in succession. To any one who knows the ordinary behaviour of proglottides during their migratory passage from the human bearer, it will appear simply incredible that, during the act of childbirth, enough of these segments should have gained access to the infant's mouth to ensure such an eventual secondary wandering, so to speak, from the baby's rectum. Dr. Haussmann will now perceive that his explanation, in the face of these additional facts, cannot be reasonably entertained.

From a passage quoted by Dr. Haussmann from his own treatise (*Die Parasiten*, etc.), it is evident that, in other cases of infantile parasitism "now and then recorded in literature", he thinks an explanation of such phenomena may be sought and found in the fact of his having himself detected the ova of parasites in the mouths of infants. Dr. Haussmann's experiences in this relation are most interesting; but, notwithstanding his discovery of all sorts of strange things, including "the eggs of *Tenia solium*", I will not do him the injustice of supposing that he thinks there could be any tapeworms formed in the intestinal canal of a baby only a few days old, from such a source. However, I fear some persons will have interpreted his quotation in the sense to which I refer.

Be this as it may, it is quite clear that Mr. S. G. Armour supposes that some such explanation is possible. He says, "How did the *tenia* or the parasitic embryo gain entrance to the intestinal canal of the foetus *in utero*? for the child was fed on nothing but the mother's milk." He evidently supposes that the tapeworm in the infant might have been developed in a direct manner from the eggs or their contained embryos within the space of a few days. By no possibility could this be the case. The researches of Von Siebold, Leuckart, Küchenmeister, Haubner, Röhl, Möller, Mosler, and others, which I have myself both verified and extended in certain directions incontestably prove that the sexually mature tapeworm can only be developed from the so-called cysticercal phase of tapeworm-life. To assert, or even to assume, that a tapeworm can be developed in a few days from eggs or embryos introduced into the infant's mouth at the time of birth, is to hazard an explanation which utterly ignores every legitimate conclusion deducible from experimental inquiry. The sequence of phenomena observable in the life-history of any human tapeworm is, in its larger phases, as invariable and uniform as are those more striking changes which we recognise in the succession of the seasons. In the case of the tapeworm, something like three months is necessary for the perfection of the cysticercal stage alone; whilst a similar period is requisite for the development in the body of the ultimate host of the perfect sexually mature tapeworm colony, whether it be the form called *Tenia solium*, or the still commoner species termed *Tenia mediocanellata*.

Leaving this vexed question, I will remark upon the high interest attaching to certain recent observations by other foreign writers. In a brochure (*Aus den Sitzungsberichten der physikalisch-medizinischen Societät zu Erlangen*), Dr. Arnold Heller records a most attractive *post mortem* "find" which he made in the middle of May last. In the small intestines of an imbecile (*Geisteskranken*) fifty-two years of age, he discovered eighteen examples of juvenile round worms; these being, without doubt, referable to the common *Ascaris lumbricoides*. The largest of these measured about half an inch in length (more strictly, 13.0 millimètres); whilst the smallest gave only a long diameter of 2.75 millimètres, or let us say, roughly, about the ninth of an English inch. Dr. Heller's record of the microscopic examination of these minute round worms, though brief, displays rare precision in reference to the facts of internal organisation; but the special value of his communication lies rather in another direction. As he correctly points out, between the embryos, as they appear at the time of their expulsion, and the smallest round worms hitherto observed in the human subject—including in this reckoning the recorded experiences of Leuckart, Vix, Küchenmeister, and Laennec—we have, as regards size, an immense gap to bridge over. Dr. Heller's observations, therefore, serve to supply almost all the missing links in the chain of developmental evidence necessary to prove the true nature of the life-record of *Ascaris lumbricoides*. In other words, it becomes almost certain that this common round worm completes its life-cycle without the necessity of its having to pass through or infest the body of any intermediary animal bearer. This is what obtains in the case of *Trichocephalus* and *Oxyuris*, as has recently been made known to us, chiefly through the admirable researches of Leuckart, Vix, and Dr. Heller himself.

At the Erlangen Society's meeting on the 3rd of last June, Dr. Heller also exhibited a specimen of *Ascaris mystax*, taken from the human body by Dr. Böhm at the Gunzenhausen Pathological Institute. On the occurrence of this fact, Dr. Heller remarks: "Es ist dies der vierte Fall von Vorkommen des Katzen- oder Hundespulwurms beim Menschen." In this calculation, however, I infer that he takes no account of the instance brought forward in this country by Dr. Thomas Morton.

There have been five, if not six, instances placed on record of the occurrence of this parasite within the human body.

In the present communication, I can do no more than allude to other advances in helminthology. The observations very recently brought under my notice by Dr. Krabbe of Copenhagen (*Aftryk af Tidsskrift for Veterinær*; 2 den Række: II); by Mr. Fedtschenko, the eminent Russian traveller, in reference to the structure and development of *Dracunculus* and *Gnathostoma*; further information concerning the prevalence of *Stephanurus* in the United States by Professor Fletcher of Indianapolis; and especially also certain statements by Dr. Wucherer, now in Stuttgart, in respect of the memoir and leading article on *Bilharzia* and its associated hæmaturia, which appeared in one of the July numbers of this JOURNAL;—these observations, I repeat, all and severally, are full of interest for myself and others. Drs. Wucherer and Fletcher will, I am sure, accept this apology for my not having given their valued information more immediate attention.

Lastly, one more sentence, to acknowledge the criticisms of my friends Dr. Leared and Dr. Crisp. The vigour with which the last named writer strikes out—as recorded in the report of the Pathological Society's meeting in last week's *Medical Times and Gazette*—is thoroughly characteristic, and will secure my respectful attention on a more suitable occasion. To Dr. Leared my thanks are due for pointing out Dr. Hannover's priority in relation to the discovery of peculiarly appendaged ova attached to the eyes of turtles (BRIT. MED. JOURN., Aug. 3, p. 140). I have great pleasure in recognising this priority on the part of that profound anatomist; the more so because, when Dr. Hannover visited Edinburgh in 1850 or 1851, I remember to have disputed the correctness of his views respecting the structure of the vitreous humour—basing my remarks on original investigations which were partly embodied in my prize thesis on the Eye. Having said thus much, I must, on the other hand, add that I do not think that Dr. Leared has shown that I fell into any "inaccuracy" of statement in my paper on *Bilharzia*. Quite independently of the question of priority, Mr. Canton's as well as Dr. Leared's own observations were valuable; and I alluded to the former rather with the view of fixing attention to a morphological fact of remarkable significance, than with the purpose of exhaustively analysing the literature of the subject. This must be my apology for apparently not having done justice to Dr. Leared's contribution in the same direction.

REMARKS ON THE TREATMENT OF MALIGNANT DISEASE OF THE UTERUS.*

By ALFRED WILTSHIRE, M.D., M.R.C.P. Lond.,

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ANYTHING which promises alleviation in cases of cancer, no matter what may be the part affected, deserves our earnest attention. Past experience and observation warn us to be careful of accepting the accounts of vaunted cures as trustworthy. The relief, however, when practicable, of those afflicted with cancer commends itself to our notice; and, moreover, it is our bounden duty to compass at least this much, if it be possible, for a class of sufferers whose afflictions are indeed grievous. Pre-eminently is this the case with those who are afflicted with cancer of the womb, to which disease my remarks have reference, and specially in connexion with the plan of treatment proposed by Professor Simon of Heidelberg, whose method is illustrated by two cases in which it was adopted.

My acquaintance with Dr. Simon's practice I owe to Dr. Munde of Newhaven, Connecticut, who introduced it to my notice. Dr. Munde, who is an accomplished gynecologist, having been for three years and a half assistant to Dr. Scanzoni, and having been for six months assistant to Dr. Simon of Heidelberg, was requested by the latter to introduce his method to his English *confères*, and to show the instrument he has devised—a facsimile of which I here present. At my suggestion, Dr. Munde introduced the subject, and showed the instrument at the June meeting of the Obstetrical Society.

Simon's plan consists in lightly scraping away the loose and fungating portions of the invaded tissues, thus leaving a healthier and less painful surface, which is also less prone to bleed. Simon thinks this plan preferable to the hot iron in certain cases—viz., where the tissues are largely invaded, and there are fungating and spongy masses, and in cases where, from the extent of surface, it is thought inadvisable to apply other

* Read before the Midwifery Section at the Annual Meeting of the British Medical Association in Birmingham, August 1872.

cauterants. It is thought that this plan of lightly scraping materially lessens hæmorrhagic and other discharges.

Unhappily, both hospital and private practice afford but too many opportunities for testing new methods of treating cancer of the womb; and I accordingly asked Dr. Munde to operate after Simon's manner on a patient who was sent to me by my friend Mr. Churton of Erith, with whom I attended her. She had already been greatly benefited by the application of chromic acid, but I thought there were still some loose portions of tissue which might well be removed by the scoop, and therefore asked Dr. Munde to operate. Some scrapings of partly disintegrated tissue were removed; the patient felt very little pain; there was but trifling bleeding, and the after consequences were of a satisfactory character, as far as they went, but I am bound to say that the patient did not appear to be much benefited. She was certainly not worse, and I think was rather better; the change, however, was not marked; this, perhaps, was owing to the decidedly beneficial effect already exerted by the chromic acid upon the disease.

Another case being about the same time in my service at the West London Hospital, I asked Dr. Munde to operate on her also. He removed a considerable amount of very soft tissue, exposing a large cavity. The operation was performed with extreme care, and only such portions as were quite soft and easily detached were removed. There was free but not excessive bleeding. A tampon of wool, soaked in solution of perchloride of iron, was inserted, as a matter of precaution, and to prevent any undue loss. The course of the case was as follows. Symptoms of peritonitis, of a severe form, presented themselves, and for some days the patient's life was threatened. She had a high temperature, quick and frequent pulse, distended abdomen, and all the signs of a serious attack of septic peritonitis. There was no hæmorrhage, but a free muco-purulent discharge. Pain and vomiting were troublesome. By dint of great care and skilful treatment on the part of our able senior house-surgeon, Mr. Bridgman, now of Burton-on-Trent, the patient was pulled through the attack, notwithstanding its apparent hopelessness; and great credit is due to Mr. Bridgman, and I here wish very cordially to acknowledge the ability with which he conducted the case to a successful issue. One day, after the patient had got over the more serious symptoms, Mr. Bridgman informed me, on my visit to the hospital, that she had passed a mass of something *per vaginam*. Before seeing it, I suggested that it might be a clot of blood moulded in the diseased cavity. Mr. Bridgman thought it too firm for a clot, which caused me to remark that it might be that the serum was expressed, and only the firmer portion of the clot remaining, as not unfrequently happens in intrauterine coagulation. However, on examining the mass, there could be but little doubt that it consisted of the whole of the body of the womb, shorn of the broad ligaments and ovaries. Dr. Munde, whom I apprised of this, could scarcely credit the possibility of what had occurred, but, on examining the specimen for himself, was convinced of its correctness. He also visited the patient, who had meanwhile greatly improved, and found scarcely any evidence of disease, as far as the finger could reach; but as we could not then touch the summit of the cavity, and, as the patient was too weak to allow of the speculum being used, we could not accurately determine the state of the upper part of the vagina. I have, however, since examined the parts, and find that almost complete cicatrisation has taken place, there being scarcely any puckering and very little induration. The finger passes into a kind of transverse hollow, almost like an os uteri, but with rather roughened surfaces. By the speculum, too, the parts looked very like an os, but there remained only a *cul-de-sac*, through which neither a sound nor a small flexible bougie could be passed with such force as seemed justifiable. A bimanual exploration declared the absence of any body corresponding in size, shape, or position, to the womb; and, indeed, it seemed evident from such physical exploration as it was prudent to make that the organ was absent. Generally as well as locally the patient has greatly improved, but I cannot conceal from myself the fear that the roughened state of the cicatrising surfaces may be indicative of remaining malignant disease. The pain, too, of which she sometimes complains increases my fear. However, I shall carefully watch the case, and, should occasion arise, will adopt such measures as appear called for to conduct the case to an entirely successful issue—in other words, to establish, if it be possible, which is very doubtful, and indeed not to be expected, seeing that the retro-peritoneal glands are presumably infected, a complete cure.* I exhibit the specimen, which I think no one will doubt is the uterus. I wish to say that not the smallest blame can be attached to the operator, whose judgment and skill are

beyond praise, or to the operation. The same result might have happened from spontaneous sloughing, though probably before that point had been arrived at the patient would have sunk from exhaustion. As it was, doubtless sloughing helped towards the result, and such attachments as were left might have been thus destroyed; indeed, only in this way can we account for the absence of the broad ligaments and ovaries, which puzzled us much. Neither Dr. Munde nor myself dreamt of such an issue, for, to the best of our judgment, the supports of the womb were not unduly encroached upon.

With reference to the extrusion *en masse* of the womb, it must not be forgotten, as Gaillard Thomas remarks, that Scanzoni, Rokitansky, Kiwisch, Virchow, and Klob, all acknowledge the possibility of the uterus sloughing away, and recovery following in cases of carcinoma of that organ. A case of this kind is recorded by Dr. Habit of Vienna, an epitome of which will be found in the Sydenham Society's *Year-Book* for 1864; and Aran quotes a case from Estevenet, where a cancerous uterus was spontaneously separated from its attachments and expelled in the act of defæcation. Indeed, Dr. Munde told me that Professor Simon himself has proposed to scrape away all the tissues of the uterus, except a bare shell of peritoneum: and Dr. Munde suggests that something of this kind might have happened in our case. Of this, however, I am doubtful, for it appeared to me that the whole of the organ, though shorn of its ligaments and appendages, had escaped bodily. Corresponding to the openings of the Fallopian tubes are two holes, which appear to indicate the point of severance of the oviducts from the body of the organ. The specimen was placed in chromic acid, and has accordingly undergone much hardening and shrinking. Sections show the curling arteries peculiar to the organ, and microscopical examination demonstrates the usual uterine tissues. Only the cervical portion appears to have been invaded by the disease, the body being free.

Cases of malignant disease of the uterus are, in my experience, unhappily, very common; and although few are susceptible of great improvement, yet some are amenable to, and derive much benefit from, well directed treatment. Ablation, when possible, of diseased masses by the galvanic cautery or *écraseur*, the application of the hot iron or chromic acid, and scraping after Simon's method, are all very serviceable methods of treatment. When there is much bleeding, or the discharges are copious and offensive, nothing in my experience equals chromic acid, the astringent and deodorant properties of which far transcend anything with which I am acquainted. As an instance of this, I would mention that on one occasion a pledget of wool soaked in a solution of chromic acid was, through inadvertence on the part of the patient suffering from cancer of the cervix uteri, left in the vagina for a week, and on its removal there was not the smallest foetor detectable. This case was seen with me by Dr. H. Savage. On two other occasions a like accident has happened through fear of a patient in withdrawing a tampon, and neither time was there the smallest degree of offensiveness in spite of, at other times, fetid discharges.

Chromic acid should be used in solution, and with great care, lest it attack sound tissues. Half a drachm of the crystals to one ounce of water makes a very strong solution. It may be used as a vaginal injection in the strength of a drachm to ten or twenty ounces of water, or even weaker. I have thought that it might be an improvement on Simon's method of scraping, if the diseased parts were previously dressed with a strong solution of chromic acid. This, however, as yet lacks the test of experience. I am inclined to think favourably of the plan of scraping, if cautiously done, in suitable cases. The remarkable and very exceptional case which I have related, while it is eminently instructive, should yet make us very cautious in our procedures, and is not one which we should lightly seek to imitate, or at any rate is not one which in our present state of knowledge it would appear desirable to imitate; seeing that, although the internal organs are usually not affected in epithelial cancer, which is the form most commonly affecting the uterus, the retroperitoneal glands are sooner or later involved, and therefore it is only in the earliest stages of the disease, when a serious operation would hardly be justifiable, that any attempt at ablation of the organ is likely to be attended with permanent success.

My friend, Dr. Fordyce Barker of New York, has kindly given me a copy of some observations of his on the subject. He thinks very highly of early ablation of the diseased structures. He also gives an account of some cases in which a cure followed the application of acid nitrate of mercury.

The principle of Simon's method is by no means new, though perhaps his instrument may be. It was long ago practised by Récamier, who used a scoop or *curette* for scraping cancer of the body of the womb. Dr. Marion Sims, I believe, has also employed the same plan, and so I think has our distinguished Associate, Dr. Barnes. Indeed, whatever may be our views as to the pathology of malignant disease of the uterus,

* This surmise has, I regret to say, proved but too accurate. Though the process of erosion is not rapid, the parts surrounding the top of the vagina are now invaded, and the blind excavation at the extreme upper end of that canal will now admit half a walnut. The patient suffers a considerable amount of pain around the loins, leading me to infer extensive implication of the lumbar glands, etc.

the plan of treatment in question commends itself to our notice, and it is one which, cautiously used, may be capable of rendering valuable service.

It is not desirable to stop here to discuss the different pathological doctrines as to the true nature of the various affections which, clinically, we all recognise as more or less malignant, since that, deeply interesting as it is, would lead us too far from our present purpose. On this subject, the work of my friend Mr. Henry Arnott will be found full of instruction; suffice it to say, that the cases to which I have referred are instances of what is called epithelial cancer or epithelioma, the least malignant of the cancers, owing probably to the rarity or tardiness with which it affects the internal organs.

One word, however, on the etiology of uterine cancer. Setting aside hereditary influence—on which point the most conflicting statements have been made—it is difficult to arrive at any satisfactory conclusion as to the causation of malignant disease of the womb. On the one hand, there is much in favour of Rokitsansky's view, that cancer probably originates in a peculiar state of the blood, which consists mainly in a preponderance of albumen with a hypinotic condition, or diminution of fibrine, seeing that the disease chiefly affects persons who are subject to miscarriages, over-suckling, and other pre-existing depressing conditions, such as menorrhagia and like exhausting discharges occurring before the disease has arisen.

But in opposition to this view, it may be urged that thousands of women suffer in like manner and never become cancerous, implying the existence of some more recondite cause than mere exhaustion of the vital fluids. On the other hand, the view of those who, like the late Mr. Moore, hold cancer to be originally a local affection, and to be excited by blows and such like, is supported by the fact that the womb is certainly exposed to the chance of injury in parturition, abortion, etc. Scanzoni's tables, which show that cancer of the womb is more frequent in multiparæ than in nulliparæ, may be quoted indifferently by the advocates of either hypothesis.

There is just one possible cause of malignant disease of the uterus which I should like to mention—it is syphilis. I have seen undoubted syphilis of the tongue of long standing develop into true cancer, so recognised by Sir James Paget, who saw the patient; and the possibility of a like occurrence in the uterus appears worthy of consideration.

Before concluding, one word seems called for on general therapeutic treatment in carcinosis, as Billroth calls the cancerous cachexia. I am convinced from observation that cancerous patients derive great benefit from judiciously directed general treatment, and am of opinion that it should never be omitted. Iron (especially the astringent forms), ergot, iodide of potassium, arsenic, cod-liver oil, and such like, for the improvement of the general health (besides opium, cannabis Indica, chloral, etc., which latter are obviously indicated for the relief of pain), are all highly serviceable, and should never be omitted in dealing with malignant disease.

ON PYÆMIA.*

By EDWARD COPEMAN, M.D., F.R.C.P.,
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THE occurrence of several deaths, during the past year, in our hospital from pyæmia (so called), induces me to bring before you a few remarks upon the subject, in the hope of being able to elicit by discussion some more definite ideas as to the nature and treatment of this very unsatisfactory disease. When fully established, it appears to resist treatment, and cannot be controlled; at least, such is the conclusion at which most of us must arrive from what we have recently seen. One of the theories with respect to its causation, is that pus is absorbed into the blood, poisons this vital fluid, and leads to secondary deposits in some of the important organs of the body. But my own firm belief is that the theory is without foundation, and that pus is never absorbed into the blood. Pus is a bland fluid, unirritating, healing, and the result of one of the natural processes for the cure of inflammation. In a healthy individual, I do not believe that pus ever does any harm unless it be confined in a space which will not give way to it, and make injurious pressure upon other organs. How often do we see abscesses, when favourably situated, become absorbed without any injurious consequences; how disappointed we feel if wounds which do not heal by the first intention do not suppurate; we augur from it that in such a case the patient's strength is not sufficient to allow the formation of pus, and we have little hope of recovery. How often do large wounds, after operations of various kinds, continue to suppurate for days, weeks, or months, and we have no fear of ill consequences arising from it so

long as the pus is *laudable*. My own opinion is that the true cause of pyæmia, which I would rather call *caecæmia*, rests in the constitutional condition of the patients, and that the disease is by no means confined to cases of surgical operations. We all know that the blood always contains the ingredients, so to speak, of pus, and can at any time form it when Nature seems to require it for the cure of inflammation. Whenever inflammation occurs, there is what Cullen called *spasm of the extreme vessels*; and this frequently results in the formation of abscess. In inflammation, whether acute or chronic, active or passive, there is congestion in the inflamed part, and generally there is matter formed for the relief of this congestion. We see it in the formation of matter in the glands under the jaw in scarlet fever, in the skin in small-pox, in the liver in hepatitis, in the lungs in certain forms of pneumonia, in the prostate gland, in the eyeball, in the cellular tissue in carbuncle and erysipelas, in various organs in different kinds of fever—in fact, in any part attacked by inflammation; and I look upon the occurrence of secondary abscesses after operations as instances of partial stagnation of the blood (passive inflammation) when it is in an unhealthy state, and unable, for want of nervous influence, to eliminate any morbid elements it may contain. This is especially the case with respect to the liver, which contains more blood than any other organ, and so abscesses are often found there; and I do not think it would be stretching the point too far if we were to say that the same thing occurs in the lungs in cachectic subjects, where a number of small abscesses form around passively inflamed bronchial tubes, eventually forming what we call tubercle, the absorption of the watery part of the pus leaving the residue in a semi-solid form. I do not say that this is actually the *rationale* of the formation of tubercle, but it seems to me as reasonable as any other, and quite in accordance with the ordinary rules of morbid phenomena terminating in abscess.

Speaking of an accumulation of blood in an organ—the spleen, for instance—and the consequent formation of tubercle, Dr. Carswell says: "In one cell you may perceive simply the blood coagulated; in another, it may be coagulated and deprived of its colouring matter; and, in another, converted into a mass of solid fibrin, having in its centre a small nodule of tubercular matter. And speaking of the fact that the superior and posterior portion of the upper lobe is the spot in which tubercles, if any exist, are almost sure to be found, and where they first begin to suppurate and soften, he adds that, in his experience, there is no deviation from this rule, except when some other portion of the lung may have been the seat of an inflammatory attack, which has determined the priority of tubercular disease in that portion." (Sir T. Watson's *Lectures*, 5th edition.)

We must all have remarked the extraordinary similarity of the symptoms leading to death in all diseases which lapse into a typhoid state, whether from fevers or inflammations, or after surgical operations; the rough dry tongue, the rapid feeble pulse, the relaxed skin, obtuse sensorium, colliquate perspiration, panting breathing, powerless muscles, relaxed sphincters, and very frequently vomiting of green or grumous fluid. Surely some common cause for this must be at work; and that cause I deem to be a morbid condition of the vital fluid—the blood; and this whether attended with secondary abscesses or not. There can be no doubt that this fearful condition of things may be very much promoted, if not actually caused, by air impregnated with a *materies morbi*, and that pure air and good ventilation are most desirable and necessary adjuvants in the treatment; but it should be borne in mind that such cases occur often in great malignancy in country districts as well as in hospitals; and, as far as my experience will guide me, a well regulated hospital is more healthy than many country villages during the prevalence of an epidemic which terminates in what we always used to call typhoid symptoms.

A word as to treatment in cases of so-called pyæmia after surgical operations. No doubt the principal points are good nourishing food, good pure air, and such an amount of stimulants as will *strengthen* without *accelerating* the pulse; such as will *diminish* and not increase the rapidity of respiration. But surely medicine may lend its aid, and in some instances turn the scale in the patient's favour, although the general state of things appears almost hopeless. I do not intend to weary you with any long detail of the treatment in general; there are several points so evident to all of us as to the use of tonics, aperients, and opiates, that they need not be enumerated. But there is one agent to my mind of great value, and which I have seldom seen fairly tried in cases of pyæmia after surgical operations. Once, on passing casually through a ward at the hospital, I met with Mr. — at the bedside of a patient who was apparently in a hopeless typhoid condition, and in whom, if he had died, it would have been probable, from analogous cases, that internal abscesses might have been found. I suggested to him the use of oil of turpentine. Mr. — replied that he thought the man would die; and, as he had nothing more to suggest for his relief, he

* Read before the Norwich Medico-Chirurgical Society.