besides myself, not one of whom had this disease in his practice. Can any one say this is mere coincidence? Surely there can be no other explanation of the origin of the disease in these cases, than that the *materies morbi* was in some manner connected with my person.

Was it connected with my clothes? This does not seem to me reasonable; for I changed them twice during my attendance. Moreover, Mr. Wilson would have then been nearly if not quite as liable to carry the contagion; and any one who has met with this disease can realise the facility with which, *cæteris paribus*, it could be transmitted by this means, owing to the peculiarly offensive smell which emanates from those affected.

Do I know any source from which the contagious principle might have been derived? Small-pox has been suggested to me as a probable one; and it coincides with this explanation, inasmuch as we have had this disease for months prevalent here in its very worst form. But here we are met by the difficulty, "How did it happen that I was the only one to have puerperal fever, seeing that all were exposed to the cause?"

Burns or scalds, from the suppuration which succeeds them, would seem likely to furnish the means of transmission. About December 12th, a case of this description came under my treatment; and, owing to the timidity of the patient's wife, I was required to dress the case during the first few days. Yet, considering the frequent ablutions which medical men necessarily use, and the continuance of contagion so long after exposure to the supposed cause, I cannot believe that this is a satisfactory explanation. It seems unlikely that the *materies morbi* could continue to exercise its influence with such unabated vigour, unless it were reproduced. Is it not possible that it may be derived from no extraneous source, but generated in the system of the accoucheur?

It is my firm conviction, that it is the hand of the accoucheur which communicates the disease; and the perspiration the channel through which it exerts its deleterious effects.

It is worthy of note, that the few cases attended by my friend recovered without any bad symptom, though I visited them afterwards.

It may be thought strange that the cause missed taking its effect in so many instances; but we see an analogous exemption from other diseases known to be contagious. Even in vaccination, where the intention is to transmit the disease, cases are met with which seem to be for a time exceedingly difficult to take effect.

Case XIII appears to have been a type of the rest, and determines the nature of the disease to have been phlebitis. I venture to express an opinion that there are three diseases classed under the term "puerperal fever", each having a distinct origin:

1. Sporadic cases, such as arise from conditions solely connected with the person affected;

2. That form of the disease which I have attempted to define, communicated by the accoucheur—phlebitis;

3. The true epidemic, allied to typhus fever.

I should not omit to mention that, about two years ago, I had three cases of puerperal fever under treatment at the same time, when I was not aware that it was prevalent apart from my practice. I should also state that my health has been uniformly good, so that I could not adduce any evidence from this fact in support of my view of the origin of this disease. I have come to entertain this opinion by a negative process of reasoning.

AUTOPSY OF A CASE OF LATENT CARCINOMA OF STOMACH AND LIVER: WITH A FEW REMARKS.

By PAUL BELCHER, Esq., L.R.C.P.Lond.

THE examination was made on July 5th, 1865, twelve hours after death. The deceased was forty-one years of age, married, and had had one premature still-born child. The body was much emaciated; and the decay of the muscular was even more striking than of the adipose tissue. The rigor mortis was slight, and overcome by the gentlest effort.

The thorax, liver, stomach, duodenum, small intestines, and spleen, were examined. The organs in the thorax were healthy, except the apex of the left lung. This was firmly adherent to the ribs posteriorly for the space of an inch square. It was infiltrated with an evidently cancerous exudation, in which all lung-structure was lost. The stomach was distended by gas, and contained a little semi-digested food, looking like egg-flip. Its lesser curvature, and also the approximating edge of the liver, were a mass of carcinomatous matter, of a dirty white colour, and nodulated externally. The convex surface of the liver, as it was exposed *in situ*, presented three chief cancer-deposits, of a size varying from a hen's egg to a cob-nut. One of these was very striking in appearance, from the arborescent injected appearance of the surrounding structure. After removing *en masse* the stomach, liver, and duodenum, the former, when opened, presented an immense dirty white fungous-looking infiltration occupying all the lesser curvature, and in which the pylorus was lost. The œsophageal opening and the larger curvature were normal. The duodenum, except at its very commencement, was healthy; and the pancreas was not observed to be diseased. The structure of the liver, where the cancer had not encroached, was paler than natural; the capsule separated easily, and its total bulk was not much increased. The cancerous masses with which the liver was crammed were of firm consistence, almost crying under the knife. The spleen was free from disease.

REMARKS. The history of the case of Mrs. M. is an instructive one, and illustrates the fact that a fatal amount of cancer may be developed so quietly and insidiously as to give but few and uncertain tokens of its presence. We may call it a case of *latent* cancer. Fourteen years ago, Mrs. M. was a remarkably fine, blooming, healthy woman, and as strong as she looked. She had been married some years, and there had been no issue. About the date mentioned, she began to fall off slowly, but progressively and surely. She had little medical advice, however; and there was no tangible evidence of distinct disease; no lung-disease; no heart-disease; no uterine disease; nothing that you could put your finger upon and say, "*There* is her complaint." She lost in appetite and flesh *pari passu*; ate little meat; felt weak, and sometimes dyspeptic. And so she went on year by year. About five years ago she became my patient—as fragile-looking a one as any would care to "take to". She had the aspect of one who was nursing a fatal disease, but rather that of a tuberculous than cancerous dyscrasia. She complained of ordinary dyspepsia, with sickness often matutinal. She had not been regular in her uterine functions for a year or more: now too much, now too little, and so on haphazard. She had had no catamenial relief for two months. I examined, and found the uterus healthy; but suspected pregnancy. She gained considerably in flesh and strength, and eventually was prematurely confined at between the sixth and seventh months.

She soon relapsed into her habitual ill health—the debility and want of appetite. At one time, considerable anasarca of both legs appeared, for which she took quinine and iron with diuretics, and which soon passed off. So she went on till, a month ago (during her husband's illness from hæmoptysis), overcome by anxiety and nursing, she seemed visibly to fade away. There had been a little hæmorrhage from piles: no other fresh symptom. A week before her death, she took to her bed; had vomiting and intense pain, horrid fœtor of the breath, tenderness, and an undefined thickening in the præcordial region. She had, moreover, percussion-dulness, harsh respiration, and bronchophony at the posterior part of the left lung apex.

This was her history. So vague were her symptoms, that not till the week before her death did I venture to express my diagnosis, which in every particular was confirmed by the autopsy. At what date was the first cancer-germ? Was it cancer *quia* debility, or debility *quia* cancer? There is no hereditary history of cancer; and I have little doubt that the commencement of cancer-formation and the debility were simultaneous; that the exudation was remarkably slow, and the symptoms more than usually obscure.

I must add, that a sister, twelve years younger, is wasting away as Mrs. M. did; and "there is no disease, *only* debility."

Reviews and Notices.

CONTRIBUTIONS TO ASSIST THE STUDY OF OVARIAN PHYSIOLOGY AND PATHOLOGY. By CHARLES G. RITCHIE, M.D., etc. 8vo., pp. 208, with Woodcuts. London: 1865.

IN medicine, as in every other branch of science, theory and practice are mutually dependent. Now theory is in advance, teaching us how to direct our treatment. Now we collect our facts, and by inductive reasoning string them into a theory.

Probably few of the surgeons who have practised ovariotomy reckoned among the advantages of the operation that it would provide a store for the labours of the pathologist; yet such is the case. Art has opened up a wide field to science; and there is now no internal organ for the study of whose morbid anatomy more opportunities exist than the ovary. Instead of wearily waiting for the chance of getting an ovarian case into the *post mortem* theatre, all that is necessary for the pathologist is to make interest with one of our busy surgeons, and he will be supplied almost *ad libitum*. Dr. RITCHIE was fortunate enough to have Mr. Spencer Wells as his purveyor of ovarian tumours, and he now publishes the results at which he has arrived.

"The great mass of ovarian disease is due to slight errors of nutrition. More serum is poured into a vesicle than is normal. The secretion is retained, instead of being got rid of, or is poured into a cavity which was not intended to receive it. Cells go on multiplying *ad infinitum*, and thus produce hyperplastic growths. Changes which normally occur in the contents of the vesicle, after they have been discharged, take place within the vesicles themselves, either in a perfectly regular series, or slightly modified by their novel situation. Such are the different situations in which ovarian cysts are produced." (Page 183.)

It has long been known that some of the more simple forms of ovarian cysts are due to simple dropsical enlargement of Graafian follicles. Rokitansky proved this by discovering ova in some of the smaller cysts. Dr. Ritchie has also discovered ova in ovarian cysts; and he has thus been able to demonstrate that not infrequently a space becomes hollowed out in the ovary, the walls of which space are formed by ovarian tissue with Graafian follicles, in which healthy ova may be found.

Dr. Ritchie argues, that if the cyst—perhaps a foot in diameter—has walls of ovarian tissue, it is evident that ovarian tissue is in excess; that there is hyperplasia of the ovary, or adenoma. He is here at issue with the later German authorities, who, while admitting that a healthy ovary may contain thousands of germ-follicles, still maintain that no new germ-follicles are produced after birth.

However, this is not the chief point which Dr. Ritchie wishes to establish. His chief object in writing the book before us is to show that some ovarian tumours are due to development of ova while still in their ovisacs; that they are unsuccessful attempts at reproduction; and that they ought to be classed along with "moles", which are found in the uterus. In proof of the correctness of this supposition, Dr. Ritchie first compares skin-cysts of the ovary with skin-cysts found in other parts of the body. He shows that these two varieties of skin-cysts have "an apparent but no real analogy"; that they differ in every respect, except that cutaneous structures may be found in each. He then gives a detailed description of a dermoid ovarian cyst which he examined after its removal by Mr. Wells from a virgin, and in which he found hair, skin, bone, and cartilage.

Dr. Ritchie next proceeds to show that dermoid cysts identical with those of the ovary may be found in the uterus, where there can be no reasonable doubt that they are "so-called blighted ova".

Having proved that an unimpregnated ovum may go on to the formation of such highly organised structures as bone and cartilage, it is by no means rash to hazard a conjecture that it may stop short at an earlier degree of development, and form a cystic tumour, the analogue of the hydatid mole found in the womb. And this is what Dr. Ritchie believes to be the case.

The question of the malignancy or non-malignancy of ovarian tumours has often been discussed. Some of them are undoubtedly cancerous; some of them are as undoubtedly benign; but many are very doubtful.

In speaking of dendritic growths, Dr. Ritchie says: "There is a great deal of doubt as to their nature; but, on the whole, I am inclined to look upon them as a kind of epithelioma, having some affinity to, or some tendency to degenerate into, true cancer." He, however, confesses that as yet we are almost entirely ignorant of the relation of cancer to ovarian disease.

Dr. Ritchie has considerably lightened the labours of future investigators by giving an historical summary of the different opinions which have been held on ovarian physiology and pathology at different periods and by different writers. This summary is carried down to the latest date, embracing the antagonistic views of Grohe and Pflüger in Germany, and giving the results which were recently arrived at in this country by Dr. Wilson Fox.