

the bones are small and numerous, the ligaments and surrounding tendons are strong, and it is not easy to apply such a degree and direction of force as will cause such an accident in preference to fracture. When dislocations do occur, they are mostly combined with laceration of the soft parts, being caused by severe machinery or gunshot accidents. The displacement of the bones then becomes of secondary consideration.

In combination with such severe injuries, the following dislocations have been observed. (a) Dislocation of the radius forward or backward, the ulna remaining attached to the carpus. (b) Dislocation of the ulna forwards, backwards, or inwards, the radius remaining attached to the carpus. (c) The tearing away of the carpal bones from the forearm. (d) Dislocation of the first and second row of carpal bones. (e) The scaphoid, pisiform, magnum, and trapezium have been dislocated separately. (f) Dislocations of the thumb are well known; it may be thrown backwards, forwards, or towards the index-finger.

In speaking, then, of dislocation of the wrist we must take into consideration the whole set of articulations. Such accidents are very uncommon, except as associated with severe compound fracture and laceration.

Margaret-street, Cavendish-square, 1870.

### ON THE LOCAL AND SPONTANEOUS ORIGIN OF ENTERIC FEVER.\*

BY CHARLES E. PRIOR, M.D.

(Concluded from p. 290.)

WE trace small-pox not unfrequently from house to house and from village to village, scarlatina and typhus with a little more difficulty, enteric fever with still more; and yet that enteric fever is a contagious disease has, I think, been already proved, not only by the essays of Gendron and Piedvacke, but to a minor extent in a paper which I had last year the honour of reading before this Society, and by the admission and experience of practitioners of reputation in this district.

How and where these diseases commenced their dismal pedigree is buried in the mists of time—beyond, perhaps, the age of flint weapons, ere yet the troubles of Israel or the plagues of Egypt had begun. Small-pox, we know, existed in China before historical records; deserts and want of communication, a horror perhaps of its ravages, barred for centuries its passage to Europe. Of typhus and enteric fever, whose nature is only just ascertained, and whose characteristic eruptions with their differences have scarcely yet fixed themselves in the professional mind, we have no reliable record as to their origin. It is enough for my purpose that they belong to the same group of exanthemata, and are subject to similar laws. We have no more right to infer a spontaneous origin for enteric fever from the analogies of exanthematous disease than we have to assume a new creation in a freshly discovered insect. Spontaneous generation has been assumed for measles produced by the dust of a certain vegetable during the American war. I have not the account at hand, but I believe it rests on the authority of Dr. Richardson. Such a statement needs close investigation, and, in the absence of that, I must express my decided conclusion that the analogies of nature and the study of kindred disorders are all evidences against the spontaneous generation of enteric fever.

But there is yet another side to this branch of the question. Some febrile diseases are of spontaneous origin—of local origin. Granted. What are those diseases? Have they any peculiar characteristics? Do they differ in any way from the exanthemata? They have; they do so differ. Intermittent and remittent fevers form a group by themselves: their origin in malaria is unquestionable. The non-connexion of that malaria with foul odours or organic refuse has been repeatedly demonstrated. It has been found in its most deadly form on the barren sands of the Tagus, or the rock of Gibraltar; in the stony ravines of India, where scarcely a blade of grass can grow, or a trace of organic

matter exists. I have myself met with it in the elevated valleys of the Malvern range. It is the specific gravity of malaria alone that indicates its existence; to chemistry, to smell or to vision it is alike unknown.

It is the characteristic of the fever of ague and of the paludal form of yellow fever to be incommunicable by contagion, nor have they any typical eruption; in fact, their non-contagion would appear to be almost a law of nature for the preservation of our species. Were it otherwise, in the words of Dr. Bancroft, "every dunghill, every collection of decaying animal or vegetable matter, might become the source of a fresh contagious and pestilential disease, and mankind, surrounded by such accumulated perils, would soon cease to exist."

Never having seen a case of yellow fever, or resided in a hot climate, I speak with diffidence on the subject. It would appear, however, that there exists in tropical latitudes a certain specific form of the disease, communicated by contagion and continuous in type. If this be so, it may eventually turn out to be a disease having the same relation to the paludal disorder that our own enteric fever bears to ague. Possibly, with further research a typical eruption may be discovered. Be this as it may, the grand fact remains, that in England at least, and probably in other countries, the intermittent fevers of local or paludal origin are non-contagious and non-eruptive.

It would be begging the question to say that the converse of this is true—namely, that eruptive contagious fevers are not of local origin; but this much may be granted—(1) that they are not of paludal origin; and (2), a point which I have discussed with some minuteness, that enteric fever is beyond all question a contagious disease.

"Typhoid fever," says Piedvacke (p. 124), "reigns everywhere—in the dwelling of the rich as in that of the poor, in places low and damp as in places high and dry, in town as in country." Again: "Pathological observation teaches us that the affections which are the product of external and well-demonstrated causes are but very rarely, perhaps never, contagious. A simple enumeration would be sufficient to prove it." (p. 17.) Again: "Some local epidemics have appeared produced by emanations disengaged from vegetable and animal matters in putrefaction. But it is easy to demonstrate that the most part are independent of this cause. Hygiene has hitherto made little progress in the country. One often finds there dunghills before the doors and stinking water; dirt reigns in a great number of rural habitations. But typhoid fever is not in conformity with these circumstances: one sees it in houses rendered, by position and arrangement, most salubrious; in high places as in low, in town as in country." (p. 22.)

To this the experience of M. Gaultier de Claubry, in his report to the Academy (1848–49) on the epidemics of France, corresponds, and the testimony is of great value, as it comprises the experience of 162 communes in 28 departments, and of 122 separate reports on nearly 10,000 cases. "Relatively to the conditions of topography," says he, "the situation of places has thrown no light on the causes of the disease. A similar remark is applicable to the state of salubrity or of insalubrity of the villages where the disease has shown itself, as also to the variable degree of ease or of misery of the population who inhabit them. Singular as such an assertion appears, it is the rigorous expression of the numerous facts contained in the reports."\*

There yet remain for consideration two branches of the argument—namely, the argument from experiment, and the argument from experience.

*Experiment.*—The late Dr. Barker of Bedford endeavoured, by exposing animals for shorter or longer periods to cesspool air in a box constructed for the purpose, to ascertain the connexion between the gaseous products of faecal fermentation and certain febrile diseases. Four dogs and a mouse were thus exposed. In all the dogs diarrhoea and vomiting, more or less severe, set in; but all the animals, with the exception of the mouse, recovered. Beyond the symptoms named, there was no reason to suppose that the disorder produced was any stage of enteric fever.

Other observers and the same gentleman also have exposed animals to various gases the products of decomposition—namely, sulphuretted hydrogen, carbonic acid, ammonia, and sulphide of ammonium.

Fifteen experiments on sulphuretted hydrogen are re-

\* Read, Oct. 1868, before the S. Midland Branch of the B.M. Assoc.

\* Acad. de Méd., vol. xiv., p. 4 (1849).

corded by Dr. Barker and by Dr. Richardson, performed on dogs, hedgehogs, and birds. In most of the cases accelerated respiration, jactitation of the muscles, tremors, insalivation, and disorder of the bowels appear to have ensued, passing, if the experiments were pushed further, into narcotism, coma, and death. Careful post-mortem examinations were made in several of the cases, but in none was the characteristic intestinal lesion of enteric fever discovered.

Of the well-known effects of carbonic acid it is unnecessary to speak.

Dr. Barker also made eleven experiments on sulphide of ammonium introduced into the box containing the animals (dogs, jackdaws, and hedgehogs). General distress, lachrymation, diarrhoea, and salivation were the principal symptoms produced. Two dogs and one bird were poisoned, and their bodies were examined after death. There was nothing in the post-mortem appearances characteristic of enteric fever.

It appears to me that there is one grave deficiency which vitiates all these experiments. It is this: we have no evidence that any of these animals are susceptible of typhus or of enteric fever. In fact, I never heard of any domestic animal that had contracted the disease, unless it be the pig; and surely, if the local origin of enteric fever be not a myth, the conditions of piggy's existence ought to be causing daily victims.

The only animal in whom enteric fever can be studied is man. To experiment upon the human species is out of the question; and therefore it only remains for us to select from the records we possess those histories of the exposure of a healthy population to the products of decomposition supposed to be most capable of generating enteric fever, which, from their scale and circumstances, realise most fully the conditions of an experiment. Such a history is that of the removal of the Cimetière des Innocens at Paris in 1785—a space of two acres, which contained the remains of at least half a million of bodies in various stages of decomposition, yet which was carried out during two years without the slightest accident or any appearance of epidemic disease among the workmen. Such again is the great stink—if I may so term it—of 1858, when the Thames became in such a revolting condition as led to the enormous works of the metropolitan main drainage; yet, according to the testimony of Dr. McWilliam and others, the health of the police employed on the river, and of the population along its banks, was absolutely unimpaired, and no epidemic ensued. Such too is the history of the horrible Montfauçon, whose vile odours, arising from the bodies and refuse of thousands of dead horses, dogs, and cats, and from large pools of excrement collected from the entire city of Paris, were perceptible at two miles' distance, and were absolutely insupportable on the spot to the uninitiated; and yet, according to the testimony of Parent du Châtelet, the workmen employed there enjoyed the most robust health, and their families have shown no peculiar tendency to enteric fever. Beside observations of this magnitude, a few experiments upon animals with poisonous gases sink into insignificance.

It has been conjectured that sulphide of ammonium may be the peculiar deleterious agent which produces enteric fever. But nothing in the way of proof exists: no animal which has been exposed to the vapour has shown the usual symptoms of the disease after the ordinary period of incubation. In fact, I think myself justified in saying that the experimental proof of the spontaneous origin of enteric fever utterly breaks down.

*Experience.*—The last appeal lies to that most fallacious of all tests, "experience."

Many cases from many authorities have been cited in illustration of the spontaneous generation of enteric fever from fæcal fermentation, vegetable decomposition, and foul odours of all sorts. The late Dr. Barker collected thirty-six cases of the sort; but the diseases are not well discriminated. In fact, I believe that in many of the cases they are not correctly described, some of them having been gathered from non-professional sources; the history of some others is, to my own knowledge, incorrectly stated; and some are from professional men who were apparently unaware of the distinguishing characteristics of typhus and enteric fever. Many of them also are far too short; but, on the whole, there appears reasonable ground to suppose that about one-half of the number were cases of enteric, or, as it is termed, "typhoid" fever.

To Dr. Murchison, as the great apostle of pythogenesis, and one who has thereby, according to Dr. Aitken, "rashly committed science to an hypothesis of a highly doubtful nature" (p. 413), one naturally turns for the best evidence that can be produced in support of his dogma. Dr. Murchison has collected 13 cases, which are set forth with considerable detail. Of course it is out of my power to quote them in a paper of this sort. They include the Croydon and Windsor epidemics, the Clapham School case, the Westminster epidemic, alluded to by Dr. Watson, and others equally singular.\*

Many of these outbreaks may be accounted for on the explanation of Dr. Budd—namely, the transmission by the sewers of the poison from intestinal discharges. Some of them, such as the "Westminster fever," have been a matter of question as to whether they were fever at all. There still, however, remains a puzzling residuum. But then, on the other hand, we must consider the forcible facts brought forward by Dr. Chisholm, Dr. Hughes Bennett, and others, and briefly alluded to in this paper, in proof of the non-origin of enteric fever from simple fæcal fermentation. It is fair also to state that the experience of Piedvacke gives a certain proportion of cases in which the disease could not be traced to contagion, and appeared of spontaneous origin, children in the cradle being in two or three instances the first to be attacked, and the disease spreading from them. "In the country I noticed, out of 312 cases, 12 typhoid fevers at least where I acquired certainty that there was no anterior connexion, direct or indirect, with other patients; and they have been the stem from which sprung a great number of others." (p. 127, 128.) Piedvacke, however, does not name any peculiar connexion between these cases and defective cleanliness; on the contrary, he expressly denies such an origin.

Such is a brief review of the case from experience. It appears to me that there are certain considerations of which gentlemen who favour us with their observations are too prone to lose sight. Has the explanation of Dr. Budd been sufficiently considered, and has its applicability to the case in question been tested? Is it the rule, I would ask, or is it the exception, even in diseases of the most markedly contagious nature, to obtain an explanation of the origin of the disease? When is it so obtained in a case of scarlatina? How often in typhus, or even in small-pox? Yet what medical man for that reason doubts either their transmissibility or their transmission? The fact is that these diseases are always in existence around us, but their method of diffusion is far too subtle to be traced under all circumstances.

For a hundred and twenty years this country had remained exempt from rinderpest, sanitary defects notwithstanding; and again now it is verging towards extinction. Yet every now and then see how the cases crop up: one in Windsor, after seven months' intermission; others in Derbyshire, after an interval nearly as long; another in Wales, or it maybe even in Ireland. But do we therefore assume the spontaneous generation of rinderpest?

It is one of the laws of nature that animals and plants which propagate by continuous succession should appear in alternating periods of profusion or scarceness. The deeper the knowledge of the naturalist, the more instances will occur to him. The wasp—nay, the very birds are subject to this law. Whence came the humming-bird moths which appeared in such remarkable numbers in the summer and autumn of 1866?—whence the clouds of locusts or of gnats?

But when we descend to the lower forms of life observation is fairly baffled. Their rapid growth, their minute spores, their subtle powers of penetration, their mysterious disappearance and equally mysterious return, their wonderful migrations, are such as the present powers of man are totally incapable to follow—scarcely even his philosophy to comprehend.

At this point I think it fair to leave the present subject.

\* Among Dr. Murchison's cases will be found one, supposed to have occurred in Bedford, of spontaneous generation of enteric fever from polluted water. It is very extraordinary that neither myself nor my friend Mr. Blower, though both living and practising here at the time, and anxiously looking for evidence on this subject, should have been aware of the occurrence. This case, or supposed case, has found its way into other works of authority, and into the Privy Council Reports. Cullen observes that there are in medicine "more false facts than false theories." There was at the time in Bedford a great pressure for the introduction of the Board-of-Health system; "shocking examples" were needed to demonstrate its necessity, and in due time they arrived.

From what I have had the pleasure of laying before you, the following conclusions occur to me with more or less of distinctness. That some of them have not been worked out as they should be, is due partly to the limited time at my disposal; partly, it may be, to an inadequate rendering of what presents itself, nevertheless, forcibly to my own mind.

1. Spontaneous generation of plants and animals is a figment which is constantly receding as means of observation extend and improve.

2. Spontaneous generation of parasitical diseases is a figment.

3. The exanthemata may be a low form of fungoid life.

4. Small-pox, a contagious exanthem, is proved by indisputable negative testimony to be incapable of spontaneous generation.

5. Several other contagious exanthemata cannot originate spontaneously.

6. Enteric fever is a contagious exanthem.

7. Febrile diseases of local origin are not contagious.

8. Experiment\* gives strong evidence against the spontaneous origin of enteric fever.

9. Observation, as usually conducted, is a treacherous and insufficient test of the origin of febrile diseases.

St. Peters, Bedford.

## CASES IN SURGICAL PRACTICE.

By JOHN EWENS, L.R.C.P. LOND., L.R.C.S. EDIN.

THE following cases occurring recently in my practice, being at least unusual, may, I think, prove of interest.

In March this year I was consulted by a woman, aged sixty-eight, who informed me that she had for the last five years suffered from uterine hæmorrhage to a greater or less extent. She was very anæmic and depressed. As the administration of astringent remedies failed to give relief, I at once proposed a vaginal examination, expecting to find a polypus. I found nothing abnormal in the vagina, and could at first detect no os uteri, although a hard substance, immediately behind the bladder, revealed the presence of the uterus. On the most careful examination, an elevation, with a slight depression in the centre, very similar to, but smaller than, the opening of the urethra, could be made out. This I concluded to be the os; and, as no other source of hæmorrhage could be detected, I determined to dilate and examine the uterine cavity. This I did by means of the smallest laminaria tent that I could get; but even this I could not introduce until I had passed an ordinary probe with some difficulty. The tent, being hollow, was placed over the probe, and guided into the canal of the cervix. The patient was ordered to remain in bed for twenty-four hours, so as to retain the tent; but twelve hours after, on rising to pass water, it slipped out. However, the next morning I found that sufficient dilatation had occurred to enable me to pass a larger tent. Next day another difficulty presented itself. The portion of the tent within the uterus had expanded in far greater proportion than that in the cervix, and could not possibly be removed with any force which I considered it safe to use. (There was a highly-offensive sanguineous discharge; so I used an injection of carbolic acid and water, 2½ per cent., which answered admirably in removing the offensive smell.) I therefore introduced another tent by the side of the one contained in the uterus; and next morning, to my great gratification, I found the os uteri sufficiently dilated to admit of the introduction of one finger, and cleared out from the uterus what I at first thought to be some fibrinous clots. I could detect that the anterior wall was very much thickened, but could not reach to the full extent of the uterine cavity, which was enlarged, and therefore was not sure as to its condition beyond my reach; but as the patient was very intolerant of pain, very nervous, and somewhat exhausted, I was obliged to defer further examination, intending to repeat the process of dilatation next day. But, at my next visit, I found the patient not only free from pain, but that there had been no bloody discharge since the operation. I therefore concluded that what I considered fibrinous clots were really small warty or polypoid growths. Further examination was

objected to. The discharge ceased; but, about two weeks after, a clot of blood escaped with some pain, and with the exception of a trifling sanguineous discharge, following the use of a purgative about a month since, the patient has continued well.

*Remarks.*—The patient, a widow, never was pregnant—a circumstance explained by the almost impervious condition of the os uteri. She first menstruated at seventeen years of age, and ceased at forty-five. The periods were always attended with pain, and the quantity of discharge was scanty. She always suffered from leucorrhœa. The points of interest in connexion with this case appear to be—1st. The long interval between the cessation of the catamenia, at the age of forty-five, and the re-appearance of the sanguineous discharge, as evidence of polypoid growths, at sixty-three. (By the way, I may mention that some years ago I met with a case, in a woman aged seventy, in which the polypus descended into the vagina, and was thence removed by me in a sloughing condition, and hæmorrhage ceased immediately.) 2nd. The nearly obliterated os, and the extreme shortness of the canal of the cervix. Had the cervix been taken up into the body of the uterus by the thickening and consequent expansion of the anterior wall? 3rd. The unintentional and unexpected destructive inflammation set up among the contents of the uterine cavity by the tent introduced on the second day, which, on removal, was found thickly coated with fibrinous deposit. The polypi were so thoroughly disorganised that they were easily detached from the uterus, and (as stated above) looked like clots of fibrin. This inflammation was not attended by pain or any serious symptoms. The chief complication in the case arose from the extreme difficulty of reaching the seat of disease, owing to the very contracted state of the vagina and the constricted os uteri.

The second case alluded to is an instance of the comparatively rare accident of fracture of the humerus by *muscular action*, which occurred to a boy, aged fourteen years, in attempting to throw a stone. He is quite positive that he did not strike against anything. The fracture is exactly in the middle of the shaft, and apparently is due to violent action of the biceps. It is of the transverse kind, and there has been no tendency to shortening or displacement. Firm union has already taken place (in sixteen days). The boy is short and stout, with fair bony and rather extra muscular development.

Cerne-Abbas, August 5th, 1870.

## PUBLIC VACCINATION.

By THOMAS NEWHAM, M.D.,

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THIRTEEN years of Union work (which, of course, has included a vaccination contract) will be sufficient to prove that I have had some experience of the subject upon which I write; and, although the district I hold is by no means an extensive one, yet I trust the remarks I have to make may not be devoid of interest to the numerous readers of THE LANCET.

I propose in the following paper to lay before the profession—first, the method I have pursued in order to secure efficient vaccination; second, the difficulties to be contended against, and how they have been overcome; and lastly, the results.

1. *The method I have pursued in order to secure efficient vaccination.*—The guardians of this Union have appointed one day in every week for the performance of the operation for the town of Winslow, and one day in every three months for the villages composing the remainder of my district, at the pay-room of the relieving officer. At intervals of three months I obtain from the registrar the names of all children born; and before they are three months old I issue a printed notice to remind the parents when and where the child must be vaccinated. I find that the paper given them on the registration of birth frequently finds its way into the fire, or is otherwise lost. By a little management, I have generally been able to secure fresh lymph immediately before visiting my outlying parishes, and thus, if vaccination has

\* That is, the only kind of experiment which is permissible.