connective tissue in the liver after ligation of the portal vein, as I have found after ligation of the bile-ducts.

This overgrowth of the connective tissue was not altogether unknown to the older pathologists. Albers (*Beobachtungen auf dem Gebiete der Pathologie*, Bonn, 1836, p. 19), in speaking of cases of obstruction and dilatation of the bile-ducts, says the parenchyma of the liver is granular, rough, coloured yellowish brown; and that in one case the liver was really cirrhotic. It appears, he continues to say, that the cellular tissue of the liver is partly atrophied, partly indurated. Andral (*Clinique Médicale*, vol. ii, p. 534, 4th edition, Paris, 1839) noticed, in a man whose bile-ducts were much dilated, that the liver was small, deep green, very dense, and granular on the surface and section. These changes Andral seems to attribute to a fall on the right hypochondrium two years before death; but it should be noticed that the man had also been a drunkard.

The changes which the liver-cells undergo are less striking. Leyden (*loc. cit.*), from his experiments, drew too hasty a generalisation that a fatty infiltration of the cells followed all obstructions to the bile-ducts. This is not universally the case either in man or in animals. In man, I have yet to meet with the case in which fatty infiltration was the most prominent appearance. In some cats who died within a short time of the bile-ducts being tied, I have, indeed, seen this appearance; but I am inclined to attribute it to the liver having been fatty before the duct was tied—a state not at all uncommon in domestic animals.

The chief change in the liver-cells is atrophy. The size of the cells varies from a little smaller than natural, to twice that of a leucocyte when the growth of the connective tissue has been great. When the connective tissue shows less appearance of growth, the cells are less altered; they then preserve very nearly their natural shape and size. In other cases, the shape varies with the size; they tend to become oval or rounded; their outline indistinct. The nucleus is well preserved, shrivelled, or invisible. The contents of the cells seem to be chiefly fat and a few pigment-granules: the fat is rarely in large drops; and the pigment, in the cases which I have observed, not so abundant as the writings of others led me to expect. The arrangement of the cells in rays around the hepatic vein is altogether lost, when the obstruction has existed for any length of time.

One of the most important functions of the liver known to us is the preparation of glycogen. I have made several observations bearing upon the question, whether the liver continues its glycogenic functions when the gall-ducts are completely obstructed. I have found that, in cats, the glycogen disappears from the liver within a few days after the bile-ducts have been tied; and also that, on the fifth or sixth day after ligation of these ducts, irritation of the fourth ventricle is not followed by the appearance of sugar in the urine (*Archiv für exper. Pathologie*, 1874). It would appear, therefore, that within a few days after complete obstruction to the gall-ducts, the liver ceases to secrete glycogen.

Another function of the liver, that of secreting the bile-acids, is believed by Golwin (*Archiv für pathol. Anat.*, 1871, vol. liii, p. 433) to be abolished in long continued jaundice. He found that, in dogs in which biliary fistulae had been made and again closed, the bile-acids could not be discovered in the urine; and he concludes therefrom that, in long continued jaundice, the bile-acids are no longer produced. Further evidence will, however, be required before this opinion can be universally assented to.

Some physiologists appear to believe that the urea and uric acid excreted in the urine are formed in the liver. The experiments of Gscheidlen (*Studien über den Ursprung des Harnstoffs im Thierkörper*, Leipzig, 1871) seem to make this theory more than doubtful. However this may be, in complete obstruction to the gall-ducts I have several times found that, although the urine was much increased in quantity, yet the amount of urea excreted in the twenty-four hours was quite natural.

In some cases of long continued obstruction to the bile-ducts, the liver-cells have been found completely destroyed; nothing but a fatty *atritus* being seen under the microscope, and not a single liver-cell being found in any of the preparations. I have myself seen this appearance only in acute yellow atrophy of the liver. Yet it is noteworthy, that the very first case in which the liver-cells were found to be dissolved was one in which a permanent obstruction to the ducts existed (Thomas Williams, *Guy's Hospital Reports*, 1843, p. 444). The relation, as to cause and effect, which exists between obstruction to the bile-passages and dissolution of the cells, is dubious. I am unable to admit Oskar Wyss' explanation, that the solution of the cells is simply a *post mortem* change. I have frequently kept a liver from a jaundiced patient, even in the heat of summer, without any preservative fluid, and yet been able to recognise the cells many days after death. In one case, where the obstruction to the ducts was complete, and the jaundice

had lasted a year, the liver-cells were easily recognised on the seventh day after death, when the liver gave plain signs of putrefaction.

This solution of the cells was attributed by Theodor von Dusch (*Untersuchungen und Experimente als Beitrag zur Pathogenese des Icterus und der acuten gelben Atrophie der Leber*, Leipzig, 1854, p. 36) to the action of the bile upon the liver; but his statement, that the bile dissolves the liver-cells, has been disproved very satisfactorily by Kühne (*Archiv für pathol. Anat.*, 1858, vol. xiv, p. 324). It is true that the bile renders the outline of the cells indistinct, and the cells themselves become difficult to see; but the outline of the cells can always be restored by the addition of some colouring matter to the slide. I have verified these experiments on several occasions; and there is no likeness whatever between the sudden and complete disappearance of the red blood-corpuscle when acted on by bile, and the slow obliteration of the outline of the liver-cell under the same circumstances.

It is still important, however, to keep in view the results of Leyden's experiments (*Beiträge zur Pathologie des Icterus*, Berlin, 1866, p. 60) upon the slow absorption of the bile-acids. He found that, after injection of the bile-acids under the skin of frogs, the liver-cells showed a marked fatty infiltration; the muscular fibre of the heart a diffused granular appearance. In one case, it is stated that the liver-cells were destroyed. The bile-acids had produced a parenchymatous degeneration of the liver and heart—a common effect of many poisons, such as phosphorus, sulphuric acid, alcohol, etc., and, as I have recently shown, of raising the natural temperature of the body (*Trans. of the Pathological Society*, 1873, vol. xxiv, p. 266). It is quite possible that in these cases, where the liver-cells are found destroyed after death, the long continued circulation of the bile-acids in the blood may have produced their physiological effect, as described by Leyden; and the liver-cells may have thus been dissolved. The appearance of solution of the cells of the liver is, however, not often met with outside acute yellow atrophy; and it would be desirable to have recorded further cases, well authenticated by competent observers.

ON ERYSIPELAS DURING THE PUERPERAL CONDITION.

By JAMES REID, M.A., M.B.

It is generally agreed among medical men that to lying-in women there is no greater danger than exposure in any way to the poison of erysipelas. Among the many sources to which puerperal fever can be traced, this is regarded, and apparently on the most ample grounds, as one of the most potent; though there are authorities who maintain that the poison of puerperal fever is a specific one, and that other specific poisons will not engender the malady. The following case seems, at the first blush at least, to favour the latter theory.

On being called one morning lately to see a woman who was daily expecting her confinement, I found her suffering from a severe attack of erysipelas of the head and face. She had been ill for a couple of days, and was steadily getting worse, the tumefaction over the whole face being very considerable by the time I saw her. There were no symptoms of impending labour in the morning; but in the evening, after a very few pains, she was suddenly delivered of a healthy child. Not having hitherto had a case where labour came on during the height of an attack of erysipelas, and having in view the current theory, I formed in my own mind a grave prognosis, and my opinion was shared by a medical friend who saw the case. The treatment from the outset and throughout was simply beef-tea, milk, eggs, and port wine *ad libitum*; with fifteen minims of the tincture of iron every two hours. During the first two days after parturition, the erysipelas increased in intensity, the contour of the features being in a great measure lost, and the neck being invaded. At one time, there was slight delirium, but this wore off. By the third day, there was a change for the better; and there was now steady progress towards convalescence, the erysipelas terminating in resolution, with desquamation of the cuticle, after a duration of ten days. During this time, the patient consumed a large quantity of the remedies mentioned, and took in all three or four ounces of the iron tincture. The lochia were at no time suppressed or unnatural, nor was there ever the slightest symptom that the uterus was affected. There was, further, no febrile state, more than was accounted for by the erysipelas; and this was not intense, nor did it take on in any way a typhoid aspect. In short, the erysipelas ran its course in a most favourable manner, ignoring as it were, and not interfering in the slightest degree with, the mother's puerperal condition.

Now here is a case where, on the current theory, puerperal fever might have been confidently looked for. If puerperal patients be so susceptible to the poison when conveyed by the atmosphere or by con-

tact, one would, *à priori*, expect that untoward consequences could not fail to follow in a case where the poison was already present in the circulation, and where circumstances were so favourable for its operation. But such was not the case; and either erysipelas cannot invariably be so very fatal a source of mischief to lying-in women as is generally believed, or there must be some explanation of such a case as the present. I do not believe the case is by any means unique, as a friend assured me he had a lying-in patient lately where erysipelas affected the left arm entirely and part of the left side of the trunk, with no more unfavourable result than the formation of large bullæ; and I have no doubt some of the readers of the JOURNAL can recall similar cases.

One explanation may be, that the poison, having already become localised before labour came on, remained so, and wore itself out without invading the uterus or the system at large; though this view is at variance with the spreading and metastatic nature of erysipelas, which might have been fairly expected to come into play under circumstances so favourable as in the case at issue. Or, assuming, as some maintain, that erysipelas and puerperal fever are essentially the same disease, it might be suggested that the erysipelas here acted as a prophylactic against the more severe form of the same disease. Unfortunately, however, for this suggestion, one attack of erysipelas confers no immunity from subsequent attacks. Very probably the case in point can be explained by competent authorities; but, failing any reasonable explanation, it seems to favour the view held by Dr. Snow Beck, that puerperal women have no influences at work in the system whereby the poisons of scarlatina, erysipelas, typhus, etc., can be resolved into the poison of puerperal fever, which poison, he believes, is specific.

CLINICAL MEMORANDA.

HÆMATOMA AURIS.

II.

THIS subject having received some attention in the JOURNAL, the following brief notes of a case which has lately come under my notice may prove interesting from the fact that, in one or two important points, it seems to differ from the cases hitherto observed.

A. B., a Dutchman, healthy-looking and well-built, aged 29, presented himself with a tense, elastic swelling about the size of a small chestnut, involving the helix of the left ear, and almost entirely obliterating the fossa. This swelling was somewhat purplish, and bulged more anteriorly than posteriorly; it did not pulsate, nor was its temperature sensibly increased. He stated that it began to appear about three months previously without any apparent cause, and had been increasing gradually; it had never caused him any pain, nor was it at all tender on pressure. There was no history or evidence of any cerebral symptoms; the man seemed quite up to the ordinary intellectual standard, and denied the existence of insanity in any member of his family. On cutting into the swelling anteriorly through the hypertrophied cartilage, florid, somewhat frothy, blood began to ooze out from a cavity, which, on introducing a probe, was found to exist between two distinct layers of cartilage. When all this blood had oozed out, the swelling was somewhat reduced, but, after a few days, returned to its original size.

If this be a true case of hæmatoma auris, it differs from those previously recorded in the following particulars: 1. In the absence of any mental affection, and of all cerebral symptoms whatever, either in the man himself or any of his relatives; 2. In the gradual appearance and slow increase of the swelling, and absence of all pain and tenderness. I shall keep the case under observation to note its further progress.

JOHN CROSS, M.B.Camb., Spital Square.

III.

DR. PHILLIMORE, in his paper published in the BRITISH MEDICAL JOURNAL of April 25th, upon hæmatoma of the external ear, says he has never observed it "unless accompanied by disease of the brain or its membranes". If Dr. Phillimore mean coarse lesion of the brain or its membranes, these two cases which I now record will tend to disprove it; but, if he mean functional, it is almost useless to make that distinction, seeing that in every case of insanity there must be functional disturbance. For sixteen years, my experience led me to believe in the connection of hæmatoma with cerebral disease; but, during the last four years, I have met with two cases which have convinced me that it may occur independently of grave cerebral lesion; one was that of a girl, aged 22, with acute mania; the other that of a man, aged 45, and similarly affected, but whose case was much more chronic. I do not suppose such cases are extraordinary, but they show that hæmatoma auris has no *essential* connection with cerebral disease. At one time, it

was my conviction that this singular morbid condition was found in those only who were incurably insane, and was, therefore, a ground for an unfavourable prognosis; but I have good reason to know that, in many cases, a blow has produced the hæmatoma; it is not always easy to say precisely how the injury was inflicted, but one can readily imagine that a restless patient might, in tossing about in bed, receive a slight contusion on the external ear, and in this way get hæmatoma. In short, it seems to me highly probable that the hæmatoma of insanity may have such an accidental origin, and that we are, therefore, not justified in asserting that there is any necessary connection between the aural lesion and confirmed disease of the brain or its membranes.

WILLIAM C. HILLS, M.D., Superintendent of the Norfolk County Asylum, near Norwich.

REPORTS OF SOCIETIES.

CLINICAL SOCIETY OF LONDON.

FRIDAY, APRIL 25TH, 1874.

PRESCOTT HEWETT, F.R.C.S., President, in the Chair.

CERTAIN British, American, French, and German physicians and surgeons were recommended by the Council for election as honorary members of the Society. The names were ordered to be suspended for one month, in accordance with the bye-laws.

Cancer of Transverse Colon, producing Sphacelus of Wall of Abdomen.—Dr. ANSTIE read, for Mrs. Garrett-Anderson, notes of the case. The patient was a woman, aged 68, and presented, on first being seen by Mrs. Anderson at the Women's Hospital, December 9th, no distinctive signs of malignant disease. A slightly movable lump, which existed on the right of the navel, was quite painless; and it was at first hoped that it might be mere fecal accumulation. Purgatives at first diminished the swelling, and seemed to make it softer. Thirteen days after her first visit, however, some pain came on, and she was taken into the hospital the next day. By this time, sphacelus of the skin had already commenced, and it was evident that there was a communication between the bowel and the surface. The treatment consisted of opium and brandy, given by the mouth, and disinfected poultices applied to the sphacelated part. The woman sank and died on December 30th, or twenty-one days from her first being seen, and one week after admission to the hospital. *Post mortem* examination showed that the malignant disease of the transverse colon was primary, and that there was no cancerous disease in any other part of the body. The bowel became adherent to the abdominal wall, and a perforation took place, through which fecal matter passed into the muscular and subcutaneous cellular tissue, causing general cellulitis and sphacelus of a large portion of the skin of the abdomen. A ring of cancerous tissue almost occluded the bowel and caused fecal accumulation above; through this ring slow perforation had taken place, with inflammatory adhesion to, and subsequent perforation of, the successive tissues of the abdominal wall. It was an interesting fact that the colon had been diagonally twisted by the results of old omental adhesions, due to an old omental hernia.—Dr. ANSTIE mentioned particulars of another case, in which it was at first doubtful whether an abdominal tumour consisted of an accumulation of feces or a solid growth, which in time proved to be a mass of malignant disease. The question of diagnosis in such cases was often very difficult.—The PRESIDENT remarked upon the rarity with which cancer of the bowel led to sphacelus of the abdominal walls. He had seen such an occurrence once or twice, but then the cæcum was the part primarily affected. The bowel at that point being closely bound down, cellulitis was easily produced, and an abscess formed. Such a disease in the colon must be much more uncommon. It was not at first easy to decide upon the exact cause of the abscess; whether it were due to simple inflammation of the cellular tissue, or to malignant disease of the bowel. Another patient, a lady, died of a tumour in the bowel, which formed a hard immovable mass in the iliac fossa.—Dr. FITZPATRICK had treated a case of cancerous tumour of the descending colon, close to the rectum, which baffled diagnosis. It caused a large accumulation of feces in the colon, and was at one time regarded as a case of neuralgia and of abscess. Further particulars would probably be published.—Dr. D. POWELL inquired if there was any objection to the administration of purgatives in such cases for the sake of diagnosis. In a case of doubt, surely a purgative would clear up the diagnosis.—Mr. T. NORTON had seen Mrs. Anderson's case; the intestinal contents escaped into the cellular tissue and caused the fatal result.—Dr. ANSTIE remarked that, in some instances, a purgative might resolve all doubts about a case; but that, in any case where fecal vomiting had existed for a day or two, a purgative would be inadmissible.