their strength soon fails, and the section does not unite. In several instances I have found it necessary not only to allow meat and generous diet from the day after the operation, but to give wine and stimuli freely, the effect being merely that of keeping the vital powers up to the point necessary for the completion of the process of restoration. Instead of trusting to subsequent treatment to keep down inflammation, the wisest course, in my opinion, is to carefully prepare the patient for the operation by diet and regimen suited to each case. In one it may be advisable to keep the patient on a reduced diet, and to give a smart purgative or two; in another, a course of tonics may be necessary when the powers of life are low and the nervous system shattered. The latter plan I have found especially necessary in persons who have seen better days, and who have to struggle against poverty in their old age, having been accustomed in their youth to luxury and every comfort.

When the section does not unite kindly, the upper lid becomes swollen—at first but slightly and without discolouration, but after a day or two a dull purplish hue appears, most marked near the ciliary margin. If we open the eye, we shall find that pink, semi-transparent chemosis, so characteristic of debility; the section more or less gaping; perhaps some protrusion of the iris, and some haziness of the cornea. There may be little or no pain; but the patient is depressed, sighs frequently, and has a feeling as if she was going to die. The pulse is low, although perhaps frequent, and the skin cold. Under these circumstances we must not hesitate to give stimuli and support. A draught composed of the sesquicarbonate of ammonia, with compound tincture of lavender and camphor mixture, should be given immediately, and an opiate may be advantageously combined with it. The diet should be improved and meat allowed, and if the patient has been in the habit of taking wine, or spirits, or porter, a moderate quantity of that to which they are accustomed will do good. I have been surprised at times to find how much may be taken not only with impunity but with advantage. Quinine is also of much service where it agrees with the patient, but the more active stimuli are necessary in the first instance to check the mischief as soon as possible.

I have observed, that it is about the third day that the symptoms above described usually set in, but it occasionally happens that with the same symptoms, the patient complains of acute pain, in which case the treatment I have mentioned should be adopted.

Acute inflammation generally begins at an early period—within forty-eight hours, and is characterized by the intense pain, the purulent discharge, the bright, red, shining and swollen lids, the scarlet redness of the eye, and the symptoms of general febrile disturbance. In such cases active depletion must be at once practised, and every other means to subdue the inflammation excepting tartar emetic; that might cause vomiting, which would be most injurious.

In conclusion, I may remark, that if possible, we should operate when the weather is fine and mild and the wind not north east. The last May was most unfavourable, and I know of several cases which did badly in consequence.

2, Tenterden Street, Hanover Square.

NOTES AND ILLUSTRATIONS OF THE DISQUISITION ON PUERPERAL FEVER.

By EDWARD BLACKMORE, M.D.

(Continued from page 371.)

Mr. Nunneley, of Leeds, so well known by his admirable treatise on erysipelas, has favoured me with an interesting summary of his views of the origin and forms of puerperal fever, which he allows me to incorporate among these notes. Mr. Nunneley says—"That erysipelas and puerperal fever occur together as epidemics, there is most abundant evidence to show; to many of which authorities I have referred, (in the above-named treatise,) nor can it be doubted that the same atmospheric condition conduces to the two disorders; and, further, there is also much testimony to the fact of one disease being directly induced by the other.

"As to how far the proposition you mention, (in a private note,) of the fever being always induced by causes extrinsic to the body, be correct or not, I am not altogether prepared to speak positively; but I should scarcely be inclined to admit it. That this fever is not necessarily confined to lying-in women, if we are to be guided by every other thing than the mere name, I feel confident. I have seen cases, I believe, where puerperal fever has arisen sporadically, in which no extrinsic cause could be traced; but there was always sufficient in the state of the patient to account for the disease. These cases are, however, more manageable, and exhibit a more phlegmonous character.

"I have noticed, on two occasions, in which I saw two or three cases of puerperal fever occurring near together, that in several women after delivery, in whom there was no manifestation of the fever, the after-pains were unusually severe and prolonged. This I attributed to atmospheric conditions; and supposed that the fever would have shown itself if the patients had been in a fit state for its development. In these cases, I have always immediately well cleared out the primæ viæ, and given opium with calomel and antimony, in which, combined of course with local applications, I have great faith, when early resorted to.

"Limiting the term specific to what is, I think, its proper sense, I should not call either erysipelas or puerperal fever a specific disorder, since they do not, in all cases, run an uniform course, nor affect the same tissue, nor produce the same effects, nor lessen the chances of a future attack. They can be cut short, which I do not think possible in a specific disorder.

"There are, I consider, two types of constitutional disease, as well as of local affection. In one both the nervous and sanguiferous systems exhibit not only excitement but increased power, and the local affection is limited; as there is more fibrine formed, so there is an effusion of plastic lymph—a vital material. In the other type, while there is great excitement there is great loss of power; not only is there no increase of fibrine, but the vital property of that which still circulates is lessened; little of it is poured out, and that little is not capable of organization; and the local affection is widely spread.

"I believe erysipelas and puerperal fever present every intermediate form between these two extremes.

When sporadic, they generally approach the first, unless in old or very young persons, or in the weak; when epidemic, *cæteris paribus*, they approach the last extreme; and I believe that much is to be done by judicious early treatment to avoid the latter type."

A most eminent living professor of midwifery also appears to believe with Mr. Nunneley, that puerperal fever may arise from some peculiar condition of the lying-in case — certain intrinsic uterine changes, by which the system becomes contaminated, independently of any external exciting cause, of any known or probable application of an animal poison, as well as of the influence of malaria. While I would not positively reject the former view, I conclude from the facts on record, that an inoculated animal poison, or a similar poison diffused in the atmosphere, or simple malaria, a noxious constitution of the natural properties of the air, the influence of which is shown in the other diseases co-existing with puerperal fever cases, is, in a vast majority of cases, the exciting cause of the disease.

The coincidence of unusually severe after-pains with the fever cases, mentioned by Mr. Nunnely, is curiously paralleled by an analogous fact, the occurrence of muscular neuralgic pains, at Plymouth, when puerperal fever and gangrenous erysipelas were prevailing, and also by a phenomenon mentioned by M. Scipion Pinel, in his remarks on Cerebral Pathology, in which he says: "There prevailed at Paris, fifteen years ago, (that is in 1830,) an epidemic of local pains in the palms of the hands and soles of the feet, to which the name of acrodynia was given; the patients compared these pains to those produced by the insertion of pins and needles. The skin became red and insensible, and peeled off in scales. This epidemic was replaced by the influenza and cholera, and has not reappeared since!" These analogies confirm the truth of Mr. Nunneley's view of the atmospheric origin of his hysterical lying-in cases.

CAUSES OF THE DISEASE.

On this point Dr. Hamilton said: "It has occurred after the easiest labours; it is unconnected with the act of parturition. Look for the cause in the state before delivery; there is high susceptibility of the nervous system, and increased action of the digestive organs; and look for it in the state after delivery, there is increased activity of the lymphatic system, and a large quantity of fibrine, and of the other elements of milk, in the circulating mass. There is no disease similar to it, in any other condition of the human body. In hospitals a specific contagion produces it; I have traced cases in private practice, that were infected from the hospital. It may be communicated by personal infection; and simple peritonitis and inflammation of the uterus may be thus communicated. (This he founds on his view of Dr. Gordon's cases.) In the Dumfries case the disease was communicated through the attendants. Some attribute it to the absorption of putrid bile, and of the lochia. No one ever saw a case of putrid lochia, so as to afford a focus for absorption. The great quantity of bile seen in some cases is from the greater quantity of blood in the digestive organs. The exciting causes are irregularities of diet, impurities of the air, and contagion."

It would appear then, that Dr. Hamilton supposed that

the disease might originate from other causes than contagion; that it did not generally arise from contagion in private practice; that a predisposition is formed in the natural conditions of pregnancy and of the child-bed state; and that with this, certain occurrences in the lying-in room would suffice to induce the disease.

If this be so, might we not expect oftener to meet with sporadic cases of the disease? Dr. Fordyce thought it might arise from exposure to cold. Mr. Hey thought it not contagious, because it is not, in his opinion, an idiopathic putrid fever, with an incidental uterine inflammation; and because persons in puerperal fever do not communicate common putrid fever, (typhus,) to the attendants. It prevailed at Leeds in all seasons and states of the weather; but chiefly in the autumn, and in those winters which chiefly resemble autumn, *i. e.*, in mild rainy weather. Dr. Lee thinks the exciting causes mysterious; something in the air; and he has no doubt of its contagiousness. In Mr. Hey's cases it often began immediately after the first attempts at suckling. Dr. Dewees thinks it was formed before delivery. Dr. Gooch never met with cases in immediate succession in private practice; "never saw the disease carried from one patient to another, although he did not then change his clothes, yet he believed it to be contagious, and that the poison was even conveyed by an accoucheur seven miles in a gale of wind! and that the miasma of puerperal women infected pregnant women, and occasioned abortion and then death, for the symptoms and morbid appearances were the same as in puerperal women." Dr. Burns never could trace it to contagion: "it might be more or less." Dr. Ryan thinks, "antecedent disorders in the system an important remote cause; many of the patients, before parturition, and before admission into the Lying-in Hospital, had a burning heat in the lower part of the abdomen, shivering, pains in the shoulders, erythema on the extremities, and many had suffered from severe privations." Velpeau thinks, "that peritonitis in lying-in hospitals originates from moral causes; in many patients he found some affection of the mind." Other French pathologists have thought that puerperal fever was occasioned by the absorption of putrid placenta, or putrid lochia, by which the uterine veins were inflamed. Others refer it to erysipelas.

These quotations will show how much more exact and more extended observation of facts is required on this most important subject. How does the disease originate and spread in private practice, in places where no lying-in hospital exists, to form a source of contagion?

The facts which bear on this question, appear to show three modes of origin:—

1. Extraordinary natural conditions of the atmosphere, such as excite other specific diseases, as the influenza, and which give a peculiar character to various common diseases.

When the specific malady prevailed at Plymouth, the weather was very variable, sunny days were succeeded by cold nights; there were sudden alterations of cloud and sunshine, attended with an undefinable character of insalubrity; and not long afterwards, when malignant cholera raged there, the barometer

was for a long period extraordinarily high, as observed by my friend Mr. Snow Harris.

Unusual atmospheric conditions, which may be local to some degree, do seem to be important concurrent causes at least of the prevalence of the fever.

2. Contagion, conveyed (*a*) by direct communication with the sick; (*b*) by fomites; and received from either source, (*a*) through inhalation? or (*b*) by inoculation? and operating by a nervous impression, or by absorption.

3. A diffused atmospheric infection, a vaporisable animal poison, that extends beyond the range of personal communication.

The facts are few that relate to the propagation of the disease, by direct personal communication with the sick; and in private practice the intercourse of pregnant women with puerperal fever patients must be rare. Yet a disease resembling the fever is said to have been thus communicated, to the pregnant, occasioning abortion and death; and also to the nurses who were not pregnant. The records of lying-in hospitals may cast a light on this point.

The spread of the disease by fomites, is the more likely, mode; washerwomen are said to have got the disease from the clothes and bedding of the patient. Accoucheurs have attached to themselves the misfortune of its propagation, because for a time it has been confined to their several practices; but how they may have conveyed the poison, has not been generally agreed upon. The clothes are often thought to be the noxious vehicle; the hair, the breath, the hands, are quite as likely to be concerned in the case. In many cases in which the accoucheur has blamed himself, it is not stated whether the same nurse has attended in various cases.

The modes of infection may be either (*a*) that of inhalation and deglutition of the poison; or (*b*) of introduction of it into the uterine organs; or (*c*) both of these modes conjoined. To determine the mode is of some importance to settling the means of prevention.

This is not a superfluously refined inquiry. Some of the contagious exanthemata, as the measles, and probably the plague, are communicable by inhalation, and not by inoculation; the exanthematous poisons communicable in the latter way, are also in the former; but the converse is not true in all the species of this order of diseases. The puerperal fever contagion is probably received in both modes; that it can be inoculated is not unlikely, for the fluids of the subject, living or dead, are evidently poisonous; wounds in the dissection of puerperal cases, in my own experience, are especially dangerous.

Then the accoucheur may retain some poisonous matter *subter unguis*. It would be interesting to know whether he could ever have conveyed infection from a puerperal case in its early stage? Yet this mode of infection is not the most likely, for the uterine organs are not in a state to admit of absorption during the act of parturition; and if the accoucheur does possess the poison, it would also exhale from his person or clothes, supposing it capable of assuming the gaseous state, and thus get into the patient by inhalation, and it is not easy to believe that it is not capable of becoming gaseous.

The physiological action of the contagion is not of

so much consequence to be ascertained; it may be (1) by an impression on the nervous system—by a direct action on the nerves of the organs of the senses of smell and taste, or of the more extended mucous membranes; or (2) by absorption from those membranes; or (3) in both ways. A more practical inquiry is, how it so particularly affects the abdominal organs, and in some cases these alone? If it be by a special determination to those parts, from the mysterious relation of the poison to certain textures, (as of the variolous poison to the skin,) we might expect that the disease from a dissection wound would be abdominal; but the poison has usually generated a disease of the lymphatic glands, such as occurs from the dissection of any morbid bodies. Then some peculiar circumstance of the puerperal women, if we are to exclude the natural conditions of the state, must determine the localisation of the disease. It becomes then most important to ascertain what these circumstances are, if we may hope to prevent the disease. Our observation has not yet been able to assign any concurrent circumstance sufficient to account for the formation of the disease. There is in some cases no evidence of previous bad health, or of a morbid disposition of constitution; it prevails as much among the higher ranks as the lower; in cases of flooding as well as of scanty lochia. It appears to be more common and more fatal, where the constitution is weak and irritable; in large towns, and in low situations, but sometimes over wide country districts; and it prevails in autumnal weather; and its attack has often followed the first attempt at suckling.

Thirdly, contagion, that is a poison of a circumscribed range, that requires for its action personal communication with infected persons or things, is not the sole origin of the disease, as the diffused infection seems also to occasion it.

It may be difficult to separate the influence of this source from that first-named,—extraordinary insalubrious but natural conditions of the atmosphere; and probably no positive proof of the operation of this diffused animal poison in any particular case can be obtained, unless it shall have been proved that the natural variations of the atmosphere can never excite a disease which might become infectious. I believe, however, that a merely insalubrious uninfected air would only modify the character of such diseases as evince contagiousness. My observation has convinced me that puerperal fever did occur in persons who were not subjected to any known source of contagion, as I have defined it, either immediately or mediately; and the analogical evidence is very strong, from other concurrent diseases of a putrid character also spreading without personal communication. On this point it would be important to know, whether, whenever the disease has appeared in a lying-in hospital, it has also spread in the neighbourhood without a direct infection from the hospital.

One fact that seems to oppose this third opinion of its origin, is, that an accoucheur, on retiring from practice for a short time, after having had fatal cases, meets with no more of the disease on his return. This, however, is not conclusive on the question; and could only be determined by the combined observations of all the practitioners of the district where the fever was prevailing. And most valuable would be the

evidence that would relieve the conscientious accoucheur from such bitter reflections as have been entertained by some able writers in this Journal.

Assuming, now, that contagion or infection is a cause of the disease, if not its essential cause, what is the specific poison? The balance of evidence is certainly against its being that of typhus, and in favour of its identity with that of erysipelas. I mean not only that the puerperal inflammation is of the erysipelatous sort, but also that the specific febrile exanthem, cutaneous erysipelas, originating in contagion, and itself becoming contagious, is a source of puerperal fever. Not that I conceive it possible for every case of surgical erysipelas, (so called,) a mere local disease, to originate it; something more is required than the touch of a hand that has just before opened an unhealthy abscess. The opinions and self-reproaches of some estimable accoucheurs have, I think, gone beyond the evidence on this point. Possibly some unhealthy concurrent atmospheric conditions are requisite in order for external erysipelas ever to become contagious, and when these are present, no one can say what erysipelatous case may not become the source of a wide spreading puerperal disease. It may be objected that cutaneous erysipelas, when contagious, should excite a cutaneous disease, and not an abdominal; but it is enough to reply, that, as erysipelas is not confined to the skin, the puerperal state is enough to concentrate the influence of the poison on the abdominal organs, and then external disease is prevented on the principle of substitution and counter-excitation.

I trust that no apology is required for this extended discussion of so important a subject; if it is a means of adducing other more combined observations from the members of the Association, my pains will not have been thrown away.

ON THE PITUITARY GLAND.

TO THE EDITOR OF THE PROVINCIAL MEDICAL AND SURGICAL JOURNAL.

SIR,

Through your widely circulated columns permit me to call the attention of your anatomical readers to the hitherto, I believe, unexplained function of the pituitary gland, situated, as is well known, in the cerebral hollow of the sphenoid bone, surrounded on all sides by the dura mater, with the exception of its superior aspect, where it is in contact with the infundibulum, having the internal carotid arteries on either side. Upon the external coats of these arteries numerous small filaments of the sympathetic nerve may be traced ramifying, and forming a beautiful plexus, bearing some resemblance in appearance, though not in reality, to the air-vessels in the wing of an insect. By careful manipulation these delicate branches may be traced, if I am not in error, to the inner and under parts of the process of dura-mater surrounding the hitherto described gland; and something in the manner of the cribriform opening in the sclerotic, through which the optic nerve passes, to expand into the retina of the eye. The before-named transit of the sympathetic may be seen, under a moderate magnifying power, passing to the substance in question.

I should hence be inclined to consider the pituitary gland as the first or true cerebral ganglion of the great sympathetic. In this view I am confirmed by the

ultimate structure of the ganglion itself, as under a magnifying power of five hundred diameters the ultimate structure of the superior cervical ganglion of the sympathetic, the cineritious matter of the cerebral masses, and the pituitary ganglion, are precisely similar, with this exception only, that the cineritious matter of the cerebral masses contains more aqueous fluid. I admit the difficulty in examining these structures under a high power of the microscope; to this I attribute my inability to detect the tubular arrangement described by Ehrenberg, under the name of varicose tubes, which he states to exist in the brain, spinal cord, and nerves of spinal sensation. Medullary matter has at all times presented the appearance of cylindrical tubes, whenever I have examined it under a microscope, without the dilatations he has figured and described.

The comparative anatomy of the nervous system still further corroborates the view of the pituitary ganglion I have ventured to offer. I have now before me a beautiful drawing on a large scale, made by a first-rate entomologist, of the *Cossus lignipenda* of Lyonet, where the sympathetic nerve is clearly delineated in the same relative situation in the head of the insect; and would space permit, I could much increase the number. The ganglion of *Ribes* I have not been able to discover, unless certain granular masses, very similar in ultimate organization to cineritious matter, loosely connected, in the substance of the infundibulum, are to be considered as such. In passing I may be permitted to state, that the usual description given of the infundibulum is not confirmed by microscopic examination.

While upon this subject, I wish to remark that I have reason to believe a decided connexion may be traced between the axillary plexus in the bat with the optic lobes of the cerebrum. Should further enquiry establish this position, a satisfactory explanation may be afforded for the exquisite sensibility, amounting almost to an additional sense, these animals possess in avoiding injurious objects while in pursuit of insects; and might be carried out still further in elucidating that very delicate sense of touch, persons deprived of sight by congenital or accidental misfortune, almost invariably manifest, upon the principle, or rather law, alluded to by Paley and others, of the *balancing of organs*,—or, in other words, "that the deficiency of development in one organ or sense produces a corresponding increase of development in another," and *vice versa*.

As in the larva condition of the insect, the assimilation of food, and the increase of bulk, seems the sole object of its existence; the reproductive being dormant; in the imago or perfect insect, the reproductive system is the chief object of its existence, the nutritive being passive, or entirely dormant, as in the silk-worm moth.

From the little attention I have been able to give to this subject, I am inclined to the opinion that something analogous to the reflex action of the true spinal axis, so ably detailed and investigated by Dr. Marshall Hall, may be established in the ganglionic masses of the cerebrum and cerebellum; that an intimate union may be traced between certain nerves of specific sensation, as I venture to consider the fourth pair and eighth, or portio mollis, may be easily demonstrated in the ruminantia, though with more difficulty in the human brain.