there are abundant opportunities for their entrance into the circulation.

This same principle was made use of by Mr. Lister in his experiment for obtaining unboiled urine. He simply washed the glans penis, and the meatus urinarius. He did not wash out the urethra at all. The urine passed in this way remained absolutely pure showing that no organisms were present in the urethra. And yet the urethra contains a putresicible mucus, putrescible outside the body, & there is sufficient time between the acts of micturition for bacteria to spread quite up to the bladder. (This was experimentally determined by Mr. Lister.) And yet they do not penetrate at all into the urethra. In other words they cannot develope [sic] in this putrescible mucus when lying between 2 healthy living surfaces. Or to state the fact otherwise, the healthy living tissues have somehow or other the power of preventing the developement [sic] of or-

To render tissues capable of resisting putrefaction

Avoid all causes of irritation

Blood clot is a tissue

*Insert here Lister Dublin Med. Journal p.105
I have found [unfinished sentence]
In recent address at Cambridge he mentioned some experiments.
John Hunter in his work on the living principle in the blood mentions the following curious fact:
That the blood alone if at perfect rest can under [?] of the tissues bring about this state of asepsis is shown by the following quote from a paper of Esmarch's in L.A. Vol. V[?]

Methods of carrying out this principle

-ganisms in their immediate vicinity.

These facts sufficiently reconcile the fact of union by first intention in cases not treated aseptically with the germ theory of putrefaction.

Now in order to have tissues in the state in which they are capable of resisting the developement [sic] of bacteria in their immediate vicinity they must be as much as possible in a condition of perfect health. To attain this the health of the patient must be attended to & kept good and all causes which irritate & cause the wounded part to inflame or become weaker must be avoided. The causes which weaken the part are various forms of unrest, mechanical or chemical.

Now blood clot itself may be looked on as a tissue, though one which is of very low vitality. That it is a tissue is well known by Mr. Lister's experiments on coagulation of blood for he found that a tube or cup of blood clot acted in the same way as regards the prevention of coagulation – as the healthy living walls of the vessels. He further found that movement of the clot leading to its laceration destroyed its vital power as a tissue.

*Thus then this principle may be carried out in two ways. By accurate approximation of the cut surfaces & absolute immobility the tissues are kept

General health be also attended to

Gangee's method.

Imperfect from not understanding the principle.

Suggested improvement

in such a state as to resist the developement [sic] of organisms in their vicinity. Where accurate approximation cannot be obtained the same principle of absolute rest may be carried out as regards the clot filling up the gap & it may thus resist the developement [sic] of bacteria in its substance.

This is carried out by Gangee by accurate apposition of the line of incision in its greater extent by covering by with strips of lint dipped in styptic colloid[?], by firm pressure[,] by means of cotton wool & by absolute immobilization[,] by means of accurately & firmly applied splints.

This method is not carried out on the principle I have described & consequently does not fulfil the best conditions. Were I compelled to treat any case on this principle I should combine it with the open method, leaving the wound freely exposed to the air.

I have mentioned this as an antiseptic method as the principle is of great importance in explaining certain otherwise puzzling cases but I should not recommend it for adoption for it is in very few cases that this vital action of the tissues & blood clot can be sufficiently trusted [Addition in pencil] unless in parts of high vitality as the face in which this [? ? ? ?][end of addition]

Such are the chief principles on which antiseptic treatment can be carried out. In the class of antiseptic

methods to which I have been latterly referring the surgeon does not adhere strictly to one or other principle partly because he does not understand aright the principles on which he acts & partly because better results can be obtained by their combination.

Minor ways of aiding antiseptic treatment

In many minor ways the antiseptic principle may be aided. Thus by the use of catgut ligature we do not have a long septic thread hanging out of the wound conducting putrefaction to its interior & leading to deep seated suppurations &c.

Catgut ligatures

Wire stitches

In the same way does the use of the silver suture act as compared with the silk. Silver does not absorb the putrescent materials & they cannot therefore putrefy in it. On the contrary silk absorbs blood & serum which putrefy in it & the silk which at first was irritating soon becomes caustic & causes inflammation in its vicinity – unrest of the wound.

Attention to general health

What are the various means by which the general health & tone are kept up in septic cases but imperfect attempts at antiseptic surgery? For in this way the body may be fortified to resist the entrance of organisms into it.

Ventilation

What is ventilation but an antiseptic means? The air being constantly changed the foul emanations from wounds, containing septic bacteria, are diluted

Subcutaneous surgery.

So much for the various methods of antiseptic treatment. Which is the best?

& swept away while at the same time a better state of health is obtained.

Subcutaneous surgery might also have been alluded to as a form of antiseptic surgery. Here the whole wound is made under the skin while the opening communicating with the outer world is small & heals by fist intention on the principle just mentioned.

So far then I have executed my task. I have defined what antiseptic surgery is. I have shown what are its essential details & what are the principles which ought to guide us in all the various methods by which it can be carried out. It only remains for me to complete the answer to the question[:] How are its details best carried out in practical form? :By briefly considering which of the various methods described is the best. I shall not enter here into any detailed statistical discussions. I shall merely point out the chief advantages & disadvantages of these methods compared with each other.

And firstly I shall consider antiseptic surgery as consisting of two great divisions – methods by which attempts

are made to exclude organisms completely from wounds, & secondly methods by which organisms are admitted to wounds but where attempts are made to interfere in some way or other with their growth.

Theoretically aseptic surgery is best

At once one would be inclined to say; surely it is more sensible & effectual to exclude organisms entering if that can be done (& my researches show that all that is necessary can be done) rather than to let them in & then try to get them out, a task in many cases utterly impossible.

Experience confirms this

And experience of the course of these two cases shows that the former is the better method in every respect. With regard to mortality no record is extant of such a constant series of successes as after Lister's aseptic surgery, a success constant not only in his own hands but in those of his immediate pupils. That it is not so constant in other hands depends entirely on the fact that the treatment is not properly carried out[,] mainly because the principles which ought to guide the surgeon are not believed in or more often not known to him.

Constancy of results

I may be allowed first to mention as an example some of Mr. Lister's general results. I have lately had access to his notebooks from November 1871 till the time of his leaving Edinburgh Aug. 1877[,] a period of 6¾

Mr. Lister's general results

years & he has kindly allowed me to tabulate his statistics & use them for the present purpose.

Total cases & deaths.

In his note books I find a total record of 84 operations performed partly strictly aseptically & partly as far as possible antiseptically. Of these 845 cases 37 died or a mortality of 4.4p.c. After going over the cases carefully I find that 120 may fairly be excluded as cases of minor operation. Among these were all amputations below the ankle & below the wrist, cases of hæmorrhoids, fistula in ano, excision of small tumours, small abscesses, necrosis cases &c. Thus there remained 725 cases of undoubted major operation with 37 deaths or a mortality of 5.1 p.c. (It is to be observed that among the minor cases no deaths were included.

Deaths from blood poisoning.

Of these 37 deaths 6 cases or 82 p.c. died of blood poisoning[,] of these 2 died from pyæmia, 2 from septicæmia & 2 from erysipelas.

Aseptic cases

& cases not aseptic

Now of these 845 operations 553 were performed aseptically & of these 553 only two died of blood poisoning (one from septicæmia & one from erysipelas) while of the 292 cases treated as far as possible antiseptically 4 died of blood poisoning.

Comparison between these as to occurrence of blood poisoning

Thus in Mr. Lister's own practice in the same place at the same time the deaths from blood poisoning were 4 times as many in cases not treated aseptically

as in those where that method was carried out. But here we have hardly a fair comparison for among the 553 aseptic cases there was a very much larger proportion of great & grave operations than in the other. For among these are about 100 cases of incisions into larger joints, 80 major amputations, many cases of excision of the mamma, operations for ununited fractures, large spinal abcesses &c. &c. While among the 292 cases not aseptically treated were a large number of fistulæ, necrosis, hæmorrhoids &c.

Cases in which blood poisoning occurred

The 2 deaths from blood poisoning in cases treated aseptically occurred after excision of the mamma. In one a large raw surface was left, putrefaction occurred through a mistake of a dresser noticed at the time & septicæmia followed.

The other patient died from erysipelas which did not markedly affect the wound but which rather affected other parts of the body.

The 4 deaths from blood poisoning in the other set of cases consisted of 2 from pyæmia (one after amputation of the penis & the other following a plastic operation on the nose), one from septicæmia (a case of excision of the tongue) & one from erysipelas (a case of small abscess of the neck opened by the house surgeon without aseptic precautions.)

The period of the aseptic method may be divided into

the first from 1871 to 1875 during which several advances were being made in the treatment[,] in which period the mortality was 4.7 p.c. & a period from 1875 to 1877 in which the treatment was carried out more perfectly & in which the mortality was 3.8 p.c. The 2 cases of blood poisoning occurred during the first period.

Compare with Spence's results

Comparing these results with those obtained by Mr. Spence of Edinburgh by the use of a form of treatment closely allied to the open method we find a marked difference; for to take the period of which Mr. Spence gives complete details from Oct^{r} 1872 till Ap. 1878 (with the exception of the winter session 1874-75) he states that during that period 331 cases of major operation & injury were treated of which 58 died or a percentage mortality of 17.5. These results were obtained in the same hospital & during the same period as Mr. Lister's. Of these 58 deaths a considerable number died of blood poisoning (we are not told how many but certainly more than 12). Say 124[,] we have a mortality from blood poisoning of 3.6 or more than 4 times Mr. Lister's mortality & greater than Mr. Lister's mortality in cases not treated aseptically.

Take amputations

But take a special operation & contrast these various methods. I here give Mr. Lister's results during the period mentioned in a tabular form & I exclude

Mr. Lister's results

Mr. Spence's results

primary amputations from the list so as to exclude as far as possible cases of shock for of the 16 main primary amputations performed by Mr. Lister 4 died all within 36 hours after the operation.

Contrast with these Mr. Spence's results during the same period & exclude here also primary amputations. Of these there were also 16 of which 7 died but the cause is not given in all.

Mr. Lister's table of amp[utation] for disease & secondary to injury

	No.	Died	
Hip	2	2	both from shock
Thigh	26	1	from diphtheria 9 weeks after operation
Leg	5	0	
Ankle	16	1	from cerebral hæmorrhage 3 mos. after op
Shoulder	1	1	from hemorrhage into thigh from a sacomatous[?] tumour of the femur 3 days after operation
Arm	6	0	
Forearm	8	0	

64 cases, 5 deaths or 7.8p.c.

Thus it will be seen that in no case was the cause of death preventable by any antiseptic method of treatment

Mr. Spence's table of amputations for disease & secondary to injury

	No.	**Deaths**		**No.**	**Deaths**	
Hip	3	1	Ankle	24	2	
Thigh	26	6	Partial of foot	6	2	Total 82 –
Leg	9	2	Shoulder	3	1	18 deaths
	38	9	Arm	7	2	
			Forearm	2	2	Partial of hand 1-0
				43	9	

Rose's results

Thus giving a total of 82 cases with 18 deaths or a mortality of 22.2 p.c. as compared with Mr. Lister's mortality of 7.8 p.c.

Mr. Spence's mortality after amputations is very much the same as that obtained by Rose by the use of the open method of treatment combined with antiseptic irrigation.

The following table is copied from Krönlein's work & represents the cases treated by the open method between 1867-1871

	No.	Died
Thigh	28	10
Leg	11	2
Foot	15	3
Upper arm	14	2
Fore arm	10	0
Hand	7	0
	85	17 or a mortality of 20 p.c.[;] of these 7 p.c. died of pyæmia or septicæmia

Comparison of these.

Thus we see that looked at from a merely numerical point of view we have a great advantage in cases treated aseptically.

Aseptic	7.8 p.c. of deaths
Open with irrigation	20 p.c. " "
Somewhat less antiseptic method	22.2 p.c. " "

This advantage in favour of aseptic methods is the same whatever group of operations

is taken & there is this further difference that while in the aseptic cases there are no deaths form blood poisoning, in the others there are always some.

Were I to go into this matter in detail I might produce a great mass of facts which would show that I am justified in these conclusions but to do so would be quite outside the scope of this essay.

Operations may be done safely aseptically which could [not?] be otherwise attempted

I must however point out one or two pure facts which lead us to decide in favour of strict aseptic methods as the best means of carrying out antiseptic details in practice.

Not only does the aseptic method save lives in ordinary cases but it enables operations to be carried out which are admittedly unjustifiable by any other method.

Operations on healthy joints

Such are operations on healthy joints. As it so happens all the cases which Mr. Lister has treated since 1871 have been published by one of his pupils & as I happen to have a printed copy by me I may introduce it here & make some remarks afterwards.

I may state here that lately Mr. Lister has had another case of fractured patella (recent) treated like no.15 with similar course & a perfectly moveable joint. Here passive motion was begun 3 weeks after the operation which was performed 3 or 4 days after the injury. Thus there are 21 cases of incisions

into healthy joints.

Take notice of the following facts

Now it must be observed that the conditions here are different from those in the cases where successful incisions were made into joints without any aseptic precautions.

In the latter class of cases as soon as the foreign body is removed the wound is at once closed & union by 1st intention is hoped for but in these cases of Mr. Lister's the joint was kept deliberately open for a drainage tube was inserted into the joint, & taken out at each dressing, washed & put back again. If this were done without any aseptic precautions I make bold to say that infallibly suppuration of the articulation must ensue but here we see that all the cases followed an "aseptic course".

What is meant by "aseptic course"

As this aseptic course may not be quite understood I shall conclude my remarks by pointing out what is the usual course followed by wounds or incisions treated strictly aseptically – what is the typical aseptic course.

Example

To illustrate this I shall narrate a case of removal of an exostosis from the lower end of the femur in which it was not certain whether the articulation was opened or not.

John D. aged 43, was admitted into the Edinburgh Royal Infirmary on February 12th 1874, suffering from exostosis growing from the external condyle of the right femur. The patient stated that for the last 30 years a swelling had existed on the outer side of the lower end of the right femur. This had increased very slowly, but of late it had interfered with the movements of the knee joint.

There was a small tumour about the size of an orange, quite hard, situated on the outer side of the right knee, apparently growing from the external condyle of the femur. The patient complained of great pain in walking.

Operation: Febr 12th A longitudinal incision about 3 inches in length was made on the outer side of the knee over the tumour. Some synovial fluid escaped, probably from a wound in the capsule of the knee joint. The tumour was found to be pedunculated, & the pedicle was cut across by bone forceps. (The rough ridge corresponding to the base of the tumour extended to a point of ¾s of an inch below the level of the upper border of the patella, the limb being extended). Two drainage tubes were inserted & the rest of the wound was stitched up.

Febr 13th Morning temperature 99.2 degrees[,] pulse 70. The patient has not slept well, in consequence

of chloroform sickness. He had taken a good breakfast. There was almost no pain in the wound. The dressing was changed, & found to be soaked with a sero-sanguineous fluid. One drainage tube was removed.

Febr 14th Morning temperature 99°. Pulse 76. The patient felt quite well. As no discharge was visible outside the dressing at the usual time of visit, it was not changed.

Febr 15th Only the superficial dressing was changed today. There was only a very slight serous stain on it. There was no redness nor pain or pressure around the wound.

Febr 19th The patient had not been dressed since last date. The whole dressing was changed today. One stitch & the remaining tube were removed.

Febr 26th The patient was dressed. There was only a slight serous stain on the dressing; no pus. All the remaining stitches were removed. Cicatrisation was complete except where the drainage tube was & there blood clot filled the channel.

March 5th Healing was complete everywhere except a minute point where the drainage tube had been. There had never been any pus or granulations.

March 12th The wound was quite healed. The knee joint moveable.

Temperature in aseptic surgery

March 25th The patient was dismissed cured.

Here we observe that there was no local or constitutional disturbance after the operation; & such is the typical aseptic course.

It is unfortunate that in this case, which I have selected as being one where the knee joint was probably opened, a complete record of the temperature has not been kept. I may, however, state from careful examination of many temperature charts of cases treated aseptically that the following is the usual course. On the evening after the operation, the temperature is generally slightly below the normal, especially if the operation has been a severe one. Next morning, it is normal, or slightly elevated (one or two tenths of a degree). On the following evening it is generally at its greatest height, being rarely higher than 100°F. It then rapidly falls, & is again normal 24 hours later. It never rises again, unless putrefaction occurs, or unless other causes of elevation of temperature, such as tension, come into play.

After the effects of the chloroform have passed off, the patient who has been operated on aseptically has the aspect of perfect health, no impairment of appetite or other symptom of fever being present.

As regards the local effects, there is no pain, no inflammation, no suppuration; the discharge, which is serous in quality, rapidly diminishes in amount & hence the dressing requires to be but seldom changed. If a joint be opened, there is no impairment of its mobility.

All facts tell in favour of antiseptic surgery

Such is the ordinary course of an incised wound under aseptic methods of treatment. Such a constant course cannot be observed by any other method of antiseptic surgery or of septic surgery.

Such a constancy of union by 1st intention
Such a constant rapidity from healing
Such a constant freedom from pain
Such a constant absence of fever
Such a constant series of successes
Such an impunity from accidents where operations never dreamt of before as feasible is not attained by any other method; at least I have never seen or read of any such.

Conservative surgery also strengthened

And it is not only with regard to these joints that advantages are gained, the cause of conservative surgery is greatly strengthened for now it is very rarely necessary to amputate in case of bad compound fracture or to excise in case of wound of joint, & diseases of joints with or without suppuration may as a rule be cured by free incisions without

any mutilation of the limb such as excision or amputation entails.

Results in abscesses

Then again large abscesses connected with disease of the spine may be cured in the great proportion of cases. No more suppuration occurs after the abscess is opened. There is merely a serous discharge which may last for months but which ultimately dries up & the case is cured. And this cure takes place without any deterioration of health. I shall mention one piece of statistics to show this & it is the last which I shall introduce here.

Statistics asked by Mr. Spence

In a recent attack on Mr. Lister, Professor Spence states that when Mr. Lister left Edinburgh he left behind him several cases (he says 17) of chronic abscesses uncured. Now take these cases selected by Mr. Spence & see what are the results.

Dr. Bishop under whose care these cases were placed makes the following statement.

"The cases in question were as follows

Name	**Age**	**Disease**	**Date of admission**
W^{m} M.	17	Abscess of hip joint	Jan^{r} 7th 1876
John D.	18	Psoas abscess	March 16th 1876
Lizzie T[?].	19	Psoas abscess	Aug. 1st 1876
James B.	23	Abscess of hip joint	Jan^{r} 5th 1877
Hugh M^{c}L.	6	Lumbar & psoas abscess	Jan^{r} 19th 1877

Dr. Bishop's reply

Name	Age	Disease	Date of admission
Michael C.	21	Two lumbar abscesses	Jan^{r} 25th 1877
John B.	25	Psoas abscess	Jan^{r} 25th 1877
Alex. W.	16	Abscess of hip joint	June 22nd 1877.

Thus the cases were 8 in no. instead of 17. Knowing that these cases could have little clinical interest for any one who had not treated them from the outset, & at the same time feeling that they would still demand careful management for a more or less protracted period, Mr. Lister thought it better to remove them from the Infirmary when he went to London. He therefore wrote to ask me to take charge of such cases as the house surgeon thought it desirable to place under my care. The female patient was sent to King's College Hospital. One male patient Alex W. was considered by the house surgeon unsuitable for removal. He was in a very weak state, some bed sores having only just healed. He was, however, much improved & was regarded as in a promising condition. The remaining 6 patients were transferred to my care, & I had the satisfaction of seeing them <u>all cured</u> with the exception of the little boy, whose abscesses had become putrid before he left the Infirmary, probably from slipping of the dressings owing to the extreme deformity of his body. His father removed

him in August 1878 to the West of Scotland. He was then considerably improved, having youth on his side to resist the septic influences; &, when he was last heard of, he was running about.

The results of the rest of the cases were as follows.
W^{m} M. healed in Dec^{r} 1877; left in Feb^{r} quite strong. When last heard of, he had gone to Liverpool as clerk in a warehouse.

John D. healed in March 1878; left Edin. in May. When last heard of, he was able to walk without support & was feeling quite strong.

James B. healed in May 1878; Left Edin. in July. When last heard of he was working at his trade (as a baker).

Michael C. healed in March 1878; left in May. In July 1878 he was able to go to Peterhead to the herring fishing. He returned to the fishing in 1879.

John B. healed in Dec^{r} 1877; left in Feb^{r} quite strong & well. When last heard of, he was employed as a colporteur."

I may state from personal knowledge that Lizzie T. – the female patient – is now walking about well & strong.

Observe here that I have not selected a piece of statistics to support the statements

I have just made as to the course of abscesses. The selection was made by an adversary, I can hardly say of aseptic treatment, but of Mr. Lister, & yet the result supports in every way what I have stated.

Foreign evidence

From these & many other facts which will be found stated in the publications of Lister, Volkmann, Nussbaum, Championnière, Saxtorph &c. there can I think be no doubt that the best & most constant results are obtained by strict aseptic treatment – by the exclusion of the causes of putrefaction.

But before I can set this method down as the best I must refer for a moment to the disadvantages which the system is said to possess.

As to Risks of carbolic poisoning

It is said that there is great risk of carbolic poisoning. This I can most emphatically deny from long experience of the method. Black urine is no doubt common enough but that is not carbolic poisoning any more than a trace of Iodide of potassium in the urine is Iodism &c. In 8 years constant experience of aseptic work, the antiseptic used being carbolic acid, I can only remember 4 cases in which bad effects were produced by the absorption.

But this objection does not tell against the method. If it were worthy of notice it would merely tell

Other objections to aseptic treatment

against the antiseptic employed. So long as the principle is attended to, should this poisoning occur, some other mode of excluding the organisms may be employed, either by salicylic acid, boracic acid, benzoic acid, &c. &c.

Aseptic surgery is said to be costly. Now it cannot be denied that each individual dressing compared with the absence of dressing in the open method is costly but the matter must be look at [in] another light.

In the first place the dressings are really not costly for they are few in number & from a calculation I made some time ago they do not cost more than water dressings.

Then again in aseptic surgery no tonic nor stimulants are required.

No special nursing is necessary.

Further the time during which the patient is unfit for work is as a rule very much less & in the case of the breadwinner this is a matter of very great consequence.

Lastly the treatment is said to be troublesome & not practicable in private practice. Now if the principle be carried out & some sort of method such as I have sketched be employed the trouble of these cases is very little while all anxiety is abolished.

But granting for a moment that the treatment were troublesome[,] if it possesses the advantages I have mentioned what right have we to deprive our patients of these because we are put to a little extra trouble? Medical Education is trouble & why do we trouble ourselves about perfecting our knowledge? Simply in order to do the best for our fellow creatures. Why then should we refuse to bestow the same trouble in this case? Those who argue thus will find that any extra trouble they bestow will be well rewarded by the excellent progress of their patients, their freedom from pain, feverishness &c & by the absence of anxiety from their own mind.

Conclusion

With this I must come to an end. I have considered to the best of my ability what is meant by Antiseptic Surgery. And in striving after this meaning I have attempted to point out the principles which ought to guide us in carrying out this most important part of the treatment of wounds. I have considered under the head of each form of treatment what its essential details are while at the same time I have tried not to forget all the general surgical

[additional note following on from end of page 298] I have now finished my task. I have attempted to show what the cause of the frequent bad progress[?] of wounds is & how it may best be avoided. The whole principle of local wound treatment may be summed up in the word Rest. Causes of unrest may be mechanical or chemical. The mechanical causes are at once avoided by the use of suitable apparatus & removal of any mechanical irritant. Chemical causes of unrest may be divided into two classes. 1. Where the chemical substance is merely a salt or acid or alkalin added to a wound from without. This will act only in proportion to & so long as the quantity added remains in contact. When the original quantity is exhausted the chemical irritation of the wound ceases. The other mode of chemical unrest is where the chemical substance is constantly formed in the wound. Here we have the most formidable cause of unrest to deal with for there is no exhaustion of the substance but continued action till the causes of the formation of this chemical substance are removed. To remove these causes or prevent their entrance is the object of Antiseptic Surgery. The latter is the special object of aseptic surgery. This is much more easily done than the removal of the causes which[,] as must now be evident[,] is a more difficult task.

That advance can only be blind & imperfect till the true law of nature is discovered is fully evident from the history of wound treatment in former years. Through the darkness which then reigned glimmers of light have at times appeared but no true & lasting progress was made till quite recently when chiefly by the labours of 2 men, Pasteur & Lister, a flood of light has been thrown on one of the most obscure subjects in nature[,] & the formulation of rational methods of treatment on rational & scientific principles has been followed by inestimable advantages to mankind.

principles to be attended to in trying to obtain complete rest for a wound. And lastly I have attempted to show in what way these essential details may best be carried out in practical form. I have shown that where possible aseptic surgery is the best & from the remarks I have made under the other heads my views as to the next best methods may be gathered.

I may apparently have dwelt too much on scientific matters but I have done so from the strong conviction that only in this way can the best practice be carried out, that only by looking to nature, by taking nature as we find it, & by acting to the best of our ability up to the teachings of nature can the best results be obtained.