PALAEOGENESIS AS EXPLANATORY OF CERTAIN FORMS OF DISEASE.

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IN the BRITISH MEDICAL JOURNAL of January 9th, 1909, p. 85, I ventured, in writing about the condition known as leucoderma colli, to give a novel explanation as to its nature. My suggestion was that the condition thus named is not in any sense an eruption, but simply a revelation, under disturbance of general health, of a pattern which pre-existed in the skin. I have since then given much thought to the subject, with the result not only of confirming the conclusions then arrived at, but of finding good reason to suspect that the same explanation is applicable to many other phenomena of supposed "disease."

The theory which I invoke is that, in the course of hereditary transmission, peculiarities of organization may be perpetuated for quite indefinite periods and under conditions of the most absolute latency. In order to be explicit I have invented (or borrowed, I do not know which) the terms "palaeogenesis" and "palaeogenetic." The latter is applicable to all conditions supposed to be due, wholly or in part, to inheritance from ancestry extremely remote; so much so that in the majority of cases all history of such association has been lost. The possibilities of palaeogenesis are not limited by species or genus, but may be recognized in the most distantly related animals. Thus the condition known as the "dappled" skin occurs both in deer and horses, and I now claim it for human beings. It is simply a spotted skin in which the spots become visible only under certain conditions. Although there is, of course, difference in colour between the ground and the spots upon it, the precise colour of both may vary much, and is a matter of no consequence: a dappled horse may be either brown or grey, but must be well fed and well groomed, or it will not show its dappling. It seems probable that the use of arsenic may develop it, and that the blood changes in secondary syphilis, or the drugs used against it, may be influential.

I do not propose here to offer any further exposition of my theory, for it is very complicated, and I am well aware that at first sight some of its conclusions may appear far-fetched and very improbable. My desire on the present occasion is simply to ask the attention of other observers, and to mention some of the conclusions which have suggested themselves to my mind.

Not only do I hold that we may recognize palaeogenetic structural peculiarities in the dappled skin of leucoderma colli, but a similar predisposition is to be traced in xanthelasma palpebrarum, in the common form of leucoderma, and the very curious congenital conditions which have been recognized under the names of linear ichthyosis, ichthyosis herpetiformis, or Biett's bands.*

The explanations of these in reference to cause and distribution have hitherto been insoluble problems, which have exhausted the ingenuity of many observers. To take, as an example, xanthelasma palpebrarum. I have recently found that crescentic patches, curving round the inner canthus, or on the inner third of the upper lid are common in some deer, in many dogs, in young lambs, and a host of other animals. I accept these facts as evidence that there exists in these regions portions of integument so organized that they are prone to produce hair, etc., of peculiar tint, and I deduce that it is these which under various states of disturbed health, or without any, give location to the washleather patches which Rayer drew our attention to.

Perhaps the simplest example of palaeogenesis which I can adduce is the "linea fusca," which used to be regarded as a symptom attending a first pregnancy, but which obviously is "the abdominal line" conspicuous in many quadrupeds, made visible in a woman by the pigment disturbance which

often attends pregnancy. It will be obvious that palaeogenetic influences do in a certain sense affect every fibre of our organism. We are physically as well as morally "the heirs of all the ages." I suggest, however, that it will be convenient to restrict the term to conspicuous and unexpected illustrations of a law which, often overlooked, is really of universal force.

I hope to bring the subject under discussion at the forthcoming meeting of the British Medical Association in London next July, and may state that in the meantime pictorial illustrations of the conditions referred to will be on exhibition on the ground floor of the Clinical Museum at the Polyclinic, and that I shall be thankful for suggestions and criticism.

FAECAL VOMITING DURING ANAESTHESIA: A SUGGESTED METHOD OF OBTAINING ITS DANGER.

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THE extreme difficulty of avoiding fatal consequences when a general anaesthetic is administered in cases in which intestinal obstruction exists and faecal vomiting is liable to occur has led to the adoption of many methods of inducing anaesthesia. None of these has proved wholly satisfactory. It is proposed to consider these methods, and to suggest one which, so far as the writer knows, is novel and appears to offer a chance of avoiding the dangers incident to all the plans which have been proposed up to the present time. In most cases of complete or almost complete obstruction it is extremely difficult to estimate the danger incident to general anaesthesia. Some are obviously beset with the utmost peril; vomiting is frequent and stercoraceous, and the patient is collapsed. Other patients vomit at intervals, but palliative treatment is adopted and the more grave symptoms appear to be relieved. Vomiting may cease although the abdomen is distended and operative treatment is obviously necessary. In such cases until recently a general anaesthetic was administered as a matter of course, and the selection of its nature was decided by the general state of the patient. Records of deaths under such conditions demonstrate very clearly that grave dangers arise and commonly terminate fatally, either through drowning of the patient with his vomitus or through infection of the lungs causing septic and fatal pneumonia. The Clinical Report on Anaesthetics published by the British Medical Association records three deaths under ether, and there were cases in which the faecal vomiting resulted in suffocation. It is not denied that many cases of this grave condition have been successfully operated upon under a general anaesthetic, but the point of importance is that no one can assure safety even when the greatest skill and care are invoked for the administration of the anaesthetic.

Surgeons both in this country and abroad have become convinced of the danger, and have sought to avoid it by employing local analgesia, or spinal analgesia. Local analgesia is at best an unreliable method in these cases. Even if the preliminary steps of the operation are carried through without causing serious suffering—and often this is not possible—the actual operation, the examination and manipulation of the abdominal viscera, are always associated with great pain and shock. That the gravest danger from the surgeon's point of view is shock, no one who has seen many such cases will deny. For a patient who is conscious to undergo a severe operation, even if severe pain is not felt, taxes the nerve of most persons, even when in good condition, but such an experience for one who has been seriously shocked or is collapsed involves the most grave risk of further shock or fatal collapse.

When subdural injections were introduced, it was at first claimed for the intradural analgesic method that it would prove a valuable alternative to general anaesthesia for all persons whose state appeared to contraindicate the use of general anaesthesia. It was thought that this method, even if it possessed its own dangers, would be found at least safer than older plans. Obviously cases of

* Both in the human skin and in those of animals the dappled condition may become much more conspicuous in certain seasons. The Formosan sika stag, which is a relative of the Japanese stag, is, like it, "spotted" only in summer. The whiteness of the spots and their contrast with the dark ground are far less marked than those of some other stags which, like the axis, retain their conspicuous spots all the year round. A young lady who was brought to me on account of spots on her hands and forearms showed little that I could observe. It was in January. I was assured that in summer the spots were very conspicuous and annoying.

intestinal obstruction, when faecal vomiting was urgent or was imminent, were those in which this method should be especially valuable. The experience of surgeons, both here and abroad, has demonstrated that subarachnoid injection is far more dangerous than was anticipated; and many surgeons of special experience with spinal analgesia have advised restricting the method to normal patients—that is, to those who run no particular risk if a general anaesthetic is efficiently administered. Some surgeons, however, have clung to the hope that for the particular cases under consideration the spinal method, with its admitted dangers, presented less danger than a general anaesthetic. They extol the facility it offers to the operator by relaxing the abdominal musculature, and so making his work easier and more rapid. The case of death under spinal analgesia¹ recorded by Mr. F. Victor Milward has unhappily shaken our confidence in the method so far as safety is concerned. As is pointed out, the case was parallel in all ways with similar cases treated on similar lines, and yet the result was fatal. It would be quite unjustifiable to discredit any method because it fails in some instances; but such failure, provided it can be shown to have been due to no lack of skill or knowledge, must give pause, and compels us to regard the method as dangerous in certain cases. The unfortunate fact about the published cases of deaths under spinal analgesia is that they do not appear to obey any rule and so allow of correction. Further, as in Mr. Milward's case, the death is not always, or even commonly, due to the disease or operation which is being performed, but is the result of a new factor introduced into the problem—a factor which, used to promote safety, resulted in death. The method is, however, only at present in the stage of inquiry; it would be premature to assert either that it is useless, or to extol its merits in the cases we are considering; we can only say that at present the method does not always succeed, and is not free from danger.

The general anaesthetic in the cases under review can certainly be guarded against overdosage, but may, and often does, lead to a fatal issue through abolishing reflexes whose function is to prevent vomited matters entering the air passages.

The problem is, then, to discover whether it is possible to employ general anaesthetics and yet obviate the danger in question. The mechanism of lung flooding is probably explicable as follows: Vomiting as a purely reflex act does not occur under complete anaesthesia, and entrance of vomited matter into the airways is prevented so long as the laryngeal reflex is present. When the patient is put under a general anaesthetic the muscular tonus of both voluntary and involuntary muscular tissue is lowered, and the pressure *a fronte* allows the intestinal contents to enter the stomach. When distension of this viscus becomes sufficient its contents are ejected through the cardiac orifice and enter the pharynx. The process is a regurgitation rather than vomiting. As the laryngeal reflex is also less able to prevent fluids entering the trachea—if, indeed, it is not wholly abolished—the lungs become flooded. It often happens that the regurgitation of a stream of stercoraceous fluid occurs without warning and without any of the usual signs which indicate to the anaesthetist that fluid has entered the pharynx from the stomach. If a closely fitting mask is in use the danger may be overlooked until the mask is lifted or a stream trickles beneath the pad.

It is suggested that the following method may prove useful in preventing such an occurrence. Lavage may be employed, although experience has demonstrated that emptying of the stomach only makes way for further backward flow of intestinal fluids. It is, however, wise to use it and clean out the stomach and oesophagus with boric acid solution and hot water. The next step is to perform laryngotomy under local analgesia, or even under light anaesthesia, but it is essential in the latter case that the laryngeal reflex should remain active, because vomiting or regurgitation may occur while the larynx is being opened and the stomach tube may be expelled, or vomitus travel beside it into the pharynx. As soon as the larynx is opened, and a cocaine spray used to lessen spasm, chloroform is to be administered through the laryngeal tube to full anaesthesia. The mouth is opened by a gag so soon as the patient is analgesic, and a full-sized oesophageal tube

introduced into the stomach. If the preliminary lavage has been carried out by means of a large tube, and if it has not been expelled, the only further step necessary is to pack very carefully both the upper opening of the larynx and the pharynx around the oesophageal tube. As soon as this is done, the free end of the oesophageal tube must be allowed to drain into a vessel containing a deodorant and antiseptic. It is essential that the tube be introduced and maintained in the stomach, and that the packing round it be tight. The packing of the larynx is done in exactly the same way as that carried out before an excision of the maxilla, when a preliminary laryngotomy is performed. Packing round the oesophageal tube is rendered more easy by cocaineizing the pharynx, as it is essential that there shall be no leak of fluid around the tube.

It may be urged that a preliminary laryngotomy complicates the operation and increases the shock. Whether the latter allegation is valid more extended trial alone can determine, but as the danger which the suggestion aims at removing is a very real one, only serious and proved objections ought to weigh. If an assistant watched the packing and retained the tube in position and repacked should it become necessary, there is less likelihood of the air passages getting any regurgitated fluid even by leakage. Experience has shown that a laryngotomy is a simple operation and should not entail post-operative risks of lung dangers.

I am unable at present to quote cases, as the procedure until its adoption has been proved to be of real value could hardly be urged except in exceptionally grave cases. If, however, it bears the test of extended trial, it would appear to be a plan which might enter into the technique of all operations for the relief of intestinal obstruction where vomiting is a pronounced symptom. Although laryngotomy is mentioned, there exists one good reason for advising tracheotomy rather than the higher operation. The tracheotomy wound is easily closed, is less liable to be forced open in straining efforts. The great objection to the plan proposed is that it destroys the explosive power of coughing, and so clearing the lungs—a most serious point if bronchitis and pneumonia follow the operation, when the patient's life may depend upon his ability to clear his lungs of mucus. The tracheal opening promises better chances of restoring this power, since it can be more securely sealed than is possible after laryngotomy. The tracheal opening also is further from the seat of infection, and this is all to the good. Experience alone can show whether these advantages outweigh the more serious character of the tracheal operation and the increase of shock. The method suggested appears to be valuable to the surgeon in another way. The constant draining of the stomach and intestines unquestionably renders operation easier by abating the distension of the viscera and the chaotic peristaltic movements which commonly complicate such cases.

REFERENCE.

¹ BRITISH MEDICAL JOURNAL, March 26th, 1910, p. 743.

POST-OPERATIVE SUPPRESSION OF URINE:

RELIEVED ON THREE SEPARATE OCCASIONS
BY INTRAVENOUS INFUSION.

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It has been suggested to me that the following case presents at least one point of considerable interest—namely, that three attacks of complete urinary suppression were recovered from as a result of rapid intravenous infusion of saline.

I do not intend to enter into any discussion as to the cause of the suppression; it will be noted that the urine tested before the operation by the ordinary methods (boiling with acetic acid and the nitric acid test) showed no trace of albumen, and the specific gravity was 1020; it contained a small quantity of bile pigment.

The onset of the uraemic symptoms, accompanied by complete suppression, would indicate some previous renal incompetence not shown by the usual tests, and perhaps the case may well be one of so-called "latent uraemia."