Observations on Typhoid Epidemics in Auckland, New Zealand, with an analysis of over two hundred cases in which the Serum Reaction was tested.
The history of typhoid fever in Auckland dates back to the early days of colonization. The District Hospital records show that cases occurred in 1851, the year when Jenner first placed the diagnosis of the disease on a firm basis. During the last 45 years, 1904 cases have been admitted to the wards. A some idea of the prevalence may be gathered from the fact that ten years ago two buildings had to be provided especially for typhoid cases, containing in all forty beds; during the heaviest of the season even these are now insufficient. As the population of Auckland & its suburbs is only 50,000 there is rather a serious state of affairs. Unfortunately, owing to the entire absence of a system in the administration of Public Sanitation in the colony, these hospital records are the only ones existing from which the extent of the epidemic can be estimated. Notification of infectious disease is not carried out; no report is made by the Medical Officer of Health, & Sanitary matters in general are neglected.
It is not to be wondered at, therefore, that Enteric Fever has obtained such a hold in the district & is steadily increasing, as will be shown.

Dr. G. P. Baldwin, Medical Superintendent of the district Hospital presented at the Branch Meeting of the British Medical Association last January, an interesting series of charts which he had compiled from the records of the last 48 years, showing the history of the disease.

The general features of the fever appear to differ but slightly from the average. The death rate was 16:17 0/0, 9 the incidence as to age & sex accorded with the results of other observers. The steady increase in the number of admissions is apparent when the average per year is taken in the following periods:

<table>
<thead>
<tr>
<th>Period</th>
<th>Average Admissions</th>
</tr>
</thead>
<tbody>
<tr>
<td>1857-1860</td>
<td>14·5</td>
</tr>
<tr>
<td>1871-1890</td>
<td>46·2</td>
</tr>
<tr>
<td>1891-1895</td>
<td>86·1</td>
</tr>
</tbody>
</table>

The increase in population could not account for this rise, as during the last 10 years it has been slight, indeed in the city proper, has diminished by some 2000.
Percentage Typhoid Per 10,000 Population.

Chart I

Year: 1860, 1865, 1870, 1875, 1880, 1885, 1890, 1895, 1898

Numbers on the y-axis range from 0 to 120.
To eliminate the error from increase of population & Baldwin has taken the percentage of typhoid admissions to general admissions, which probably gives a fairly accurate result, while the want of statistics makes it impossible to get the cases per 1000 of inhabitants. He divides the 48 years into groups of 12 years.

<table>
<thead>
<tr>
<th>Period</th>
<th>Percentage of Typhoid</th>
</tr>
</thead>
<tbody>
<tr>
<td>1851-1863</td>
<td>4.42</td>
</tr>
<tr>
<td>1863-1875</td>
<td>4.43</td>
</tr>
<tr>
<td>1876-1887</td>
<td>6.22</td>
</tr>
<tr>
<td>1887-1898</td>
<td>7.98</td>
</tr>
</tbody>
</table>

There is therefore a marked increase in the past 24 years.

The accompanying chart is compiled from this table — for convenience I have taken the average of admissions & percentages in groups of five years. The two lines in later years correspond very closely & both show the great rise they experienced about the year 1880. It will be noticed that during the last five years the typhoid cases formed 10% of total admissions, while in 1895 the climax has been reached with 16.8% an alarming statement to make of so small a community.
We see then that typhoid is endemic in the district. In attempting to account for this prevalence, I have naturally enquired into the influence which climatic changes may have.

We find that here, as elsewhere, the disease is at its highest during the autumnal months, falling in May as the wet season comes on, rising again early in summer. This is very constant, and as a result the months of January, February, March, and April are spoken of at the Hospital as the 'Typhoid season'.

In the accompanying chart I have compared the curve representing the total monthly typhoid admissions during the 45 years with the average monthly rainfall and mean temperature. The temperature curve seems to follow that of incidence fairly closely, but generally a month or two behind it, as it does in other parts of the world. Thus the warmest month is February, in April the incidence is highest; the falling temperature afterwards in April being followed by a diminution in Typhoid in May and June. The influence
is probably through the soil and this delay of two months is owing to the deeper layers not being infected immediately.

A hot, dry summer aggravates the intensity of the Autumnal rise (Parkes). 1898 was such a year, so the epidemic was severe. It would seem as though the rainfall also had an influence, for the typhoid curve moves inversely with that of the rainfall in a very striking manner. Stevens & Murphy deny such an influence; however, and indeed when we consider the two or three weeks which must elapse during incubation it seems impossible that the sudden fall in cases from April to May is owing to the simultaneous rise in rainfall.

Yet it is not certain that the temperature alone controls the incidence of fever. The obsequent temperature decrease from February to March could never account for this sudden disappearance of the epidemic. Possibly the two act together, the falling temperature of March & April
The mean typhoid curve is for 4 years and therefore does not represent accurately the incidence of recent years which must be much higher.
producing some condition of the soil acted on by the increasing rainfalls of April & May in such a manner as to retard the spread of the disease. So also the decline in rainfall from September to November acts with the rising temperature & the conditions for reappearance are again favourable.

Consideration of the meteorological condition preceding the outbreak during the last two years would make it appear that the rainfall exercises no slight effect.

Chart III represents the end of 1897 & the beginning of 1898. The outbreak was delayed till January & then was exceptionally severe. During September & October 1897 the rainfall was higher & the temperature lower than the mean; hence, doubtless, the delay. But in November & December, while the temperature still remained low, the rainfall was very far below the average & therefore must have been responsible for the typhoid severity of January & February. The heavy rains of January again may have been responsible for a slight decline which occurred in March.
Chart IV refers to the present outbreak which began in November 1898, therefore two months earlier than the previous one, but does not threaten to be nearly so severe. September shows a low rainfall and a temperature somewhat below the mean. The early appearance of the fever seems again to correspond with the low rainfall. October, however, was a very wet month, but there is no corresponding fall in December, though it may have had an influence in checking the severity. November had a low rainfall and a high mean temperature, a combination which has a corresponding high typhoid incidence in January, practically the same as in 1898; while the draught again in February would seem to correspond to the high rainfall of December, a month which had again a low mean temperature.

I presume then that the state of the ground water as demonstrated in Basle by Pettenkofer has an influence on the prevalence of typhoid in this district, but I cannot give any true data (so far few cases have been admitted in March).
In making deductions from the hospital records it is necessary to bear in mind that patients are admitted from a very wide area of country extending north some 200 miles & south 30 or 40 miles from Auckland; & having a population of some 60,000 apart from that city. This population is responsible for about one third of the general cases in the wards. In considering the typhoid cases it is necessary therefore to divide them into the following groups.

I. Those from Auckland city & the immediate suburbs having the same water-supply.

II. Villages in the rural districts & suburbs outside the water-supply.

III. From isolated houses in the up-country settlements.

Auckland city & immediate suburbs represents a population of 57,000 (1896) & during the past five years supplied 71% of the typhoid patients. The houses, in no part over-crowded, are scattered over an irregular mask of land some six miles across, separately
the harbours on the East and West coasts of the Island; the water shed being formed by a series of volcanic hills some 800 ft in height. The town, rising from the sea beach to over 200 ft in the higher parts lies chiefly on the Northern slopes which are intersected by deep valleys. The geological formation is a thin surface of volcanic scoria rock covering impermeable tertiary clays. Towards the sea coast this covering is absent.

Disposal of Sewage.

Only a small area of the town is drained by sewers, but there are three in the more thickly populated parts, all opening directly on the fore-shore of the harbour.

Over 80 per cent of the larger part of the town there is a dry system for removal of night soil, compulsory within a certain radius. The closets are supplied with pails or boxes by a contractor empties these weekly or fortnightly according to the fancy of the household. The night soil is deposited on a farm, bordering on the upper reaches of the harbour.
about three and a half miles from the centre of the town.

General refuse is carted out to the same district and deposited in heaps. Slop water and house refuse outside the sewer area is thrown into open surface drains, some of which find their way into the many gullies intersecting the town and others form cesspools in backyards and waste spaces or ultimately sink into the ground. Incidents are common in the outlying parts, and the general arrangements for removing sewage are very imperfect. The sewer drained area is however not less responsible for the disease than are other parts of the town. The cases are generally fairly evenly distributed, but in a few instances small localized outbreaks have arisen, as though from specially infected centres.

One large suburb lying almost entirely on the clay lands, is perhaps more liable to such outbreaks; here, especially, collections of refuse are found; while at the bottoms of the gullies an evil-smelling drain is generally seen seeping silently making its way to the sea.
In January 1889 one of the local outbreaks occurred in this suburb, six cases coming in from one small road four being members of the same family and the others near neighbours. The night soil is removed by the contractor from these houses but the slops etc. are flung out anywhere to accumulate in a gully at the back of the street from which an evil odour arises in hot weather. All use city water. Only three other cases have come in from Poplar only the outbreak in question, 

They seem to be unconnected the one with the other.

In the two or three instances where I have been able to trace the conditions preceding each outbreak, soaking of the soil with sewage always seems to be a factor.

Dr. Bellman has kindly supplied me with details of another such localized epidemic which occurred among the nurses in the typhoid wards during the season of 1886. Eight nurses were attacked; all one unconnected with the fever wards. These were not probationers but had all some experience in fever nursing, I presumeably were.
careful as to cleanliness &c.
Those in one ward more especially seemed to suffer, while the patients at the South end of the two wards were especially prone to relapse.
An examination of the sanitary conditions revealed the facts. The soil-pipe of the closet attached to the females' ward was leaking, & the boards of the flooring soaked in consequence. At the same time a flaw was detected in a drain passing under the south end of the buildings, which had caused the soil for some distance round to be saturated in sewage. Although the stools from the patients were disinfected with perchloride of mercury before being emptied down the closet it is doubtful if this is sufficient to destroy their injective power unless very carefully done. Again the urine was not treated in this way, & it has been shown to be often swarming with the bacillus. Probably then the soil was a breeding ground for the germ. When these defects were rectified the outbreak stopped, & has not since re-appeared. The wards are about six feet above the ground at this place & the conveyance seems to have been by
sewage evacuations rather than direct contagion.

Water Supply. —

Rock has stated that where typhoid is occurring in increasing quantities over an area having the same water supply, or where there is no connection case with case, the water should be suspected.

This exactly describes the Auckland epidemic for, with the few localized exceptions given above, the cases occur scattered up and down throughout the water area. The water supply is to my mind the most probable source of the infection in most cases. The service was introduced in 1876, & the marked increase has been subsequent to that date.

The situation of the Springs lends weight also to this view. They arise at the foot of the slopes forming the northwestern end of the town, about three miles distant from the centre, where they supply a shallow pond.

The water, taken from the two largest of the Springs, is pumped by steam, up to concrete reservoirs of which there are three at different parts of the town, & by which a constant service is provided.
The amount of water in these springs depends on the rainfall in the locality, a few weeks elapsing before the effect is apparent. In dry summers such as 1898 there is a partial water-famine.

A shallow, wide valley represents the catchment area which is about five square miles in extent. The side of this valley is of the surface volcanic rock formation, the other a steeper side is formed by a series of clay hills without the moria covering. At the bottom of this valley is a small water course containing the surface drainage of these hills. This runs within 60 yards of the collecting pond at a level above the bottom of the lake & is merely an ill swelling open sewer. The whole area is thinly populated, more particularly at its upper end where the poorest part of the town is situated. The houses nearest the springs do not come into the districts where the night soil renewal is enforced, & consequently money bury this substance in their back yards or fields.
The Municipal Authorities contend that the moria rock forms an efficient filter to this populous catchment area. But we have seen that in parts, at least, this is abortive, and even in the parts where it is present, it is questionable whether it could cope with the quantities of sewage which are deposited over the surface.

Dr. W. H. Boore's contention that such matters are rendered innocuous by being spread out and exposed to sun and air, only holds. I presume where the work is carried out systematically; it would not apply to this instance where they are allowed to accumulate in gulleys and hollows. Then, again, these rocks contain wide fissures and cavities where the molten lava has come in contact with water-courses; such conditions are known to have been responsible in some instances for the carriage of infected material.

But it is unnecessary to further discuss this point when there are, close to the Auckland springs, two sources of infection which in all probability have a marked influence on the quality of the water.
These are, firstly, the refuse soil dumps, which is only some 600 yards to the north, and secondly, the pit refuse heaps, which lie about 400 yards in another direction, from the ponds. It is true that both are below the drainage level, but they are near enough to supply a plentiful amount of infected dust to the surface of the ponds, when the wind is favourable. The rainfall may exercise its influence by preventing the spread of this dust. That these surroundings have an influence is shown by the chemical analysis of the water. As it comes from the ground at the main spring it is fairly pure:

<table>
<thead>
<tr>
<th>Total Solids</th>
<th>136</th>
<th>parts per million</th>
</tr>
</thead>
<tbody>
<tr>
<td>Chlorine as chloride</td>
<td>25</td>
<td></td>
</tr>
<tr>
<td>Nitrogen as nitrate</td>
<td>4.1</td>
<td></td>
</tr>
<tr>
<td>Free Ammonia</td>
<td>0.1</td>
<td></td>
</tr>
<tr>
<td>Alkali Nitrate</td>
<td>0.2</td>
<td></td>
</tr>
<tr>
<td>Absorbed Oxygen (four hours)</td>
<td>13</td>
<td></td>
</tr>
<tr>
<td>Phosphate, a trace</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

The high proportion of chlorides and nitrates with the low amount of organic matter is suggestive of previous sewage contamination.

These figures are taken from the Report of the Official Analyst issued in 1897.
And is what might be expected in water after filtration, when we consider the catchment area. The chlorides, indeed, may be due to other causes; as a spring in the same formation, but further off, with less chance of contamination, has them in the proportion of 22 per million, with nitrites 1.3.

The collecting pond water gives abundant evidence of pollution on chemical analysis:

<table>
<thead>
<tr>
<th>Component</th>
<th>Concentration (ppm)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Total solids</td>
<td>148</td>
</tr>
<tr>
<td>Chlorides</td>
<td>25</td>
</tr>
<tr>
<td>Nitrogen as nitrate</td>
<td>1.4</td>
</tr>
<tr>
<td>Free Ammonia</td>
<td>0.6</td>
</tr>
<tr>
<td>Alumminoid Ammonia</td>
<td>3.48</td>
</tr>
<tr>
<td>Oxygen absorbed</td>
<td>1.2</td>
</tr>
<tr>
<td>Traces of phosphates</td>
<td></td>
</tr>
</tbody>
</table>

It is a turbid, evil-smelling water, and, from the large amount of alumminoid ammonia, suggests recent pollution. This water is allowed to mix in with the spring water before it is pumped up to the reservoirs, with the result that the water in the mains has a quality intermediate between these two.

This pond water, then, is the probable source of infection, while the high amount

These springs are too far from the sea to derive their chlorides from its influence.
of nitrates in the spring water may favour the growth of the typical bacilliferous (Parkes, Royal Comm. Report 1893).

I have on two occasions attempted an analysis of the water, by bacteriological methods.

I followed the method, described by Klein, of taking the sludge from the candles of a Pasteur filter, after some gallons had been passed through, & making cultures from this in phenol broth & phenol gelatine plates.

The amount after twenty or thirty gallons had been filtered was astonishing, & it contained, besides diatoms, algae, & much low vegetable life, a number of motile animal organisms - Infusoria, Paramecium, and so on.

I obtained the use of a small Pasteur-Chamberland pressure filter, which, after cleaning, was attached directly to the main & sterilized as far as possible by boiling. The top was screwed down, & the boiling water allowed to filter through with the rest. In removing the 'candles' again, I found it convenient to wrap the upper ends in sterilized wool, & take the
The deposit from the lower ends only, I did not attempt, therefore, to estimate the number of bacilli per cubic centimetre, in the manner described by Klein.

The water was allowed to run through the main for a time, before letting it into the filter. The water at two different parts of the town was examined, and both showed the presence of the bacillus coli in large amounts. This was especially marked in the second examination, in January 1899.

Klein, Christophers, McCreney and other observers have found great variation in the action of these coli forms in various media. The two latter have also found that there are great variations in these as to their attitude in various normal and other sera.

Recently Lorrain Smith has made some very interesting investigations into the serum reactions of these. He isolated from the Belfast water. As the Journal containing his report has only just reached this colony, I have been unable to apply his observations to the

B.M. J. Sept. 3rd 1898.
B.M. J. Jan 28th 1899
species which I have isolated. Some of them showed marked agglutination at a 1-10 dilution in all sera, but Israev Smith holds that this dilution is insufficient for accuracy. It may, however, be worth while enumerating those which I have so examined.

1. A Coli type

- Resembles coli in appearance
- Activity in growth on agar
- Gelatin forms bubbles freely in depth of gelatin stab & in glucose gelatin
- On Potato gives atypical growth
- Indol production slight. Yeast
- Milk is coagulated but very slowly
- Serum reaction was what might be expected. Negative at 1-10 of a serum reacting well to virulent cultures.
- Negative at 1-10 normal blood.
- Negative at 1-10 of my own blood which as a result of vaccination with dead culture was giving a good reaction at 1-10 to virulent stocks.

2. An A-typical form

- Smaller & rather more active than C. coli
- No gas formation in gelatin stab or glucose gelatin
- On gelatin surface resembles
- One bubble has some developed in a stab 3 weeks old.
6. Typhoid in appearance of growth on Potato - a faint creamy line not typical of coli.
Milk is coagulated in 24 hours.

**Serum reaction.**

- **Positive** 1-10 dilution of my own blood.
- **Negative** 1-10 Normal blood

Positive 1-10 1-20 of a patient's blood giving a good Widal reaction.
Negative 1-10 serum from a possible case of typhoid which however has no fever remained negative to virulent stocks.

These reactions suggest a true typhoidal nature. I have some reason for thinking this may be a mixed stock.

3. Another atypical form.

- Also smaller than coli but sluggish and tends to produce long straw-like individual colonies. It gives a very faint coagulum in milk after a week.
- Stools form in gelatin stab or glucose.
- Potato - a faint typhoid like line.
- Indol production marked.

This form agglutinates at 1-10 dilutions of all serums except 1-20 typhoid serum.
- It grows on agar like b. coli.

In another case the reactions are like those of typhoid but I have not completed its examination.

I had hoped by vaccinating myself with dead cultures to have a standard serum always at hand to probably free from reaction to coli types. 10 days after it showed a very strong reaction but now in 2 months seems to be much weaker.
I have not had time to complete the examination of these colonies so that whether or not I have in case 2 a mixture of a typhoid, with a colit type, I cannot say.

The serum reaction is apparently not such a simple means of diagnosing the nature of the bacillus as was at first hoped. The bacillus Enteritidis—especially the Särtiner variety—seems from the researches of Durham and Sydney Martin, to be to some extent intermediate between the colit and typhoid races with some interaction so far as the agglutination in their respective serums is concerned, while Lorrain Smith has shown the same thing to occur with these water-borne varieties of E. coli as distinguished from those occurring in normal foci of even typhoid spleens.

The whole matter needs further elucidation, but apparently the presence of these varieties of a colit type has some important bearing on the production of typhoid. Indeed Lorrain Smith's results would seem to indicate a preliminary
invasion by some of these forms in some cases at least. The practical conclusion is that the presence of such organisms in a water is sufficient to condemn it.

In one plate (not phenolated) I got a colony answering to the description given by Klebs of the Proteus Fermentans — another evidence that the Auckland water is unsafe.

Of other possible sources of infection the milk supply might in some instances be suspected. It is difficult to trace out any exact evidence as to this, for there are so many small dairies with a few cows, which get as a supplementary supply from one or other of the larger companies. The patient's knowledge (my only source of information) only extends to the retail source.

On two occasions during 1898 patients were admitted, suffering from typhoid, who had been milking up to the day previously.
Diseases have surroundings of the most sanitary description, yet little control seems to be exercised over them. The cows in some of the lanes, for instance, have been found to be dependent for their water supply on one of the open sewers. I have mentioned above, and it has been suggested that this might infect the milk. There seems to be no strong evidence that such a transmission is possible. Hart mentions a suggestive case which occurred near Leeds in 1876, while Pembridge's experiments with rabbits, where he succeeded in reproducing the disease by feeding them on infected lettuce leaves, may perhaps be quoted as bearing on the subject. I have not found any instance where Shellford could be held responsible.

Oysters are taken from far down the harbour and are not treated in a refrigerating bed, which seems generally to be the place whence they receive the worms. But in regard to the question of

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Harbour pollution I may give the following case:
Two men during last December were brought in suffering from typhoid, both of whom stated that they traced their illness to a bath in the Municipal salt-water baths, a few days previously. Though they had bathed on the same day, they were unacquainted with each other and gave their histories without any possibility of collusion. The fact that these baths are situated close to where one of the sewers empties itself makes me inclined to accept this mode of conveyance as possible. One or two similar instances have occurred where the disease seemed to have its onset traceable to a bath near the mouth of the sewer coming from the Hospital district; moreover, Gianna has shown that the typhoid bacillus can live for a time in sea-water, an observation which is confirmed by the Report on the Shell fish Epidemic, to the British Medical Journal in 1898.
Cases from villages or outlying suburban areas.

These cases are generally traceable to infected streams or wells, & dry weather has a marked influence; not only by producing the usual telluric conditions, but also in many such houses the drinking water is collected from the roofs in tanks, & when in dry seasons these become empty, the inmates are compelled to use less pure supplies.

These houses have commonly a shallow well in the back garden into which the surface water drains carrying with it impurities from manured soil, while cess-pools & middens privies are often situated close at hand.

Certain villages may be singled out as being specially infected centres -

Oberhagen - a seaside village of about 2000 inhabitants, about six miles from Auckland. Fourteen cases came from here in 1898.

The water supply is pumped from a spring which arises in the middle of the village. The catchment area has the same volcanic formation as Auckland, & is thinly populated, & also includes
two grave-yards not 300 yards from the pumping station.

There are no sewers, & the storm water from the streets is conducted into convenient clefs in the volcanic rocks, above the level of the spring.

A few years ago night-soil was deposited in the back yards of the cottages or accumulated in privies, but after a very severe epidemic in 1894, a compulsory dry system of removal was introduced which has had the effect of greatly lessening the number of cases.

The water is said to be chemically pure, but from the situation it stands in constant danger of pollution.

**Otahuhu** — A village of 1000 inhabitants, about 8 miles from Auckland & on similar formation.

Isolated cases have arisen there for two or three years, but last year a severe outbreak occurred — nineteen cases were admitted to the Hospital.

This summer, so far, seven cases have been sent in.

The water supply is almost entirely from surface wells.

At the highest part of the village is
an Hotel at which some cases arose in November 1844. The excreta were deposited without disinfection in a yard some fifty yards from the house & the well lies below the floor of one wing.

The cottages lie fairly close together on the slopes below this, each having a shallow well round which excreta & slops are, as usual, deposited.

The ground over the whole village cannot by now be well saturated in sewage.

The cases began to arise in January, & the climax was reached in April.

Samples of water from three of the wells were sent & one for analysis—

1. That below the Hotel

<table>
<thead>
<tr>
<th>Component</th>
<th>Result</th>
</tr>
</thead>
<tbody>
<tr>
<td>Total solids</td>
<td>5.60 parts per 1000,000</td>
</tr>
<tr>
<td>Loss on ignition</td>
<td>2.80</td>
</tr>
<tr>
<td>Chlorine as Chlorides</td>
<td>0.4</td>
</tr>
<tr>
<td>Nitrogen as nitrites</td>
<td>17.5</td>
</tr>
<tr>
<td>Nitrites &amp; phosphates</td>
<td>a trace</td>
</tr>
</tbody>
</table>

Bacteriological examination showed the colonies per cc. to be many thousands.

B. coli Com. & Proteus vulgaris were present so that it was from all points of view a very impure water.
2. From a well about 300 yards down the slope.

<table>
<thead>
<tr>
<th>Total Solids - ppm</th>
<th>6.40</th>
</tr>
</thead>
<tbody>
<tr>
<td>Loss on ignition</td>
<td>260</td>
</tr>
<tr>
<td>Nitrogen as nitrate</td>
<td>15.4</td>
</tr>
</tbody>
</table>

A very faint trace of nitrite & phosphate.

Bacteriologically - the water had over a thousand colonies per cc. & coliforms were plentiful, one of which agglutinated well in typhoid serum. Three cases of typhoid came from this house.

This water was only slightly less impure than the other.

3. A well 600 yards from the hotel, but not on the slopes below, while being by no means satisfactory did not show such marked evidences of contamination.

The whole outbreak seems comparable to that of Ashfield in 1872 where a series of infected wells were contaminated by a case arising at the upper end of the town.

This year five of the seven cases come from one family who live nearly a mile from the village. They use rain water 7000 ft. far, I have been unable to track the cause.
Isolated cases in the country. Occasionally a case is brought in from some outlying district, where one would scarcely expect the virus to exist, and are very difficult to explain. Probably most of them might be traced to some infected article of diet, or a visit to some infected centre. This year two such cases came in, the one in December and the other in February. Both lived in sparsely populated districts, far from all neighbours, and where hitherto no case had been known. They were separated from each other by about thirty miles, but I discovered that both had visited the same district before they took ill, in which some suspicious cases had been occurring. At the Hotel in this place a child had recently died of fever, and as the water is obtained from a well with, as usual, a compost place at hand, it is probable these patients received the infection here. Cases, however, arise where no such possibility could be traced.

In June 1898 a specimen of blood, showing Widal's reaction markedly, was sent to me from an undoubted case of typhoid
which arose in a district generally free from the disease.
She had not been from home, even to visit a neighbour, for many months, and no other members of the family nor any of her neighbours were attacked.
It seems certain that the bacteria of typhoid can lie dormant for many years without losing its vitality.
It has been found in the fall bladder fourteen years after the attack, as in the soil of a field which had lain untilled for many years.
It is easy to see, therefore, how all traces of the origin of an outbreak may be lost. But even then it is hard to see why the re-awakened microorganisms should attack only one member of a large family, as in this instance, where as far as the ordinary means of conveyance are concerned, all must have been equally exposed. Varying degrees of susceptibility no doubt exist, and it is of course possible that out of seven or eight persons, only one should offer a suitable indicium for the bacteria.
General Summary
It is certain then that, in Auckland at
any rate, there is an infinite number
of channels through which infection
may be conveyed.
That it may be waterborne is to be
gathered from the character of the
epidemic and the results of chemical
and bacteriological examination.
In comparatively few instances has the
bacillus been detected in the water,
proving owing to its great dilution.
The presence of coli and certain saprophytes
seems to militate against its growth,
thus also may make the detection difficult.
It dies out rapidly in sewages containing
such bacilli, although not so quickly as
to prevent its wide dissemination especially
if the sewage be dilute.
It is necessary to draw a distinction between
the wide-spread epidemics, such as that
in Greatstone in 1897, where the whole
water supply is thoroughly infected; and those
cases where the pollution is less gross
such as I believe to be the case with the
Auckland supply.
That many persons escape in such cases
is probably owing to the dilution of

the vessels. This might also be accounted for by supposing an intermittent infection, not polluting the whole mass, but forming, as it were, emboli, which are rapidly carried down the main as before they have time to be disseminated.

The fact which I have noticed about the Auckland cases might be taken as an evidence of this, namely that the cases tend to come in in batches of four or five, having on the whole a simultaneous period of incubation, and even a similarity in the seriousness of the attacks. I will give two marked examples of this —

During January 1898 the cases had been coming irregularly till towards the end of the month, when on the 27th two cases were admitted — one fatal. On the 28th four cases, two fatal, one severe. All gave a history of illness beginning seven days before, and all came from parts of the city widely separated.

Again, in April 26th, four cases from about the city came in, none being severe, all giving a history of seven days illness. These certainly suggest a similarity of infection but I cannot give enough marked cases to be very sure of the fact.
It suggests, however, the possibility of desert borne infection from the filth heaps around the springs.

Cases of this mode of conveyance have been given by Dr. J. E. Izegun, as occurring in the Sudan, while Dr. Robertson strongly favours the idea of this being a frequent source of epidemics. The, in the same paper, shows that there are areas of infection in England where the death rate from typhoid remained for thirty years abnormally high. He concludes that in those places the soil favours the growth; he has made experiments on soil showing that even after the bacilli he had sown could not be detected, their presence could again be demonstrated if the earth were soaked in bacillus.

The sewage from cesspools, leaking sewers, & house slopes will then prepare the soil for the spread of the bacilli. The wells at Itahbun, as in many such outbreaks, were thus infected; and doubtless the dust from such places also contains the virus; & localized outbreaks, such as those in Bombay this year, result.

Examinations from sewers & filth heaps.

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B.M.J. 1898 p. 146 vol II

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Ibid. Jan. 8th 1898

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B.M.J. 1898 p. 421 vol II
possibly also act in this way, though in some instances, as in the outbreak among the nurses in 1896, dust could hardly have been the means of conveyance. From the many local epidemics which have been due to such eruptions it would almost seem as though the air round were charged with the infection. Carmelty & Haldane proved that this occurred in drains where the sewage splashed or bubbled. However Robertson failed to get any colonies on gelatine plates exposed over infected typhoid filth heaps.

Such, then is the state of the Auckland Province in respect to typhoid. In other parts of the colony the disease is not nearly so prevalent, though the insanitary condition is universal; so probably the warmer climate favours the life of the organism. Considering that it has been endemic here for fifty years, Auckland must by now be a thoroughly infected centre, so it is likely to remain so as long as the present disregard for sanitary laws continues.

26 Jan. 85 1898
Pathological Aspects.

Type of the Disease.—

In its general features, typhoid as it occurs in Auckland does not differ markedly from the ordinary type.

One noticeable point is the large number of mild cases.

Of the cases which have come under my observation, 34 percent might be so classed. The death rate too is somewhat low, being 10% , although for the whole number of cases since 1851 it is 16.17%.

In doubt had it not been for the serum reaction being present many would have been classed as febrile, but most were fairly definite cases of typhoid showing the usual symptoms at first, but dying out in the third week, or even sooner.

A study of the influences affecting the virulence of the disease, reveals several interesting facts, the chief factors being apparently (1) Place of residence of the patient (2) The season in which the disease is contracted.
I

Place of Residence -

The disease differs in virulence according as the patient is (1) A resident in the endemic area, or (2) A visitor to such an area (3) or contracts the disease in some local outbreak in a district not hitherto much infected. It has been, of course, impossible to trace the disease whole of the cases as to previous exposure to virulence. So I have divided them up as to whether they use the Auckland water supply or not, taking as my endemic area the water-district.

The epidemic this year is, in the whole of an unusually mild type 430% of the last 44 cases being so classified. As in these cases I have been able to trace the question of residence fairly accurately, I will give them alone first -

City Residents 30 -

Severe 8 = 26%
Normal 8 = 26%
Gland 14 = 47%

Cases arising outside 14 -

Severe 7 = 50%
Ordinary 2 = 15%
Gland 5 = 35%
Taking the whole 209 cases of the disease which I have examined with the exception of those too recently admitted to be classed as typhoid fever,

Of 134 city cases
50 were mild = 36.6%
43 were severe = 31.9%
Death rate 8.0% = 11 cases

Of 63 outside cases
18 were mild = 28.5%
31 were severe = 48.6%
Death rate = 15.8% = 10 cases

This difference especially as to death rate is very striking.

I have included in “outside cases” some five or six visitors, who received the infection in Auckland during their stay there, all of which were severe.

It has been pointed out in the cases of visitors to India that they are more liable to contract the disease; a also in Paris the same thing is seen. W. Baldwin has traced the history as to residence, in 1025 cases of typhoid patients in the Hospital; omitting all those coming from,

not only Auckland, but any area where typhoid is frequently found, there remained 143 visitors from uninfective districts, which
gives the high percentage of 14
over the whole admissions.
The outbreak at Takaka last year,
while 50% of the cases were severe,
had a low death rate being 3.0%.
Only 20% were mild. This village
cannot be considered an endemic
area, however, although only 8 miles
from Auckland.

It would seem, then, as though resident
in such an area conferred some
degree of immunity.

Seasonal variation.
As the epidemic advances the number
of mild cases increases, and of severe
cases decrease.

The death rate during the latter half of
the epidemic of 1898 was much
lower than in the earlier months.

Y/n e 1st November 1897 to Feb. 26th 1898 —
60 cases 8 deaths 6.5% rate
Feb. 26th to June 30th —
82 cases 6 deaths 7.3%
from the city

Then again this present epidemic
shows a like lowering of death rate.

In November 7 December there were 19
admissions with five deaths, while
In January and February with 35 admissions there have been no deaths; but this year is exceptional. I believe such a state of affairs has been observed in other outbreaks, but the only reference to it which I can find is a casual one as to hospital cases in the Maidstone epidemic. In Chart I I have shown the two curves representing the fairly steady rise in mild cases & the less constant but still marked decline in severe ones. The number of cases admitted, indicated by the third line, seems to exercise no influence as the rise of mild attacks continues through May & June. After this, the numbers are too small to admit of accuracy, but generally the mild cases continue high in proportion till November is reached. The percentage of severe cases rises slightly in March & April owing to the bulk of the Ottawa cut cases coming in during those months, 50% of which were severe. The numbers are too small to admit of a division into city & outside cases, but from what has already been said it is evident that
the phenomenon chiefly applies to
the endemic area.

A curious point is that this
decreasing virulence applies to
the two localized outbreaks which
have occurred this year. The
numbers are very small, but the
fact is so obvious that I will give
them here.

That at the road in Pocono:

<table>
<thead>
<tr>
<th>Disease Year</th>
<th>Date of Admission</th>
<th>Remarks</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>Dec. 12th 1898</td>
<td>4th day died</td>
</tr>
<tr>
<td>2</td>
<td>Dec. 11th 1898</td>
<td>1st day - sharp</td>
</tr>
<tr>
<td>3</td>
<td>Jan. 11th 1899</td>
<td>9th day - ordinary</td>
</tr>
<tr>
<td>4</td>
<td>Jan. 14th</td>
<td>6th day - ordinary</td>
</tr>
<tr>
<td>5</td>
<td>Feb. 2nd</td>
<td>2nd day - very mild</td>
</tr>
</tbody>
</table>

Then again, at the gaming in St. Louis:

<table>
<thead>
<tr>
<th>Disease Year</th>
<th>Date of Admission</th>
<th>Remarks</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>Jan. 2nd 1899</td>
<td>7th day severe</td>
</tr>
<tr>
<td>2</td>
<td>10th</td>
<td>9th day severe</td>
</tr>
<tr>
<td>3</td>
<td>10th</td>
<td>9th day severe</td>
</tr>
<tr>
<td>4</td>
<td>16th</td>
<td>5th day - ordinary</td>
</tr>
<tr>
<td>5</td>
<td>22rd</td>
<td>4th day - mild</td>
</tr>
</tbody>
</table>

It would appear as if either there was
a weakening in the virulence of the bacilli,
or that those affected later had acquired
some degree of immunity. I will discuss
the matter in reference to the serum reaction.
Post-mortem Examinations.

I have made in all 18 post-mortem examinations at which the following points were noted.

The cause of death in 9 might be considered to be general exhaustion, with failure of especially the right side of the heart, as a prominent feature.

In 7 others death resulted from peritonitis from perforation of an ulcer. This makes the percentage of such cases very high = 35%, while Panchon puts it at 20% & Leibermeister at only 8%.

In 2 cases respiratory complications were most prominent, a double pneumonia in one which supervened in the fourth week, in the other a septic pneumonia which had a fatal result in the 2nd week, the left upper lobe being affected.

In one case death might be attributed to acute toxic poisoning, which occurred in the 12th day of the disease, the patient being for three days in a semi-comatose condition. There were in the ileum many ulcers from which the sloughs had not separated. The spleen was typical in appearance of a virulent stock of Eberth's bacillus was separated.

In another case death resulted from purulent meningitis. Six weeks from the beginning of the attack, which was not in itself severe as lysis occurred in the fourth week. Subsequently symptoms of paralysis began to appear.

The ulcers were all in a healing stage & no culture was obtainable from the spleen.

Among the cases of Peritonitis the lesion was generally within nine inches of the ileocecal valve.

In one of these the perforation occurred during a relapse in the 6th week of the illness & here some of the ulcers were almost healed.

In another death occurred after 12 weeks of vague illness. Here the perforation was two feet above the valve. A peculiar feature was that the peritoneum was stretched by adhesions to form two cavities, one on each side, which were full of purulent fluid. The intestines were in a great extent shut off & showed but little purulent matter on the surface.

The perforation was multiple in another case - a female of 52 years deep in the 6th week. The ulcers extended as high
as the feet above the valve. One perforation about two inches from the valve was plugged by the right fallopian tube, being firmly bound over it by lymph adhesions.

In another I found a double perforation, again in a female after six weeks illness; one close to the valve was shut off from the general cavity by an adherent fold of the ascending colon. The other was 8 inches above this.

Oclusion of the perforation was seen in another case - a male aged 24. The ulcer, about a foot above the valve, was at the end of a knuckle of bowel which was firmly bound down to the bladder by adhesions. There was consequently only a localized peritonitis. This patient had complained for some days before death of a severe pain referred to the tip of the penis, doubtless caused by the adhesion about the insertion of the ureter.

I found two cases of ulceration of the colon, one a large single lesion, in the other a number of minute points in the sigmoid flexure. In another case the valve was involved.

In six cases pustules, generally large, were separated from the spleen giving all the reactions of Chertesum and others.
The Serum Reaction.

While the general law is now well established, that when a serum, under the influence of certain pathogenic micro-organisms, acquires antimicrobial properties, it also develops the power to agglutinate that organism; it is apparent to all who have worked at the subject, that there are great differences between such sera as to the intensity of this reaction. During the past fifteen months I have had occasion to examine the reaction in the blood of over 200 typhoid patients at the Auckland Hospital. While doing so I have attempted, by comparing such variations in intensity with the co-incident clinical features, to discover if possible what factors are at work.

While clinical material was abundant, I was handicapped by lack of time and assistance, and therefore have not been able to make the observations as complete as I could wish; but imperfect as they consequently are, they may have some features of interest.
Before giving the details of the cases it will be necessary to say something as to the methods of observation adopted, as it is evident that technical differences in the preparation of the specimens will produce variations in the reaction.

I followed throughout Bockels' plan of preparing a hair drop specimen for microscopical examination in the manner described by Dejerine. On several occasions I used the capillary zero-sedimentation method of Wright but found a difficulty in distinguishing between a true agglutination and the natural sedimentation which occurs in the dead culture.

The collection of the serum—

As most of my cases were in the hospital wards, I merely smeared the blood drop from the fractured finger on a clean glass slide, placed it in a shallow, moist chamber. Evaporation was thus prevented, and in a short time the serum had sufficiently separated to allow of a platinum loopful being free.
from corpses, being obtained. Where the specimens came from a
distance, I found the dried blood method of Whyatt Johnstone
convenient; but I found one objection to this plan was that
the re-diluted blood is not clear; masses of fibrin & blood pigment
floating about & obscuring the
reaction.

It has been put forward as an objection to Delphine's method that the diluted
serum is brought in direct contact with a part at least of the typhoid
culture while mixing the drops. This can be easily avoided by putting
a certain number of loopfuls of saline solution, or sterile broth,
at one corner of the cover-slip, &
diluting the serum with this; subsequent
adding enough of the culture to
bring the dilution up to the required
strength. Lately I have found it simpler to make a stock 1 in 5
dilution, on one end of the slide
containing the blood. This is kept
from evaporating in the moist
chamber & a number of preparations
of different degrees of dilution can be
made as required.
If the experiments are to have any comparative value it is necessary to have some uniformity in the materials used.

1. The medium containing the bacilli. This may be either a broth culture or an emulsion.

Broth. Where broth cultures are used it seems necessary that they should not be older than 24 hours; after this they lose their motility to some extent, and tend to clump spontaneously.

Age and acidity. The chemical reaction is also important. If it be too alkaline pseudo-clumps tend to form, motility diminishes; or if too acid, the bacilli are quiescent & the reaction delayed. I found it necessary to make trial cultures with each new lot of broth till the right degree of acidity was obtained.

Emulsions. Emulsions of cultures on solid media. These I have frequently used where the supply of broth had run short. Grüntseum recommends them as being of greater comparative value than broth cultures.

I certainly found that differences
in the age & virulence of the bacilli were very evident in using emulsions. Making them from agar cultures of from one day to three weeks growth (at 35°) I found that those over a week or ten days old showed less motility & a greater resistance to agglutination. A 24 hours colony. I think, tends to clumps rather too easily & be reliable. The chief difficulty has been encountered by Mr. Kerr — if the emulsion be too thick a number of bacilli always remain motile as though the agglutinating substance were all used up & some bacilli had therefore escaped its action. It is difficult to make an emulsion of the proper density, moreover, especially when the cultures are over a week old, some masses of bacilli may remain unbroken up & give the appearance of clumping, though such masses can generally be distinguished by their closely packed, even arrangement, quite unlike the irregular tubulated look of true clumps.

II The Virulence of the Culture

This, I am convinced, has a most marked influence on the reaction. When leaving Edinburgh at the beginning of 1894 Dr. R. Fair kindly supplied me with some cultures of typhoid from the University laboratory. These I brought with me to New Zealand, and with them my earliest observations were made. During the voyage out, for some months after my arrival, these cultures were not grown in the incubator; being without the materials for fresh media, I had to leave them for long periods without transplanting. They thus acquired a considerable degree of attenuation. After I had succeeded in obtaining fresh stock from the spleen of a typhoid patient I found a great difference in the resistance to agglutination between this and the original culture. The latter had in 24 hours formed a number of long slender, straw-like individuals, which clumped very readily, while the fresh culture had few such forms and was generally more active and more resistant. I have still kept up the Edinburgh stock and find that in many cases before the reaction
on virulent cultures is developed, this attenuated culture will attenuate even to high dilutions. I can often
by this means foretell when a good reaction is likely to take place later in the disease. But attenuation
of such an attenuated culture is not reliable, as the following case shows. The serum from a child, six years old,
with a typhoid-like temperature, headache, abdominal tenderness, but no definite clinical symptoms, gave a fair reaction
when diluted 1/25 with the emulsion of this Edinburgh culture. A virulent stock gave only a few doubtful clumps
even at a 1/10 dilution. The child died shortly after, and the post mortem revealed several acute tuberculous - no intestinal
ulcers - while no culture could be obtained from the spleen.
Hence I do not place any reliance on such a stock, although when in 24 hour broth the effects of attenuation are less marked.
I have produced a like degree of attenuation in a spleen culture, originally virulent, by growing it for seven or eight weeks
on agar at the room temperature, often making emulsions from a 24 hour subculture on potato. The long sluggish
forms were very plentiful, & the
alternation was so great that they
clumped entirely in 1-25 dilution
of normal serum & even partially
in saline solution alone. The growth
on potato seemed to be more plentiful
than normal also. By re-transplanting
& growing at 37°C on agar these
characters were lost again.

Even fresh cultures show a few of these
long bacilli-chains & they are always
the first to react. By growing the colonies
on agar at 37°C changing them once a
month or so they remain healthy.

Delphine & Bates Block have found that
too frequent transplanting produces a
tendency to spontaneous clumping, while
Johnstone got a 24 hour bottle culture
from old agar colonies grown at room
temperature which clumping took
place very easily. Such a culture
when injected subcutaneously produced
in rabbit Sood the typical reaction, but
without any accompanying loss of health.
I did not find in emulsions directly from
the long kept cultures this tendency to clump;
such are always more resistant even in the embryos
stock.

Mc. Wemey has shown that this occurs when cultures
are planted in diluted serum from early typhoid cases.
B. Mem. 1898. vol. II p. 5-93
It is evident, then, that conflicting results may be got by various observers according to the virulence of the cultures they use, and it would be desirable if some standard could be fixed.

Even fresh cultures from the spleens of patients dying of the disease show variations in resistance to agglutination, according, I presume, to their virulence. Of the seven or eight races which I have cultivated the most resistant came from a patient dying from acute poisoning within twelve days of the onset of the illness. This strain has kept up through twelve months of laboratory growth.

Dead cultures I have used a good deal lately and find them very reliable. With the less powerful sera, they require a little longer time than the living ones. They show an astonishing amount of Brownian motion and the reaction differs little from the ordinary.

I cannot say whether the virulence when living has any influence, but if not they would be useful as a standard of reaction.

...
III. Dilution.

There seems to be a good deal of difference of opinion between the various authorities as to what is the lowest limit of reliability. Durham got a reaction from the blood of patients suffering from infection with B. Enterit. Sabin, at a 1-10 dilution with typhoid cultures while Lorrain Smith considers 1-100 as the proper degree.

With virulent cultures I never found a reaction with other than typhoid sera (unless febrile sera may be considered such) above a 1-10 dilution.

I have been in the habit of making a 1-25 dilution first when, if the reaction on such cultures is not definite, I repeat the observation at 1-12. If this again is unsatisfactory I use an attenuated culture. The time limit throughout has been half an hour, and I have made what must be confessed is a somewhat rough classification of the reactions:

Marked = practically all clumped, some motile
Definite = good clumps but a few motile forms
Feeble = clumps sufficient for diagnosis but many motile
Indefinite = at 1-12 dilution only poor clumping.

Such cases often give definite with attenuated cultures.

Of course such a classification can only be made where, as in my case, all the observations are taken by the same person, when after a time a fair amount of uniformity can be ensured. Some loss of mobility, I sometimes noticed, occurred where the serum was other than typhoid, but generally it indicated a reaction later on. Errors are not easy made after a little experience, but at first a beginner, like myself, might fall into various technical pitfalls, such as using the platinum loop while hot to collect the serum. I had at one time some conflicting results from the use of cover plates, for the hanging-drops, which had been used before I must properly cleaned. After boiling and placing them in pure sulphuric acid for a time they can be used again safely, but it is perhaps safer to take fresh ones each time.

I found that a temperature of 100°C. for fifteen minutes quite destroyed the reaction, J. Jennae has shown that this occurs at 70°C. after ten minutes.
Classification of Cases.

All 256 cases have been examined which may be grouped thus:

Cases probably typhoid — 209
Cases clinically doubtful or with uncertain reaction — 4
Cases having had typhoid sometime before — 4
Cases clinically not typhoid — 39

Besides these many normal bloods were examined as control preparations.

The 39 cases not typhoid clinically may be dismissed in a few words. They include patients suffering from pneumonia, from obscure suppurative conditions, from ill marked phthisical focus, from attacks of simple gastro-enteritis, or so on. I did not examine the blood systematically in all cases admitted only those where some doubt existed.

For this reason, probably, I met with but few instances in which such cases showed an agglutinating reaction, such as Bates Black Fathers have recorded. In these there was a slight reaction but never sufficient to cause error in diagnosis.

I have already mentioned the case of
general tuberculosis; while clumping the Edinburgh culture definitely at 1-25 dilution of a few feebly clumps formed in an emulsion of virulent culture at 1-10. The post-mortem showed definitely that there was no typhoid infection.

2. A child of two suffering from an irregular febrile attack of septic type. The Edinburgh culture gave poorly formed clumps through many remained mobile, while the virulent stocks were scarce affected. This was on the 16th day, 4 days later the same results were obtained. After this, definite physical signs of apical pneumonia appeared. The child recovered.

3. Another case showing a septic temperature gave on the 9th day a doubtful reaction with attenuated cultures at 1-12 dilution, 9 negative result with virulent ones. Five days later it was negative to all the stocks. A peripheritic abscess ultimately developed.

It is necessary to say that in this group of negative cases are included seven cases only which could only be called febrile. Among the series placed as typhoid some such febrile types occur which reacted well.
In these negative instances it is impossible to say whether there was a mild typhoidal infection. Their occurrence during the typhoid epidemic leaves them open to this suspicion. In the cases which reacted well were clinically identical it is possible that in these others are examples of the failure of the Widal reaction.

Of 18 such febrile cases reacted, 7 did not. 4 gave doubtful results.

From Sorenson Smith's work it seems likely that some of the coliform bacilli, which he found in a water supply co-incidently with an outbreak of typhoid, which have no interaction with Elberth's bacillus, are infective. These anomalous cases may be due to invasion by some of the less virulent species.

In the 213 cases of febrile attack possibly typhoidal, four uncertain reactions occurred among the febriculae. They had a certain degree of agglutinative power on the attenuated cultures & guinea, in one case, on the virulent. In only one was a second examination made, with a negative result in that instance.

The case probably typhoid, giving doubtful result.

on the single occasion on which it
was examined, but which ran
a fairly typical course clinically, may
be classed with these four culture
making a group of five which
may be considered the failures of the series.
This leaves 208 cases in which the
serum reaction justified a diagnosis
of typhoid being made.
Before referring to these I will mention
the four in which I made an examination
some time after the attack of typhoid.
1. A nurse, 17 months previously had
a definite attack of the disease
1-20 dilution gave feeble reaction to a
virulent stock but 1-10 was definite.

2. A nurse, attack 2½ years before
Marked reaction at 1-12 dilution with a
virulent stock.

3. A nurse, attack 2 years before
Marked reaction at 1-25 with a
fairly virulent stock.

4. A patient admitted for other causes
Attack 2½ years previously very severe.
Marked reaction at 1-25 on
virulent culture.

Definite evidence therefore had remained
for a good while after these attacks.
Others have found it after many years.
In 206 cases the serum gave a definite reaction. I have grouped them according to the variations in intensity with a view to the study of the circumstances which may have had an influence in the production of such variations. The following points have been investigated:

1. Age & sex
2. Day of disease on which the examination was made.
3. Clinical aspect of the case at the time.
4. Violence of the attack.
5. Effect of residence, within or without an infected area.

I can dismiss the question of age & sex at once as having little influence. The proportion of males to females remains fairly constant throughout the groups; while the extremes of age, i.e. children under 12 & adults over 30, are, if anything, in higher percentage in the less markedly reacting classes, the proportions are not marked enough to call for remark.
The cases group themselves thus —

Class I. Those giving a marked reaction to virulent cultures at the first examination, comprising 145 cases.

Class II. In these the reaction to virulent cultures was only definite, though they might agglutinate the attenuated stocks markedly. 31 cases.

Class III. Requiring re-examination; reaction being absent or only feeble at first. Later on in the disease they gave a full reaction. 14 cases.

Class IV. Requiring re-examination, but gave at no time a good reaction. 8 cases.

Class V. Those giving a feeble reaction, but in which for various reasons no re-examination was made. 10 cases.

It is evident that from the last group little can be learned as further observation would have placed them in classes III or IV; however as this was not carried out it is necessary to place them in a group by themselves, for they were not sufficiently strong to be classified with the second group.
Classes I & II should have been subdivided according to the time in the disease at which the examination was made, but the result would have been too complicated.

Classes III & IV are more instructive, being fairly complete so far as the number of observations goes.

It may be generally stated that in 14.5 or 70% the reaction was strong... 63% 30% of reaction was weak or delayed.

Some idea of the actual clinical value of the test may be arrived at thus—

Considering the 73 cases, classified as "definite" in regard to their clinical symptoms, as those in which a diagnosis of typhoid could be arrived at without the assistance of the Widal reaction; we have left from the whole 213 possible typhoids, 140 in which no such diagnosis could be made. 119 of these occurred in classes I, II, & IV, these being cases where the reaction was at once sufficient to settle the question. That is to say, the test cleared up 85% of the doubtful cases.

The following table gives the results of the points which I have investigated—
<table>
<thead>
<tr>
<th>Class I: Strongly morbid</th>
<th>Total Cases: 208</th>
<th>Class II &amp; IV</th>
<th>Class III</th>
<th>Class IV</th>
<th>Class V</th>
</tr>
</thead>
<tbody>
<tr>
<td>Kid Reaction</td>
<td>Reaction</td>
<td>Reaction</td>
<td>Reaction</td>
<td>Reaction</td>
<td>Reaction</td>
</tr>
<tr>
<td>145 cases</td>
<td>31 cases</td>
<td>71 cases</td>
<td>8 cases</td>
<td>10 cases</td>
<td>50.0%</td>
</tr>
<tr>
<td>70%</td>
<td>15%</td>
<td>6%</td>
<td>8%</td>
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<table>
<thead>
<tr>
<th>Class I: Total</th>
<th>Class II: Kid Reaction</th>
<th>Class III: Reaction</th>
<th>Class IV: Reaction</th>
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<tbody>
<tr>
<td>57 cases</td>
<td>24 cases</td>
<td>90 cases</td>
<td>80 cases</td>
<td>70 cases</td>
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<tr>
<td>11%</td>
<td>15%</td>
<td>33%</td>
<td>26%</td>
<td>50%</td>
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<table>
<thead>
<tr>
<th>Class I: Total</th>
<th>Class II: Kid Reaction</th>
<th>Class III: Reaction</th>
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<td>29 cases</td>
<td>71 cases</td>
<td>51 cases</td>
<td>7 cases</td>
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<td>7%</td>
<td>26%</td>
<td>50%</td>
<td>36%</td>
<td>10%</td>
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<table>
<thead>
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<th>Residence</th>
<th>City</th>
<th>Country</th>
</tr>
</thead>
<tbody>
<tr>
<td>101 cases</td>
<td>70%</td>
<td>70%</td>
</tr>
<tr>
<td>20 cases</td>
<td>70%</td>
<td>64%</td>
</tr>
<tr>
<td>9 cases</td>
<td>64%</td>
<td>64%</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Residence</th>
<th>City</th>
<th>Country</th>
</tr>
</thead>
<tbody>
<tr>
<td>41 cases</td>
<td>30%</td>
<td>30%</td>
</tr>
</tbody>
</table>
Class I.

These cases gave good reactions at the highest dilution used, namely 1:25. In all but the earlier cases, fresh virulent stocks were used. The first 30, however, were examined by means of the Edinbraugh culture. These 30 cases were remarkable for having a large percentage of severe cases, five of which were fatal, while only one could be called mild. As a fresh broth culture was used, however, it is probable that the results are fairly accurate.

This is far the largest class, representing 70% of the whole number. It contains a large percentage of cases in which the clinical symptoms were definite at the time of observation, and likewise a large number of severe cases, the death rate for the group being 11.6%. While over the whole, 20.8 it was only 9.6%.

Class II.

In these cases the reaction was a degree less intense than the 1st group. A serum giving a marked effect of a virulent culture at 1:12, but only a poor result at 1:25, would be included in this class.
It is the second largest of the groups, but three of the cases were examined rather late in the disease; that in making the comparative observation I have placed them in Class IV, as being instances of a peculiar reaction throughout. One of these was examined on the 26th, another on the 25th, and the third on the seventh week. This makes the figures in the table appear rather conflicting, but it was, I think, the most reasonable course to take, though of course they might have been instances of a diminishing reaction. The proportion of cases with definite clinical symptoms in this class is low, as also is the death rate, which is only a third of that in class I.

Class III.

This is a rather small group, the repeated examinations giving a fair idea of the development of the reaction. It is small, however, which is to be regretted as the cases seem to me to be especially interesting. There is a high proportion of cases with the symptoms clinically definite and typical in the disease; most being quite typical, with rose spots, enlarged spleen, &c. &c.
Not only two developed into severe cases, while none died. 36% were mild, so they suggest the idea that the tissues had been able to overcome what threatened to be a severe attack. That the aggravating reaction should not appear till this victory had taken place is noteworthy. Nor was the absence of reaction at first due to carelessness of examination for the 6th day was the earliest date for the 1st observation. The proportion of city to suburban cases is about normal.

Class IV.

This also is a satisfactory group. It is into this class that I have introduced the three cases from Class II.

The remarkable feature of this class of reactions is, that, though the number of mild cases is high, there is also a high proportion of severe ones, with so high a death rate as 18% of the group. Though the small number of cases makes this ratio perhaps exaggerated.

It seems to include the extremes of virulence, and to some extent the characteristics of class III, which has 50% of ordinarily severe cases, no deaths, while in only two were the symptoms distinctly definitely those of typhoid.
Class V.

This is an unsatisfactory group.
In these cases the reaction was just insufficiently strong to justify a positive diagnosis, and while generally good with the Edinburgh or other attenuated stocks, was feeble with the virulent ones.

Probably many would have been placed in class V had re-examination been made; but the latest examination was not until on the 18th day, 9 in two on the 16th. The examined on the 6th, 9 three on the 7th and might have shown a better reaction later.

This class had 60% of mild cases—the highest of the groups—three of which might be called petrificus. The death rate was again high, but owing to the smallness of the group, this is not reliable. Only one death occurred, a herring woman living in the country, aged 64—a prototypic illness of a sub-acute type. The Post Mortem was allowed.

It is noticeable that one half of this group were cases from the country, though the general rate for the whole cases examined was one in three.

A general survey of the features of this class makes one inclined to include it with group IV.
Date at which the reaction appears.

It is hard to arrive at any definite conclusions as to this point, especially as the onset of the disease is generally so vague. Patients' statements are most unreliable, but I have always taken the date on which they first felt any symptoms of illness, as the commencement. It is useless to take the average date of examination as typical of the several classes, nor does a statement of the day on which the reaction was first felt, help much, as in classes I and II, at least, it might have been found earlier had an examination been made.

It probably in most cases appears about the 7th day.

In some cases the earliest date on which a full reaction was present was the 2nd day - a very mild case. In 4 it was found on the 5th day, 9 in 3 on the 6th.

In class II, definite agglutination was seen on the 4th day in 2 cases, 5th day in one case, 6th day in three cases.

In classes III, IV, the reaction was obtained on the 6th day in one, on the seventh in three, on the 8th in three, t
it was absent also on the 9th, 10th, 11th, 12th, 13th, 14th, or 21st days in other cases; while in two it was absent till the 4th or 5th weeks, though in the two latter the history was very vague. In these cases reaction appeared generally 5 or 6 days after, once next day, and in another case on the 2nd day following.

In only two cases did I find the afflative power to diminish later on in the disease. Both were mild; in one it was feeble on the 7th day—definite on the 10th, but entirely absent in six weeks; in the other it was somewhat feeble on the 10th day & negative on the 16th.

Evident & heard found in some cases quite unaccountable variations throughout the disease; though generally the curve of intensity rose towards the end of the attack, & diminished slowly during convalescence.

Re-examinations made in a few cases in classes I never showed any diminution later in the illness.

The Clinical aspect of the case, at the time of examination, seems to have some bearing on the reaction, as if those groups, where this was less marked, began, with the noticeable exception of class III,
a higher percentage of clinically indefinite cases than has class I, in which only 80% were such.
This division of the cases was somewhat difficult, as typhoid is such an indefinite disease. Of the history of onset, the temperature, the abdomen, if the spleen suggested typhoid, it was classed as definite; if always well marked rose spots were present. This last symptom was almost invariably accompanied by a full reaction, the only exceptions being six of the twelve "definite" cases in class III.

Relapses—seemed to occur fairly evenly throughout the classes. Nidel & Stern found them to occur in cases with a high assimilative power.

Two of my cases may be noted. In one, after an ordinary convalescent period of four weeks' duration, a relapse occurred during which the patient died. On the 18th day of the primary illness a marked reaction was noted, but only an attenuated culture was used, and no examina-

mild attack of three weeks duration went out, but after a few weeks was brought back suffering from a severe relapse. The reaction was rather peculiar on the 6th day of the intervening convalescence, which would rather suggest that the tendency to relapse was coincident with a low agglutinative power. However, being only a solitary case, no conclusions should be drawn.

Durham found experimentally that fresh doses of virus diminished the agglutinative power, but in the cases recorded by Wentzel & Simple, of re-vaccination with dead cultures, the reaction was increased by so doing.

It would be interesting to know whether the relapse is always due to re-invasion by the Charth's bacillus. Isn't it possible that the bacillus coli, being in a specially virulent condition during typhoid attacks, may be responsible for some of these relapses? or, in the light of Larrain's two ferment work, which the relapse was genuinely typhoid, might not the original illness be due to invasion by the coli type present in a polluted water supply?

A series of comparative observations, as to
the serum reaction before and after
the relapse, to B. typhosus and other
resembling types would be of much
interest.

Severity of the attack.
Widal & Ricard, reviewing 21 cases,
consider that there is no relation between
the severity of the agglutinative power,
and they find that it appeared any earlier
in severe cases. Johnston states that
many of those of a peculiar type only give a
partial reaction, though in some it is marked,
while others, found the instances of delayed
reaction to immunity in mild cases.

From, on the other hand, got the best
reactions in mild cases, I suggested that
a weak reaction was co-existent with
severity of the attack, another observer
got more marked reactions in these
mild cases & late stage of severe attacks.
On the whole I found that an acute attack
was accompanied by a strong reaction
in most cases, while the contrary held
time with mild types of disease.
Thus in class I the death rate is 11.6%,
only 6.3% in the four groups of febrile reaction.
The general result may be stated thus:

<table>
<thead>
<tr>
<th>Class</th>
<th>Cases</th>
<th>Percentage</th>
</tr>
</thead>
<tbody>
<tr>
<td>I</td>
<td>57</td>
<td>76%</td>
</tr>
<tr>
<td>II to V</td>
<td>18</td>
<td>24%</td>
</tr>
<tr>
<td>Total</td>
<td>75</td>
<td></td>
</tr>
<tr>
<td>Death</td>
<td>11.6%</td>
<td></td>
</tr>
<tr>
<td></td>
<td>6.3%</td>
<td></td>
</tr>
</tbody>
</table>

Severe cases:
- Total: 57
- Death rate: 11.6%
- Percentage: 76%

Mild cases:
- Total: 41
- Death rate: 6.3%
- Percentage: 5.9%

A reference to the general table also shows that Groups IV and V, representing the feeblest reactions, have much the highest percentage of mild cases, being 55% and 60% respectively.

With the fatal cases I never found a diminution in reaction to occur before death, such as Widal records. Full reactions were put in one case 24 hours, in one 2 days, 9 in another nine days, before death. In the four cases of death with lesser reactions the last examination was made some weeks before death, and thus do not favour Widal's assertion.

But further examination of the cases shows that good agglutination power does not depend simply on severity of the disease. The large conflicting results of various observers quoted above proves that some other factor must be at work.
In classes IV and V with the feeblest reactions the death rate is highest, despite the fact of the large proportion of mild cases. Again, some explanation should be forthcoming of the fact that over a half, or 59%, of the mild cases gave marked reactions. I think I have found an explanation of these anomalies by taking into consideration the two factors which I have already spoken of as influencing the type of the disease, namely, place of residence, & time period in the epidemic at which infection took place. I have previously shown that country cases, & those arising early in the epidemic, whether this be general or localized, tend to be more severe.

Compare now, the last division of the table, & we find that whereas of the whole 208 cases 68% are from the city, in classes I & II the percentage is 70. In class IV it diminishes to 64, while in class V it is only 50%.

It is necessary, then, to review the cases and their severity in this light.
Modification through Place of Residence of the influence on Intensity of Reaction exercised of Severity.

Wild Cases

Of 70 mild cases 50 came from the city water area, 20 from districts outside of this. These groups themselves thus

<table>
<thead>
<tr>
<th>Class</th>
<th>City area</th>
<th>Class II</th>
<th>Class III IV V</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>71%</td>
<td>74%</td>
<td>81%</td>
</tr>
<tr>
<td></td>
<td>29%</td>
<td>26%</td>
<td>19%</td>
</tr>
</tbody>
</table>

The normal percentage for the 208 cases was, City water area 66%, to Rural 33%; but the ratio of mild cases generally is higher for the city, 4 much higher in the better reacting groups; while it falls below the normal in the groups of delayed or feeble reaction. Again the cases are distributed throughout the groups thus

<table>
<thead>
<tr>
<th>Class</th>
<th>City water area</th>
<th>Rural</th>
</tr>
</thead>
<tbody>
<tr>
<td>I</td>
<td>31 = 62%</td>
<td>11 = 55%</td>
</tr>
<tr>
<td>II</td>
<td>9 = 18%</td>
<td>2 = 10%</td>
</tr>
<tr>
<td>III IV V</td>
<td>10 = 20%</td>
<td>7 = 35%</td>
</tr>
</tbody>
</table>

This modification of the ratios is most marked in group IV in which the mild cases are distributed equally between city & rural.
Indeed it is in this class of continued febrile reactions all the country cases are mild.

I conclude, therefore, that the mild cases may be divided into two groups; the one, represented by the city cases, being made up of individuals who have been greatly exposed to the risks of infection before actual invasion takes place; or the other group consisting of cases in which such exposure has not taken place, as in the country. The first group react well on the whole, & the second feebly.

This effect of exposure to infection is further borne out when we apply the other principle, which I have advanced, as to the increase in mild cases in the city as the season advances.

Taking the mild cases in groups large enough to give a satisfactory proportion (the numbers in each month being small) we get the following result:

<table>
<thead>
<tr>
<th>Total mild cases</th>
<th>Class I</th>
<th>Class II &amp; V</th>
</tr>
</thead>
<tbody>
<tr>
<td>Nov. 1897 to Jan. 31, 1898</td>
<td>15</td>
<td>9 = 60%</td>
</tr>
<tr>
<td>April 1st to April 30th</td>
<td>13</td>
<td>9 = 70%</td>
</tr>
<tr>
<td>May 1st to July 31st</td>
<td>12</td>
<td>9 = 75%</td>
</tr>
<tr>
<td>Aug. 1st to Nov. 30th</td>
<td>10</td>
<td>3 = 30%</td>
</tr>
<tr>
<td>Dec. 1st to Feb. 1899</td>
<td>19</td>
<td>11 = 57%</td>
</tr>
</tbody>
</table>
It is evident, then, that the proportion of strongly reacting mild cases rose steadily to the end of July. It then dropped during the following four months in which the general epidemic had subsided and began to rise again with the next outbreak. The highest proportion was reached in June 1878, when, in a total of seven cases five were mild, all of which gave first-class reactions. Four out of these 5 cases came from the city area; indeed, the rule may be considered to apply to these cases throughout, as the number of mild country cases does not affect the periods to any extent, except, perhaps, in that from Aug. 1st to Nov. 30th, when two out of the three first-class reactions came from the country.

Turning to the two localized outbreaks, in which all the sufferers were probably exposed to an equal degree of infection, we find the same increase in strength of the reactions coincident with the diminution in severity.

It may be objected that the last cases in each outbreak were too mild clinically to be considered typhoid. Still, with so strong a serum reaction, as with the history
of the previous cases in the family. The evidence is in favour of a true typhoidal infection in the last patient. The Powsely outbreak ran thus:

<table>
<thead>
<tr>
<th>Date</th>
<th>Day of examination</th>
<th>Reaction</th>
<th>Severity of case</th>
</tr>
</thead>
<tbody>
<tr>
<td>1. 28-12-98</td>
<td>9th</td>
<td>1-12 Ed. cult. Indefinite</td>
<td>Death</td>
</tr>
<tr>
<td>2. 31-12-98</td>
<td>8th</td>
<td>1-12 Vir. cult. Definite</td>
<td>Severe</td>
</tr>
<tr>
<td>3. 11-1-99</td>
<td>12th</td>
<td>1-12 Vir. cult. marked</td>
<td>Ordinary</td>
</tr>
<tr>
<td>4. 14-1-99</td>
<td>10th</td>
<td>1-25 Vir. cult. Definite</td>
<td>Ordinary</td>
</tr>
<tr>
<td>5. 14-1-99</td>
<td>12th</td>
<td>1-25 Vir. cult. Definite</td>
<td>Ordinary</td>
</tr>
<tr>
<td>6. 14-2-99</td>
<td>2nd</td>
<td>1-25 Vir. cult. marked</td>
<td>Very mild</td>
</tr>
</tbody>
</table>

I have given the limit of reaction in these cases; thus case 2 gave no reaction to inoculated culture at 1-25, 1 case 3. marked any, though it was marked at 1-12. Thus case 6 was the only one with a full reaction. It was moreover the earliest day of the whole 204 cases in which I got a reaction.

In the Stanley family the reactions ran thus:

<table>
<thead>
<tr>
<th>Date</th>
<th>Day of examination</th>
<th>Reaction</th>
<th>Severity of case</th>
</tr>
</thead>
<tbody>
<tr>
<td>1. 2-1-99</td>
<td>9th</td>
<td>1-12 Vir. cult. Definite</td>
<td>Severe</td>
</tr>
<tr>
<td>2. 10-1-99</td>
<td>10th</td>
<td>1-12 Vir. cult. Definite</td>
<td>Severe</td>
</tr>
<tr>
<td>3. 10-1-99</td>
<td>10th</td>
<td>1-25 Vir. cult. marked</td>
<td>Severe</td>
</tr>
<tr>
<td>4. 16-1-99</td>
<td>6th</td>
<td>1-25 Vir. cult. marked</td>
<td>Ordinary</td>
</tr>
<tr>
<td>5. 22-1-99</td>
<td>5th</td>
<td>1-25 Vir. cult. marked</td>
<td>Very mild</td>
</tr>
</tbody>
</table>

The last three cases here gave a full reaction in spite of the diminishing severity.
It would seem, then, that whatever the cause of the decline in severity may be, it has also the effect of producing an increase in the reaction power.

Severe cases.

The effect of residence on the reaction in these cases is less marked. This is partly owing to the fact that the first 30 cases, which were, in large proportion severe, were only examined with the Edinburgh culture, and some good reactions.

43% of all the severe attacks came from the country, largely owing to the Strakou outbreak, which while the number of severe cases was high, there was also a high proportion of good reactions.

Listing the first examinations, for the reason given above, we are left with 50 severe cases, of which 36% gave a poor reaction. They were then distributed

<table>
<thead>
<tr>
<th>Class</th>
<th>I</th>
<th>II</th>
<th>III</th>
<th>IV</th>
<th>V</th>
</tr>
</thead>
<tbody>
<tr>
<td>City</td>
<td>25</td>
<td>15</td>
<td>60%</td>
<td>10</td>
<td>40%</td>
</tr>
<tr>
<td>Rural</td>
<td>25</td>
<td>17</td>
<td>68%</td>
<td>8</td>
<td>32%</td>
</tr>
</tbody>
</table>

From this it would appear as if the effect of residence was in severe cases, the reverse of that in mild ones.

The period in the epidemic also seems to have but little effect, but the numbers are too small for accuracy.
To summarize the results, we may say that a strong reaction is generally found in mild cases from the city area, severe cases from the rural districts, while the cases giving a lesser reaction are largely the severe attacks in the city and the mild attacks in the rural districts.

I conclude therefore—

I. That the intensity of reaction varies generally with the severity of the disease.

II. That the above rule is modified by exposure to infection previously to the attack, such as probably occurs in an endemic area, with the following results—

1. Mild cases are more plentiful, more
2. Especially towards the end of the seasonal outbreak, but
3. Such mild cases develop a strong agglutinative reaction.

III. While this modification can not apply to rural cases in general, it does so where, as in the rat-bite fever, a localized outbreak occurs, if all individuals are thereby exposed to the risks of infection. In such cases those least to be affected will tend to show a mild type of disease but a high agglutinative power.
IV. Seasonal and residual differences affect the severe cases but slightly as regards reaction. They occur less frequently in those previously exposed to infection, but when such persons do suffer from a severe attack, the reaction is more often feeble, than in persons not so exposed.

V. There is a class of febrifugae which do not react well and do not therefore come under the rule. They occur during a general outbreak but show no seasonal variation.

IV. There is a special class of cases, occurring independently of exposure to infection, characterized generally in the earlier stages of the illness, by severe symptoms which, however, soon greatly subside. In them, during the height of the disease the agglutinating reaction is absent or feeble, but later as the symptoms modify, it becomes strong. This class of case is typified by group III.
Conclusions as to the nature of the Reaction.

Whether the presence of the affluterative reaction is a sign that the individual is immune is apparently a matter about which there is some doubt. Widell calls it a reaction of infection & not of immunity owing to its presence before relapses & also because it diminishes in convalescence when immunity is highest. Grimbaum, on the other hand, considers that the affluterative power is in proportion to the immunity, it is a result of the process; in the case of relapses immunity is imperfect but not absent, as the severity of such attacks is not great; hence the presence of the reaction is not incompatible with its being an index of immunity. That it cannot be a reaction of infection he thinks is disproved by the fact that it appears in the blood after vaccination with dead cultures.

Grimbaum, while admitting that the affluterative power is distinct from the immunizing, considers that the two are in ratio of that the course of affluteration is an index of the reaction of the
organism against infection. This seems to be the view taken by
Duchams also. In his paper delivered at the meeting at Edington in August 1895
he showed that in the case of the Cholera spirillum, at least, the agglutinative,
lysozyme, & protective properties, the
which develop in the serum of the
invaded animal, are in proportion.
That a serum possessing agglutinative
properties has an antagonistic action
on the specific bacillus has long
been known from the experiments
of Pfeiffer. The effects of temperature
& of keeping are exercised over the
agglutinative & protective qualities
of a serum equally.
We conclude that they are to some
extent linked.

Wright & Temple, in discussing the value
of their vaccine, come to the conclusion
that the agglutinative power may be
taken as an index of immunity since —
1. It has been shown that the two are always
co-existent in the serum of vaccinated animals.
2. The serum of convalescent patients which
is known to be bactericidal to the typhoid
bacillus possesses this reaction.
3. In one case of a person who showed
a high agglutinative reaction, inoculation with virulent culture failed to produce any ill effect.

4. While the bactericidal properties present may ward off an attack, it does not follow that its acquisition during the disease will prevent a fatal issue.

In the whole the evidence seems to point strongly to the reaction being co-incident with immunity, to be an index of the resistance which the animal offers to invasions.

On applying this theory to many results a reasonable explanation of their causation seems to offer itself.

In an area in which the inhabitants are frequently exposed to the means of infection, whether in the water supply or otherwise, there seems some reason to expect a low degree of immunity should develop in such persons. The typhoid bacilli in a contaminated water lose their virulence to a great extent if finally die out as Klein & others have shown. Here, then, we have an attenuated virus in great dilution — as it necessary for
the production of immunity; as the season progresses this medicine containing the bacilli or their products gets more impure from further contamination or concentration with the low rainfall. Thus a gradual increase in the dose would be provided. It is surely not impossible that the individuals using such a medicine should be thus rendered to a slight extent temporarily immune. Dr. Seymour Taylor suggested such a possibility as an explanation of why new arrivals in an endemic area should show a greater susceptibility than residents, for there seems little doubt that such is the case.

Such a theory would explain the seasonal decline in the severity of cases in the City water area; those attacked late are having been longer exposed to the immunizing influences which possibly get stronger as the season advances, until the rainfalls at once destroy the infective and the immunizing properties. Why some should escape till late in the epidemic is not explained by this theory, but that such a condition of things occurs from
one cause or another is, of course, a fact. It might be objected that where there was such a degree of immunity, there should be a corresponding degree of serum reaction. I have no proof that there is a slight agglutinative power in such sera, unless the reaction in the non typhoid cases to the very attenuated stocks could be so taken. It would at all events be but slight, as Durkheim has shown that inoculation with a species of low virulence failed to produce a reaction in the blood sufficient to agglutinate species of higher virulence. A very low degree of immunity is all that could be expected. In the two local outbreaks I have detailed the severity of the early cases suggests a high degree of virulence in the infection, & this corresponds with the rapid immunity (should my theory be admissible), & the rapidity with which the immunity showed itself; for the last cases occurred within two months of the first. Again, in comparing the two outbreaks, the latter cases in the one which occurred in the city showed the immunity
in a greater degree than in the rural
epidemic, the case being much
milder. This would explain by the
former individuals having super-added
the degree of immunity which I contended
is conferred by residence in the endemic
area, for Statikus cannot be considered
such, that of 1898 being the first general
outbreak: nor would this outbreak affect
the family in question who lived isolated
from the village.

The theory also explains the high agglut-
inative action of these mild cases which
have occurred after they have been
partially immunized. For such immunity
would enable the body to react more
strongly against the infection and therefore
the index would rise higher. This
result of the invasion by a virulent
culture after a degree of immunity
has been acquired by inoculation of less
virulent ones is just what has been
done in the artificial immunization of
animals by Pfeiffer with the typhoid
bacilli.

Thus, as the season advances a higher degree
of immunity is conferred, the milder will
be the type of disease, if the stronger. The
agglutinative power after the ultimate invasion.

The same principle can be applied to the explanation of the cases in Class III. These cases for some reason react strongly against invasion. The severity of the early symptoms indicates a powerful dose of the virus; later, as the organism overcomes this dose there is a rapid disappearance together with the development of a high agglutinative reaction, which again can be taken as the index of the resistance shown to the invasion.

The poor reaction in a fair percentage of the severe cases is more difficult to explain. Grinbaum has advanced the reasonable explanation that owing to the large number of bacilli concerned in the attack the agglutinative material is used up and the weakened body cannot produce more. But then certain of these severe cases show a good reaction, so I found a larger percentage of these in the presumably unprotected cases. Possibly the severe cases of the latter type are those reacting to a moderate dose of the virus, which had serious
effects owing to the lack of protection, but which was not virulent enough to overcome all reaction of the body against such invasion. Indeed the石家庄 cases, which form a large proportion of this class of reaction, showed a final triumph over the invasion for severe as they were the death rate was only 5 per cent. The city cases, being partially immune, required a strong degree of virulence in the invading Bacillus to produce any effect, so virulent, indeed, that the organism is powerless to resist it, when so powerful a weapon does invade.

The conditions in the city might also favour the production of this very powerful virus. Again it must be admitted that among the number of persons in a city some may lack the disposition necessary for the full response to the immunizing conditions I suppose to exist.

Again we must consider the possibilities of some other agent besides the typhoid Bacillus being at work. It might be that not only these non-reacting severe cases but also the mild cases of febrile which
grieve but a quiet suggestion are the result of an infection, mixed or not, by the coliform bacilli which Christopher and others have demonstrated.

If a water be infected by typhoid bacilli, it is reasonable to suppose that the bacillus is in the virulent form which it takes on during an attack of typhoid, is also present; it may then be the cause of the cases of diarrhoea and fever which so often accompany an outbreak of typhoid but which give no reaction to Bertolet's bacillus. Lorraine Smith's observations at Belfast strongly favour a mixed infection with such types; moreover, that these types have the effect of preparing the body for a severe invasion by the typhoid bacilli. Samaelli's experiments also point in this direction.

Regret that I have not made any observations in these severe but non-reacting cases as to their action on the coliform bacilli which I found in the water supply; but the time has been too short since the report on the Belfast outbreak came to hand.
There is a striking resemblance between these type of the city outbreak, the localized outbreaks, & the cases in class III.

In each, the early severity — whether it be the cases in the epidemics or the symptoms in the class — is accompanied by poor agglutinative power; while later, when there are milder clinical features, the serum reaction is strong. It suggests the idea that there must be a general law acting towards the production of the same effects in all three cases. And though my explanation involves the assumption that a whole community has conferred on it some degree of immunity, that this immunity rises & falls, each outbreak, according to the season, still it seems to me to fit in with the phenomena which I have observed, fairly reasonably.

I thought at first, when I noticed the autumnal decline in severity, that this was due to the virions getting more attenuated owing to meteorological conditions. But this would not explain the rise in agglutinative power resulting from the attacks; nor would it explain
why the country cases & visitors of the town were more severely attacked even late in April 1848 when very severe cases occurred among visitors to Auckland & again class III could not be explained by a weakened virus.

I thought also, that the stronger reaction in May & June which first called any attention to the subject, might have been owing to my stocks becoming attenuated. But when I came to compare these with fresh stocks isolated in December I found the latter showed no more resistance to agglutination. Again the local outbreaks showed that this explanation was untenable.

I regret that my position in so isolated a part has made it impossible for me to obtain full accounts of the work done by the many authorities on the subject.

New light would doubtless have been thrown on my results which, after all, only apply to one epidemic; while my observations have been, through want of facilities & the absence of anyone who could give me expert advice, anything but complete. While, therefore, I do not consider that I have in any way proved my point, I can claim that the idea is so far as I am concerned - original.