accurate conception of the form of the curved surface may be gained from the curves of the three series of experiments that have been made.

The region included within this curved surface represents every possible variation in the doses of atropia and physostigma, and in the intervals of time separating the administration of the two substances, that is compatible with the production of successful antagonism between physostigma and atropia. Its existence shows us how an investigation on antagonism may lead to very fallacious results, even when every care has been taken in obtaining a large amount of experimental data. I have already pointed out that, almost without exception, the instances of lethal antagonism asserted to exist cannot be regarded as certainly established, because sufficient care has not been taken in proving that recovery took place after an undoubtedly lethal dose of one of the substances concerned. In attempting to *disprove* the existence of any asserted instance of lethal antagonism, a fallacy of equal importance may originate from ignorance of the fact that the antagonism does not necessarily occur throughout an unlimited range in the doses of the two substances, or in the intervals of time separating their administration: in short, that there is a region of death as well as a region of recovery in connection with probably every instance of lethal antagonism. Unless, therefore, the factors I have mentioned be greatly varied in a large series of experiments, it cannot be positively asserted that the antagonism does not exist. It appears to me that the fallacy to which I have now drawn your attention, has not been sufficiently attended to in much that has recently been written on the subject of antagonism.

Bearing of Antagonism between Active Substances on Therapeutics.—

An eminent authority in pharmacology has recently published the statement, that the only method by which the injurious action of a poison can be made to terminate is by the employment of such means as will cause or hasten the elimination of the poison. This statement, fortunately, does not accurately describe our remedial resources. The existence of so undoubted an example of physiological antagonism as that which I have brought before you shows that the toxic action of a morbid agent may be directly opposed by the physiological action of an antidote or remedy; and, therefore, that recovery may be produced not only by removing the cause of the abnormal conditions, but likewise by directly influencing these abnormal conditions themselves in such a manner as to cause their return to a normal state.

It does not seem, however, that, in order to effect this return, the dose of the remedy must necessarily be increased in proportion to that of the morbid agent. This general principle has hitherto been somewhat vaguely recognised as a guide for treatment. The greater the severity of the symptoms, the greater the need for administering the antidote in large doses. When it is remembered that the action of poisons—whether these be the known substances with which toxicology is concerned, or those unknown substances on which the symptoms of many diseases are dependent—is rarely a simple one, but a series of independent actions directly involving many structures, and that the action of the antidote or remedy is in like manner the aggregate of several independent influences, we at once see how improbable it is that each of these several actions should be mutually antagonistic. In the case of the antagonism between atropia and physostigma, only a limited number of the different actions produced by each substance are of an opposite, and therefore counteracting kind; while others of these actions—either of a similar or of a different nature—are not mutually counteracting. Successful antagonism occurs when the doses are so proportioned that the non-counteracting actions are not permitted to acquire an undue prominence. When, however, they are permitted to acquire this prominence, death, and not recovery, occurs; and this result may be induced by an increase, beyond certain proportional limits, of the dose either of atropia or of physostigma. When the dose of physostigma is a large one, therefore, we find that a comparatively small and not a large dose of atropia is the proper one to administer; and, when the dose of atropia is a large one, we find that it can successfully antagonise only a small, and not a large, dose of physostigma.

I cannot avoid thinking that, were our knowledge of the conditions produced by disease as accurate as that of the conditions produced by many active substances, it would, for similar reasons, be found that a remedy which exerts so perfect a counteraction to a disease as to be able to prevent its fatal effect, would aid, and not prevent, the lethal action, when given in a somewhat large dose, even when this dose is considerably below the minimum-lethal. Just as we have seen that the actions of atropia which are not employed in counteracting those of physostigma, may increase the fatal power of a dose of the latter substance to such an extent that death occurs, even when the dose of neither substance is of itself sufficient to cause death.

The occurrence of this anomalous result is well worthy of consideration for another reason. The symptoms that are produced by a dose of physostigma *slightly* below the minimum-lethal are of so serious a character, that it is impossible to predict from their evidence alone whether death or recovery will occur. This can only be done by previously defining the minimum-lethal dose; and, unless this precaution be taken, the greatest errors may arise in judging of the effects of antidotes. Do we not find an analogy between this cause of error and that which frequently characterises the inductions of therapeutists? A disease that produces symptoms of the most serious import, does not necessarily terminate in death, even although this termination be a frequent one. The *dose of disease* present may not be so large as a minimum-lethal one, and still the symptoms may be sufficiently urgent to induce us to consider that they are caused by such a dose. If a remedy be applied in these circumstances—and, in the present state of our knowledge, they are probably always present—what surety can there be that the remedy has cured the disease? or that any remedy which may have been employed is not an efficient counteragent to its fatal effects? or even that the so-called *vis medicatrix nature* is not alone sufficient to counteract its lethal action? In presence of these uncertainties in reference to the exact degree of diseased action which is necessary to produce death—the exact dose of the disease that constitutes the minimum-lethal—there is little cause for wonder that scepticism regarding the power of remedies should exist, or that the unfortunate irrationalism of an indiscriminate expectancy should be revived as a therapeutic dogma.

I venture to think, however, that even the few facts which I have this evening brought before you are sufficient to show that a series of abnormal actions which, if unchecked, would inevitably terminate in death, may be so modified by an antidote or remedy, that the tendency to death is averted, and recovery produced. The existence of such an antagonism as that between atropia and physostigma encourages the hope that the power of directly counteracting disease is far from unattainable; and it supplies a strong incentive to efforts designed to determine the conditions of disease and the actions of remedies with an exactitude sufficient to show how the remedial action may be applied as a counteracting influence to the diseased condition.

REMARKS ON THE PROGRESS OF HELMINTHOLOGY, AND ON DR. HAUSSMANN'S OBSERVATIONS RESPECTING TÆNIA IN INFANTS.

By T. SPENCER COBBOLD, M.D., F.R.S.,
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ALTHOUGH I hope shortly to publish a course of lectures embodying some of the most important recent discoveries in helminthology, the progress of research is so rapid, that I crave indulgence for space sufficient to indicate the source and character of such of these scientific inquiries as may be supposed to have an especial interest for the profession.

Dr. Haussmann's observations in the JOURNAL for October 26th are extremely ingenious in the view of supplying a possible explanation of the alleged occurrence of tænia in the new-born infant; but I fear they are altogether inadequate. He makes it appear possible that the segments or proglottides of a tapeworm might actually pass from the maternal perineum into the mouth of the partially expelled infant; and then, after these portions of the worm have sojourned four days in the baby's intestinal canal, they might be finally discharged on the fifth day—as is supposed to have happened in Mr. Armour's case. Such a circuitous route for these cucurbitini appears to me impossible, when all the circumstances are duly weighed, without laying any stress upon the likelihood of their being partially digested in the infant's stomach. But it is possible that the notice, as recorded in the JOURNAL for August 17th, may, by reason of its brevity, have misled Dr. Haussmann. Here, therefore, I may mention that, through the kindness of Dr. W. S. Playfair of King's College, my attention was early called to the particulars of this case, as given in the *Medical Press and Circular* for February 14th of the present year. Since then I have had an opportunity of consulting the original communication itself; and, from a careful study of Mr. Samuel G. Armour's letter, it is quite clear that the facts are of a very different order from that implied by Dr. Haussmann's conception of them. Thus it is said that, commencing on the fifth day, the child "continued to pass segment after segment for five or six days"

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.