

light of other recurrent disease, give us a solution. Enchondroma, fibroma, &c., we find, return after removal in distant parts. We have the evidence that these structures may be carried by the lymphatics or bloodvessels, and when arrested at any point may grow. If this may occur in the case of structures so coarse (if one may use the expression) as those named, how much more likely would be its occurrence in connexion with a structure the very essence of which is absence of coherence of its elements. It may be illustrated by the difference we see when a recurrent non-cancerous growth and a true cancer are cut into. In the former case, in order to get at any of its component parts for microscopic purposes, we must cut off or scrape up a portion of the tissue. In the latter, the cells, which constitute the active part of the cancer, exude on the cut surface, which presents a creamy character. Nothing is more easy to conceive than that the elements which compose this creamy matter, lying free and without limitary membrane in the midst of soft tissues, would be taken up by them, by their lymphatics or bloodvessels or connective-tissue spaces, and be carried anywhere and everywhere through the body. Principally, one would expect to find them within the range of the primary tumour, on the same side of the body; and so, in reality, we do. But there is no real limit. Just as we find that when a paper of pins is swallowed they may course through the body to any extent, upwards or downwards or in any direction—so, only infinitely more readily, might these minute germs of disease travel to any extent or in any direction. The differences observable in the various forms of cancer offer a support to this view. Take the four forms of rodent cancer, epithelioma, scirrhus, and encephaloid. It is, I am well aware, a disputed point whether rodent ulcer is really cancerous. I believe it to be so, as the structure of the deposit around the ulcer is like that of epithelioma, and on account of its strong tendency to recurrence. Its elements are, however, very localised—it does not travel usually even to the neighbouring glands. Epithelioma, of which the elements are grosser and more coherent than those of the higher forms, will affect the neighbouring glands, but will not readily pass beyond them, though it may do so, and give rise to secondary growths, just as will scirrhus. Scirrhus readily affects the glands, and will contaminate distant parts, but with nothing like the rapidity that is often seen in encephaloid, the structure of which is far softer, and the fluid more abundant, and laden with minute elements, nuclear and granular; yet it is found that of all the true cancerous diseases encephaloid is the one which may go on to spontaneous cure or to long quiescence. This would appear to militate against the view that cancers contaminate in proportion to the fluidity of their contents. But an explanation is found in this: encephaloid is often the least infiltrating of cancers; it pushes the tissues aside, it forms connective-tissue capsules around it, and hence in a large number of cases it is not brought into connexion with the tissues, as scirrhus is, so as to be disseminated amongst them. And this very fact, that the most malignant of cancers, the most rapidly growing, and the most destructive when it is disseminated, is often encapsuled, often non-infiltrating, and often less venomous than other forms, confirms the notion that it is not to constitutional but to local conditions that we must attribute the malignancy of the disease.

The laxity of tissues and the activity of vital action going on in them exercise a marked influence on the dissemination of cancer. In young persons—in those in whom there is much fat—tumours grow more rapidly, and are more quickly and widely disseminated. I have removed cancers from the midst of abundant fat, where minute points of disease have been traceable in all directions around, to the distance of from two to three inches. Surgeons know that they cannot take the fat away from around a cancerous breast too widely. Surely no one would consider these as evidences of blood-disease setting up cancers *de novo*, considering that the fat is not a tissue in which primary cancer is formed.

(To be continued.)

THE Town Council of Longton, Staffordshire, held a conference recently with the medical practitioners of the town as to the best mode of suppressing the small-pox. It was decided to make a house-to-house visitation with the view of persuading people to be vaccinated or revaccinated.

ON THE ETIOLOGY OF TYPHOID FEVER.

By P. W. LATHAM, M.D.,

FELLOW OF DOWNING COLLEGE, CAMBRIDGE; PHYSICIAN TO ADDENBROOKE'S HOSPITAL.

IN THE LANCET of July 1st the following account, on the authority of Dr. Flint, is given of the propagation of typhoid fever; and an annotation is appended that "it would scarcely be possible to devise an experiment which would better test the communicability of typhoid fever by contagion than this":—

"A stranger, who had been sick for several days, stopped from off a stage-coach at a tavern in a little village eighteen miles from the city of Buffalo called North Boston, and, after a few days, died. The village consisted of nine families, closely grouped together around the tavern. All these families, save one named Stearns, used water from a common well, and visited the sick stranger at the tavern. Prior to the arrival of this stranger there had been no typhoid fever there, but twenty-three days after his arrival a member of the landlord's family was taken down with the disease. Other cases quickly followed, and in a month more than one-half the population, numbering forty-three, had been affected, and ten had died. Stearns' family, being on ill terms with the tavern-keeper and most of the other families, who were his tenants, did not visit the sick stranger, and had been forbidden the use of the common well, and as they alone, of all the families immediately surrounding the tavern, escaped the disease, he was accused of poisoning the water of the said well—a charge which chemical analysis showed to be entirely unfounded."

Most certainly these facts conclusively prove the *communicability* of typhoid fever, but I hesitate to accept them, though with considerable diffidence, as any proof that the disease is *directly contagious*. An equally remarkable instance, which I shall presently relate, of the spread of the disease came under my own observation three months ago, and seemed decidedly to prove that the disease had spread by personal contact; further investigation showed the contrary.

The theory which my own experience leads me to support is as follows:—1. That *almost* invariably the disease proceeds from a special poison contained in the alvine excreta. 2. That this poison is *directly* introduced into the alimentary canal, either in the food or, most generally, in the water drunk. 3. That it is not yet proved that the poison is contained in, or is disseminated by, the exhalations from drains, privies, &c., or that it can be absorbed into the system through breathing air contaminated with such exhalations.

The epidemic at North Boston lends support to my first point, and Dr. Flint himself says: "The fact that all the families in which the disease prevailed were supplied with water from a common well, and the fact that the family in which no case occurred did not obtain water from this well, afford ground for supposing that a virus derived from the excreta was conveyed in the water drunk." Though the stranger died within a few days after coming to the tavern, it was not until twenty-three days after his arrival that the first case occurred. This is a long period of incubation, assuming that the disease was contagious, but not at all too long if we assume that the cesspool or drain into which the excreta were poured was leaky, and so allowed the poisonous material gradually to filter through the ground into the well.

The epidemic to which I have referred above as coming under my own observation occurred at Harston, a small village about five miles from Cambridge. Between the 1st of January and the end of April of the present year, no less than twenty patients were placed under my care in Addenbrooke's Hospital suffering from typhoid fever. A large proportion, and those the most severe cases, were sent from Harston, where I learnt that whole families were being affected, and several deaths had occurred. I wrote to my friend Mr. Trestrail, asking him to investigate what I had heard respecting the introduction of the fever into the village, also for information about the wells and drainage, stating my belief that the disease was spreading through contaminated drinking water. He replied: "I find that you are correct in stating that there is a distinct history of the introduction of typhoid fever into this village. It ap-

pears that a man who had barely recovered from the fever came as a lodger to Mrs. P—, who resides in the village, and whose child was subsequently attacked. The second case was that of a Mrs. C—, a patient of mine, who had been in the house of Mrs. P— occasionally. Mrs. C—'s sister was afterwards taken ill with typhoid fever. The next case was that of a child who had also been to see the child P— frequently. The next two cases were persons living very near; and then cases around them. Nearly all the cases have been near each other, where the houses are overcrowded—no gardens nor any proper drainage. It is usual for these persons to drink water from one of the springs. Now these springs are continually running at the rate of many gallons per minute; they come from upwards of 200 feet below the surface, and the water is conducted up from this distance by tubes. It is therefore highly improbable that these springs can become contaminated."

Here, then, we have the disease introduced into a district and spreading rapidly, the first persons attacked being in direct communication with each other, and the drinking water not liable to contamination. Evidently a clear proof of its contagiousness! But wait. In a subsequent letter Mr. Trestrail writes: "I have discovered the cause of the spread of the fever—namely, there is running directly through the village a stream of water, into which nearly all the drains from the houses enter. The water of this stream has been generally used by the poor for cooking purposes, as the springs are at some little distance from their houses. Since the attention of the people has been called to the great importance of using only spring water I have not heard of any fresh case of fever in the village."

The outbreak of typhoid fever last year in Islington is strongly corroborative of the view that the disease spreads in the manner I have mentioned. As stated in THE LANCET for Nov. 26th, 1870, at a certain dairy the milk cans were washed in water taken from a tank communicating with the drains, and a little water of course remained behind: there was no charge of gross admixture of water with the milk. The dairyman himself died of typhoid, and, of 140 families supplied with the milk, 70 contracted the disease. The majority of the cases occurred in close proximity to the dairy; but "in one long road a mile and a half from the dairy there were three families thus supplied with milk; two of these had typhoid in them, and they were the only houses in which the malady occurred."

Another point against the contagiousness of the disease is the rarity of its infecting, in hospitals, the physicians, nurses, or the other patients. I have, however, seen this happen, and I have no doubt that such cases have led to the assumption that the poison is oftener introduced by way of the trachea than by way of the œsophagus; but, to my mind, the simple explanation is, that after examining the rose spots on the back or abdomen, or after auscultating the chest, the hands of the physician have been imperceptibly soiled by contact with the skin or linen; as he continues his round he perhaps unthinkingly brushes his hand across his lips, and so the poison finds an entrance. With respect to the nurses, there is sometimes, after removing the soiled linen of the patients or the bed-pans, not that scrupulous attention to cleanliness which is so essential, and thus, eating or serving out the diets of other patients with unwashed hands, some little speck of fever-poison is introduced with the food. I say "in hospitals"; for in private houses the nurses and friends are often exposed to the same influence which caused the illness of the patient, and so may be infected, but not necessarily through the patient.

I would, in conclusion, make two practical suggestions with regard to the treatment of typhoid-fever patients:—1. That every evacuation, as soon as passed, should be disinfected with carbolic acid. 2. That no surface-well water or pump water, whether boiled or not, should be used in a district where there are any cases of typhoid fever, but that all drinking water should be obtained from some spring not liable to contamination. I need not say that this precaution is of the greatest importance to the patients themselves. I am quite sure that neglect of it gives rise to many of the relapses in typhoid fever cases, perhaps more frequently than any other cause except the too early administration of solid food. And with respect to this last point, though I am now going beyond the subject of my paper, I would just point out the supreme importance of

the thermometer in telling us when solid food may safely be given. It should not be given until the temperature of the patient at 8 A.M. and 6 P.M. has remained, for two days at least, about the normal point, or between 98° and 99° F. The patient's tongue may be clean and moist, the appetite ravenous, the patient crying out for food, and yet the typhoid ulcers still unhealed. The thermometer alone will tell us this; it will probably show at this stage an evening temperature of about 101° F., with a morning temperature 1·6° to 2° lower; and a mutton-chop now might be sufficient to induce fresh irritation of the intestinal ulcers, fatal hæmorrhage or perforation. It is only after the evening temperature has remained, on at least two successive days, below 99° F. that we can be sure the ulcers have healed, and that solid food may be given without risk. Out of the twenty patients mentioned as admitted into Addenbrooke's Hospital only one died; but had not careful thermometric observations been made, the mortality would, I think, have been greater. In two who recovered there was, after the fortieth day of the disease, considerable hæmorrhage from the bowels, and this after the tongue had become moist and clean, the motions perfectly formed, and the patients for some days had been bitterly complaining at only being allowed beef-tea, milk, and wine. The evening temperature still rose as high as 101° F., and no solid food was given. If it had been I have little doubt it would have turned the scale in each case against the patient's recovery.

Cambridge, July, 1871.

A CASE OF EXCISION OF HALF THE LOWER JAW.

By WM. THOMAS, M.B. LOND., F.R.C.S.,
DEMONSTRATOR OF ANATOMY, QUEEN'S COLLEGE, BIRMINGHAM.

R. P—, aged thirty-one, a milkman. With the exception of the illness produced by his tumour, has always enjoyed good health. Seven years ago he hurt his jaw in trying to untie a knot in a hayband with his teeth. The pain of the injury passed off in a few days, but soon afterwards a little lump appeared on the left angle of the lower jaw; this gradually increased until it had the appearance shown in Fig. 1. The

FIG. 1.



From a photograph taken shortly before the operation.

growth of the tumour was at first slow, but much more rapid during the six months previous to removal. It encroached but little on the cavity of the mouth, the enlargement being for the most part on the outer and under surfaces and in front of the ramus. The molar teeth were gradually pushed