acknowledged, more, or not much more, voluminous than is the motor province of the middle level. I now consider this matter. Evolution is not an "even process," not one to be properly symbolised by the raising of an expression to a higher power, called Involution in algebra. If I may put it so, increasing evolution in the nervous system may, at least in some cases, be likened to the raising of some parts of an expression to a higher power than other parts of that expression are raised to. From theoretical considerations I think that in man the motor province of the highest level (a division of the "mental centres") represents very many movements of parts of the body which have small muscles (properly small movements) and that it represents comparatively very few movements of the parts having large muscles; if so it especially represents most complex movements of the ocular muscles, of the muscles of the hands, and of those of the tongue, lips, and palate; these are movements represented (of course, with corresponding sensory elements) in the physical bases of visual and tactual ideas and of words in the highest level ("mental centres").

The main elements of that part of mind which is commonly distinguished as intellect from the other part called emotion or feeling, are Visual and Tactual ideas and Words. Much the greatest part of mentation, both in the sane and the insane, is carried on in visual ideas; if all visual ideas were cleared out of a man's mind he would become practically mindless; there will be a very great representation of ocular movements in the highest level. Further, much mentation is carried on in tactual ideas. Herbert Spencer¹³ has pointed out that intelligence in animals is proportionate to the development of tactual organs; to use his words: "a highly-elaborated tactual apparatus comes to be the uniform accompaniment of superior intelligence"; there will be a great representation of movements of the hands in the highest level. I suppose no one denies that words serve in all higher thought, in what has been called conceptual thought. I think that the physical bases of the psychical things we call words are audito-articulatory nervous arrangements and suggest that highly complex and special movements of the tongue, lips, and palate are represented in the highest level.

It is interesting that, according to Weir Mitchell, after amputation of limbs the parts of the lost limbs which remain spectrally, or I might say mentally, present to the patients are the terminal parts, hand and foot; the sufferers are, in most cases, unconscious of the part between the phantom hand or foot and the stump, if I may be pardoned this expression; they are "conscious of the parts" having small muscles; here is some indirect evidence that those parts are much more represented in the highest level than the parts having large muscles.

In this connexion I draw attention to a very interesting paper by Mr. F. H. Bradley.¹⁴ In introductory remarks Mr. Bradley asks: "Why, when we strive to move in dreams do we not always move?" Perhaps this accords with the hypothesis that large movements are but little represented in the highest level ("mental centres"). Mr. Bradley makes the interesting remark that dream movements are easier in some dreams; "thus, for example, it is common to move the lips and tongue and fingers" (parts having small muscles).

I think we may conclude, taking into account not only the physical bases of visual and tactual ideas and of words, but also the vast number of connecting fibres implied by the innumerable combinations into which these ideas and words enter, that the motor province of the highest level, if not more voluminous, is yet more intricate than the motor province of the middle level; and that destruction-lesions of the former are more tolerable than equal-sized destruction-lesions of the latter; the reverse for discharge-lesions.

NOTES AND REFERENCES.

¹ *Med.-Chir. Trans.*, 1890. ² *Brain*, vol. iv. ³ *Lancet*, December 22nd, 1894. ⁴ *Journal of Mental Science*, October, 1874, p. 401. ⁵ I do not say that all fits called epileptic (so-called idiopathic) are owing to discharge-lesions of parts of the highest level. There are seizures, called epileptic, depending on discharges beginning in parts of the temporo-sphenoidal lobe; convulsions of these lobes may not be parts of the highest level. ⁶ *Brain*, April, 1888. ⁷ For an able criticism of my opinions on this matter, see a paper by Dr. Gay: *Brain*, January, 1890. ⁸ *Clinical Journal*, March 1st, 1893. ⁹ The word "comparatively" is to be understood here and in many other places. ¹⁰ On the Spinal Cord and Medulla Oblongata and on Epilepsy, *New Syd. Soc. Trans.*, p. 64. ¹¹ *Psychology*, vol. i, pp. 35 and 55.

¹² *Phil. Trans.*, vol. cxxix, 1888, B. ¹³ *Principles of Psychology*, vol. i, pp. 35 et seq. ¹⁴ On the Failure of Movement in Dreams, *Mind*, July, 1894, p. 373.

NOTES ON AN EXPERIMENTAL INVESTIGATION INTO THE GROWTH OF BACILLUS TYPHOSUS IN SOIL.

By JOHN ROBERTSON, M.D., B.Sc.,
Medical Officer of Health, Sheffield.

UNTRACED SOURCES OF INFECTION.

In probably over 80 per cent. of the reported cases of typhoid fever the origin of the infection cannot be ascertained with any certainty. Usually some insanitary conditions in the surroundings of the patient are credited with giving rise to the disease. It is common experience that in a majority of cases when the sanitary condition are remedied, further cases of the disease are prevented. Again, it is also a common observation that so long as insanitary conditions exist cases of typhoid fever are apt to recur at longer or shorter intervals.

From observation and experience during a number of years it has become now, I think, generally accepted that the typhoid poison is generated outside the human body, and gains access by a great variety of ways.

The evidence, too, which has accumulated during the past few years must put it as almost beyond doubt that the bacillus typhosus of Eberth is the exciting cause of the disease we term typhoid fever. The theory, therefore, that the bacillus typhosus is fairly widely distributed in Nature, and that it is capable of living, and possibly of growing, for long periods outside the human body is one which, I think, is generally accepted, and is one which experience has shown to be of much value in the prevention of typhoid fever.

"TYPHOID AREAS."

It was my good fortune to have for several years exceptional opportunities of observing the special features of endemic typhoid fever in my capacity as Medical Officer of Health at St. Helens, one of the most typhoid-stricken towns in this country. Among the many interesting points in the natural history of the disease which one observed was that there were certain areas in which the disease showed a special tendency to recur at varied intervals.

One of the most marked examples of such an infected area occurred in a group of seven cottages in which 18 cases of typhoid fever were reported in eight years and a-half. Apart from the fact that these cottages had unpaved yards and privy middens, they were in a good sanitary condition. Perhaps 5 or 6 of the total number of cases were caused by direct infection, but the remaining cases occurred at intervals and under such conditions that one could arrive at no other conclusion but that there was some local source of infection which had existed in this locality during the whole of that period.

The above is an exaggerated example of what one observed in a great many other districts of the town.

EXAMINATION OF THE SOIL.

The question naturally arose as to where the actual propagation of the disease-producing organism took place. It appeared to be a matter of the first importance to determine this; because, having done so, one could the better formulate a scheme for the destruction of the infecting material.

With this object in view, I undertook a rather laborious research, during the first portion of which I was associated with Dr. Maitland Gibson, my assistant, to whose zeal, accuracy, and careful work I shall ever owe a debt of gratitude.

In the first instance, samples of soil—using the term "soil" in its widest sense—were collected from what I considered the most "probable" of the areas. These were examined by the methods which are detailed later with a view of finding the typhoid bacillus if it were present. Thirty such samples were collected and examined. In not one single instance was *B. typhosus* found. In many instances parallel control experiments were carried on with duplicate samples to which had been added small quantities of fresh culture of *B. typhosus*.

As a result of considerable experience of our best known methods of isolating the typhoid bacillus from infected material, I feel certain that it would be unwise to conclude that the organism was entirely absent from all of these soils. The number of organisms present in the "soils" examined was always great, but what made these investigations specially difficult was the number of organisms which were, either morphologically or from their cultural characteristics, very like the bacillus of typhoid fever. Dr. Klein has truly said that the search for *B. typhosus* under such circumstances is about as difficult as searching for the proverbial "needle in a haystack."

On many occasions similar researches have been undertaken by Continental bacteriologists, but in so far as I am aware in none of these, when positive results have been reported, has the investigation been quite satisfactory. Within the past few months Professor Delépine has been able to demonstrate the presence of *B. typhosus* in the soil of an infected area.

In this connection, too, how frequently have waters been examined by the most experienced bacteriological technologists, waters which one would say must have contained *B. typhosus*, and yet the results have been entirely negative. Several marked instances of this nature have occurred within the past few months in this country. Again, the careful and accurate investigation of Andrews and Parry Law on sewage from one of the London hospitals in which typhoid dejecta were actually thrown into the sewers without disinfection, went to show how extremely improbable were the chances of finding the bacillus typhosus in the presence of other filth organisms.

EXPERIMENTAL INOCULATION OF SOIL.

Having thus entirely failed to demonstrate the presence of bacillus typhosus in the soil from presumable infected areas, I next proceeded to ascertain if it were possible for the typhoid organism to grow in the ordinary earth of the district, and if so, under what conditions. For these purposes inoculations were made on patches of soil in a field, and laboratory experiments were carried out in large sized chloride of calcium towers, which were filled with a variety of soils. In character the soil in the field, which afterwards yielded such interesting results, was as follows: On the surface about 8 inches of clayey loam, under which there was a stratum of sand and clay 10 inches thick, below which was a thick layer of stiff yellow boulder clay. The ground water level was high—it often came within 3 feet of the surface. A daily meteorological record was kept, which included 4 feet soil temperature and ground water levels. A daily estimation of the amount of carbonic acid in the ground air at 1 foot below the surface was made with a view to its giving an indication of the bacterial activity of the soil at any particular time.

The field where the outdoor experiments were carried on had not been manured for over ten years. Each experimental patch of ground had the turf removed, and nothing was allowed to grow on it:

Patch No. I was inoculated on May 30th, 1896, with about 200 c.c.m. of a twenty-four-hours' old bouillon culture of bacillus typhosus, which had been recently isolated from the spleen of a man. The bouillon was added to about 1½ gallon of tap water, and the patch was slowly drenched with it on its surface.

Patch No. II had 9 inches of soil removed before it received a similar quantity of the culture. The soil was replaced as nearly as possible in its original order of stratification. Great care was taken that the sides of the excavation should not be inoculated.

Patch No. III was similarly treated, but at a depth of 18 inches from its surface.

Nothing was done to these patches until August 26th. On this date samples were taken from the surface of each patch—at about 3 inches below the surface of each—at 9 inches below the surface of Patches No. 2 and 3, and at 18 inches below the surface of No. 3. The surface samples were taken by skimming the surface with a sterile knife. The deep samples were taken by excavating at one side of the patch, and punching samples from the exposed face.

In every one of the samples bacillus typhosus was found with comparative ease. No attempt was made to compare the number of colonies of bacillus typhosus found in given quantities, but both Dr. Gibson and myself were convinced that extensive new growth had taken place. Between the sowing of these patches and August 26th the weather had

been on the whole dry, with abundance of bright sunshine. Total rainfall, 5.2 inches.

Another series of samples were taken on October 20th, the weather having been in the interval dull and very wet. On this occasion practically identical results were obtained.

On November 27th a third series of samples were taken and examined by identical methods and with equal care. In none of these could bacillus typhosus be found. Prior to the taking of these samples the weather had been cold, damp, and unsettled; the minimum shade temperature in the Stevenson's screen had been on several occasions below freezing point. The average of the readings of the thermometer for the week ending November 7th was 23° F.

Toward the end of August, 1896, three other patches, Nos. 4, 5, and 6 were inoculated in an identical manner to the first series. The only difference between the two series was that each of the patches in the second series was 3 feet square, while those of the first series were 18 inches square. Patches 4, 5, and 6 were examined on November 27th, and found also to contain no bacillus typhosus as far as could be ascertained.

Nothing was done until the middle of January (17th), 1897, when patches Nos. 4, 5, and 6 each received about 2 gallons of very weak beef tea (the solution containing about a teacupful of ordinary bouillon). At intervals of about a fortnight these patches (4, 5, and 6) received doses of various dilute organic solutions. This was continued until June 3rd (over 5 months). During the whole of this time patches Nos. 1, 2, and 3 received no organic matter.

On June 3rd, 1897, samples were taken from each of the six patches. In the case of the samples from patches Nos. 1, 2, and 3, no typhoid organism could be found. This was in very abrupt contrast to the fact that in patches Nos. 4, 5, and 6 the typhoid organism was easily found.

On July 11th, 1897, another series of samples were taken from each of the patches, with the same very interesting result. On this occasion samples were taken at 6 inches below the surface of Nos. 2, 3, 5, and 6. In those from Nos. 2 and 3 no bacillus typhosus could be found, while in those from Nos. 5 and 6 it was found.

At this point my investigation had to be brought to a conclusion, owing to my leaving the district, and as there is obviously some difficulty in continuing such an investigation in a new locality, I have thought it best to put in evidence what has been found, in the hope that others may repeat the experiments, and perhaps confirm them. One is convinced, however, that there are a very large number of points which require further investigation before the results can be absolutely accepted. The length of time which these experiments necessitate will make it many years before the main conditions of growth of the typhoid organism in the soil can be satisfactorily inquired into.

METHODS OF ISOLATING THE BACILLUS FROM SOILS.

The amount of labour and difficulty entailed in the examination of a sample of more or less polluted soil for the typhoid bacillus can only, I think, be fully realised by those who have attempted such examinations. Such soils as have fallen to my lot to examine have contained enormous numbers of organisms which resemble in many of their morphological and cultural characteristics the bacillus of typhoid fever. It therefore becomes a matter of much importance in an inquiry such as this to make sure that reliable methods are adopted. As is well known, there is no one method which can be easily and generally applied.

Briefly, the method which was found most reliable, though by no means expeditious, and the one which was adopted for all the later experiments, was as follows:

I. The samples were collected in sterile test tubes with a sterile spatula; Each consisted of numerous small portions of soil, which were well mixed in the test tube.

II. About ½ to 1 gramme of the sample was then added to about 200 c.c.m. of distilled water in a sterile flask, and this was incubated for 24 hours at 37° C. Several methods which depend for their success on the inhibition of the growth of many non-typhoid bacteria by the addition of chemical substances were investigated, but in my hands these proved not to be reliable.

III. Sterile agar plates were poured and allowed to set, with a sterile camel-hair brush from 2 to 4 such plates were inoculated by brushing the surface with the soil water. This has the great advantages of producing only surface colonies.

IV. These plates were incubated and examined under a low power

(Zeiss obj. A. oc. 2). Every colony resembling in its appearance the *B. typhosus* was picked out with a platinum needle and examined in hanging drop with a Zeiss obj. $\frac{1}{2}$ and oc. 6.

V. Those which were found to be short motile bacilli were at once subcultured into milk.

VI. Those milk tubes which became coagulated after incubation were rejected.

VII. The milk tubes which did not become coagulated were subcultured into bouillon for indol and also into glucose gelatine for gas formation or into Theobald Smith's tubes and on to potatoes.

VIII. Pure cultures on agar were then obtained, and the organism examined by Van Ermengem's or Loeffler's method as to the disposition of their flagella, and also as to its reaction with typhoid serum of known strength.

I feel certain that it is a matter of much importance to know exactly how a culture of *B. typhosus* grows on the various media used. There are often slight differences in media prepared at different times by apparently identical methods which alter to some extent the character of the growth. To obviate this it was found important to prepare large batches of the different materials at one time. In the case of potatoes the slices taken from one potato ought to be kept by themselves, and each batch tested as to its reaction with a typical typhoid culture.

CONCLUSIONS FROM THE EXPERIMENTS.

The above experiments were carried out under conditions as natural as could be devised. Experiments carried on with soils in the laboratory were not found to give satisfactory results. The results obtained from the outdoor experiments are very suggestive, and, I think, throw some light on the natural history of *Bacillus typhosus*. They prove that the typhoid organism is capable of growing very rapidly in certain soils, and that apparently under certain conditions the organism can survive from one summer to another. The rains of spring and autumn or the frosts and snows of winter did not kill them off.

What the exact conditions are which caused the organism to live for over twelve months in the one series and not in the other series of soils cannot at this step be definitely stated; but whatever these conditions may have been, there remains the one obvious fact that in the one series the organism was fed while in the other it was not. If this be the real cause of the difference it would appear that very similar conditions exist in the so-called typhoid infected areas. A leaking or defective drain is constantly feeding with organic matters the surrounding soil. If cases of typhoid fever are attributed to this drain and the leak is repaired, it is possible that a similar action will take place to that which occurred in the soils which were not manured.

So also the infected privy would appear to be admirably adapted for the preservation of the typhoid bacillus, owing to its constantly receiving fresh additions of organic matter. Whether all soils will support the healthy growth of the typhoid organism equally well is a subject for further research.

SUNLIGHT.

Many experiments during recent years have gone to prove most conclusively that direct sunlight has a most powerful bactericidal influence on the typhoid organism. The results obtained by the above soil experiments go to indicate (as indeed one would expect) that the effect of sunlight is limited to those organisms actually on the surface. Samples of soil which had been collected by scraping a layer of soil not more than one-sixteenth of an inch in thickness from the surface of patches which had been exposed to abundant sunshine showed the organism to be present. It must be borne in mind, however, that the particles of soil are more or less opaque, and that it was impossible to roughly separate the exposed from the unexposed soil.

SPREAD OF THE TYPHOID ORGANISM IN SOILS.

With the object of ascertaining whether the typhoid bacillus had spread from the original infected area on to the surrounding soil (which happened to be grass land), samples were collected at distances of 1, 3, and 9 feet from two of the patches in July, 1897. No bacilli were found in any of these.

Some experiments on similar soils in the laboratory, with and without grass went to show that vegetation was detrimental to the healthy growth of the organism. These experiments could not be carried as far as was originally intended for reasons already stated. The subject is one of some im-

portance, as it may go to explain why typhoid fever is so much more prevalent in towns than in rural districts. It also may throw some light on the fact that persons employed and living in the neighbourhood of sewage farms do not suffer from typhoid fever to the extent that might have been anticipated.

It has been already stated that cultures of the typhoid organism planted at a depth of 18 inches grew to the surface, so also patches inoculated on the surface showed that at least the organism could grow downwards to a depth of 3 inches. It is possible under the existing conditions that this downward growth may have been assisted by mechanical means, that is, rain or artificial watering.

So far as the results go no lateral spread could be ascertained.

Both in laboratory experiments and in experiments under natural (?) conditions it was abundantly proved that the typhoid organism could grow over moist surfaces of stones, etc. It was attempted in another series of experiments to detach the organism from the stones when dry by causing a current of air to impinge on them; this, however, in each case proved unsuccessful.

How far the typhoid organism can be given off from decomposing masses of semiliquid filth was also examined into. For this purpose sterile plates of agar or gelatine plated were inverted over masses of semisolid excrement, which had been largely contaminated with a culture of the typhoid bacillus. Such plates when kept at the ordinary temperature remained sterile for a period of nearly three months. Those that were kept at 37° C., and which presumably were exposed to a very actively decomposing mass, soon showed growth of various organisms. In no case did the typhoid organism appear among the growths. These plates were distant 1½ and 4 inches from the decomposing faecal matter, and at both distances apparently equal contamination took place.

NOTE ON THE SPECIFIC ACTION OF NORMAL HUMAN SERUM UPON THE BACILLUS COLI COMMUNIS.

By S. R. CHRISTOPHERS, M.B. VICT.

(From the Pathological Laboratory, University College, Liverpool.)

THE application of the serum diagnosis to the differentiation of the typhoid bacillus from the various varieties of the *Bacillus coli communis* has not hitherto been as satisfactory in its results as might have been anticipated.

Peckham gives a table showing the action of typhoid human serum upon seventy cultures of typhoid and coli bacilli obtained from various sources, and lays stress upon the anomalous results obtained by her application of this test to the differentiation of the two forms. In her list the majority of cultures giving the biological characters of coli were undoubtedly agglutinated by human typhoid serum; one in particular she mentions as giving the reaction as fully, that is, clumping, and almost entire cessation of movement, as did any of the typhoid cultures.

Ziemke states that whilst using cultures of coli as controls in the performance of the Widal's test, he frequently found that they were agglutinated by typhoid human serum.

Bensaude also states that the agglutinins of typhoid serum act upon the *B. coli*, though in a less degree, than upon the typhoid bacillus, and explain this action as being due to the close biological relationship of the two organisms.

Such marked action of typhoid serum upon the *B. coli* would naturally invalidate the strict specific nature of the reaction between typhoid serum and the typhoid bacilli. There is, however, a source of error, which I have not seen mentioned, which may be to a large extent responsible for these puzzling results. It is that many have used as the differentiating test the blood of human typhoids. Now in the following table it will be seen that normal human serum has in the great majority of cases as specific an action upon the *Bacillus coli communis* as has the serum of a typhoid patient upon the *Bacillus typhosus*; that, in fact, the serum of typhoid patient probably has two distinct specific actions, one upon the typhoid bacillus due to infection by that organism, and one upon the *B. coli*, an action which is present alike in typhoid and non-typhoid sera.