On the other hand, would the diagnosis of rickets be correct if only one or two of the foregoing conditions were present, and perhaps not over well marked? For instance, should enlargement of the radial epiphyses be regarded as rickets when no other signs are present? Such a condition, whatever it be called, is immensely common, and often the only sign of disease. Yet this condition, and the histologic-pathological changes of which it is the result, are by some considered as pathognomonic of rickets, and, indeed, as the only pathognomonic signs we possess. Even in its worst form, however, this enlargement is only premature development, physiological in kind, though pathological in time, and one which seldom leads to any permanent change.

Passing on from the definition of rickets, the question of food shall be briefly considered. Do children, who are suckled and well suckled, ever suffer from rickets? The answer is, most undoubtedly they do; this at once introduces some element as a cause, other than mere artificial feeding. Little need be said in this place on the subject of suckling; its advantages both to mother and child are so obvious that one can only wonder why so many children are deprived of their natural food. Excluding a certain number of mothers who die; some whose breasts do not secrete milk, or secrete very scantily and for a very short time; some whom special forms of acute disease part from their infants—a very small percentage of the total number of mothers—what a considerable number of the remainder forego, on very trivial grounds, the duty of suckling; and of this number what a considerable proportion—I regret to say it—are encouraged by their medical attendants to do so! It may be replied, Where is the harm, if suckling is not a certain preventive of rickets? But, on the other hand, if an infant develop rickets even when breast-fed, how much worse will be its condition, if it have to trust entirely to artificial feeding. Experience amply confirms my statement (that suckled children get rickets), and it alone would suggest that privation of natural food is not the only cause of rickets. Very often, too, a child who is not suckled fails to receive other motherly attentions; it may be either before or after its birth, or both, for a mother's duty begins not with the birth of her child, but with its conception.

The constitutional element which thus enters into the argument raises the question of predisposition and receptivity. It is in this way, I imagine, that rickets occurs among the socially well-to-do. For no class can with impunity long neglect the laws of hygiene. The relatively close circle from which husbands and wives can be drawn, obliges a certain class of the population to utterly disregard questions of heredity, and the weakening influences of frequent intermarriages are well known. Among other classes, late hours, and the various exactions of social life, or the hurry and worry of keeping up appearances, take the place of the physical hardships and not unfrequent semi-starvation which obtain among the lower classes, and both act alike on the constitution of the pregnant mother, and on the foetus *in utero*. After birth, however, the child of the rich, even if not suckled, has every attention and every care which money can procure, while the poor child continues to experience the deprivations inseparable from its surroundings.

The former is probably less frequently suckled than the latter, given child for child, and then artificial food begins to play its part. Among the poor, it is a very exceptional circumstance for the child not to be suckled, though few are brought up on breast-milk entirely; either the milk is not secreted in sufficient quantity, or it is not "satisfying" in quality; or the mother has to go out to work, and must needs trust her child during the day to the care of a neighbour, who feeds it "on the bottle." Here, again, artificial food comes into play. There is a widespread belief among poorer women, that, so long as they continue to suckle their infants, they will not again become pregnant; and for this reason many mothers continue to suckle their children for long periods (more or less up to two years of age): a practice which is a fertile source of rickets, less by supplying insufficient food to the child than by exhausting the mother's strength, by depreciating her health, and so acting indirectly on her next child. On the other hand, if there be some truth in this widespread belief—and there doubtless is—it indicates that suckling is one of the means which nature adopts to keep children from harm, but mothers from too frequent pregnancies; and it should be an inducement to mothers to practise the little self-denial which these maternal duties entail, and which save them from the more exhausting and exacting cares incidental to too rapid child-bearing.

It is unusual, at the present day, to see a child who has not some of the signs of rickets; and this is due, in the writer's opinion, to what, in the present paper, he has spoken of as the "constitutional factor." It is, however, though not at all exclusively, among the very poor that the extreme degrees are met with, that is to say, among

those in whom both the constitutional and dietetic shortcomings are combined, aggravated, and persistent.

The artificial food to which reference has often been made, under whatever high-sounding name it may be disguised and sold, is, after all, either some form of flour (baked or unbaked, and containing more or less starch), or milk, fresh or preserved, alone or combined with other ingredients. Neither of these substances in itself is hurtful, as many thousands can testify who have thriven upon them. Even if some other kinds of food besides breast-milk be occasionally given, "bits off the table what we eat ourselves," as the phrase goes in the out-patient room, how very difficult it is to prove that the rickets depend on these causes!

The ingredient in food to which is ascribed the chief part in the production of rickets is starch; and the object of the various methods of "preparing" artificial food for infants is the reduction of the insoluble starch into soluble dextrine. Is it not a little strange that so harmless and bland a substance as starch—one which forms a large proportion of our food in later life—should lead to the serious lesions which we find in severe rickets? If starvation were to result, there would be less surprise; but the severer forms of rickets are beyond understanding, unless other than food-causes be invoked. It cannot be questioned that many children thrive on artificial food, and that many would die if it were not given to them. It is also certain that a varied diet proves very useful in many cases, where even breast-milk fails to satisfy a child. Lastly, the question of starch-indigestion rests, at best, on a narrow foundation, although it has attained such a widely spread acceptance.

The immense amount of rickets which is now met with, not alone in hospital, but also among well-to-do patients in private practice, renders it incumbent on all, and especially on those who concern themselves with children's diseases, to insist on the importance of due care and precautions on the part of the expectant mothers. It can never be too strongly impressed upon them that maternal duties commence at the time of conception, and that neglect of common-sense precautions during this all-important period will surely react on the infant, and entail upon it evils which no subsequent care can entirely remove.

The inquiry is important, and is one on which each reader should individually come to a conclusion without being too much biassed by the "influence of authority" in these, as yet, largely "matters of opinion."

"Est turba semper argumentum pessimi."

The writer hopes that the question of diet, as the sole or chief cause, will be reconsidered from the present standpoint; for, unless and until it is generally admitted that other and important causes are at work, and probably responsible for the increase of the disease, little can be done to arrest it.

The views set forth are offered as suggestions rather than as dogmatic assertions; and for the most obvious of all reasons. The object will have been attained if more attention is in future directed to the circumstances surrounding mothers during gestation, with a view to minimise the "constitutional factor" in the causation of rickets.

It would be beyond [the limits of the present paper to enter into the connection, if any, between rickets in children and dyspepsia in adults. Who shall say that some of these rickety children do not represent the naturally dyspeptic portion of mankind, and that both suffer less from the effects of unsuitable food than from a natural weakness of the digestive and assimilative systems?

NOTES ON RESUSCITATION OF THE NEWLY BORN AND ON THE TREATMENT OF POISONING BY ANÆSTHETICS.

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REFERRING to the debate, at the Royal Medical and Chirurgical Society on March 11th, on "Artificial Respiration in Stillborn Children," it seems that the chief point in connection with the subject was either omitted in the discussion, or not reported in the JOURNAL.

Whilst resident accoucheur at the London Hospital, I was once performing artificial respiration, after Silvester's method, with insufficient assistance, in a cottage, on an infant, when it appeared that the chest could not expand, since the air, immediately on entering the larynx, forced the epiglottis, *ipso facto*, like a valve, over the superior orifice of the organ.

The nurse—an ancient one, and not one of an intelligent order—

could not be trusted to hold the tip of the child's tongue, small and slippery, with firmness. Perceiving that a darning-needle lay threaded on a pincushion, I thrust the needle through the tongue, and directed forcible traction to be made on the loop of thread, whilst artificial respiration was recommenced. Never do I recollect a case, of like character, where signs of animation appeared so quickly; which, since they vanished on relaxation of the tongue, and reappeared at once on the renewal of traction, were clearly due to that proceeding.

I venture to attach the greatest importance to forcible traction on the tongue, as a preliminary to artificial respiration in the newly born, not merely having tested the efficacy of the plan on subsequent occasions, but having read in an article in Holmes's *System of Surgery* (third edition) that Sir Joseph Lister considers it invaluable where life is jeopardised by inhalation of chloroform.

But a careful consideration of the arguments which this great surgeon adduces to prove his view that "the beneficial effect of this procedure cannot be explained mechanically, but must be developed in a reflex manner through the medium of the nervous system" (*op. cit.*, vol. iii, p. 604) fails, to my mind, to convey conviction of the stability of the theory, since it seems more probable that, in chloroform-poisoning and in the asphyxia of new-born babies, the laryngeal apparatus is flaccid, owing to paralysis of its muscular apparatus, caused in the former instance by the specific action of the anæsthetic, and, in the latter, due to previous disuse.

If this explanation be accepted, nothing is easier to understand than the frequent failures which follow artificial respiration, when the tongue has been previously withdrawn from the mouth, and the success which attends Silvester's method (the precaution having been adopted) as a means of resuscitating the newly born, or of preventing death from anæsthetic poisoning when solely threatened by laryngeal obstruction.

Should death appear imminent from causes other than laryngeal obstruction, only one method remains which can possibly avert dissolution—when the respiratory movements have ceased, when the circulation has stopped or is stopping, when galvanism has failed, and when the administration of diffusible stimuli, either in the form of inhalation or *per cutem*, would be useless—namely, the intravenous injection of fluid (medicated or otherwise), accompanied, under most circumstances, by simultaneous depletion. Five points would thus be gained; first, the re-establishment or the maintenance of the circulation; secondly, the removal of part of the poison, and dilution of the remainder; thirdly, where loss of blood had occurred, repletion of the vascular system; fourthly, the opportunity for administering a physiological antidote, *e.g.*, in the case of chloroform, ammonia; and, fifthly, the employment of the most powerful cardiac stimulant with which we are acquainted.

It may be added, in support of this method, that last summer, when about to commence an experimental transfusion (*vide Lancet*, September 1st, 1883), by the kind permission of Dr. Pye-Smith, in his laboratory at Guy's Hospital, I noticed that one of the animals, deeply under the influence of mixed narcosis, seemed on the point of expiring, but re-animation speedily followed the performance of the operation.

The report of a fatal case of inhalation of ether, from St. George's Hospital, by Mr. Holmes (*BRITISH MEDICAL JOURNAL*, March 15th, 1884, p. 508) is my excuse for calling attention to this plan of treatment.

ENTERIC FEVER FROM CONTAGION.

By ROBERT DENHAM PINNOCK, M.B., C.M.,

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THE following notes on an outbreak of typhoid fever, which recently came under my notice, will, I think, be read with interest by those who are still gathering arguments in favour of the contagion-theory of the above disease. The village of Greendale, with a population of about one hundred all told, is about thirty miles from this city, and is situated partly in a valley, and partly on the sloping ground rising from one side of a watercourse which runs through the valley. This watercourse, in the dry season, is merely a chain of waterholes. The cottages are irregularly distributed, and every one is separated from its neighbour by a distance of from fifty to two hundred yards. The water-supply is chiefly from the creek, though some of the residents have wells; and a few collect their roof-water in iron or galvanised tin tanks. Only a few of the more important dwellings, such as the school-house, hotel, and store are provided with closets; and these are generally separate from the house, and are ordinary cess-pits, except at the school-house, where the excreta are received into

receptacles, which are emptied when necessary, and the contents buried. By the great majority of the inhabitants, the calls of nature are responded to in the most convenient spot in the vicinity of their dwellings, and household slops and refuse of all kinds are in most cases thrown out of the back door. There is thus every opportunity, during the summer, for noxious odours and vapours to rise around the dwellings, and, during the rainy season, for the fecal and other accumulations on the surface to be washed down to the creek, or to be carried directly, or by soakage through the soil, into the wells and water-holes at a lower level. Yet, in spite of all these unwholesome conditions, which have always existed, the schoolmaster informed me that to his certain knowledge there had been no case of typhoid fever for the last eight years, and, on inquiring for me amongst the oldest inhabitants, he could not find any record of a single case having ever occurred before.

The three nearest settlements are respectively six, seven, and seven miles distant.

The first case of the disease occurred in a girl named H., aged 22, who had been servant in a house in Melbourne where the complaint existed at the time she came home (March 14th). She became delirious; and took to bed seven days after arrival, and was over two weeks before showing any signs of improvement. Her mother sickened eighteen days, her sister (aged 19) sickened twenty-two days, and her brother (aged 23) twenty-seven days after her arrival, and all went through regular attacks of typhoid fever. They were all remarkably strong and healthy before. They lived in a small cottage on the slope of the hill; the pigstye was a few yards from the house; slops and refuse were thrown out at the back; excreta were also thrown out at the back during the time of sickness, and not covered with earth. Their water-supply was from the creek, but after the first member of the family became ill they got it from the school-house tank.

A child, P. (aged 4), living two hundred yards lower down the hill, took the fever six weeks after H.'s arrival from Melbourne. The P.'s got their water from a well close to their house, and their milk from the H.'s. The next attacked was Mrs. M., who used to go and nurse the eldest H. girl and her mother. After about three weeks of this self-appointed task, she complained of pain in the head and back, and shivering; and, though she did not lie up, she felt unwell, and suffered from diarrhoea. The M.'s lived on the slope of the same hill as the H.'s, but three hundred or four hundred yards on one side of them, and not much below the same level. Their water-supply was from a zinc roof, and collected into an iron tank; and their milk came from a dairy several miles away.

Mrs. M.'s little girl, aged 3 (the only child), sickened a few weeks after her mother gave up nursing the H.'s, and went through a rather severe attack of the fever. In fact, all the cases narrated were severe attacks except Mrs. M.'s.

Mrs. M.'s child was the last case of which I heard. The cessation of the disease I attribute to the fact of a heavy and continuous rainfall occurring just after she became ill, which flooded the surrounding country, and no doubt washed away the remaining germs of the disease.

CLINICAL MEMORANDA.

CASES OF FACIAL ERYSIPELAS WITH LOW TEMPERATURE.

DR. CAVAFY's article, under the above title, in the *BRITISH MEDICAL JOURNAL* of March 29th, I have read with much interest, and especially because certain points discussed therein have recently engaged my attention in preparing a paper on the contagiousness of erysipelas, which appeared in the *Practitioner* of December last.

In that paper I stated my conviction—a conviction based upon some exceptional opportunities of observation—that "in cases of facial erysipelas, as they ordinarily present themselves in practice, whatever secondary varieties may exist, there are primarily two distinct forms to be dealt with, viz., the infective and the non-infective forms, or, as the distinction may otherwise be expressed, true genuine erysipelas and spurious or pseudo-erysipelas." I added, "Without venturing to indicate in what ratio these two forms stand to one another as to frequency of occurrence, I am quite sure that I have again and again seen and treated cases of the latter—cases in which the local affection has followed exposure to heat or cold, or other injurious influences, and the fever and general disturbance have been comparatively insignificant. Mr. Jonathan Hutchinson, in his paper 'On Certain Diseases Allied to Erysipelas,' clearly recognises their existence when he says, 'These forms of erysipelas, which are due to local irritation are, I think, almost always mild.' It is when the disease is due to contagion that it assumes its greatest severity."