An Experimental Inquiry into the Influence of Certain Factors in the Causation of Peritonitis

by

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The following Thesis is the outcome of a wish, which I have had for some time, to make a series of experiments in order to throw some light upon the deficiency of our knowledge of the etiological relations to pirituris of certain pyogenic microorganisms, animal secretions, and other substances. In January 1888 my attention was painfully directed to this question by the death of my brother-in-law Lieutenant Colonel G.A. from perforative pirituris. It was at that time that I felt acutely how little certain was known regarding the etiology of this disease, and I resolved to attempt to solve some of the many problems connected with the 'Caution of Pirituris' in the earliest possible period. Fortunately I had not long to wait for in the summer session 1888 the Senate of Academia, on my application, granted me the sum of £40 from the Dickson Travelling Scholarship Fund in order to assist me in investigating the 'Etiology of Septic Pirituris' in a German University. I chose the University of Göttingen and have conducted this research in the Pathological Institute under Professor Dett. I wish to take this opportunity of thanking most heartily those who have rendered me assistance in carrying out this series of experiments. To Prof. Dett I owe more than I can well express.
He placed a room in his Institute entirely at my disposal, allowed me full liberty to make what use I wished of his valuable library and apparatus, and gave me his opinion on several experimental procedures and on the conditions found at the post-mortem examinations. I am also deeply indebted to him for much kind assistance in reading the literature (chiefly German) of the subject.

To my friend Mr. J. F. Pride, Mr. Edin, I owe my warmest thanks for his everready help in the performance of the experiments. I desire also to thank Prof. Wolfflinrgel for much instruction in bacteriological methods and for cultures of micro-organisms, Prof. Rosenbach for cultures of the lyssenic organisms and Mr. W. C. Sprague, Mr. Edin, for assistance in the carrying out of some of the experiments after the termination of the Winter Session when Mr. Pride was absent from Gottingen. I beg in conclusion to express my thanks to the Senate Academicians for awarding me the grant from the Dickson Travelling Fund without which I should have been unable to prosecute this research.

For one thing a word of apology is perhaps necessary. Have used the metric system in all my experiments and have not employed English systems in the writing of
In this Thesis, I have done so intentionally for any one wishing to compare the results described in this research with those of former experimenters. Ziegler, Corning, and Pawlowsky, will find that in their works the same system is employed and that therefore comparison is rendered easier. A full list of Authors quoted will be found at the end of the Thesis.

In investigating any disease, we must endeavour before we can pretend to treat upon scientific principles a patient suffering from the malady in question to obtain a perfect understanding of the etiology of the disease. It is not sufficient that we should have a sound knowledge of the pathological history and clinical symptoms. We require to understand the causes which have originated the disease. Concerning many diseases, our knowledge of their etiology is very slender, of others as for example, Peritonitis, it is extremely confused. The object of the present research is to endeavour to unravel some of the confusion which surrounds the etiology of this very lethal disease. It has been truly
said that Pathology stands on a tripod. It supports being Pathological Anatomy, Clinical Observation, and Experimental Research. As regards Peritonitis, I think I may venture to assert that concerning the two former our knowledge is fairly complete and satisfactory. It is however to Experimental Research that one must turn in order to gather new information. It will therefore be a matter of surprise to us as we acquainted with the subject that I should have chosen this \textit{direction}, in which to pursue my investigations. In the following pages I intend to bring forward a short account of 150 experiments carried out on animals, and to draw as briefly as the exigencies of the case demand, to the best of my ability, the deductions therefrom having reference especially to the \textit{Surgical aspect of the Treatment of Peritonitis.}

While I have no intention to enter at all fully into the Pathological Anatomy and Clinical Symptoms of Peritonitis, I must with reference to what is to follow later to make a few remarks on these subjects. Acute Peritonitis is certainly one of the
most frequent and most fatal of all diseases to which the human body is liable. Thus, according to Prof von Graef, out of a total of 8,421 consecutive post-mortem examinations conducted in the Pathological theatre of the Charité Hospital in Berlin, in more than 800, i.e., in nearly 10 per cent, peritonitis was the cause of death. Still larger is the percentage given by the late Dr. Hilton Fagge who, marking the year 1873 at random, found that out of 4,384 post-mortem examinations performed at Guy's Hospital in that year 52, or 12 per cent of all the deaths were due to acute peritonitis. Taking the lower of these two percentages, we find it stated on high authority that one out of every ten patients who died in one of the most important hospitals in Europe peritonitis was the cause of death. A fact which calls upon us urgently to do all that we can to arrive at a better understanding of the causation of this too frequently avoidable disease, and which surely justifies the expenditure of even a large amount of animal life in the attempt to elucidate the history of the condition. It was this fact...
coupled with the assurance of those whose opinion I valued that no useful work on the subject could be done without a long series of well conducted experiments on animals, which forced me to the conclusion that very many experiments must be carried out. For every new result must be made certain, every disputed point must be settled; firstly, by repeating every important experiment at least three times in cases in which all the results agree, and more frequently where there exists any doubt as to the result. Therefore it will be seen that in this series of experiments several are described together being confirmatory the one of the other.

Inflammation of the peritoneal membrane is in the overwhelming majority of cases not a primary disease but is caused by diseases or inflammation of organs covered by peritoneum. I would here emphasise what I am sure is a fact that almost invariably peritonitis is at first a localised inflammation, though undeniably it may spread withondrous rapidity of the many organs inflammation.
or disease of which extending to the serous membrane may cause peritonitis, in
the foremost rank is the alimentary canal; next in order the female
genital tract.
Inflammatory processes in the peritoneal membrane arise as a rule from
a microbic origin, and it is this fact that explains what the older
pathologists were accustomed to regard as the predisposition of the peritoneal
serous membrane to the acute forms of inflammation. For whereas in
the pericardium and pleura inflammation is as a rule sero-fibrinous
that is non microbic, in the peri-
toenum it is usually purulent
that is microbic in origin.
Typical exudative peritonitis is certainly
the purulent form, though of course
sero-fibrinous exudations are not rare.
In the case of microbic peritonitis the
fibrin exuded which, in the non microbic
form is so apt to form permanent
adhesions, is rapidly liquefied by
the peptic and action of the pyogenic
microbes. Upon the kind of micro-
organism prevailing in the peritoneal
cavity depends the rapidity of the
Spread of the inflammation; but in microbic peritonitis it may be taken as a general rule that the longer the inflammation has existed the more purulent it will be.

All clinical observers are agreed that peritonitis is frequently the cause of death in Bright's disease. Probably the blood change has produced a predisposition to inflammation, that is a lowering of vitality, thus rendering the membrane more susceptible to the action of the exoters of inflammation — the pyogenic organisms. Also I venture to suggest that the deteriorated blood is less able to destroy the organisms which have in some way or other obtained entrance into the blood stream. It is worthy of note that the inflammation of the peritoneum in Bright's disease is nearly always purulent, that is microbic; and I think that few will doubt especially after reading the account of some of the experiments which are described further on, that the exoters of inflammation are conveyed to the peritoneal membrane by the blood.
In connection with the above I may draw attention to the fact that suppurative inflammation of another serous membrane, the pericardium, is a not infrequent cause of death in Bright's disease. I have drawn attention to the pericardium in Bright's disease for, especially if ascites be present, it represents the "abnormal peritoneum" of which more hereafter.

Of the many other varieties of peritonitis which have been described I do not intend to treat e.g. aseptic purulent and haemorrhagic. I agree with Gronvold and Hilton Fazge in being very sceptical about cold as a cause of peritonitis. I have on this subject performed only a few experiments as they coincide entirely with Gronvold's more numerous experiments on this subject.

Idiopathic peritonitis, if it exist at all is exceedingly rare and in my opinion should never be diagnosed. Hilton Fazge remarks that at Guy's Hospital only two cases of acute peritonitis are recorded in 20 years, which could not be attributed either to renal disease, or to extension from a viscus. That is in only two cases could no cause be found.

Foetal gas and intestinal contents
may be so quickly absorbed that they may cause death from septic intoxication before inflammation can have time to occur. Tubercular peritonitis is strictly speaking a septic inflammation but it presents so strikingly different a pathological cause from its chronicity to other forms, caused by saprophytic microbes, that I have not attempted to discuss it in this Thesis. I wish to draw attention however to the fact first mentioned by Lusson in 1883 of the frequent dependence of tubercular peritonitis on cirrhosis of the liver, for it bears out the fact which will be continually referred to in the following pages, that micro-organisms are especially prone to attack, and to flourish best in parts whose vitality is depressed, where there is in fact a "locus minoris resistenciae."

Before leaving this part of the subject I must briefly refer to the fate of foreign substances introduced into the peritoneal cavity. Much of course depends upon the amount in which they are introduced, upon the size of the particles of which they consist as in the case of fluid,
but far more upon whether they are putres-
cline or not, and whether they are introduced
associated with, or free from, microorganisms.
As a general rule it may be stated that
a moderate amount of a substance whose
particles do not exceed the dimensions of a
red blood corpuscle is absorbed rapidly.
Larger particles unless capable of serving
as a food ground for organisms (if any
are present) are encapsulated. Putrescible
substances, free from organisms, are absorbed
or encapsulated, but putrescible substances
if in considerable quantity and accompanied
by pathogenic organisms, cause death
by septic intoxication or by septic peritonitis.

The important question now arises. How is
a fluid such as blood introduced into
the peritoneal cavity absorbed, and by what
route does it enter into the bloodstream?
I shall be able to bring forward evidence
which will show I think, that it is absorbed
chiefly by means of the pumping action
of the diaphragm, whose stomach lead by
lymphatic vessels into the glands of the
anterior mediastinum, whence it is carried into
the thoracic duct, and thus gains entrance
into the general blood stream. This
latter however is merely conjecture on my
part for flares not been able to trace blood and coloured particles beyond the glands of the anterior mediastinum. Of this I am certain that blood introduced into the healthy peritoneum of a rabbit in a fluid state is absorbed to the extent of 15 c.c. within 4 days and that it leaves behind no trace of its presence, always provided that no organisms have obtained entrance with the blood into the peritoneal cavity.

We have now to consider the present state of our knowledge regarding pyogenic micro-organisms and their action on the tissues. It would carry me far beyond the purpose of this paper were I to attempt to enter into all the questions concerning this interesting but complicated subject. Fortunately this is not necessary, so I feel that I could add very little of importance to the masterly survey of the subject contained in the lectures on suppuration and septic diseases delivered by Mr. Watson Cleeve only a year ago. Still there are some points which have so very distinctly a bearing upon the subject with which I have to deal, that I cannot refrain from mentioning them.
On few subjects in the domain of Pathology at the present day is there more dispute than on that of the effect of pyogenic microorganisms on the tissues of the human body. Whilst some regard microorganisms as the potent exciters of suppuration and imagine that if a few stray microbes gain entrance to any wound suppuration must inevitably occur; others again regard them as of no import whatever and consider their presence in pus as an accidental occurrence. But both these extreme parties are wrong. I venture to think I shall be able conclusively to prove in connection with this subject I would wish to quote from the latest text-book on Pathology published only a few days ago, as showing how great a change has been manifesting itself in the minds of Pathologists during the last few years on this regarding the action of pyogenic microbes. Prof. Hamilton says, the matter is still sub judice. It is certainly very questionable whether the micrococci found in abscesses are the invariable cause of the inflammatory effusion suppuration, so whether their presence is in any way more than a mere coincidence
One chief object of this paper is to draw attention to the fact that in the consideration of the action of pyogenic micro-organisms, the condition of the soil on which they act if one may use the term, is of prime importance.

It was Billroth who first drew attention to the fact that micro-cocci are the prevailing organisms found in pus from acute abscesses. But it is to Eggert, Rosenbach, Passat, and others that our exact knowledge of the pyogenic organisms is due. It would serve no useful purpose were I to endeavour to enumerate all the microbes which have been by various writers credited with pyogenic properties. Suffice it to say that in the vast majority of cases the organisms found in acute pus are micrococci; rarely but still undoubtedly, as we find bacilli alone. I have experimented with but few organisms viz.

*Staphylococcus pyogenes aureus*

*Klebsiella pyogenes*

*Bacillus pyogenes*

And a peculiar bacillus found in the feces of rabbits.

Zuckermann has tabulated 495 cases of abscesses the pus of which was
examined by well known observers. Rosenbach, Oster, Pascoet, Hoffa, Tricomi, and others and finds that

Staphylococci are present alone in 71 percent. Streptococci: 16 percent.

And the two above combined in 5 1/2 percent of all cases.

The most frequently present pyogenic organism is certainly the Staphylococcus pyogenes aureus, next to it in frequency, though far behind it, comes Staphylococcus pyogenes albus, and then Streptococcus pyogenes.

Staphylococcus pyogenes albus is very closely related to aureus, from which it is chiefly distinguished by the fact that it forms no pigment. According to Rosenbach, it is less virulent than aureus, whilst Watson Cheyne inclines to regard it as rather more virulent.

The similarity of the two, however, is so close that I have not thought it necessary to make any experiments with albus, but have taken the far more commonplace present aureus as the type of the staphylococci. Streptococcus pyogenes present in 16 percent of cases.
alone, and in 5½ per cent in conjunction with *Staphylococcus*, I have taken as the type of the chain cocci. I have also undertaken two experiments with bacillus *pyogenes*, the peculiar bacillus of green-blue pus, as a type of the rare class of pyogenic bacilli.

It may be well to describe the above-mentioned organisms somewhat more in detail.

*Staphylococcus pyogenes* aureus occurs as a rule in little heaps whence the name given them by Lister. The individual organisms are never found arranged as are the streptococci like beads in a chain. They grow at ordinary room temperature, but flourish best and grow most rapidly at a temperature approaching that of the human body 30-37.5°C. Although in some organisms as indeed in all other forms of micrococci, spore formation has never yet been observed, they possess a wonderful power of resistance to unfavourable external conditions. Whilst several factors influence their action especially the age of the culture, their food, ground, the temperature at which they have been reared, and
the presence or absence of oxygen, they can
bear 16 days drying on a cover glass
and still reproduce their species
apparently unharmed. When brought
again under conditions favourable
to their growth and development
Passet states that a colony may
be exposed to a temperature of 99 C
for 4 hours without being entirely de-
stroyed, and Dubreuil has frozen
the organisms for 24 hours several
times and found that when thawed,
they resume their functions apparently
unscathed for their exposure to
so low a temperature. Although
they flourish best in the presence of
sufficient oxygen, they have no
special need for it. They seem to
almost the specific organisms of
acute supplicative periostitis (oste-itis, eczema of some authors) in which disease
they may be associated with other or-
organisms, especially albus. A gelat-
ine culture keeps fresh and capable
of reproduction for 12 months. In
nature they are found chiefly
on the surface of the skin especially
where sweat glands are in abundance, as in the axilla,
also in the digestive and respiratory tracts; in 3% water (Pezzato) in earth (Dübber) in air (C. Fränkel), in the dust under the finger nails (Bockhardt), and in the sputum (Brioli). I have confirmed this last. They grow readily in gelatine peptonising the medium as also albumin very powerfully, and they form ammonia as their chief product of decomposition (Brieger).

They grew excellently on agar agar. In my experiments I used almost exclusively agar cultures. Injection of these organisms into rabbits gives various results dependent on many factors of which almost important one is the dose. Large amounts, say 12 oz. rubbed into the skin produced no result. This is distinctly at variance with that which one would have expected from the experiment of Garre. Dübber also obtained no result when he rubbed these organisms into the skin of rabbits. Subcutaneous injections of small quantities is as a rule unproductive of abscess formation. Larger quantities generally produce localised abscesses.
which have little tendency to give rise to metastatic formations. On injury to bone in young animals, and subsequent injection of Staph. aureus into the blood stream, suppuration in the injured parts is often set up; in older rabbits seldom. Intravenous injection of small quantities provided there be no lumps in the suspension of organisms in water gives rise to no effect unless there be a local and depressed part of the body where the organisms can settle and grow before they are destroyed in the blood stream. If there happen to be larger masses of cocci of some substance rich in cocci, as in Rabbit's experiments, abscesses in the liver, and sometimes in other organs may be produced but these in my opinion are always embolic.

The staphylococcus pyogenes is the most frequently occurring member of the group of chain cocci. Indeed it has been maintained by many and apparently with reason, that it is the only chain coccus, and is identical with the staphylococcus.
of erysipelas which is associated with
the name of Flechlein. With this
question however I have here nothing
to do. Still I have carefully avoided
taking staphylococci from cases of erys-
pelas for any experiments. I have
obtained only staphylococci from cases
of peritonitis, and have employed
these in my cases exclusively.
These micro-organisms grow in
chains something like beads in a
rosary. They grow but very slowly
at room temperature but much rapidly
between 30 and 37.5°C. Unlike the
staphylococci they do not liquefy that
is peptosine gelatine. It may be
mentioned that peptosine power
and pus producing capacity seem to
stand in some relationship the one
to the other. Rosenbach maintains
that staphylococci can peptosine albumen
in vacuum. Staphylococci are according
to all authors extremely unsuitable
for experiments on animals.
Prof. Rosenbach informs me that
he has frequently injected large
quantities of staphylococci into the
knee joints of rabbits without
harm resulting. Although staph-

coccii are frequently present in peritonitis in man. I have made comparatively few experiments with them on account of the fact that they are much less virulent to rabbits than are staphylococci, although I have little doubt that as regards man they are the more dangerous organism. Still emphatically repudiate the statement made by some that these organisms are non-pathogenic to the lower animals. The results of my experiments show I think conclusively that under conditions favourable to them they can occasion fatal inflammation. Prof. Sir F. Westropp, and Dr. B. W. J. Wright have obtained ulcerative endocarditis with streptococci as with staphylococci in their experiments on rabbits. Dr. Storchfield also bears witness to the fact that streptococci can cause in the rabbit ulcerative endocarditis. Streptococci are normally present in the secretion of the vagina, frequently also in the lacteal (Brunner, Döders, Kuliscioff). Certainiy they are present in many of the inflammations which are termed diphtheritic; perhaps they are the organisms of diphtheria.
It was both who first drew attention to them in 1873 though he did not name them. He described cocci as always occurring in chain forms in peridental inflammations and suppurations. It only remains to be added that they are par excellence the organisms of pyaemia, being present in more than 80 per cent of all cases (Rosenthal).

It may not be out of place here briefly to compare these two foregoing kinds of cocci with which I have chiefly worked in these experiments. Staphylococcus is essentially the micro-organism of acute localised inflammation, thus for example in acute abscess, acute supplicative periostitis, carbuncle, boil, etc. it is almost always found. It has little tendency to form metastatic abscesses, and is rarely the only organism present in pyaemia.

Very different is the case with streptococcus which gives rise to inflammations which tend to spread by the surface and have little tendency (infinitely less than staphylococcus) to liquefy the tissues. Progressive phlegmon-
Our inflammations of serous and squamous membranes are those in which streptococci are almost invariably found. Streptococcus is far more apt to give rise to a general infection of the system and to form metastatic abscesses, tending as it does to spread by the lymphatics causing lymphangitis and lymphadenitis.

Of the bacilli which are associated with the induction of pus the Bacillus pyogenes foetides is probably the most frequently met with. I have never seen with this organism, and have therefore carried out the two experiments which I thought necessary for completeness to make with that organism which gives rise to greenish-blue pus. The Bacillus pyogenes. This is, I must confess not generally regarded as a pyogenic organism for it seems to produce scarcely any disturbance in the healing of the wound when it appears in surgical practice. Still I have caused suppuration in the rabbit with this organism, and in regarding it as pyogenic I have the support
of two observers who have recently investigated its qualities more closely (Baw- 
slowthy and Letterhize). Excessively rare in Scotland, it is by no means infrequent in Germany. In 6 years of constant attendance on Surgical practice in the Edinburgh Royal Infirmary I never once saw this green-blue pus, whereas in the small Göttingen University Hospital I saw it 3 times in 2 months. I noticed in my experiments a curious fact that the pus, which was caused by the subcutaneous injection of this organism was nearly colourless, but rapidly developed its peculiar colour on exposure to air i.e. when the abscess was opened.

The only other organisms with which I experimented was a short bacillus found in the feces of rabbits (invariably). It was this organism which was chiefly found in the organs of the animals who died from peritonitis caused by the introduction of fecal matter into the peritoneal cavity.

Gravewy was the first to draw attention to this microbe which he described without naming it.
Pastovskij has called it "Baillus peritoneitis ex intestini curculi". It is a short thick bacillus showing no innate movement. On gelatine plates cultures there are seen greyish-white slimy colonies. It does not liquefy gelatine. I found that it possessed pyogenic qualities when injected subcutaneously in large quantity.

Having thus described some of the characteristics of typical pus-producing organisms I think it will be well briefly to dwell on a subject which for some years has been one of the most keenly debated questions in Pathology, viz. the relation of pyogenic organisms to suppuration and the possibility of producing suppuration without micro-organisms. That the so-called "pyogenic organisms" are found in the vast majority of acute abscesses no one can deny. But the question with which we must first concern ourselves is this: Can suppuration occur without the presence of micro-organisms? In other words, was Hunter right when he laid down the principle "No suppuration without..."
Whilst I fully agree that the practical Surgeon may rightly consider that every case of suppuration with which he meets is the result of the action of micro-organisms, for I have never failed to find them in a single case out of more than 120 which I have examined acute pus for micro-organisms; I think that it is proved absolutely that suppuration can, in the hands of the experienced, be artificially produced, without the aid of micro-organisms.

The discovery of the nature of suppuration by cholera in 1867 (though Addis had virtually discovered it some years previously), viz. that pus cells are in fact derived from leucocytes which have penetrated the vessel walls, was a revelation in pathology. For us it is interesting to know how near to the truth that great Surgical Genius, John Hunter came 3/4 of a century before Whim, marvelled as he was by the improvements in microscopic apparatus which made Cholorearns discovery possible, he maintained that pus is formed from some change, decomposition, or separation, of the blood which that...
undergoes in its passage out of the vessel. Columbus's theory that all pus corpuscles proceed from the blood; the connective tissue elements playing in typical connective tissue suppuration only a passive part, as far as suppuration in origin and progress is concerned, is now I think, almost universally accepted by pathologists. This being the case, there is surely no inherent improbability that certain agents should be able to cause this separation from the blood. The majority, perhaps of observers incline to the opinion that suppuration can only be produced by the action of micro-organisms; but I think the following experiments of Leber, and Gravely, suffice convincingly to the opposite conclusion. Leber aseptically introduced minute fragments of copper into the anterior chamber of the rabbit's eye, and from a large number of observations he found that when they were in contact with the iris they produced a purulent inflammation. When they stuck in the lens substance, or floated free in the anterior chamber,
they never caused pus formation. The pus produced in those cases where the metallic particles were in contact with the iris was always examined for micro-organisms both microscopically and by the far more important and trustworthy culture test, but never were any microbes found. Let it be noted however that this suppuration was only in the neighbourhood of the injured spot in the iris, and that unlike suppuration caused by micro-organisms, it exhibited no tendency whatever to spread from this point into the surrounding tissues, always remaining strictly localised. Copper particles aseptically introduced into the vitreous likewise cause a limited localised suppuration. The same is seen in the case of metallic mercury which introduced into the anterior chamber or vitreous, always causes suppuration. Later frankly admits that all metals do not act in the same way.
and cites de Winker's so called drainage
of the eye as a proof that gold
wire does not cause suppuration.
Still I venture to think that the
numerous and careful experiments
with copper and mercury which
have been carried out by Leber, who
is undoubtedly one of the highest
living authorities on the pathology
of the eye, prove amply that
in the eye at least suppuration
can occur without micro-organisms.
Still more convincing to my mind
are the experiments of Grauvink and
de Bary who worked with turpentine
as their irritant. They come to the
conclusion that whilst turpentine
does not cause suppuration in rabbits,
in dogs it acts as a pus produc-
ing agent par excellence. After
proving that turpentine is a strong
antiseptic, as strong as carbolic acid
and corrosive sublimate in their
usual surgical solutions, they
then conclude that it is a
matter of no consequence whether
organisms are mixed with tur-
entine or no, for if present they
are immediately killed.
I take the following experiments described by Frank and de Bary. Four healthy dogs received each 3 subcutaneous injections of turpentine in various parts of the body. Of these 12 injections 6 were pure turpentine, 6 contained Staphylococcus aureus. In all cases the result was the same; suppuration occurred, and the pus proved free from organisms by the fact that 36 agar tubes inoculated with pus i.e. 3 tubes for each abscess produced no trace of microbes. These experiments taken from a number of similar ones, prove that suppuration can occur without micro-organisms. Rosebach and Kshetema reference in a recently published paper entirely support this statement. The next question that comes up for consideration is this: Can pyogenic organisms cause suppuration? The answer must unhesitatingly be given in the affirmative. The most remarkable experi-
ment in this connection is the off
gusted one of Garre, who rubbed
into the skin of his left forearm
a gelatinous culture of streptoco-
coccus auris, with the result
that on the 4th day the place
of inoculation showed a large
carbolure which took weeks to
heal, and caused a number
of permanent scars.
I and my friend Mr Bradie
have repeated this experiment
on our own persons 6 times
i.e. 3 times each without at any
time causing more than a slight
erythema, which lasted for 24 hours,
and then passed away, without
causing any pus formation. I
cannot say how far the erythema
was due to the vigorous rubbing
in of the organisms with a steril-
ised glass rod, and how far to
the influence of the organisms them-
selves. I incline to think that
the rubbing had more to do
with it than the organisms.
Still I must admit that
we did not repeat Garre's experi-
ment exactly. For whereas
he rubbed in a whole culture, gelatin and all, we rubbed in organisms alone without the gelatin in which they had grown. At the time I considered this an unimportant omission but from what will appear subsequently I think it possible that in this way exist the cause for the difference in result. The reason why we did not use the gelatin culture was that I made all my cultures in agar for the weather being very cold the gelatin cultures did not grow at the ordinary room temperature, and therefore I cultivated everything at a temperature of 30 to 37.5° C. which would have liquefied gelatin. I must here mention that Lüttorf has repeated Garre's experiment in rabbits without obtaining any suppuration or inflammation what ever. Therefore I think that we require more positive evidence on this point before we assume it as proved, that pyogenic organisms can, acting on the
healthy unbroken skin, cause suppuration.

Substantially injected in healthy rabbits in large quantities staphylococcus aureus always causes suppuration; in small quantities never, unless in connection with some irritant substance which lowers the vitality of the affected part.

To illustrate this point I will here record a few experiments which I made on this subject.

Experiments. A suspension of staphylococcus aureus in water consisting of 5-10 of organisms in 10 c.c. of sterilised distilled water was prepared and of this 1 c.c. injected under the skin of a healthy rabbit produced no suppuration, whereas 5 c.c. caused a localised acute abscess in which staphylococcus aureus was found in abundance.

Again, in order to prove the effect of derivalising the soil previous to the injection of pyogenic organisms, I injected 4 c.c. under the
Skin of a rabbit at a spot where I had previously 20 minutes of turpentine had been injected and the tissue was there pre-irritated and inflamed. An enormous abscess was caused which resulted in the death of the animal. A control experiment in which distilled water was injected I hour previously and then 0.2 cc. i.e. double the number of organisms gave no result.

These experiments I think clearly prove that pyogenic organisms in healthy tissues in small amounts are not productive of harm, but that the case is vastly different when they gain access to tissues, the virality of which is lowered. To show that these experiments (which were carried out on rabbits) that the suppuration was not caused by the turpentine alone, but by the micro-organisms acting on the tissues, prepared for their reception by the turpentine, I performed the following experiment:

60 drops of turpentine were mixed with 6 one of staphylococci and
the whole injected under the skin of a rabbit. Inflammation and a necrosis of tissue took place but no suppuration. The animal recovered. Thus in this case the turpentine acted as an antiseptic and killed the organisms which we injected with it, then it manifested its irritant and caustic qualities and caused necrosis of the tissue. In this case no Staphylococci were found in the affected area, whereas in the former experiment the organisms were introduced into an area desensitised by the action of turpentine, which by the time they reached the part had been absorbed. In the latter case the organisms were mixed with the turpentine before injection, so they were destroyed before introduction into the tissues. These experiments bear out the contention of Frozen that turpentine does not cause suppuration in rabbits (although in dogs it acts as a pus producing agent par excellence).
That the medium in which organisms are introduced has an effect upon their action is shown by the following experiments.

Experiment. One cc of Staphylococci in 10 c.c. of distilled water produced on the left side of a rabbit's back, when injected subcutaneously, no result. Whereas one cc in 10 c.c. of liquified gelatine (10 percent meat-peptone gelatine of Koch) produced on the right side an abscess as large as an acorn. This also is in agreement with results of Snell.

From the above mentioned experiments, I deduce the following principles:

1. Staphylococci aureus, the commonest pyogenic organism, produces pus when introduced (1) in large quantity or in some medium which hinders the absorption of the organisms
2. Into a debilitated tissue or as we shall see afterwards into the blood of an animal in which there is a focus of organisms resistant.

With the former of these I am not so much concerned as
with the latter, still as confirmation of the statement I may relate the following experiments.

Experiments. ⅔ oz of staphylococcus aureus in 5 c.c. of water, injected simultaneously into two rabbits, produced no effect; whereas 12 oz in the same quantity of water caused large abscess formation. The cause of this is probably not far to seek. It has been calculated that a staphylococcus can reproduce itself at body temperature in ½ hour (C. Fränkel). Now if a quantity is introduced of which more than one-half can be absorbed in ½ hour, the healthy tissues get the victim, absorb the cocci by the lymphatics, and thence carry them into the general blood stream where they are destroyed by the white blood corpuscles, or carried to various organs especially perhaps the spleen and bone marrow there to be destroyed. It is now I think proved that organisms are not excreted by the kidneys and bowel as was formerly held. The experiments of
Wyss-Kovitsch and others have shown that bacteria both saprophytic and pathogenic, introduced into the healthy body are not eliminated by any of the secretions except where a diseased condition of the eliminatory organ exists. My own observations conform entirely with the above. I have never found pyogenic organisms in the urine taken from the pelvis of the kidney and bladder in 9 cases examined, although the kidneys were swarming with them. They are chiefly found in the kidney solely because of the rich capillary network present in these organs. Prof. Rosenbach was formerly held the opinion that organisms were excreted by the urine in forms one that he is now convinced that the theory is erroneous.

To return to our organisms.

If there were the number present in the healthy subjacent tissue does not exceed that which can be rapidly drained by the abscesses, no suppuration will occur. But the matter is
not quite so simple as it appears on the surface. Another factor enters upon the scene in the shape of waste products. Staphylococci and others form waste products very rapidly of which the chief is ammonia, and these acting as irritants may give rise to a local depression of tissue which thus affords a suitable nidus for the organisms. Therefore we must say that a certain time must elapse before all or almost all the organisms are absorbed, and this seems to be about 1 hour.

Regarding the influence of the medium in which the organisms are introduced into the tissues I shall have much to say when I describe the experiments on the peritoneum that I have made until then, the consideration of this subject.

Now we come to the second division of the subject, the "locus minoris resistance", the most important point in the pathology of its formation, and one which has been far too much overlooked.
In order to obtain a secure minor incision resistance to test its effect I performed the following experiments.

Experiments. Having ascertained that I could, without fear of producing abscess formation, inject 5 c.c. of a suspension of 1 oz. of Staphylococcus aureus in 5 c.c. of water by injecting it under the skin of my abdomen I used this small amount in the following experiments.

Taking the loosest skin covering the body i.e. that of the scrotum I pulled a small fold about the size of the terminal phalanges of my little finger through an indiarubber ring such constructed it only moderately tightly. I allowed this to remain on for 2 hours. At the end of that time it was intensely congested being purple in colour. Into this congested space I injected 5 c.c. of the Staphylococcus fluid, and allowed the condensation agent to remain on 2 hours longer. The result was a small localised abscess the size of a cherry which being
opened by a Graefe knife was found to contain 9 or 10 drops of pus swimming
with staphylococcus aureus. On the opposite side of the serotum I injected
\textit{a c.c.} of the same fluid without interfering in any other way with
the tissue. Abscess formation resulted. Dr. Ritchie kindly repeated
the experiment on his own
person (as I also had done) and
obtained exactly the same result.
In these experiments there are
two factors which would aid the
pyogenic organisms. In the first
place there is the undoubted deposi-
tion of tissue, and in the second
the fact that absorption would
certainly be hindered, if not absolute-
ly prevented, by the constriction
of fluid which, it will be remem-
bered, was allowed to remain on
for ½ hour, after the injection of
the organisms.
I will not dwell here further on this
point. Its importance will
be manifested time after time
in the intraperitoneal experi-
ments. Yet with regard to intra-
venous injection the experiments
of Boe and Wysokowski on the artificial production of ulcerative enteritis and the recent serous congestion theory, and have no distinct bearing on my subject, that I must here briefly refer to them.

When small numbers of pyogenic cocci are injected into the blood stream care being taken that no masses of organisms are present in the fluid, which should have the appearance of a uniform slightly turbid liquid, they cause an ill effect, a healthy animal. They are killed in the blood in the course of a very few hours at the latest. By what means they are killed in the blood is not quite certain. Metchnikoff's phagocytic theory is certainly the best working hypothesis we at present possess. It is interesting to note that this theory was virtually pronounced by Lister many years ago, when he maintained, that leukocytes were the active agents concerned in the removal of foreign substances.
from the tissues, and that blood acted like a living tissue in resisting the development of micro-organisms. If however, in the animal whose circulation they are injected there exists a "locus minoris resistentiae", they can produce a very intense and rapid inflammation, leading to results dependent on the importance of the part concerned to the animal economy.

Otte and Wysko konsteh injured the valves of the heart in rabbits by rubbing them with a metal rod introduced down the cut carotid artery and thus lowered their virility by removing their endocardial covering. They then injected Staphylococcus and streptococci into the blood stream and invariably caused thereby ulcerative endocarditis. Albert has since declared that preliminary injury to the valves is not necessary but that the injection of pyogenic organisms into the blood stream is of itself sufficient to cause endocarditis. His mode of procedure was as follows. He made
Potato cultures of the organisms and then scraping off the cocci from the surface, taking good care to obtain little masses of potatoes at the same time, he injected them into the blood stream. Was I protest against this being regarded as any proof that the locus minoris resistentiae theory is unnecessary. For the injection into the blood stream of large particles as potato crumbs which cannot pass the capillaries alters the case entirely. In Stitt and Wyman's experiments the cocci suspended in water in a state of fine division passed the normal capillaries without harm, whereas they attacked the debilitated tissue of the injured vessels. In Rebert's experiments the crumbs of potatoes loaded with organisms were caught in the smaller vessels, and gave rise to the train of symptoms which septic emboli cause. I have little doubt but that a number of potatoes swarming with staphylococci firmly stuck between the chlorella tendiniae could give rise especially in an
animal the subject (as Ribbert's animals admittedly were) of multiple septic emboli. But I cannot see that the experiments of Ribbert diminish in any way the strength of the position of those who hold the theory that a locus minoris resistance is necessary to cause death, in cases where only a small amount of pyogenic organisms are injected into the bloodstream. I may here mention that Orth and Wegorkowitch found in their cases endocarditis as well as endocarditis, and in some cases, in which the valves had purposely not been injured but only the endothelium of the aorta, endocarditis alone. In Ribbert's experiments no endocarditis ever was caused owing to the fact that the endothelium of the aorta was uninjured, and no opportunity existed in the smooth interior of the aorta for any organisms to settle. The case was thus very different from that of the valves with their network of chordae tendineae.
and sinuses of Valsalva, in which a little mass of potato and organisms might be fixed. It must always be remembered as had been mentioned before, that in Hilbert's cases, multiple septic emboli were admittedly present, owing to the fact that some of the masses of organism-containing potato scrapings had abstracted various systemic arteries. Fränkel and Sänger, Weichselbaum, and others have repeated Orth's and Wyczoski and Schütz's experiments and agree with them entirely in their contention. The reason I have drawn so much attention to Hilbert's experiments is that they have been used by Bauninger and others as strong evidence against the loco minoris resistentiae theory. Orth's, Wyczoski and Schütz, Fränkel and Sänger, Hilbert, Dübbert, and others have described multiple abscesses of the kidneys in these cases of artificially produced endocarditis and bacteriologists have therefore come to regard these abscesses as the result of the intravenous
injection of pyogenic organisms. I dissent in to from this view. I have on very many occasions, with virulent staphylococci, and streptococci, performed intravenous injection and have only once found multiple abscesses in the kidneys. My own view of the matter is this, that these abscesses are always septic, and are caused by the detachment of particles rich in organisms from the valves. These particles are then carried away in the blood stream, and stick in the capillary network of the kidneys where, acting as septic emboli, they cause abscesses. In Robert cases, where the particles (Plate) were especially numerous, the abscesses in the kidneys were particularly noticeable. The one case in which I obtained renal abscess was one in which to test this point I took my organisms from an agar culture and injected them directly into the blood stream, without taking the precaution of stirring them in a
test tube in sterilised water, with a glass rod, in order to break up the larger masses into their smallest possible particles. I therefore maintain that the introduction of pyogenic organisms into the bloodstream does not of itself cause multiple renal abscesses as most think. I need scarcely refer to clinical experience in this matter for I think that all clinicians will agree that pyogenic organisms may circulate in the bloodstream without giving rise to renal abscesses.

Large quantities of pyogenic organisms injected into the bloodstream seem to produce death.

Before we leave the subject of intravenous injections, I cannot help referring to Comrie's well-known experiments in which he set up a septic nephritis by ligaturing the renal arteries for a few hours and then injecting pyogenic cocci into the bloodstream. A result comparable to some
which I shall presently describe
in connection with the production
of peritonitis.

The last point before I leave this
part of the subject. One question
arises. Do pyogenic organisms
enter the bloodstream in health?
I think the balance of evidence lies
distinctly in favour of the contention
of Fother that bacteria are
constantly entering the organism
of men and of animals from
the alimentary and respiratory
tracks, but that, unless they
are both excessive in number,
and virulent and (which can
but rarely happen) they are quick-
ly destroyed in the blood. I would
add to the above as another very
important point that, so long
as they are unable to obtain
a nidus suitable for their
development, they are harmless,
but if they find such a nidus
as I shall presently show, they,
though present only in small
numbers, may act with fatal
effect.

That organisms such as Staphy-
Bozzi read the peritoneum in acute suppurative peritonitis by the blood stream in the vast majority of cases there can be no doubt. Rosenbach has proved this conclusively in cases in which he has found acute suppurative peritonitis in new born children. This of course could only have received the pyogenic coci from the maternal blood. The mother in such cases remained quite healthy though transmitting the organisms to the foetus.

In this connection I made the following experiment:

Experiment: A rabbit pregnant nearly at full term received an injection of 2 c.c. of a suspension of Staphylococcus aureus in water into the femoral vein. 7 minutes later beaker glass preparations taken from the blood of 3 different veins of the right ear showed the cocci present in small amount in 5 of the 6 examined. Abdominal section was then performed and the uterus being opened, the young were removed.
24 cover glass preparations were obtained from the blood of these foetuses, of these 24, 5 showed cocci present in small amounts. It is admittedly difficult to obtain organisms in the blood, and I think that these experiments clearly show that microbes can pass from the maternal into the foetal circulation, and this we doubt explains the etiology of many intra-uterine diseases variola for example. But in nature suppurative inflammations as acute suppurative periodontis frequently may generally arise without any preexisting wound by which the microbes could have gained access to the blood stream, and in these cases I think we are forced to the conclusion that they must obtain entrance by means of the alimentary or respiratory tracts. No one doubts that the lungs are able to take up and deposit in the bronchial lymphatic glands pigment from the inspired air. But if pigment can obtain
entrance into the interior of the gland why should not a bacterium do so likewise. Once in the bronchial glands the way into the blood stream is easy. Probably far more frequently but still harder to prove is the taking up of organisms by means of the digestive tract. Best by feeding rabbits with substances containing paracelsum found in certain cases that there were evidences of tuberculosis in several organs although he could discover no change whatever in the intestine. The conclusion seems clear enough, the tubercle bacilli were absorbed by the intestine and thus gained entrance into the general blood stream through the mesenteric glands and thoracic duct. So again how frequently as we find in children who die of tabes mesenterica no change whatever in the intestine whereas the mesenteric glands which are nearest the bowel i.e. furthest from the blood stream are almost always those which are the
most enlarged and contain the greater number of bacilli. In the same connection may be mentioned Kocher's experiments in which, after injury to bone, he produced acute supplicative periostitis (osteomyelitis) in dogs fed on a large amount of purplish food. He thinks that some of the numerous micro-organisms present in the intestine owing to the administration of this food were absorbed and carried to the seat of injury where, meeting with a place of lowered vitality, they were able to settle and develop. The last point to which I wish to direct attention is the acquired violence which if I may use the term, organisms in training seem to possess. In this I am told to find streptococci (Bunnn, Doléans, Kuliscioff) which certainly usually do not give rise to preretal inflammations, though present in large numbers. Still, let a careless obstetrician whose hands have been in contact with the secretion of preretal inflammation, thus
introduce a few virulent organisms (Staphylococci) into the genital passages, and in all probability pelvic fever will result. I can only explain this fact by saying that the Staphylococci are the offspring of organisms which have been actively producing virulent inflammation, and thus have inherited a like tendency from their forefathers. Whereas those in the healthy body have for generations been accustomed to vegetate without producing any effect, and thus have lost the power of producing inflammation. I have proved by experiment that an one of Staphylococci of the 1st generation is more effective than 2 one of the 7th generation. I ascribe this to the same cause namely, that the organism whose forefathers have been accustomed to live on gelatine or agar have lost much of their power to prey on the human organism.

Having thus briefly considered the points of Pathology and Bacteriology.
log which have a direct bearing upon the experiments shortly to be described. I think it well, in the outset, give a short résumé of the previous experimental work done on the subject of septic peritonitis.

In spite of the vast importance of this disease it has been very little studied from the experimental side. There are only 3 works on the subject which demand our attention.


2. A valuable essay by Prof. Grunk of Gießen in Virchow’s Archives 1878.

3. A paper by Dr. A.D. Pastorsky in the Centralblatt für Chirurgie 1887.

Besides the above which treat of peritonitis alone, I would wish to refer to other papers which though not written specially in regard to peritonitis have great importance in this connection. They are

Prof. Oyston’s Report upon Micro-organisms in Surgical Disease British Medical Journal 1881.

and the exhaustive work on the staphylococcus aureus entitled Biologische Spaltstoffe unter der Leitung von Dr. Anton Lieber 1886.

Taking the above in chronological order, Wegner in his important work in 1877 proved by numerous experiments

1. That healthy urine injected into the peritoneal cavity of a rabbit produces no bad effect being absorbed rapidly even when injected in large quantity.

2. That a rabbit is capable of absorbing a quantity of a non-contractile fluid (such as urine) injected into the peritoneal cavity equal to from 3 to 8 per cent. of the whole body weight of the animal, in one hour.

3. That a certain amount of rabbit bile (4 c.c.) may be injected into the peritoneal cavity of a rabbit without causing the death of the animal from peritonitis.
(4) That air can be forcibly blown into the abdomen of a rabbit without causing death.

(5) That milk may be injected into the abdomen of a rabbit without causing septic peritonitis.

(6) That the peritoneum has a wonderful power of healing its wounds by first intention and that accidents, gangrene, thrombosis, and phlegm, which are so common in wounds of joints (speaking as wounded 12 years ago) are as sequences of wounds of the peritoneum almost unheard of.

(7) That small quantities of pus-like fluids are absorbed by the healthy peritoneum before they have time to act.

I from my own results concur with these conclusions of Beecher in almost every particular.

We now come to Ogston's papers, the earlier of which appeared in 1881, the latter in 1883. These papers proved conclusively that pus from an acute abscess caused septicemia.
When injected under the skin, whereas pus from a cold abscess (containing as it does no cocci) never does, Osgood states that minimal amounts of pus introduced into the peritoneal cavity does not cause peritonitis. Thus minimal acute pus introduced into the peritoneal cavity does not cause inflammation.

Further we find this remarkable feature. Pus containing micrococci is resisted by animals if the dose be minute or if it be injected into the peritoneal cavity. With these statements of Osgood it will be seen that my own results are in complete accord. If one regards pus as a fluid containing micro-organisms (most likely staphylococci for the pus was taken from acute abscesses) it will be seen that many of my experiments resemble those of Osgood.

Confirmation of the above statements are experiments (quoted by Osgood in his Gulstonian lectures Medical News March 1883) of Heiberg who introduced small pieces ofreg.
tations from a case of ulcerative endocarditis, into the peritoneal cavity without causing peritonitis. No one will doubt that these vegetations were not "swarming with pyogenic organisms."

Libbert performed 3 experiments with guinea-pigs. He took a mass of Staphylococcus aureus, the size of a pea and shook it up in 10 c.c. of water. He injected 1 c.c. of the suspension of organisms in water into the peritoneal cavities of 3 guinea-pigs. 2 of these animals recovered perfectly, the third died 2 days later from other causes. No peritonitis was present at the post-mortem examination. The peritoneal membrane was the contrary appearance quite normal. In a fourth experiment performed by Libbert death resulted from the introduction of pyogenic organisms but not from general septic peritonitis. As this is quoted by Baumgarten as proving that "pyogenic organisms can cause septic peritonitis when introduced..."
into the abdominal cavity I will describe it somewhat fully.
A mass of staphylococcus around the site of a pea was introduced into the abdomen of a rabbit.
Death occurred on the 20th day.
Post mortem examination showed colon adherent to abdominal wall at the site of the incision wound.
All around this spot, for a space the size of the palm of the hand, the peritoneum was covered with thick creamy pus. An abscess in the abdominal wall between peritoneum and muscle extended to the groin on each side and upwards reached above the navel. Skin wound healed. The bowel loops in the neighbourhood of the wound are likewise covered with pus and adherent to one another.
The greater part of the bowel and of the peritoneum is intact. There is no change in the other organs. I have no desire to say that an enormous number of cocci introduced into the peritoneal cavity cannot cause
death for I have proved that they can do so. But still I maintain that this case with its escaped peritonial abscess and huge abscess of the abdominal wall with death on the 10th day does not prove what Baumgartner desires to deduce from it. My idea of the case is this that the abdominal abscess formed first beneath internally, but owing to the adhesion of the colon to the incision wound it became localised and was shut off from the general peritonial cavity. I cannot from my own results conceive that a large mass of staphylococci could remain in a rabbit's abdomen for 6 days before they caused death which in cases of intraperitoneal injection was never delayed beyond the 5th day. Baumgarten's large great shock in this case but one of all seven of the 3 former experiments of Lübert. Lübert's 3 cases with guine pig are strongly in agreement with my own results.
I now come to the consideration of the two most recent and absolutely contradictory accounts of experimental work on the subject of peritonitis, viz., those of Prof. Gravitzky and Dr. Pavlovsky.

The following is a short resume of Gravitzky's work:

He agrees with Wagner that serum, water, bile, normal salt solutions, can be absorbed from the peritoneum without bad results. Fluids which contain formed particles as milk, blood or pus, to take examples from the cervical fluids or water containing charcoal granules or starch corpuscles as also fluids containing bacteria and micrococci are absorbed by the peritoneum, provided always that the contained particles are no larger than red blood corpuscles.

Defibrinated blood injected into the abdomen of a rabbit together with ordinary air never produces, as regards experiments with pus producing cocci, Gravitzky says that the introduction into the peritoneal cavity of
Scleromyxitis is not a necessary preliminary to the setting up of peritonitis but that it does not of itself suffice to produce in the absence of other auxiliaries septic peritonitis.

Gram found that the peculiar odor to which rabbits' feces injected in a watery suspension are absorbed without harm. The boiled rabbits' feces introduced into the peritoneal cavity of rabbits and found that no inflammation was thus caused. But he justly remarks he does not lay much stress on this result as the intestinal fermentations were probably destroyed by the boiling as were also the organisms.

Most important is this statement: "The introduction of bacteria which cause suppuration into the healthy peritoneal cavity is without danger provided that they are suspended in a non-infecting fluid and that they are introduced in an
amount not exceeding the absorption capacity of the abdomen. A suppurative diarrhoea continues of water containing a number of staphylococci or streptococci sufficient to cause a distinct tinge in the water in which they are suspended, injected into the abdomen is unproductive of harm.

The introduction of pus producing organisms into the abnormal abdomen produces suppurative peritonitis when the abdomen contains a suppurating fluid which can act as a food solution for organisms. Thus if a considerable amount of brain is mixed in a mass of pyogenic cocci and injected into the peritoneal cavity, it serves as a food ground for the organisms and owing to the slowness of its absorption (compared with water mixture) it acts as a nidus where they can develop until in ever increasing number they aid by their waste products can attack the peritoneum and
set up acute peritonitis.

Caustic substances which kill or injure the tissue of the peritoneum prepare the ground for the introduction of pus-producing organisms so that these can settle at the injured spot and produce a general infection. Also the presence of an external wound discharging organisms into the abdomen will produce peritonitis.

In this connection, the presence of a suitable vehicle differs very much from the mere injection of a certain number of organisms for whereas in the former (the case of the rabbit) there is a definite number of organisms are injected into the peritoneal cavity once and for all; in the latter (the case of the rabbit) there is what Bichowsky has called a peritonemia. There is here a centre from which ever new organisms are being produced until their number is unlimited. For there is from this spot no absorption of organisms, and these and their waste products increase until the absorptive power of the peritoneum (in other parts than the
nulus is over-taxed, or the body unable to destroy the organisms as quickly as they are produced. The neighbouring viscera are then infected by contact with the phlegmonous surface as we so frequently see in ulcerative subcarditis. The opportunity for the spread of the inflammation is increased by the paralysis of the bowel which is infected, and more than all by the decreased activity of the diaphragm on account of the pain. Thus the pumping action which is so much avail in absorption is lost.

Gray says further says "On the ground of my experiments I maintain that the immediate cause of every peritonitis is the settling of the cocci in any position in the tissue of the serous membrane. The actual cause is either a wound in a state of phlegmonous inflammation which is in immediate relation to the peritonitis, or so great a collection of fluid that this does not follow the laws"
of normal absorption but stagnates in
the abdomen.
Cold alone cannot produce peritonitis
Grayson punctured the abdomen of
rabbits and then exposed them
to an ice cold draught. Increased
peristalsis alone resulted.
Cold was likewise ineffectual in produc-
ing peritonitis even when pus producing
cocci were present in the peritoneal
cavity (after injection).
In perforative peritonitis, according to
Grayson, the quantity of focal vitality
extravasated into the peritoneal cavity
is more important than the
quality. Also, the state of the tissues
must be taken into account.
Thus, a traumatic is always more favour-
able than an ulcerative lesion. For
in the one case, the tissue of the
bowel was previously healthy, in the
other it was infiltrated with pathogenic
organisms. Thus, the formation of
an artificial anus after a traumatic
rupture is of more favourable
prognosis than the same after a
strangulated hernia.
Israel and Grayson cut one ureter
in rabbits and when the animals
had lived a month killed them
and found post-mortem examination only thickening of the bowel.
No peritonitis whatever was present.
Gravity records 8 cases of peritonitis
in the human subject in which
fluid at first serous, became purulent
after paracentesis abdominis.

Pasteur, working in Göttingen under Koch's
back performed in all 101 experiments.
He produced haemorrhagic peritonitis
by the injection of minute quantities of
crude oil. 1 drop always sufficient to
produce death to drop being borne without
harm by the animals (rabbits).
He next experimented with kipsin
and found that ½ gramme in 15 c.c.
of water produced death from acute
haemorrhagic peritonitis in from 4 6½
hours. ⅓ to a gramme caused
death in 20 - 24 hours. Neither by
crude oil nor by kipsin was any
suppuration ever produced nor any
organisms ever found in the peritoneal
exudation. The inflammation in
all cases was of the haemorrhagic
type. In his experiments
with Staphylococcus he made
Experiments and found that
(1) An agar culture injected intraperitoneally in 1 to 2 syringefuls of water caused fibrinous purulent peritonitis and death.
(2) Two cc. in one syringeful of water
   Same result.
(3) 1/2 cc. in 1 c.c. of water. Same result.
(4) Very small amount of Staphylococcus aureus (i.e. 2 cc. were suspended in 5 cc. water and 7 cc. water mixture 2 cc. were taken and injected in 1 c.c. of 7 cc. water. Same result.
(5) Half the above gave no result. This is most important.

Bacillus pyocyaneus also caused death from peritonitis when injected in large amount.

Unfiltered fluid fecal matter caused septic peritonitis and death. Filtered fluid fecal matter did not.

The peculiar bacilli of rabbits feces also caused death when injected intraperitoneally.

He found that minimal amounts of Staphylococcus aureus which of themselves cannot cause peritonitis, do so when mixed with 1/10 - 1/20 of a drop of cortival...
I.e. the ester oil depresses the vitality of the past and the organisms can then act on the less vigorous residues.

I have now given as fairly as I can, a brief description of the previous work on this subject. I do not think that there was any work of importance omitted for Prof. Orth very kindly searched the German literature for me and I the English and French. I was unable to find mention of any such work in French and my friend Dr. Pierre Kestor of Lige also kindly at my request searched among the French literature came to the same conclusion as that which I had previously arrived at, i.e. that there was no work on the subject written in French.

I therefore think that I may fairly say that the above represents all of importance which has been published on this subject. Now Professor Wegener, Libbert and Grampitz all agree that pyogenic organisms can be introduced into the peritoneal cavity without producing microbic peritonitis. Even Pawlowsky who
no point stands alone agrees that a minimal amount of staphylococci may be introduced into the peritoneal cavity without causing peritonitis. Thus all observers, myself among them, agree that the mere introduction of a few pyogenic organisms into the peritoneal cavity does not in the healthy rabbit produce peritonitis.

This is a matter of such extreme importance that I cannot help drawing particular attention to the fact that on this point at least we are all agreed.

I admit freely that Pawlowsky says that only with a minimal quantity did he obtain a negative result but still his minimal quantity comprised a considerable number of organisms. On other points I fear there is disagreement. When I read the papers of Frankly and Pawlowsky, for it is to these that I shall confine myself, for they are the two most recent and contradictory contributions to the subject.

I frankly admit that I felt sure braced as I was by preconceived notions, than Pawlowsky was not.
and Gravity. How different is my opinion to-day with the completed result of my experiments before we will be manifest to anyone who reads this account of them. I may say in short that there is scarcely a statement of Gravity with which I do not entirely concur whereas I disagree in almost every particular with Pawlowsky. Pawlowsky's paper was intended evidently as a reply to Gravity essays published a year previously. In it Pawlowsky says the statements of Gravity that the injection of physiological organisms into the peritoneal cavity was unproductive of harm led me to false conclusions which cost me much time and many animals. I can only here say that the statements of Gravity have in my hands in almost every particular met with the most entire corroboration.

Gravity adopted the mode of injection into the peritoneal cavity using a hypodermic syringe which caused virtually no wound of the peritoneum. Pawlowsky made an incision
down to the muscular layer of the abdomen and then plunged a blunt trocar and cannula through the muscular wall and the parietal peritoneum into the peritoneal cavity. In this way he sought to avoid injury to the intestines but in doing so produced a wound of the abdominal wall.

At first I thought that in this wound the difference of result might be found. Pavlovsky's cases proving to be cases of general infection conveyed from a suppurating wound in connection with the peritoneum, but my results did not bear out this idea.

One further point and I have done. Pavlovsky carried out his work in Prof. Rosenbach's private laboratory in Göttingen and I have no doubt that the fact that his research was carried out, under, Rosenbach gave to many the idea that Rosenbach coincided entirely with his conclusions. This is not quite the case for as time after time repeated Pavlovsky's experiments with almost always a result different to that which he described through the kindness of Prof. Rosenbach.
precisely similar to those which he had employed. I asked Prof. Rosenbach if he had seen Pasteur's experiments. He answered that he had been aware of them as he was obliged always to be at the Polichinche from 9 to 1 and that Pasteur always performed his experiments between those hours, i.e. in Prof. Rosenbach's absence. Therefore I think the case stands thus: that my results agree entirely with those of Graywick, and are strongly supported by those of Osterhout and Weijer. Whilst Pasteur stands alone. I therefore respectfully submit that the balance of evidence is entirely in favour of Graywick and myself.

Now proceed to detail my own somewhat numerous series of experiments. At the commencement it occurred to me that it would be better that I should procure try micro-organisms for experimental purposes from several sources in order that the objection could
not be raised hereafter that I was working
with no virulent organisms. The majority
of my experiments being conducted
with staphylococcus aureus I carefully
secured 5 different specimens.
A. Perfectly fresh staphylococcus aureus
kindly given me by Prof. Rosenbach,
was obtained from a case of acute
suppurative peritonitis. It will be noticed
that I obtained these cultures with
which I chiefly worked from the same
source as Pavlovsky. Prof. Rosenbach
assured me that they were in
every respect exactly similar to the
organisms with which he had
supplied Pavlovsky.
B. I obtained cultures of staphylococcus
aureus from Prof. Wolfflinigel. With
these I did not carry out many
experiments as the organisms
seemed to me to be less virulent
than the others.
C. Staphylococcus aureus cultivated
from pus from an acute human
abcess by Dr. Alvinus Assistant
to Prof. Ditt.
D. Staphylococcus aureus cultivated
by myself from an acute
abcess.
E. Staphylococcus aureus cultivated by myself from a case of acute suppurative peritonitis artificially produced in a rabbit.

All the above were cultivated in a thermostat the temperature of which was kept at 39.5°C (body temperature). In almost every case agar cultures were employed. All the more important experiments were performed with at least three of the above. In every case one of the three being those obtained from Prof. Rosebach. As regards Staphylococcus aureus all out of the 5 were undoubtedly virulent.

I obtained Staphylococcus pyogenes from two sources.

A. Fresh cultures from Prof. Rosebach obtained from a case of septic peritonitis after abdominal section in the Surgical Clinic.

B. From fresh cultures obtained by myself from a case of septic peritonitis the result of sepsis of the umbilical cord in a new-born child.

As regards the other organisms I cultivated the Bacillus of
rabbits feces from a plate cultivation of diluted fecal fluid. For cultures of bacillus pyocyaneus and micrococci pyogenes which latter I used merely for two control experiments I am indebted to Prof. Wolffhügel.

I was anxious to prove the virulence of my organisms and therefor performed the following experiments which as they do not concern the peritoneum I do not enumerate among my list. Experiments. Two young rabbits rather more than half grown were anaesthetised and the lower end of the right femur of each was broken across, and the two ends of the bone were then rubbed together somewhat violently in order to produce some extravasation of blood at the seat of fracture. The two fractures were then carefully set and maintained in position by splints. Half an hour later 6 millims of a suspension of staphylococci obtained from
Prof. Rosenbach) in water were injected into a vein of each ear in one rabbit, and the same quantity (12 minutes i.e. 6 in each ear) of a suspension in water of those obtained from S. albus was similarly injected into the blood stream of the second rabbit.

The result in both cases was the same, a virulent acute supplicative periostitis. The animals were killed on the 6th day and the two broken ends of bone were found in both cases bathed in pus which was swarming with staphylococci aureus.

Another somewhat similar experiment performed with the same quantity of staphylococci taking those obtained from acute pus (human). Experiment - A young rabbit having been anaesthetized its right femur and tibia were broken with a pair of stout scissors. It was somewhat severely for 15 minutes; somewhat. It was felt that a haematoma formed over the femur. The left femur and tibia were
the very gently beaten for 2 minutes. Half an hour later the organisms were injected intravenously just as in the preceding cases. The result was an acute suppurative peri-
titis on the right side both femur and tibia being affected. The
animal was killed on the 6th day and the pus examined both under the microscope and by the culture test proved to be
swarming with staphylococci. On the left side no suppurative
ocurrence and the parts at the post-mortem examination
revealed nothing abnormal.
How strong a support these experi-
ments lend to the theory of the
necessity of a hens inoculation
resistance I need scarcely men-
tion. I consider that they proved
the staphylococci with which I
have been working to be in reality
vivulent pyogenic organisms.
These experiments are placed at the beginning though they
were performed towards the
latter end of the experiments.
Still I have mentioned them
here in order to prove that my organisms are open to no criticism on the score of deficient virulence. At the commencement I contented myself with the following experiment—Experiment. The right-eyes of two healthy rabbits were injected with 1 minim of a suspension of staphylococcus aureus in sterilised water. Suppuration and phthisis resulted with total loss of sight in both eyes. One burst and left a shrivelled stump, the other formed a huge staphylococcus.

I think that from the above experiments most will agree that there is really nothing to take exception to as concerns my organisms.

Now I come to the most important part of this Thesis viz. the description of my experiments performed on the peritoneum.

Let me say here that in all the important experiments which terminated fatally the organs kidneys, liver, spleen, and sometimes bone marrow.
and blood were examined for microorganisms. In the kidney according to my experience we find most abundantly and easily organisms which have been absorbed from any part of the body. In every case the exudation in the peritoneal cavity was examined microscopically. All these examinations were made with the aid of my own microscope a Winkel of Göttingen with \( \frac{1}{4} \) inch oil immersion lens, Abbe condenser and all requisites. I worked generally with the oil immersion lens and ocular No. 5. which combination gave a magnification of 550. Sometimes I employed ocular No. 4. which increased the magnifying power to 600.

For cover glass preparations I employed Löffler's solution of methyl blue, whilst for sections of organs containing micro-organisms I obtained by far the best results by Francis's method using Gentian violet as the stain for the organisms and bichlor.
picro-carmine as a contrast stain.

I wish to state that in all experiments even when a hypodermic syringe was used for injecting the fluid containing organisms the strict antisepic precautions were employed. The abdomen was shaved, washed with turpentine and then a 1/1000 solution of corrosive sublimate was drenched over the part. All the instruments were soaked in 1 to 20 carbolic. The wounds were sutured with carbousied silk and dressed with isotonic corrosive sublimate wool and collodion. That any antisepic precautions were sufficient may be judged from the fact that I never met with any kind of organism in the peritoneal cavity which I had not myself knowingly introduced except in two cases in which I injured the bowel by accident. Let me once more repeat that the order in which these
Experiments are given here by no means the exact order in which they were performed. I have grouped them and arranged them for convenience.

Experiments with pyogenic organisms.

I. With *Staphylococcus pyogenes aureus*.

The first series of experiments I undertook in order to decide the question in dispute between Gramsky and Pawlowsky. It will be remembered that Gramsky states that the introduction of pyogenic cocci into the normal abdomen is harmless. Pawlowsky on the other hand maintains that it causes (except when only minimal quantities are employed) death from septic peritonitis.

Experiments 1 and 2.

I in these experiments injected 6 c.c. of sterilised distilled water containing a sufficient number of *Staphylococcus aureus* to cause
a slight full distinct opacity in the fluid into the peritoneal cavity of two healthy rabbits. Both made perfect recoveries with peritonitis being caused. In these cases I used a hypodermic syringe for injection according to my own description. After waiting 10 days No. I was killed. No trace of peritonitis was to be seen on post mortem examination. No 2 was allowed to live.

Experiments 3, 4, and 5.

In these experiments Pawlowsky's method of operating was followed out for I wished to prove whether or not the wound in the abdominal wall was the cause of the difference of results in Pawlowsky's and Grauvig's experiments. I therefore made an incision in the middle line down to the linea alba through which I pushed a blunt trocar and cannula into the peritoneal cavity and 6 c.c. of the same fluid as was used
in cases 1 and 2 were injected into the abdomen. Wishing to avoid infection of the wound, I introduced a very fine aseptic silver cannula about 1/2 English calibre, through the superficial opening into the peritoneal cavity, down to the pelvis, and then injected the staphylococcus fluid through the cannula. I then introduced a stitch and dressed the wound with isoform collodion and wool of these 3 cases Nos. 3 and 4 made perfect recoveries. No. 5 died on the 5th day after the experiment. The post-mortem examination showed clearly what had caused death. Had plunged the blunt point against the colon, which was distended with feces, and had thus caused a local necrosis. Though there was no perforation of the bowel. The rabbit being a vigorous one, I had used more force than was wise in order to pierce the abdominal wall and thus had caused the injury to the bowel. There was a place where
The virality of the bowel was lowered (locus minoris resistentiae) and thus opportunity was given to the pyogenic organisms to find a nidus for their development, of which they had taken full advantage.

This case, though a failure owing to my clumsiness, taught me a valuable lesson and gave me an indication of the direction in which to proceed in my further experiments, i.e., by establishing a locus minoris resistentiae somewhere in the peritoneal cavity. Still though staphylococci prevailed in the purulent exudation which was seen in this case, bacilli were also present, identical with those which I afterwards learned were the bacilli of rabbits foeces. I carefully examined the necrotic spot of bowel but there certainly was no perforation. This necrotic spot was then examined microscopically and presented the appearance described by Wepfer as coagulation necrosis. The tissue could be seen bacilli which evidently
were traversing the bowel wall from the intestinal canal into the peritoneum. This leads me to remark that whilst the bowel is still living organisms cannot pass out of it into the peritoneal cavity. But the moment the bowel wall dies the organisms commence to travel outwards. I have thus in two cases of severely strangulated hernia examined the watery fluid in the sac, the bowel being intensely congested but not gangrenous. In neither of these cases were organisms found. In a third case where at the time of operation the bowel was evidently gangrenous an immense number of organisms were found in a small drop of fluid taken from the sac of the hernia. In case No. 5 organisms were found in the spleen, liver, kidney, diaphragm, and in the necrotic area of bowel. This necrotic area was no larger than the bore of the cannula but there was intense congestion of the bowel all around it. Drs. 2 and 4 are still alive and well.
Experiments 6 and 7.

Gravity having proved correct in respect to comparatively large numbers of organisms these next experiments were performed in order to test the case with very large numbers of organisms. The following was injected into each of the 4 successive cases. i.e. of sterilised distilled water (when water is mentioned sterilised distilled water is always meant unless the contrary is stated) containing sufficient Staplococci to cause a distinct through light-microscopical in the fluid of this fluid i.e. were injected by gravity method in to hos. 6 and 7.

Hos 7 recovered perfectly but unfortunately in hos 6 the animal who was an anaesthetised as this was only the prick of a hypodermic needle gave a start which was sufficient to cause the needle to produce a tear in the bowel from which an extravasation of faecal matter resulted and the animal died of acute supplicative peritonitis on the 8th day after the injection.
Experiments 8 and 9

There were performed according to Pauw-
lowsky's method with 10 c.c. of the
same fluid as used in Nos. 6 and 7
which of course contained a very large
number of Staphylococci. The two
animals recovered perfectly.

The above 9 experiments having been
exclusively confined to rabbits, as were
Gravitz and Pauwlowsky's cases. I
thought it would be well to repeat
them on other animals.

Experiments 10, 11, and 12

In these 3 experiments the intro-
duction of organisms was done by
means of the hypodermic syringe
(Graebe). A solution as used in
the former experiments 6 to 9 was
employed; of this,
5 c.c. injected into the peritoneal
cavity of a healthy male cat produced
no effect. (No. 10).
3 c.c. similarly were without effect
on a female pig. (No. 11)
and 1 c.c. caused no ill result
injected into a rat's peritoneal cavity. (No. 12)
Experiments 13 and 14 were performed by Pawlowski's method with 100 cc and cancella.

5 c.c. of the same staphylococcus fluid were introduced intraperitoneally into a healthy male cat. Its peritonitis was caused (see 13).

3 c.c. similarly produced no effect when injected into a guinea-pig's peritoneal cavity.

Now these 14 experiments undertaken on 4 different kinds of animal in order to test the point in question decide unanimously in favour of Graving. Deducting from the 14 experiments the 2 in which had done injury to the bowel. There remain 12 cases i.e. 7 rabbits, 2 cats, 2 guinea pigs, and 1 rat in whom the experiments were made. One-half the number according to Graving's method, one-half according to Pawlowski's directions. Now of all these animals not one died from the introduction intraperitoneally of virulent staphylococcus aureus. Some of these were killed and
at the post mortem examination revealed nothing. In trace of pyogenic peritonitis was to be seen. From these results agreeing entirely as they do with those of Grams I consider as proved my first proposition. That considerable quantities of staphylococcus aureus suspended in water can be introduced into the healthy abdomen without causing peritonitis.

Now having proved that the introduction of & c.e. of a suspension of staphylococcus aureus in water into the healthy abdomen does not produce peritonitis the next question to decide was. Can these pyogenic organisms introduced into the healthy peritoneal cavity in other media produce peritonitis? In other words can the organisms be introduced in such a manner as to cheat the absorption without the medium in which they are introduced interfering the peritoneum and thus depress its vitality? To ascertain this point I performed the following experiments.

Experiment 15. I injected into
the peritoneal cavity a gelatine culture of staphylococcus aureus which had previously liquefied at a temperature of 38 C. (liquefied meat-pepton-gelatine of Koch, passes quite readily through a fine hypodermic needle being when properly made limpid like water). The animal died on the 5th day the post-mortem examination revealing the presence of general acute purulent peritonitis. Organisms (staphylococcus aureus as proved by culture) were found in immense number in the pus and also in the kidney, liver, spleen, and bone marrow. No liquefied gelatine was to be seen in the peritoneal cavity.

Experiment 16. In this case an agar culture of staphylococcus aureus was introduced into the peritoneal cavity by means of an incision 1 inch in length. Great care was taken that the cocci did not come in contact with the wound. The mass of the agar culture was placed on the posterior wall of the
abdomen and covered over with loops of small intestine so that it should not come in contact with the incision in the abdominal wall. The wound was sutured and dressed. The animal died on the 5th day of acute supplicative peritonitis. The piece of agar was found adherent to the intestines just to the right of the vertebral column and bathed in pus. The agar of course being a solid substance with a melting point far above that of the temperature of the body could not be absorbed. Concerning these two experiments, nos. 15 and 16 the question now arises: How was it that the liquid gelatine which was entirely absorbed was able to give opportunity to the organisms to exhibit their lysozyme qualities whereas water cannot do so. My explanation is this: that although it was eventually entirely absorbed still that the absorption took place slowly and further that before it had disappeared it had given the organisms which it
contained time enough to commence to produce their pyogenic effect. Thus a peritonitis had already been set up before the gelatine was entirely absorbed. Once started of course it is easy for the work to proceed uninterrupted. With regard to the agar culture it is clear that the cocci could only with difficulty and very slowly be absorbed because of the fact, that they adhered to the agar and that they were in one mass, and not suspended in a fluid.

Still there remained this point to be cleared up. It is well known that staphylococcus aureus produces in gelatine cultures as elsewhere waste products especially ammonia, and it might fairly be maintained that the microbe peritonitis was due to these irritants acting by depressing the vitality of the peritoneum and thus enabling the cocci to act on the debilitated tissue. In order to prove that the organisms can act probably I freely admit by these waste
Products (which act as I have indicated above) whilst inside the peritoneal cavity and therefore that these do not require to be performed in the gelatine culture. I performed Experiment No. 17. 3 c.c. from an agar culture were mixed with 5 c.c. of liquified gelatine and injected intraperitoneally. The result was the same as in the two former experiments: the animal dying on the 5th day of acute purulent peritonitis. Now in this case there can be no question of waste products already present in the gelatine for the organisms had only been in the gelatine for about 1 minute before they were injected into the peritoneal cavity. As confirmation of No. 17 I take the next experiment:

Experiment 18. 6 c.c. of thick bouillon containing 3 c.c. of Staphylococcus aureus were injected into the peritoneal cavity of a rabbit. The animal died on the 4th day of acute supplicative peritonitis. Staphylococci were found in the peritoneal pus and in all the organs examined.
From these 4 experiments Nos. 15, 16, 17, and 18, I think we may fairly deduce the following: That septic peritonitis can be caused by the injection of streptococcus aureus in moderate quantity when introduced into the peritoneal cavity in a medium which can serve as a suitable food-ground and which is suitably absorbed. It will be seen from later experiments that gelatine and bouillon injected into the peritoneal cavity unaccompanied by pyogenic organisms are unproductive of harm.

The next experiments undertaken were with the object of endeavouring to ascertain if the presence of a clean-cut wound in the abdominal peritoneum was sufficient of itself to cause a locus minoris resistentiae where pyogenic organisms injected into the abdominal cavity could settle and whence they could spread and cause acute peritonitis.

Experiment 19. In this, as in all the following experiments, I took a mass of organisms about the size of a pea and stirred it up with 10 c.c. of sterilised water (distilled).
until there resulted a slightly milky fluid, the opalescence uniform. I shall henceforth refer to this as the Staphylococcus (or staphylococcus) fluid for the sake of brevity.

Experiment 19. A rabbit was anesthetized and a long abdominal incision from uniform cartilage to pubic symphysis. Into the cut edges of the wound a dose of staphylococci aureus were rubbed and the wound closed by sutures. The animal died in the 5th day, the cause of death being general suppurative peritonitis which had resulted from the appearances seen at the post-mortem examination proceeded from the wound which was in part not healed on the peritoneal surface. The skin wound was healed but the whole abdominal wall was one huge abscess. The intestines were adherent to the wound which was filled with pus. In this case an enormous abdominal section was made in a weakly animal and a large number of staphylococci were rubbed into the wound. I felt that far too many organisms had here been introduced.
into the cut edges of the wound, and that it would be well to repeat the experi-
ment with a smaller incision and with the rubbing in of fewer organisms.
I therefore performed the two following experiments.

Experiments 20 and 21. An abdominal incision 1 inch in length was made in each of these cases, and the wound was then painted with a small camel’s
hair brush with staphylococcus fluid. Two applications were made to each cut surface at an interval of 5 minutes. I then sewed the wound accurately, and dressed the skin sparing the usual way with iodoform wool and collodion. The rabbits both recovered perfectly without any sign of suppuration in the neighbourhood of the wound. Being anxious to try the effect of infecting a clean cut wound of the human body, I cut the terminal phalanges of my little finger almost to the bone with a razor. When the bleeding had ceased I painted three times the cut surfaces with staphylococcus fluid, and then brought them together with plaster. Union by first intention did not occur, but still no suppuration.
Enucle and after the first half-hour no pain was felt in the finger.

**Experiment 22.** An abdominal incision 1 inch in length was made and the wound was sutured in the following way which I would strongly recommend in practice. The needle was entered on the skin surface 3/4 inch (in the rabbit) from the edge of the incision and brought out just at the curv edge of the peritoneum without perforating the latter. It was then introduced just above the curv edge of the peritoneum on the other side and brought out through the skin 3/4 inch from the incision. A simple diagram will explain what I mean.

All the stitches are passed before any are tied. If once tied I venture to express a confident hope that it will frequently be adopted. It has as far as I can see no disadvantage and many advantages compared with the usual way of passing the stitches through the peritoneum. I have used this in more than 40 cases and am exceedingly well pleased with the result.
The peritoneal edges are brought exactly in contact and all danger of infection of the peritoneum through septic organisms spreading along the stitches is done away with. Ten minutes after suturing the wound in this experiment I injected through a hypodermic syringe & c. of staphylococcus fluid. The animal recovered perfectly without any purulent matter and also without any abscess formation at the seat of the wound.

From these experiments I think we may draw the following conclusions. That a clean cut wound does not constitute itself readily in a healthy animal into a locus minoris resistentiae for a small amount of pyogenic organisms applied to the raw surface of such a wound are absorbed without suppuration occurring. On the other hand however if a very large number of organisms be applied to the cut surfaces suppuration ensues because owing to their number they are not absorbed in time before they commence to form their irritant waste products which derivate the
tissues and thus aid the cocci to manifest their pyogenic qualities. Of course the
absorption from a cut wound is not
nearly so rapid as from the peritoneum, therefore the dose required to produce
suppuration is much smaller.

Experiment 23. I made an incision
down to the peritoneum but not through
it. Then dissected away the extra-peritoneal
fat for a space about the size
of a three-penny piece and placing in
this little cavity a drop of staphylococcous
aureus I sutured the wound. My intention
was thus to produce an abscess which
would burst into the peritoneal cavity
and thus cause septic peritonitis.
The animal died on the 6th day with
an enormous abscess of the whole anterior
abdominal wall. In fact the whole
abdominal parietes formed one huge
abscess cavity. There was however
some astonishment on perforation of
the peritoneum which however was
thickened. The skin wound had
healed. In this case the animal
died doubtless from effects of this
huge abscess.
Having found that a clean cut wound did not afford a nidus for staphylococci when introduced in small numbers only I wished to try their effect on a wound which was already inflamed and for this purpose I performed this experiment.  

Experiment 24. An abdominal section 1 inch in length being made the edges of the wound were painted once with turpentine and the wound was then sutured. 1 hour later it was reopened and its edges were then painted once with staphylococcus fluid and the wound sutured once more. The animal died of septic peritonitis on the 4th day. The inflammation evidently originated from the abdominal incision which was covered with pus on the internal surface and adherent to the bowel. There was also an abscess in the anterior abdominal wall. The skin surface to which I took good care not to apply the turpentine had healed. This I think is an experiment of extraordinary interest for it shows the extreme readiness
with which pyogenic organisms attack the tissues when they are in a state of slight inflammation i.e. when their vitality is somewhat lowered.

Experiment 25. I performed precisely the same experiment on a swine pig with exactly the same result except that the animal died one day earlier.

In order to be quite sure that the mere resuming of the wound 1 hour after suturing it had nothing to do with the result, I performed the following experiment—Experiment 26. In this I acted precisely in the same way as in the two preceding cases except that I did not apply tincture of benzin before closing the wound for the first time.

The animal recovered perfectly without any suppuration.

From the foregoing experiments nos. 24, 25 and 26 I think we may say that if there exist a wound of the abdominal wall communicating with the peritoneal cavity and this wound...
be a locus minoris resistencia of pyogenic microbes gain access to it
the danger of septic peritonitis resulting is very great.

Having proved to my own satisfaction the contention of Graminy that the mere
introduction of a suspension of staphylococcus aureus in water into the healthy
abdomen was harmless I next conducted a series of experiments in which I artificially produced in the
healthy abdomen a locus minoris resistencia.

Experiments 27 and 28 In these two experiments I wished for
purposes of control to see what would
be the effect of dissecting off a piece of
peritoneum. Therefore in 27, I roughly 1/2 square inch from the
peritoneum lining the anterior abdominal wall and in 28 a portion
the same size from the mesentry.
I would draw particular attention to the fact that in these experiments
the piece of peritoneum was torn off
i.e. roughly done. Still all was aseptically
done. The abdominal incision in each case was carefully closed.
and dressed. Both animals recovered perfectly. In two weeks afterwards that the ascending colon had become attached by permanent adhesions to the wound in the parietal peritoneum. I consider that these two experiments prove that the removal of a portion of peritoneum from the parietes or mesentery produces no dangerous results when the operation is aseptically performed. The following 5 experiments prove however that the spot whence the peritoneum has been removed is a less micrinos resistantia.

Experiment 29. \( \frac{1}{2} \) square inch of the parietal peritoneum having been dissected off the right side of the abdominal incision, the wound in the mesial line was sutured and dressed. 10 minutes later 5 c.c. of Staphylococcus fluid were injected through the abdominal wall on the left side of the incision. The animal died on the 5th day of general suppulsive peritonitis which had evidently started from the wounded surface from which the peritoneum had been torn off. The bowel was adherent to this spot and pus was seen in large quantity.
Experiment 30. In this case 2 square inch of peritoneum was torn off, and then 5 c.c. of staphylococcus aureus fluid were injected into the peritoneal cavity. The result was exactly similar acute suppurrative peritonitis causing the death of the animal on the 4th day.

Experiment 31. A piece of peritoneum about 1 inch square was dissected off the right kidney of a cat. The abdominal wound being sewn up, 5 c.c. of staphylococcus fluid were then injected into the peritoneal cavity. The animal died on the 5th day of acute general peritonitis.

Experiment 32. A piece of peritoneum 1 inch square was dissected off the kidney of another cat and 4 c.c. of staphylococcus aureus were then rubbed into the lacerated surface. Death from acute peritonitis occurred on the 3rd day. The kidney was found boiled in pso.

Experiment 33. In this experiment instead of dissecting off a piece of peritoneum from the anterior abdominal wall I painted the peritoneal peritoneum over an area about the size
of a stilling durance with temperature
5 minutes intervening between the
two applications of the irritant. I
then closed the wound in the abdominal
wall and 1 hour later injected
intraperitoneally 5 c.c. of staphylococcus
aureus fluid. The animal died on
the 5th day of septic peritonitis which
had evidently had its origin in the
area which had been painted with
tartrazine. In all the above 5 cases
staphylococcus aureus was found as the
only organisms present in the peri-
toneal pus.

Need I stop for one moment to
draw attention to the powerful support
which these 5 results lend to the
theory of the locus minoris resista-
tiae? Let me however just state
the conclusions to be drawn from these
experiments. "The introduction of
pyogenic organisms into a peritoneal
cavity which there exists a locus
minoris resistance must if
the organisms reach this weak-
ened spot inevitably cause death
from septic peritonitis."
I now proceed to describe several experiments which I performed on the bowel by making a portion of it a focus which resisted. I will describe the method adopted very carefully, for as far as I can ascertain it is quite original. The object to be attained was to produce a condition as nearly related as possible to that of the bowel in strangulated hernia. For this purpose an abdominal section having been made a loop of bowel was taken and constructed in the following manner. A knuckle of bowel about 2 inches in length having been pulled out through the abdominal incision a very thick ligature of the softest silk was tied round the bowel so as to include a loop of it as is shown in the diagram and the constricted knuckle of bowel was returned into the abdomen. All these proceedings were carried out under the strictest aseptic precautions. But now a difficulty arose how to remove the ligature...
When required for owing to the construction of the bowel the oedematous swelling was so great that the ligature could not be seen being quite buried in a furrow between the constricted knuckle in which portion of course it could not be cut without risk of injuring the bowel. In order to obviate this difficulty I tied the knot over a spatula-shaped piece of aseptic cork. Thus when it was desired to release the ligature all that was required to be done was to find the piece of cork and cut boldly upon it. The accompanying diagram explains I hope what I mean.

I always left one end of the ligature long and bringing this out at the abdominal incision I tied it to one of the sutures. In this manner I could always with great ease by following up this ligature find the constricted knuckle of bowel with the least possible disturbance of the other contents of the abdomen. At first I tied the ligature far too tight but afterwards I found
that the object was much better attained by tying the ligature much more loosely so as to impede only the venous return of the blood from the part but not cut off the arterial flow to the part.

I performed ligature of the bowel 4 times by the above method then cutting the ligature returned the bowel into the abdominal cavity once more. In these 4 I brought us staphylococcus aureus of other organism into contact with the bowel. These were merely control experiments to show that the ligature of the bowel for a moderate length of time did not cause death without the presence of micro-organisms.

Experiments 34, 36, 36 and 37.

In these experiments I ligatured a loop of bowel in 34 and 35 large and in 36 and 37 the small intestine for the following lengths of time No 34 ligatured for 4½ hours.

35 - 5
36 - 4½
37 - 6

In those cases in which I ligatured
large intestine I took the first patient which presented itself. This was almost always transverse colon. When I ligatured a loop of small intestine, I always chose the last few inches of the ileum. This I reached easily by feeling along the large intestine until I came to the colon. I would strongly recommend any future experimenter who follows our this method of experimentation to choose always the small intestine for several reasons, chief of which is the fact that as it does not lie in contact with the wound in the abdominal wall against which the transverse colon which in rabbits is very large rests, there is no risk of the complication of wound infection. In all the above cases the knuckle of bowel was alwaysriers as far as possible of fecal matter by pressing from above downward, i.e. in the direction of the peristaltic wave. In all cases the knuckle of bowel was very much swelled and varied in colour from dark cherry red to purple or
black but was always glistening.

The ligatures having been cut and the knuckle of bowel released, returned into the peritoneal cavity, the abdominal incision was resutured. All the 4 animals recovered perfectly and on no occasion did they exhibit any sign of ill health afterwards. I should mention that after ligature of the loop of bowel, the abdominal wall was in each case carefully sutured. The ligatured loop having been previously returned into the peritoneal cavity. I would also here mention that in every case in which a cutting or other painful operation was performed an anesthetic (ether) was given but in such experiments as injection by means of a hypodermic needle it was dispensed with. Chloroform found to be very fatal to rabbits and therefore ether or a mixture of one part chloroform to two parts ether was employed. In cats chloroform was always used. All prolonged operations were
performed with the animal tied down on a small operating table specially constructed for the purpose. With regard to hypodermic injections etc, the following method was carried out.

The rabbit was seized by an assistant who holding the animal on its back with its head in the assistant's right axilla so that the eyes were covered. Pressed his right thumb firmly into the animal's right groin and with the palm of his right hand supported the animal's pelvis. In this position the animal would lie motionless and apparently felt no pain. Prof. Bith suggested that the rabbit was a very convenient animal for carrying out various experiments with rabbits although from personal experience I never recommend it with cats.

To return to the experiments

It will be remembered that all the 4 animals recovered. In 37 in which the bowel was
ligatured for 6 hours was killed 27 days after the experiment was performed and the loops of bowel which had been constructed was clearly seen. It was thickened and had lost its shine. Prof. Dott kindly examined it for me and informed me that it presented the appearance of having suffered haemorrhage into its substance. It was slightly green in colour and there had evidently taken place a new formation of connective tissue in the wall of the bowel. Its 28 was killed 14 days after the experiment was performed. It presented a thickened dilated appearance and was still some what distended. These observations go far to corroborate the contention of Erichsen, that the bowel does not entirely recover itself in 28 days after the reduction of a severe strangulation. I think that its 37 shows that it never recovers completely as there is always some thickening resulting. They further more than justify surgeon in insisting on patients
obtaining from solid food for several days subsequent to the performance of herniostomy in strangulated hernia.

These 4 cases showed that it was possible to ligature a kinked loop of bowel in this manner for a moderate length of time without risk of causing peritonitis. I wished to find out what course the peritonitis ran in rabbits whose bowel was ligatured until it became necrotic and allowed the passage of organisms from the interior of the intestinal canal into the peritoneal cavity. I therefore in Experiment 38 ligatured a loop of large bowel for 28 hours and then opening the abdomen found that the ligatured kinked loop of bowel was dull, lustreless, collapsed in short gaseous and that peritonitis had commenced to spread especially on the colon of each side of the ligatured gaseous portion. I released the bowel and returned the wound. The animal died in 20 hours later of general acute peritonitis which was fibrinous consolidated.
After the 23 hours ligature I examined some cover glass preparations which I obtained from the peritoneal surface of the gangrenous portion. These swarmed with organisms especially micrococci and the short thick bacilli of rabbits' feces.

In order to test the efficacy of surgical treatment in such a condition i.e. in one virtually of strangulated hernia, in which the bowel is gangrenous, I performed Experiment 39.

A loop of large intestine was ligatured for 23 hours, then examined, it was evidently gangrenous and there was petechiae extending for 1/2 inch on each side of the ligature. The wound in the abdominal wall with which the bowel was in contact also presented the appearance of commencing inflammation (from contact). There were also as I proved afterwards by culture and microscopic examination organisms present in the abdominal cavity.

My friend Sir Pridie and I agreed that the case was well nigh hopeless but still that bold treatment might possibly be of some avail. Therefore drawing the gangrenous
mass and the bowel for an inch on each side of it out of the abdomen, irrigated the peritoneal cavity very thoroughly with sterilised water at body temperature for 15 minutes and then for 5 minutes with 1 to 3,000 corrosive sublimate solution. I wrung it out all this time retaining the gauntness hunk of bowel outside the peritoneal cavity. I then amputated 3 1/2 inches of the bowel taking away the gauntness part and the somewhat inflamed portions adjacent to it and cutting into healthy bowel tissue. At this time I pressed a ring of tissue from the abdominal incision for I felt that it was useless to leave this inflamed margin which was certainly infiltrated with organisms and then to expect a good result. I now wished to unite the two ends of bowel by suture but as I knew that this would require a long time to do properly and the animal was showing great signs of collapse I brought the two ends of bowel out at the wound and made there an artificial anus serving as Lawson Tait suggests peritoneum.
to skin. The animal being subjected made an excellent recovery. As it caused so much noise in the room in the institute in which I worked (for I had the rabbits running about in the room as it was so intensely cold in the winter) that I was obliged to have it killed on the 11th day after ligature i.e. the 6th after the formation of the artificial anus. There was no sign of peritonitis and the artificial anus acted remarkably well although from the large size of the opening in the abdominal wall there was at times great tendency to prolapse of the bowel (mucous surface outward). This was easily returned and the animal did not seem at all disturbed by its pretentious anus.

The above experiment having been conducted on a rabbit who is not a suitable animal for a severe surgical operation owing to the fact that it becomes quickly depressed owing to the shock of the operation and the effect of the anaesthetics I performed the following experiments on healthy male cats.

Experiments 40, 41, and 42
In these cases the bowel (large in 40 and 42, small in 41) was ligatured for 14 hours and then the steps described in the above operation were exactly followed except as regards the after treatment of the two cut ends of bowel. In 40 2 inches of bowel having been cut away an artificial anus was formed and the animal recovered.

In 41 and 42 the two cut ends of bowel were united by Lambert's suture. In 41 made a complete recovery. In 42 died on the 3rd day after the operation owing to a stitch having given way and thus allowed extravasation of fecal matter into the peritoneal cavity to occur. This caused fatal peritonitis.

Thus of these 4 cases, 1 recovered and 1 died from any mistake in not suturing the bowel properly.

I cannot help regarding these as great successes and I think they teach us some lessons of importance.

They show us clearly I think in operations for strangulated hernia in cases in which the bowel is manifestly gangrenous that amputating the gangrenous portion by cutting into sound bowel and then forming a little
an artificial anus or at once restoring
by suture the continuity of the intestinal
canal offers a much better chance for
the patient than merely being content to
form an artificial anus without setting
off the decidualised and organism-infilt-
trated boundaries of the gangrenous portion.
Every surgeon knows that the formation
of an artificial anus after a hernia-tomy
where the bowel has been found
gangrenous is much safer than
resecting the intestine and the
reason is clearly that if there be
a resection of the bowel with puerperal
organisms in the peritoneal
 cavity, i.e. those which have
penetrated the gangrenous bowel there
may be as will be seen in a later
experiment a nidus for the organisms
to settle in the sutured part where
they may develop and cause peri-
ritis. I do not say that this
always happens for in case of
the animal recovered perfectly
after the vast majority of the organisms
had been washed out of the peritoneal
cavity, but it does without saying
that there is more fear of the
organisms settling in the sutured
place than elsewhere. In the case of the artificial anus the best results are obtained by the peritoneum because it is not forced into a state of inflammation. Still in a case of artificial anus I think it would be well to pare the edges of the bowel cutting into sound tissue for in this way one avoids all denervated permeated tissues and one has a good chance of obtaining union by first intention between peritoneum and skin which is not so difficult to secure as some would lead us to imagine. For much information on this point that is known between skin and peritoneum we are indebted to Lawson Tait.

A very important question for us is when are pyogenic organisms present in the peritoneal cavity in case of rupture of the tubercle from various causes or in the fluid in the sac of a manipulated hernia? I have already indicated my opinion that...
they are present very soon after necrosis
of the bowel occurs. I am certain that
they are not present so long as the
bowel is not necrotic. Our great
difficulty is knowing what to do in
cases in which the bowel is in
jured that although not
necrotic we fear it will leave
no power to resist the reaction
which follows the release of the
strangulation and the folk will
die later on.

Whilst we are on this subject I
will describe two operations which have
much bearing upon the question
of resection of the bowel

Experiments 43 and 44. In these
experiments, 1 inch of the small intes-
tine was resected in 2 healthy
rabbits. In both the operation was
performed in precisely the same
manner the bowel being stitched
by Lambert's nature and the abdomen
closed and dressed. In thirteenth
was done in case 43. But in case
44 15 minutes after the completion
of the operation injected into
the abdominal cavity 0.1 c.c.
Staphylococcus aureus fluid. In case 43 in which resection alone was performed, complete recovery ensued. No 44 died in 3 days of acute supplicative peritonitis which was especially severe around the sutured bowel area. In this case the sutured bowel seemed to be the focus minoris resistance. The bowel was covered with pus but on washing this away the stitches (of very fine silk) were seen to be holding perfectly, and the bowel in this area was watertight. There can therefore be no question of extravasation of feces here.

Having proven this conclusively that there was ligation of a knuckle of bowel for from 4 to 6 hours did not suffice to produce peritonitis when aseptically performed, I carried out my next experiment by ligaturing the knuckle of bowel before described and then injecting into the peritoneal cavity a considerable number of staphylococci, thinking that these organisms would be able to settle
and flourish on the injured bowel, i.e. they would find this a focus minimal resistant. The results fully bore out my anticipation. Experiments 45, 46, 47, and 48. These experiments were very similar. In the 45 the demnoble of bowel was ligatured for 40 hours.

I wish to draw attention to the fact that these were all ligatured for periods of time which I had already shown the intestine could bear with impunity. All the animals died of acute suppurative peritonitis after the intraperitoneal injection of staphylococcus fluid varying in amount from 3 c.c. in the 46 to 60 c.c. in the 45. 85 and 46 died on the 4th day. 45 and 47 died on the 5th day after the experiment. We may therefore take it for granted that the injection of staphylococcus causes septic peritonitis provided always that the organisms are able to come in contact
with this debilitated state.

Still more interesting are the results of the last series of experiments in which after ligature of a loop of bowel as in the above experiments a few minutes of staphylococcus aureus fluid were injected into a vein of the ear of a rabbit. I may mention here that whereas it is always recommended to inject the fluid centripetally, i.e. in the direction of the blood current, I have found it far more convenient to inject it centrifugally. If one holds the ear of a rabbit to the light and then injects into a vein peripherally one sees the blood driven rapidly through the vessels and its place occupied by the injected fluid. I feel sure that any one who has tried this method once will always adopt it in future. I speak only of intravenous injection.

The great advantage which in my opinion this method
injection possesses is that it is easy to see the injected material pushing the blood out of the vessels and thus one has no doubt as to whether the fluid which we desire to inject has obtained entrance into the blood stream.

**Experiments 49, 50, 51, 52 and 53**

In these cases a knuckle of bowel (large intestine in 49 and 50, small in 51, 52 and 53) was ligatured for the following periods of time:

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The bowel was then released and the abdominal incision was sutured and dressed then to 30 minutes after words 4 minutes of Staphylococci fluid were injected into a vein of each ass. I always took 8 or 9 ass to make sure that the blood received the organisms i.e. 8 minutes in all were injected in each experiment.

I noticed more particularly than ever in these cases that there were no little lumps of organisms for it seemed to be quite certain that
Embolism was not produced. Now the results were most extraordinary for omitting his 50 who died whilst I was injecting the fluid into a vein and who as shown by post-mortem examination was suffering from very advanced phthisis pulmonalis all the animals died of peritonitis death occurring in all cases within 24 hours. Still in spite of this short duration of the disease the condition was always pretty severe peritonitis and even some few pus cells were to be found in the neighborhood of the damaged portion of bowel.

The peritonitis in all cases abounded in organisms which proved on cultivation to be Streptococcus aureus. The inflammation was always septic for it had lasted so short a time that still as I have mentioned above some few pus cells were always to be found. Why there should be this extra ordinary difference in time that in these cases death should always have occurred within 24 hours whereas in the cases of intraperitoneal injection the average time for death to occur was 4 or 5 days I cannot explain.
Probably in the former case the reason of the rapidity of death occurring is that the exciters of inflammation reach the locus minoris resistance by the blood stream whereas in the latter the inflammation spreads superficially by continuity of tissue. Whilst I cannot pretend to explain this point further, I would venture to remind my readers that this is no isolated instance of the difference depending on the seat and extent of inoculation. According to Pasteur, whereas the average time for death to occur after bites is 6 weeks, the animal usually dies in 7 days after sub-dural inoculation.

These results were so striking and I believe so entirely novel that Prof. D'Her was advised me to repeat as control experiment the same steps of the operation in every respect with some non-pathogenic micrococci. This I did choosing the well-known micrococci prodigiosus which is familiar to us all as causing blushing bread to which our Saxon Forefathers used to attach so much importance in
judicial test. With this organism admitted by all to be entirely devoid of pathogenic qualities I performed—

Experiments 54 and 55. I followed out the steps of the above described operations in every particular except that for staphylococcus pyogenes aureus I substituted micrococcus prodigiosus. In Nos. 54 the intestine was ligatured for 4 and in 55 for 5 hours. Both animals recovered perfectly and No. 55 being killed 10 days later exhibited no trace of periostitis. No. 54 continued to live.

There still remained however the possibility that the organism might be able to cross the incision in the abdominal wall and in order therefore to test this point I performed Experiment 56. An abdominal incision of the same length as in the other cases (say 1½ inch in length) was made then sutured and dressed. The bowel did not being interfered with at all. Then 4½ hours later the wound was reopened and once more closed. 16 minutes later 8 minutes of staphylococcus...
fluid were injected into the veins of the ears. An suppuration followed at the
seat of the wound which in the ordinary healed by first intention. The suppur then
therefore that a clean cut wound in a healthy animal is not a tissue
sufficiently depressed to serve as a
suitable nidus for pyogenic organisms.
I maintain that these control experiments fully prove the pathogenic action of the staphylococci with which
I worked and that further these
organisms found their locus major
resistance in the damaged bowel
being conveyed there by the blood
stream.

very closely related to these experiments
is the following:

Experiment 57. A loop of small
intestine was ligatured in the usual
way and released after 2½ hours
the bowel being dark cherry red in
colour. The abdominal incision
was then sutured and dressed.
10 minutes later the right femur
was divided by means of an osteotome
into each fractured end of bone
one of staphylococci arrears
was rubbed. The fracture was then set and splints applied. The animal died 26 hours after the operation and infection of fractured ends of bone with the organisms. The post-mortem examination revealed the presence of extensive fibrino-pusulent peritonitis especially around the injured loop of bowel. The fractured ends of the femur were covered with a blue layer of pus. On opening the chest it was seen that whereas the right lung was at all appearances normal the left was in a condition of pneumonic consolidation and dark purple in color. On making microscopic sections of the two lungs which were first treated with oxalic acid fat emboli were seen in both especially numerous on the left. On staining by Gram's method with gentian violet presenes of organisms apparently all micrococci were seen in the tissue of the left lung, but very few in the right. Into the important matter of the causation of this commencing septo-pneumonia it is not my purpose here to enter. Still I thought it well to mention the fact of its existence.
In this case it appeared that the organisms gained access to the bloodstream by entering some open ramified veins or veins in the tissue of the femur.

A somewhat similar result was obtained in Experiment 58. A loop of small intestine was ligatured for 3 hours and then released. The abdominal incision was sutured and dressed. Then a somewhat deep cut was made into the muscles of the thigh. When all bleeding had ceased 2 or 3 staphylococci were rubbed into the wound. The animal died on the 3rd day of acute supplicative peritonitis. The wound was red and angry and a little pus was seen oozing.

In this case there is no doubt that the organisms were conveyed from the wound to the injured bowel but it remains a question as to whether it was by the bloodstream or indirectly by the lymphatic system. I incline to think the latter.

The next two experiments were carried out by subcutaneous in-
section of staphylococcus aureus after preliminary ligature of the bowel.

Experiment 59. In this case a loop of small intestine was ligatured for 4 hours and then released. Then 2 c.c. of a very opaque mixture of staphylococcus aureus in water were injected into the subcutaneous tissue in Scarpa's triangle. The animal died of septic peritonitis on the second day. An acute abscess was forming at the seat of inoculation in this case. The number of organisms injected was enormous.

Experiment 60. In this case the bowel was ligatured for 2 hours only, then released and the incision sutured and dressed. Then 2 c.c. of a faintly opaque mixture of staphylococcus aureus in water were injected subcutaneously into Scarpa's triangle. No abscess formed, as peritonitis resulted. The animal made a complete recovery and on being killed 16 days later presented no trace of peritonitis. It is somewhat difficult to compare these two cases because in the former the injury to the bowel was far more
Severe and the number of organisms infected was much greater than in the latter. I do not know whether the bowel was not sufficiently injured in its 60 to constitute a locus minoris resistitiae or whether the organisms were destroyed in the blood before they reached the loop of bowel. I incline to think the latter the more likely explanation for they were few in number and being absorbed by the lymphatics they might readily be caught in the lymphatic glands and destroyed there or in the lymph stream before gaining access to the blood current.

That these experiments may have a practical importance will be gathered from the following fact. It is well known that patients do sometimes die after biliary surgery performed with the most careful antiseptic precautions. Peritonitis is most frequently in such cases the cause of death. These pyogenic organisms have in some way, or other attacked the bowel, few will doubt. It will be said that the organisms have gained entrance into the peritoneal cavity and thus have set up the mischief. That this is probably the most frequent cause of the peritonitis I do not question.
Still I feel confident that in some cases the
organisms reach the injured bowel by the
blood stream. When I was House Surgeon
at the Royal Infirmary a case of strangu-
lated hernia was admitted. As far as had
been roughly applied before the patient was
sent to hospital. Herniotomy was immedi-
ately presented to. The bowel was deeply congested
but by no means gangrenous. The
patient died suddenly 44 hours after
the operation, the only symptoms prior
to death being a rapid pulse and a
somewhat disturbed abdomen.
After death I noticed that there was
in the right thumb an abscess which
had been caused by a splinter of
wood. The post mortem examin-
ation showed commencing suppurative
peritonitis. At the time I thought
nothing of the abscess but these
experiments brought the case
widely to my mind. Of course
it may be merely a coincidence
and as I was at the time un-
acquainted with Bacteriological method
I did not examine the pus or make
cultures and therefore I cannot say
whether or no the same organism
was present in the abscess of the
Stump and in the peritoneal exudation.

Still I think this case taken in connection
with these experiments gives us
an indication which may clear
up some of the many obscure points
connected with the "Etiology of Peri-
tonitis." To my own part if I were
to meet with a strangulated hernia
in a patient—whom I had reason to think
had pyogenic organisms circulating
in his blood, as in a case of acute
abscess or septic compound fracture.
I should advise very strongly that
herniotomy be not once performed
and that no attempt at taxis be made
perhaps the worst be made lest the
bowel being bruised and injured the
organisms may settle out and
this cause a septic peritonitis.

Now having shown that these pyogenic
organisms can by acting on the
injured bowel produce peritonitis
by attacking the bowel both from the
peritoneal surface and through the blood
stream it remains for us to see
whether they can set up peritonitis
by passing through the injured
intestine from its mucous to its
serous surface. In order to decide this point I conducted the following experiments and I can answer this question in the negative provided that there be no loss of epithelium (alteration) on the mucous surface.

Experiment 61. A rabbit was kept without food for 48 hours in order that its small intestine might be fairly empty. It was then fed by means of a catheter introduced into the stomach with 15 c.c. of a solution of *staphylococcus aureus* in water. 3 hours later when it was judged that the organisms must have reached the small intestine an abdominal section was performed and a loop of jejunum was ligatured. At the end of 3 hours the intestine was released. The animal made an excellent recovery. Importantly resulted there being a doubt as to whether the cocci had reached the ligatured loop in large numbers. I repeated the experiment—varying somewhat the details.

Experiments 62 and 63. In the former of these two experiments a rabbit was employed in the
latter a cat. These two animals were kept without food for 48 hours. Then
the usual medical abdominal incisions having been made a loop of ileum
about 6 inches in length in each case was taken and a ligature was
loosely applied around this. The bowel was then punctured with a hypodermic needle
2 inches above the upper end of the loop
of bowel. Then in the case of the Cat
1 1/2 c.c. and in that of the rabbit
7 c.c. of an opaque mixture of staphy-
lococcus aureus in water were injected
and allowed to flow downwards into the
loop of bowel which then filled fairly
fully. This loop was then ligatured
for 4 1/2 hours in the case of the cat
and for 3 1/2 in that of the rabbit.
Both were purplish but shrunken when released
no bad effect followed. The organisms
were apparently digested, absorbed, or
evacuated with the defecation. Certainly
they were unable to attack the mucous
surface or to penetrate the bowel wall.
This result is only what one would
a priori expect for if organisms
could penetrate debilitated congested
bowel our mortality from strangu-
lated hernia would be appalling.
As I have stated before I am certain that organisms can only penetrate the bowel when it is quite necrotic and various experiments of mine bear out this view entirely.

The next series of experiments were undertaken with some of the normal fluids of the body, both with and without the addition of Staphylococcus aureus. In the first place we will take a series of 12 experiments made with blood. Of these 12 experiments 9 were performed on rabbits and 3 on cats.

Experiment 64. The spleen of a rabbit was removed and the vessel of the pedicle were allowed to bleed outside the peritoneal cavity for a short time until some 5 c.c. of blood were shed. These vessels were then ligatured and the pedicle returned to the abdomen and with it the three 5 c.c. of blood which had coagulated by this time. 10 minutes later 5 c.c. of Staphylococcus aureus fluid were injected into the peritoneal cavity. The abdominal incision having in the meantime been sutured and dressed. One animal
died within 24 hours and the post-mortem examination revealed the presence of extensive fibrinous-purulent peritonitis which was especially severe around the pieces of blood clot. Now it may be said that the ligatured pedicle was the starting point of the mischief and that the clot had little or nothing to do with the causation of the peritonitis.

Against this would urge the two following facts: (1) that the peritonitis in the neighborhood of the clot was far more severe than around the splenic pedicle; and (2) that in the case of this splenic pedicle acting as the cul-de-sac one would not have expected death judging from the other experiments before the 3rd day at the earliest. Still as there was ground for doubt I performed these further experiments.

Experiment 65. In this case I injected 2 c.c. of living blood obtained from the cut femoral artery of a rabbit into the peritoneal cavity of another rabbit. It is particularly worth attention to the fact that it was living blood which was introduced into the peritoneal cavity 15 minutes later. I in-
jected 1 c.c. of staphylococcus aureus fluid.

The animal made a complete recovery.

Experiment 66 in this case operated
as follows. I dissected out the common
carotid artery of a living rabbit in its
whole course and cutting its upper
end I introduced it into the perito-
tenal cavity of another rabbit keeping
it there until the giver of the blood
was nearly dead from shock.

I cannot tell exactly how much
blood escaped but I think we
may safely assume that it was
not less than 15 or 20 c.c.

Then 60 minutes later I
injected 1 c.c. of staphylococcus
aureus fluid having previously
sutured and dressed the abdominal incision.

The animal died on the 4th day of
general suppurative peritonitis. The
peritoneum was covered with the
micrococi and so also all the
organs. There was not a trace of
blood to be seen in the peritoneal
cavity, though the anterior glands
and the diaphragmatic lymph
spaces were full of blood corpuscles
and debris. As these experiments
were of great interest from a Surgical
point of view it was deemed well to carry them further. Accordingly I performed the following experiments.

Experiment 67. In this case I intro-
duced into the abdomen 5 c.c. of a blood
coagulum and 3 c.c. of staphylococcus
fluid. The rabbit died within 24 hours.
in exactly the same manner as in Case
64. To further test this very rapid
occurrence of death after the introduction
of coagulated blood together with organisms
and the much slower course of the
peritonitis when fluid instead of coagulated
blood is employed. I conducted Exper-
iments 68, 69, 70, 71 and 72.

Experiment 68. Some 15 to 20 c.c.
of fresh blood from the caudal part
of a rabbit was allowed to flow into the
peritoneal cavity of another rabbit.
After closure of the wound 1 c.c. of
staphylococcus fluid was injected into
the peritoneal cavity. The animal died
on the 3rd day from general suppurative
peritonitis. There was very violent
inflammation with much pus forma-
tion. In this case merely a trace
of blood remained in the abdomen (not
coagulated but looking as if it had
been smeared over the bowel in parts)
In the previous case in which death under similar circumstances had resulted in 4 days, there was not a trace of blood to be seen in the peritoneal cavity. In both of those cases, blood was found in the lymphatics of the diaphragm and in the anterior mediastinal glands.

Experiment 69. In this case 1 c.c. of fluid blood was introduced into the abdomen of a rabbit and then 5 minutes later 1 c.c. of staphylococcus fluid was injected. The animal remained quite well.

Experiment 70. Introduction of 1 c.c. of caseous blood clot broken into pieces then 10 minutes later 1 c.c. of staphylococcus amnios fluid was injected intraperitoneally. The animal died of peritonitis within 24 hours.

Experiment 71. In this experiment a very small amount of coagulum i.e. a piece the size of a pea was introduced floating in 1 c.c. of staphylococcus fluid into the peritoneal cavity. The animal died of peritonitis within 24 hours. The peritonitis was especially marked in the neighborhood of the clot which was adherent to the bowel.

Experiment 72. As a control expen
I introduced 5 c.c. of coagulated blood into a rabbit’s abdomen. The animal made a perfect recovery.

In order to more fully prove these surprising results I carried out the next 2 experiments on cats.

Experiment 73. 5 c.c. of coagulated blood were introduced with all antisepsic precautions into the abdomen of a cat. The animal recovered completely. I was anxious to ascertain what had become of the coagulum but the cat on the 5th day, the very day on which it was to have been killed and examined, escaped.

Experiment 74. 25 c.c. of living blood from the carotid of a cat was allowed to flow into the peritoneal cavity of another cat. Then the abdominal incision having been sutured and dressed 1 c.c. of staphylococcus fluid was injected intra peritoneally. The animal died on the 5th day of acute supplicative peritonitis.

Experiment 75. 2 c.c. of blood all were introduced with 1 c.c. of staphylococcus fluid into the peritoneal cavity of a cat. The animal died on the 2nd day.
Nay; these 12 experiments astonishing in several respects call urgently for our earnest consideration.

But the various physiological questions which here generally crop up I cannot enter except to refer for a moment to the consideration, that several of these experiments give to the theory first suggested by Thunhammer (with regard to the tunica vasculosa testis) and since more convincingly demonstrated by Winter that the endothelium of a serous membrane exercises a retarding influence upon the coagulation of the blood.

A short résumé of these twelve experiments may be given as follows.

1. That the introduction of living blood even when accompanied by pathogenic organisms is without danger when very small quantities are employed, but that if a quantity too great to be absorbed in a short time be injected that peritonitis results for the organisms have thus been given time to exert the absorptive power of the peritoneum.

2. That it is an extraordinary, but still true fact, that although in parts excitation is going on by other parts of the peritoneum...
the blood still testify because it is concreted
in a space which being lined by endothelium
resembles a blood vessel, is absorbed into the
blood plasma. When we turn to the case of blood clot I feel
I can give no satisfactory explanation of
the suddenness of death in those cases
in which along with the blood clot
stephanoceous curvus was introduced into the peritoneal cavity. Of course
it is easily conceivable that death
should occur even when minute
quantities only of clot are present
for these serve as a food ground for the
organisms where they can settle and direct
the absorption. Still why should death occur
so rapidly (within 24 hours)? Can it be
that organisms acquire a special
virulence when grown in blood clot
or must we attribute it to the action
of the fibrinolysis... I know not.
I can only state the facts of the case
as I found them by experiment
and confess that I am able to give no
satisfactory explanation of them.
Since writing the above I have received
a letter from Prof. Dr. in which he
says that he was so struck with the
extraordinary difference between my
results with fluid and with coagulated
blood that he has himself with one of his pupils undertaken an experimental research on the subject: I hope that this may clear up many of the difficulties connected with the subject: difficulties to which I believe attention has never before been drawn. I cannot help feeling that these experiments show that it is quite unjustifiable to transfuse blood into the peritoneal cavity for the risks of conveyance organisms and of the blood clotting are great and the dangers if enough blood to be of any use be injected are tremendous.

Let me quote such a case. Insulin injected into the peritoneal cavity of a human being suffering from anaemia 40 c.c. of defibrinated blood his evil effects followed though no improvement—in the case resulted. Emboldened by the first attempt Insulin injected 130 c.c. of blood. Acute peritonitis resulted and death occurred on the 5th day. My explanation of this is that in this case pyogenic organisms obtained access to the peritoneum.
during the injection of the blood and thus
flooding in the slowly absorbed fluid
had ample time to start peritonitis before the blood was all absorbed.
This case agrees with the results of
my experiments. It is a matter
of regret to me now that I did
not experiment at all with defibri-
dated blood. I think that after
reading these results Surgeons would
be more careful than ever to get rid
of all coagula in a septic wound.

The next series of experiments were
undertaken with urine with and
without the admixture of Staphylo-
cocci.

Experiment 8. 76 and 77 injected
in these cases in the 76, 7 and
in the 77 12 c.c. of fresh healthy
human urine into the abdominal
cavities of two rabbits. The animals
both recovered without a bad symptom.

Experiment 78. 70 c.c. of healthy
human urine were injected into
the peritoneal cavity of a cat. The animal
recovered completely.

Experiment 79 10 c.c. 79 rabbits
urine were injected into the
abdominal cavity of a rabbit. The animal recovered completely.

These experiments show that healthy urine is not an irritant fluid but
on the contrary a bland unirritating secretion which when injected
into the peritoneal cavity causes no irritation or inflammation.

Experiments 80, 81, 82, 83, and 84.

In these 5 experiments different quantities of staphylococcus aureus
and human urine were injected intraperitoneally in rabbits.

The 1st 4 c.c. of urine containing 6 c.c.

- 81 4 c.c. __________ 3 c.c.
- 82 4 c.c. __________ amount unknown
- 83 6 c.c. __________ 3 c.c.
- 84 8 c.c. __________ 4 c.c.

Of these 5 experiments were
performed with the 5 different
cultures of staphylococcus aureus I
possessed and they show conclusively
the harmlessness of the procedure
for excepting no 83 who was a weak
animal and died 3 days after the
injection all the animals recovered
completely. In the case of no 83 the
post-mortem showed no peri-
tions, but extensive pustules of the left lung which was riddled with cavities, and also consolidation of the right lung.

From these experiments we may conclude that normal human urine even when mixed with staphylococcus aureus in considerable quantity does not produce peritonitis when injected into the peritoneal cavity of a rabbit. It has just the same effect or rather it exhibits the same absence of result as the same quantity of water mixed with organisms gave.

The next series of experiments, 5 in number concern ammoniacal urine. Healthy human urine was kept at body temperature for 9 days until it gave a distinct smell of ammonia, was dull and abundant in organisms.

Experiments 85 and 86

In these two cases injected in his 85, 3 c.c. and in 86 10 c.c. of ammoniacal urine. In 85 died 5 days afterwards. His 86 4 days from the date of the experiment.
micro-organisms of various kinds were found in the peritoneal cavities. Both animals died of general peritonitis.

Experiment 87. 20 cc of ammoniacal human urine were injected into the abdomen of a cat which died on the 4th day after the injection. Post-mortem examination revealed acute suppurative peritonitis.

Experiments 88 and 89.
In these cases in 88 2 and in 89 5 0.2 cc of staphylococci were mixed with 5 cc of ammoniacal human urine and injected into the peritoneal cavities of rabbits. The animals both died on the 3rd day after the injection.

Thus we see that whereas normal human urine is not an irritant to the tissues of the peritoneum, ammoniacal urine has very irritant qualities. Ammoniacal human urine always contains organisms and hence its fatal effect when it gains entrance into the peritoneal cavity. One am-
macerated urine in virtue of its irritant qualities depresses the tissue and the organisms thus obtain opportunity to settle thereon and produce their harmful effect.

Before leaving this section I must describe Experiment 40 in which I performed an experiment previously done by Sarnitz and Israel. The abdomen of a rabbit having been opened in the middle line I exposed one inch of the right ureter and stitched the cut end nearest to the kidney to the peritoneum of the posterior abdominal wall and lower end of kidney by stitches so as to keep this cut opening patent. The animal never seemed at all ill. It lived for 3 weeks apparently quite, and when killed and examined the only change observable was that the peritoneum in the right renal region was thickened and had lost its shiny appearance. There was no free urine to be seen in the peritoneal cavity though the opening in the ureter was quite patent. Washing however
From the peritoneal cavity revealed after treatment with nitric acid crystals of nitrate of urea thus affording proof that the fluid had been recently urine in the peritoneal cavity. This case confirms the opinion of Franks and Israel that healthy urine can flow for weeks into the peritoneal cavity without causing death.

As soon as one day if urine is secreted by the kidney it flows into the peritoneal cavity and is at once absorbed. Yudnline however to the opinion that this would not go on indefinitely and that in time the peritoneum would become so changed that it would cease to absorb for I am sure it was somewhat changed in 3 weeks in this case.

In the next experiment I injected 20 c.c. of pus containing as I afterwards proved a pure culture of staphylococcus maura into the peritoneal cavity of a rabbit. The animal died on the second day of acute general peritonitis which was
commencing to be purulent.

**Experiment 92**. One drop of pus mixed with 2 c.c. of water and injected into the peritoneal cavity gave no result. This is in agreement with Goppert's results. This we see that a large amount of acute pus introduced into the healthy abdomen causes peritonitis and death. Still a minimal amount (one drop) does not do so.

Having thus far discussed only the normal abdomen, let us turn our attention now to three cases of intense interest for they are concerned with the introduction of staphylococcus aureus into the abnormal abdomen.

**Experiment 93**. This case occurred true before I commenced this series of experiments and therefore did not inject organisms alone but also along with them a few pus cells. An enormous cat-intensely dropsical was anesthetised. The abdomen was greatly distended; fluctuation could not
be very distinctly felt owing to the tautness of the abdomen. I introduced an aspirator needle and drew off 30 c.c.m. of peritoneal fluid which was straw coloured and contained an enormous amount of albumen. I then injected into the peritoneal cavity a drop of pus taken from a case of acute suppurative peritonitis shaken up in water (one drachm). One animal died of acute suppurative peritonitis on the 4th day. From the peritoneal pus a pure cultivation of staphylococcus aureus was obtained.

Although I did not examine the organisms microscopically I found that there was cirrhosis of the liver and renal incontinence. The flap of the mitral valve being apparently calcified and partly broken off.

Experiment 94. A rabbit in which when making a small opening into the abdominal wall I perceived the peritoneal cavity to contain a large quantity of a watery fluid which on being tested
was found to contain a considerable quantity of albumen. I then injected 1 c.c. of staphylococcus fluid into the peritoneal cavity after closing the abdominal incision. The animal died on the 3rd day of typical suppurative peritonitis.

Experiment 95. An ascitic cat with marked cirrhosis of the liver seemingly and an uncommon disease in our feline cats was anaesthetized and a needle of a hypodermic syringe having been introduced into the peritoneal cavity a small amount of fluid (straw coloured) was drawn off. The c.c. of staphylococcus fluid was then injected. The animal died on the 4th day of suppurative peritonitis.

These cases unfortunately only 3 in number suffice I think to prove that the introduction of staphylococci into the abdominal peritoneal cavity where there is an amount of albuminous fluid capable of acting as a fluid food and
where the absorption is prevented can multiply rapidly and by the formation of their waste products irritate the tissue of the peritoneum, once having gained a foothold, they continue to spread and cause general peritonitis.

I am writing for the present to tell two cases in which various other secretions and excretions of the body were injected extra peritonitically. I could wish now to mention experiments undertaken with other organisms. Staphylococcus pyogenes, bacillus pyocyaneus, and the bacillus of foci in rabbit.

Staphylococcus pyogenes with these organisms the following experiments were made which gave in my hands results almost identical in kind though less active independently than with Staphylococcus aureus. I have also stated very fresh potent cultures obtained from two cases of septic peritonitis. One is a case of peritonitis resulting after
abdominal section in a woman in the Surgical Clinic, the other a case arising in a newborn child from gangrene of the umbilical cord.

As these experiments are mere repetitions of those done with staphylococci and already discussed I shall describe them very briefly.

Experiment 96 5 c.c. of a suspension of Staphylococcus pyogenes in water were injected into the peritoneal cavity of a rabbit. The animal made an uninterrupted recovery and on being killed 14 days later no trace of peritonitis was to be seen.

Experiment 97 Exactly the same experiment was performed upon a cat with the same result.

Experiment 98 8 c.c. of Staphylococcus fluid were injected intraperitoneally in a guinea-pig. The animal recovered entirely.

Experiment 99 A sterile gelatin culture of Staphylococcus pyogenes liquefied at 38 C. was injected
into the peritoneal cavity of a rabbit. The animal died of acute purulent peritonitis on the 5th day. In the abdomen, streptococci were found though there was very little pus formation. The inflammation was more of a fibrous, deep, thickeened kind. Experiment 100. 5 c.c. of healthy human urine containing 8,000,000 streptococci were injected into the peritoneal cavity of a rabbit. The animal recovered completely.

Experiment 101. 5 c.c. of ammoniacal human urine containing 8,000,000 streptococci plus pyramids were injected into the peritoneal cavity of a rabbit. The animal died of general fibrous purulent peritonitis on the 3rd day. In this case the inflammation was far more purulent than is usually the case with streptococci, but this was doubtless due to the fact that there was an admixture of other organisms in the ammoniacal urine.

Experiments 102 and 103. In these cases the bowell was...
ligatured for a certain time and then 12 millims of staphylococci fluid were injected intravenously. Experiment 102 A small segment of small intestine was ligatured for 3½ hours and then when cherry red in colour it was released. 12 millims of staphylococci fluid were now injected into the veins of the sars. The animal died on the 2nd day of peritonitis which was far more fibrinous than febrile.

Experiment 103 A loop of small intestine was ligatured for 5 hours and then released. The bowel was nearly black in colour was then released and 12 millims of staphylococci fluid were then injected into the veins of the sars of the rabbit. The animal as in case 102 died on the 2nd day with a peritonitis more fibrinous than febrile.

Experiment 104 A square inch of the parietal peritoneum was torn off and then 5 c.c. of staphylococci fluid were injected.
intra-peritoneally. The animal died on the 5th day of general peritonitis which was fibrinous purulent in nature.

Now these 9 experiments taken together show that *Staphylococcus pyogenes* although it does not act quite so rapidly as a rabbit's peritoneum never the less produces in the end very much the same result as *Staphylococcus*. There can be little doubt of its pathogenic qualities. I have already in an earlier part of this Thesis indicated that as regards the human body it is a far more dangerous organism than is the *Staphylococcus aureus*. In the human subject peritonitis of the fibrinous form i.e. caused by *Staphylococcus* may prove very rapidly fatal as in the case of the woman from whom my organisms were obtained who died in 28 hours after abdominal section and in whose case the peritonitis was very general. In the rabbit the *Staphylococcus* seem to act
less violently though scarcely less dangerously. The inflammatory process as seen in the peritoneum of the rabbit which is caused by Shpòboces is a fibrinous diphteritic inflammation as opposed to the typical supplicative inflammation caused by staphylococcus aureus. In every experiment the final result was the same i.e. death or recovery whether Shpòboces or staphylococcus was employed but in the case of the former death when it resulted occurred always after a longer interval than was the case when staphylococcus was the organism used. I may mention that in the 4 cases in which I examined the organs for shpòboces I found them in the kidney, spleen, liver and in 2 cases in the bone marrow of the femur. In every case they were found in the peritoneal exudation.

The next 9 experiments I carried out in order to test Parlow'ski's statement that Bacillus pyocyaneus and the Bacillus of Rabbitfores cause when injected along with water
into the peritoneal cavity, death from peritonitis.

Experiments 105 and 106

6 c.c. of a mixture of Bacillus pyocyaneus in water containing 3 c.c.
of the organisms were injected into the peritoneal cavities of two rabbits.
Both animals recovered without a backward symptom and one being killed and
examined 10 days later showed no trace of peritonitis.

Experiment 107: 5 c.c. of water

containing 5 c.c. of the bacillus of rabbits' feces (a peculiar short-stick
bacillus) were injected into the abdomen of a rabbit. The animal
recovered perfectly and the peritoneal cavity being examined 10 days later
showed no trace of peritonitis.

In these experiments again my
results were absolutely at variance
with those of Pasteur's.

Having described several cases in
which various organisms were
injected from pure cultivation
I now proceed to a series of experi-
ments which gave a very
different result. Instead of taking
the organisms from a pure culture I took them contained in the fluid (purulent
sero-purulent etc) which I found in the peritoneal cavity in cases of death from
peritonitis. These results seem to
contradict at first sight many of
the foregoing but when to show that
in reality they were carried out under
very different circumstances. In the
earlier cases I introduced pure cultures
in fluids which contained an
irritant substance whereas in these
experiments now to be detailed
organisms were introduced surrounded
by their irritant stroma and in fluids
which are not readily absorbable such
as pus sero-purulent fluid etc.
Thus these experiments seem to me
to be intermediate between the other
series of experiments in both of which
death was produced. In the one hand
they bear a close resemblance to the
cases in which tuberculosis as an
irritant was applied to the peritoneum
and then pyogenic organisms
which were introduced into peritoneally were
enabled to settle and the violence thus
caused. In these present experiments
of course the stromas are the viri-
tions which depress the tissues. On the other hand, they resemble those experiments in which death was caused by the introduction of organisms contained in media which are not easily absorbed as bouillon, etc. In this case the organisms have time to act before the fluid in which they are suspended is entirely absorbed. In the present cases pus
non-pusulent fluid as correspond to the less slowly absorbable media. Another point of importance is that the organisms when introduced into the peritoneal cavity were at their maximum point of aggressiveness. They had just produced severe inflammation ending in death and were thus far more potent than organisms which had been raised through two generations on gelatine on agar.

Experiment 108. 3 c.c. of fluid obtained from the peritoneal cavity of a child who had died from peritonitis, were injected intraperitoneally into a rabbit. The fluid swarmed with Staphylococcus pyogenes but contained
also a few bacilli. The animal died from peritonitis faecium produced in 72 hours. Immediately after the death of this animal (i.e. if the fluid contained in its abdominal cavity was injected (Experiment 109) into that of another rabbit. This latter animal died within 60 hours of acute general peritonitis. The peritoneal exudate examined microscopically revealed the presence of a nearly pure culture of Staphylococcus pyogenes with but very few bacilli.

These two experiments having been performed with fluid containing staphylococci, I resolved to repeat them with peritonitis fluid containing staphylococci. I therefore chose the peritoneal fluid from Case 74. As this was was rather thick I mixed it with a little water, injected 2 c.c. of pus with 1 c.c. of water into the peritoneal cavity of a rabbit. The animal died early on the 2nd day of acute supplicative peritonitis. All the organs swarmed with staphylococci.
As soon as possible after death, 2 c.c. of the fluid from the peritoneal cavity were injected (Experiment 111) into the peritoneal cavity of another rabbit who likewise died in exactly the same manner on the 2nd day.

These experiments are of extreme importance for they show us I think clearly that pus from an acute suppurative peritonitis is in its effect a vastly different thing from the mere organisms it contains. Pus and peritonitis fluids from cases of acute inflammations have an immense power of causing peritonitis whereas the organisms can act only when circumstances are favourable to them.

Experiment 112. Having found that whereas 2 c.c. of pus from an acute peritonitis was always productive of death from acute peritonitis when injected intraperitoneally I rushed to try the effect of a minimal quantity. I therefore took 1 minims of peritoneal pus from case 111 mixed with 2 c.c. of water and then injected it into the peritoneal cavity of another rabbit.
Recovery without any illness resulted. This result is quite in agreement with Case 92 and with Osgood's experiments in which he found that 1 minim of antipeur injected intraperitoneally was borne by the animal. The explanation may be that in 1 minim of pus there is not a sufficient amount of pneumonic to enable it to act as an irritant or in any case that the pneumonic was so diluted with water that it no longer possessed irritating qualities.

Experiment 113 5 c.c. of pus collected from some of these above mentioned cases were mixed with 8 c.c. of water and the whole was sterilised for 1 hour in a steam steriliser and successive days in order to destroy all trace of organised life. The mixture was then introduced into the peritoneal cavity of a rabbit. The animal on recovering from the anaesthetic seemed to be in pain for some few minutes as it lay doubled up. It soon however recovered perfectly. This experiment
was undertaken to show what effect the filtrates in acute peritonitis pus had upon the organisms therein contained. I say however with the slightest sketch in this result for I had boiled the fluid and therefore it is impossible to say what change I had thereby produced.

That there is at least one ferment in pus which possesses lytic properties has been proved by the experiments of Luber but I fear we will not be able to advance our knowledge concerning the action of these substances present in pus until we can separate the organisms from their waste products in some way without subjecting the latter to boiling or chemical processes for we can never tell how much we have by these processes changed the original qualities of the substances.

The next two experiments were carried out with human sputum. Experiment 114 8 cc. of normal healthy sputum was injected into the peritoneal cavity of a rabbit. The animal
Recovered completely.

Experiment 115. 3 c.c. of excessively tough viscid sputum from any person suffering from an attack of tuberculosis was introduced into the abdomen of a rabbit. The animal died on the 4th day of paralysis. I think that in this case there can be but little doubt that the death was due as much to the fact that there were more organisms present in the sputum as to the very viscid nature of the sputum which offered such a resistance to the absorptive power of the peritoneum that the organisms had time to develop and commence their attack before the viscid medium in which they were contained was absorbed. I may here repeat that Staply-bacillus aureus is as a rule, present in the normal human sputum (Biardi).

The next series of experiments were undertaken with focal matter.

Experiment 116. 2 c.c. of liquid
Secal matter taken from the lower part of the ileum of a rabbit was mixed with 4 c.c. of rabbit urine and injected into the abdomen. The animal died on the 3rd day of acute suppurative peritonitis. Various organisms were present in the peritoneal pus.

Experiment 117/20c. A very small quantity of fluid fecal matter (rabbit) was introduced into the peritoneal cavity of a rabbit. The animal lived for 12 days and then died. Acute peritonitis was found at the post-mortem examination. The peritoneum was normal but there was extensive tubercular disease of the lungs.

Experiment 118. An enormous quantity of fluid fecal matter 12 c.c. was injected into the abdomen of a rabbit. The animal died next day. The post-mortem examination revealed the presence of chronic necrotic general peritonitis which was especially marked around the flanks of fecal matter.
All the fluid had been absorbed.

**Experiment 119.** In this case 4 grammes of semi-fluid human foecal matter obtained from the lower part of the ileum of a post-mortem subject were introduced into the abdomen of a rabbit. The animal died on the 3rd day. Peritonitis was found especially severe around the flakes of foecal matter.

**Experiment 120.** 4 grammes of foecal matter taken from the lower part of the ileum of a child who died of diphtheria when only a few days old were introduced into the peritoneal cavity of a rabbit. The animal remained to all appearance quite well but after I had left Göttingen I heard from Prof. Böttcher that an abscess had burst through the abdominal wall and discharged the debris of this foecal mass balled in pus. Rapid recovery ensued.
This was evidently a case of complete absence of the peritoneum.

Experiment 121. A recently passed fecal mass from one rabbit was introduced into the peritoneal cavity of another. No peritonitis resulted. The rabbit was killed 8 days afterward, and on examination the bulk of the fecal material was found impossible but otherwise unchanged in the left iliac fossa.

The following experiments were carried out with filtered fecal matter for Pasteur to maintain that the filtrate cannot produce peritonitis. I may here remark that the filtrate always contains many microorganisms though in course vastly less than the residue.

Experiment 122. 2 c.c. of fluid fecal matter (human) were mixed with 2 c.c. of water and filtered. When 2 c.c. had passed through the filter they were injected into the peritoneal cavity of a rabbit. The animal died 7 days later. Abdmon was normal peritoneum.
in no way influenced. On opening the thorax an enormous abscess of the anterior mediastinum, purulent pericarditis and double empyema were found. No tubercle bacilli could be found in any of the affected parts and the lungs showed no evidence of tubercular disease. My opinion is that the mediastinal abscess was caused by the absorption of the faecal fluid (filtered) by the mediastinal glands from the peritoneum as will be seen in a later experiment. And that the other purulent conditions had their origin in the mediastinal abscess. This of course is merely a guess at the etiology but I think it likely from what will appear later that it is a correct one. The pus was loaded with organisms of various sorts especially the short thick bacilli.

Experiment 123. In this case 20 c.c. of filtered faecal matter was injected into the abdomen of a rabbit. The animal died on the 4th day of purulent peritonitis. Bacilli and micrococci were found.
swarming in the fluid in the peritoneum especially the short thick focal bacilli of Scharitz.

Experiment 124. 5 c.c. of fluid similar to that used in the above experiment were injected into the peritoneal cavity of a rabbit. The animal recovered completely. Inspector was caused. The difference in the result of these two experiments seems to consist in the fact that in the former case the quantity of fluid was so large that it was not absorbed in time before the organisms began to act whereas in case 124 the quantity of fluid being so much smaller it was entirely absorbed in time i.e. before the organisms had begin to take effect.

I would wish to draw attention to the fact that injection of filtered focal matter is a very different thing to that of a mixture of organisms in water for in the former there are held in solution digestive ferments and other substances which tend to aid the action of the
organisms. Still Pawlowsky maintains that the former is harmless, the latter deadly.

Experiment 125: 4 grammes of human faeces were mixed with 2 c.c. of water and the whole mass filtered and the filtrate injected intraperitoneally in a rabbit. The animal recovered completely.

The deductions which I think one may draw from the above experiments are that it is possible for a minimal quantity of faecal matter to escape into the abdomen (solid being less unfavourable prognosis than fluid) without setting up fatal symptoms, but that the entrance of a moderately small amount will most promptly removed in all probability kill the patient. Filtered faecal matter in small quantity is not productive of death, but in large quantity does produce it.

In order to test the effect of the digestive fluids without the admixture of bacteria I infected (Experiment 126) 1/4 grammes of
pepsin in 5 c.c. of a 0.2 per cent solution of hydrochloric acid. The animal recovered completely and the peritoneum examined 10 days later showed no trace of peritonitis.

Experiment 127 In this case 1/4 gramme of pepsin was injected in 5 c.c. of the 0.2% HCl solution. The animal appeared to ingest pain for 1/2 hour but eventually made a complete recovery.

Experiment 128 1/8 gramme of trypsin in a 1 per cent solution of 0.9% Co2 (5 c.c.) was injected into the peritoneal cavity of a rabbit. The animal recovered.

Experiment 129 1/4 gramme trypsin in 5 c.c. of a 1 per cent-
lar Co2 solution was injected intra-
peritoneally in a rabbit. The animal
recovered entirely.

Now the results of these 4 experiments again are entirely at variance with Pawlow's results. Yet I am sure I am correct in this
matter for my friend Dr. Bernauer repeated my experiments with pepson and trypsin at my request and used substances obtained from other sources than rumin. He concurred entirely with my results. Prof. Ott also from experiments he had made some years ago informed me that he is quite sure that pancreatic juice injected in moderate amount into the peritoneal cavity does not cause peritonitis.

These results show very clearly the slight influence which digestive ferments have on living tissues. I have very little doubt however that they produce some irritation and thus predispose the tissue of the peritoneum to the action of the organisms which are always present when gastric or intestinal contents escape into the peritoneal cavity in disease.

Having regard to the importance at the present moment of hepatic and gall bladder surgery the following 7 experiments were
Experiments 130 and 131. A small piece of the wall of the gall bladder of two rabbits was excised and the abdominal incisions were closed. The bile was thus allowed to flow into the peritoneal cavities of these two animals. Both these animals died of intense haemorrhagic peritonitis. No pus in organisms were found in the peritoneal sac. Death in the first experiment occurred in 24 in the second within 48 hours.

Experiment 132 I repeated the above experiment on a cat in whom I had much difficulty in finding the gall bladder owing to some old standing adhesions between it and the intestine. The result was the same. Death occurred from peritonitis on the second day. The peritonitis was again marked by the haemorrhagic type.

Experiments 133 and 134
In these cases 15 and 10 cc.
respectively of human bile were injected into the peritoneal cavities of two rabbits. Both died rapidly of haemorrhagic peritonitis within 24 hours. The animals after injection lay doubled up and evidently in great pain.

Experiments 135 and 136.
In the former of these 1 c.c. of aseptic human bile and in the latter 1 c.c. of rabbit bile was injected intraperitoneally into two rabbits. Both animals remained well.

Before these experiments were undertaken there existed a great doubt whether aseptic bile escaping into the peritoneal cavity could cause death by peritonitis or no. Grassi and Boggi maintained that bile in the peritoneal cavity was harmless, whereas both and others were equally certain that it caused haemorrhagic peritonitis. I think my experiments clear up the doubt on this head. They show that whilst a continuous
flow of bile into the peritoneal cavity on
the single injection of a large quantity
causes death from hemorrhagic peritonitis
a small amount can be borne with
impunity (i.e.e). Therefore in
this case it is the dose which is the im-
portant matter.

Finding that bile in large amounts acted
as an irritant causing peritonitis,
I wished to try the effect of a chemically
pure irritant and therefore
Experiment 137 I injected 40 minims
of pure turpentine into the abdomen
of a rabbit. The animal cried so pitifully
and was evidently in great pain
that it was obliged to keep it for ½ hour
under the influence of ether. It then ap-
peared very much better and after 1 hour
began to run about and apparently
was not much the worse. On my arrival
next morning at the Institute I found
the animal dead and the abdomen
disgorgously distended. On opening the
abdomen I saw the most intense congestion
have ever seen in any organ. The
small intestine was for some inches
almost of an arterial line and
myriads of minute vessels normally
invisible were seen with great distinctness.
In this case I noticed that I had also observed in the cases of death from injection of bile a rather large amount of blood jelly in the abdomen. This was a peculiar substance I had never before observed. It consisted of blood corpuscles entangled in fibrous masses. It was much lighter in colour and less firm than blood clot.

Experiment 138. 10 minims of turpentine were mixed with 60 of sterile bocceus fluid and then 40 of water were added and the whole injected intraperitoneally. Much to my astonishment no effect was produced. I imagined that turpentine as a strong irritant would depress the tone of the peritoneum and would thus cause a loss minoris resistance of which the organisms would take advantage. Not being able to understand why this had not occurred, I referred to an experiment of Grandy in which he also had obtained no result. This he attributed to the fact that turpentine was not a sufficiently strong
important. Having seen that this could not be the case from my experience of the previous case I examined more closely into the matter and found that turpentine is a very powerful antiseptic when my case was clear. I had mixed the styphloepcess with turpentine and thus killed them before injection. Small wonder then that no suppuration inflammation had resulted. The turpentine present I had previously diluted with water before injecting it so that it had not the violent effect of pure turpentine and from this cause as well, less also from the fact that it was present only in very small quantity (10 minims) it had not set up haemorrhagic peritonitis. I deduce from the 2 above experiments that whereas a small amount of pure turpentine can cause rapid death from peritonitis (haemorrhagic) when diluted with water it can act as an efficient antiseptic and may well be employed for want of a better in cases of emergency. I am quite of the opinion of Gravity that pure turpentine is at least as strong an antiseptic as carbolic acid and corrosive
substitute in their usual surgical solutions (1 in 40 carbolic and 1 to 2000 salicylic).

The remaining experiments must very rapidly dismiss for none of them produced peritonitis the subject of this thesis.

Experiment 139. 20 c.c. of ordinary tap water injected into the abdomen of a rabbit were rapidly absorbed and no ill effect resulted.

Experiment 140. 60 c.c. of cows milk injected into the peritoneal cavity. No peritonitis ensued. I surmised that I had mixed a considerable number of staphylococci into the milk in order to see the effect on the peritoneum. I rather fancy the result would have been different for the milk is probably not very quickly absorbed. This being quite fresh warm milk probably contained comparatively few organisms.

Experiment 141. As a control experiment to some of the earlier ones 60 c.c. of sterilized meat...
Peptide gelatin liquefied at 33°C was injected intra-peritoneally into a rabbit. The animal recovered without any trace of pertainitis. When examined 16 days later, the abdomen appeared absolutely normal.

Experiment 142. 1 gramme of finely powdered charcoal suspended in 10 cc of water was injected into the peritoneal cavity of a rabbit. The animal was killed on the 6th day after the injection. Scarcey a trace of charcoal was to be seen in the abdomen, but the anterior mediastinal glands were black with the pigment. The mesenteric glands did not show a trace of black pigment nor did the posterior mediastinal glands.

Experiment 143. The previous experiment was repeated on another rabbit. This was killed and examined on the 3rd day. The result was very similar except that there was a considerable amount of charcoal present in the abdominal cavity. The glands in this case also of the anterior mediastinum were choked with charcoal granules where as the mesenteric and posterior medi.
astriol glands were perfectly free from pigmentation.

These two cases lead up to the generalisation that the glands of the anterior mediastinum serve for the filtration of substances absorbed from the peritoneal cavity where as the mesenteric glands subserve only the function of collecting from the alimentary tract and viscera.

This of course is only demonstrated in the rabbit, but it is quite likely that it is the case in man.

Experiment 146 Having in a fatal case of peritonitis (human) found that the suffering of the patient were immensely relieved or entirely alleviated by puncturing the bowel and thus allowing the escape of the distending gas I wished to ascertain whether this puncturing the bowel by a fine needle was devoid of danger. I therefore made a small abdominal incision and drawing successively a portion of stomach small and large intestine out of the abdomen through the incision I punctured each twice with the fine needle. I then closed the abdomen and the animal recovered completely. The puncture wound seems to close up.
immediately after the withdrawal of the needle. This being the case there can be no reason why puncture should not be resorted to in hopeless cases as a "dernier resort" for it alleviates very much the sufferings of the patient quickly and effectively for a time at least.

Experiment 165.

Wishing to test the statement of Granzi that cold does not cause peritonitis, and as the rabbit is rather a chilly animal, he used a rabbit whose rectal temperature at that time was 38.6°C. I then opened the abdomen and flushed the whole cavity with cold water for 1 hour. At the end of this time the animal was reactionless. Rectal temperature was only 29.6°C. I then as the animal was sinking rapidly flushed out the abdomen for 1 hour with warm water at the temperature of 40-41°C. The animal improved rapidly. At the end of this time the rectal temperature was 35.8°C. The abdominal incision was then closed and the animal placed before a fire in warm blankets. It recovered completely. On being examined 10 days later the peritonitis appeared normal. No trace of peritonitis.
was to be seen. Now in this case the
great change of temperature had seemingly
no tendency to cause inflammation.

Experiment 146. A thin rabbit was shaven
over the whole anterior abdominal wall
over the shaved area were then placed
heated poultices as warm as the animal
could bear them. After 2 hours of this
Treatment the warm poultices were
suddenly removed and iced compresses
were applied. These were changed every
3 minutes for 1 hour.

Now if cold can cause peritonitis
Surely it should have done so in this
case. For here we had the warm
abdomen with its vessels dilated
suddenly and severely chilled by
ice cold. Yet the only effect of the appli-
cation of this intense cold seemed to
lie to increase the peritonitis exceedingly.
The animal recovered perfectly and on
the abdomen being examined 10 days
later no trace of peritonitis was to be
seen.

Experiment 147. In order to show
the depressing effect of the rapid loss
of heat which the body undergoes
in cases in which the bowel is exposed and the patient under the influence of an anaesthetic, I performed the following experiment. An abdominal section was made and 3 inches of small intestine drawn out of the abdomen and covered loosely with 1/2 of a strip of muscle which was kept just moist with 1 to 3000 corrosive sublimate solution. The animal lost in 2 hours 6.2 C. i.e. the rectal temperature fell from 38.8 to 32.6. The abdomen was then flushed with water (warm) and the abdominal incision sutured. The rabbit made a good recovery. This case shows how the mere duration of an abdominal operation may itself be sufficient to cause death.

Experiments 148 and 149. 2 rabbits were bled from the cut carotid (148) and femoral (149) to the extent of 15 c.c. Then 20 c.c. of sterilised normal salt solution were injected intraperitoneally. The animals both recovered quickly.

Experiment 150. Proving that the intra-peritoneal injection of sterilised normal salt solution
might prove of some value in practice I performed Experiment 150. A dog was anaesthetised and the right femoral artery being divided the animal was allowed to bleed until the bleeding stopped. It was then allowed to recover from the anaesthetic when it struggled violently and the bleeding returned to stop once more. At this time the animal was in a state of syncope and the left femoral pulse could not be felt. The cardiac pulsations were according to Dr. Porter who was auscultating with a binaural stethoscope scarcely audible. I rapidly injected 50 cc of sterilised normal salt-solution into the peritoneal cavity. Slowly the pulse returned increased in strength and decreased in rapidity and within an hour the animal who had been as near death as can be conceived could move its limbs and mew. The next day the creature in spite of a large bandage round the groin and abdomen was walking about and eventually made a complete recovery. Now I cannot help thinking that this case gives us a limit which may turn out of considerable value. We all know that the intraperitoneal injection of blood has often been tried once it was suggested and practised first if I mistake not by Dupuytren. But only
Those who have looked into the literature of the subject have any idea of the mortality of septic peritonitis which has attended its employment. Of course now-a-days with antiseptic precautions and by previously defibrinating the blood this will be immensely decreased. Still I think that there is always danger. On the other hand I venture to think that the injection of sterilised normal salt solution is open to hardly any objection and it is from my experiments absolutely free from danger provided the peritoneal cavity be previously normal. I think that my experiments shew clearly that it is a matter of small moment if a few hairs remain after access to the cavity is provided away that they are in a readily absorbable medium such as normal salt solution. I venture to urge this intra-peritoneal injection of normal salt solution very strongly. Its greatest advantage is its rapidity of action and the ease with which it can be performed. Unlike transfusing whether in hospital practice it is so difficult to obtain blood, all that is
required is a pint of water and a cupful of salt. The only instrument required are a hypodermic needle, an enema syringe and a piece of rubber tubing to connect the two. If time allows it is well to cool the fluid but I maintain that my experiments show that in urgent cases this is not necessary. The fluid should however always be about the blood temperature for the injection of cold water into the peritoneal cavity is very depressing. A great advantage of this method of intraperitoneal injection is that a large amount of fluid may be injected whereas an intravenous injection it is seldom practicable to inject more than 12 ounces. It may be objected that it is only water that is injected. This is no objection for when a patient dies from severe traumatic haemorrhage he dies not because there is not enough blood left in his body but because there is not enough fluid in his circulatory apparatus to enable the circulation to be carried on.

I have thus described the 1874 exper..
ments which I have performed in order to attempt to solve some of the problems connected with the important question of peritonitis. This thesis I call an "Experimental Inquiry Into the Influence of Certain Factors in the Cause of Peritonitis." And now I would like to as briefly as possible state some of the more important points which we have observed in these experiments. It has been shown that pyogenic organisms provided they are contained in a readily absorbable and irritant media as water urine are harmless when injected in moderate amount into the healthy peritoneal cavity. But on the contrary that when the media are irritating or not readily absorbable or when there exists a focus non-union resistance or the abdomen is abnormal the introduction of pyogenic cocci is the necessary commencement of septic peritonitis. The experiments on urine healthy and ammoniacal and feces fluid and solid bile jns and the digestive fluids doubtless have great clinical importance. But I think that perhaps the greatest interest
centres on the extraordinary results of the experiments on blood fluid and coagulated, and the great difference displayed by these in their relation to pyogenic organisms, a point which was never as far as I know before been observed. The experiments on the ligature of the bowel open, I think it as a new and promising field of research. They are I believe absolutely new and original. I would also desire to call attention to the last few experiments which indicate I think to us some points of importance in abdominal surgery. I have great hopes that intra peritoneal injection of normal salt solution may be found of value in cases of severe haemorrhage as met with in surgical practice. Many of the results here may appear to others extraordinary, I confess they appeared so to me. Still I can only say that I entered upon the research with no preconceived ideas, and that I have tested as thoroughly as I can the truth of every statement made in these pages. I have endeavoured to verify every in
important result by repeating it when possible at least twice.

And now comes up for consideration the important question: do the results of this research in any way that ever give any strength to the position of the live antiseptic school? Answer emphatically no! I have I admit proved that a suspension of pyogenic organisms in water can be introduced into the normal abdomen without harm, but what of that? In surgical practice we have little or nothing to do with the normal, our concern is with the abnormal—the diseased—in which there is as a rule a chronic invasion resistant or a general atrocity as in chronic ascees, and in these cases the entrance of pyogenic organisms means the setting up of one of the most fatal of diseases.

But these experiments and their results show up clearly that microorganisms are not the only factors to be considered. They call upon us to devote some of our attention to that which is perhaps almost more important the tissues. Consider the seed by all means but do not forget the
soil. I claim also for my research that it has done something to advance the claim of the theory of the locus minoris resistancee for more attentive consideration.

But what is the chief lesson that these experiments teach us? Surely it is this that penticritis is in the great majority of cases merely a local disease that is a curable one. That in penticritis which has become general and ends in death there was a time when probably the focus of sepsis might have been removed and general infection prevented. Let it be granted as I think will be allowed by all that acute penticritis is almost always if not always septic, i.e. microbic in origin, that it is when general if hopeless progress then I say it is our duty to treat the in-\text{flammation surgically in the early stage whilst it is still localised and by removal of the cause prevent the spread of the deadly disease. When the disease has become general it is in fact an abscess of the abdomen and surely we should treat it as any
abscess elsewhere. Let it continue and
the pyogenic organisms ever produc-
ing their poisonous poison the
patient and rob him of life.
Surely in such cases it is our
bounden duty to wash away this
poison and thus give the
patient his only chance of life.
Also in cases where we diagnose
rupture of some abdominal organ these
experiments show us with what little
danger we may explore the abdomen
and repair any injury before a sufficent
time has elapsed to change the
peritoneum from a normal to an
abnormal condition.

And now I have done. Of many
points connected with the subject
I have not been able to hear, but
my work will be judged rather by
what I have done than by what
is left undone. I venture to express
the hope that the labour I have
spent on this research has not
been spent in vain; that I have
been enabled to throw some light
of light on some of the many
afflictions which beset the path
of those who strive to elucidate
In understanding design that is
there, shared are complete
in the Goodsir Prize.
the Etiology of Peritonitis. At least I can claim to have pointed out some of these difficulties and thus to have rendered, perhaps, easier the task of others who will follow in this direction.

For this record of my work I think I may say that it contains something which are new and not unimportant, and as an attempt to elucidate by honest original work the etiology of a disease as interesting as it is all-important. I venture to present it in this form as my Thesis for the degree of Doctor of Medicine and to hope that it may be deemed not unworthy to be considered in the competition for the Syne Surgical Fellowship.

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