who would hesitate to concur in the opinion of a very high authority—Dr. Williams—that cod-liver oil is more beneficial in the treatment of pulmonary consumption than any other agent, medicinal, dietetic, or regiminal, that has yet been employed. We may justly congratulate ourselves on the possession of cod-liver oil; but it becomes us to remember that the ancient physicians used oil inunction in phthisis. This is expressly stated by Aretæus; and he, as well as Hippocrates, lauds the use of milk in the same disease, preferring it to all other kinds of food. "For", says the former, "milk is pleasant to take, is easy to drink, gives solid nourishment, and is more familiar than any other food from childhood. In colour it is pleasant to see; as a medicine, it seems to lubricate the windpipe, to clean as with a feather the bronchi, and to bring off phlegm, improve the breathing, and facilitate the discharges downwards. To ulcers, it is a sweet medicine, and milder than anything else. If one will, then, only drink plenty of milk, he will not require anything else. For it is a great thing that, in a disease, milk should serve both for medicine and nourishment." In fevers, Hippocrates did not allow milk, more particularly in such fevers as were attended by bilious discharges from the bowels. The modern practice is not, in this very important particular, in agreement with the ancient; the highest authorities, as, for example, Gairdner, Murchison, and Parkes, the last mentioned on weighty theoretical grounds, considering milk the best food in fevers.

We have been alluding to the manner in which the practice of medicine has been advanced, and may doubtless be still further advanced, by the simple method of observation and experience. It is true that we cannot entirely depend upon empirical laws. We cannot, for example, feel assured that quinine will certainly cure an attack of intermittent fever, mercury syphilis; or, for that matter, that a dose or doses of any given medicine will exert their thoroughly ascertained physiological or therapeutic action; still, such laws are of the highest value, and we cannot help employing them.

The recent progress of chemistry, physiology, and pathology, has naturally led to the establishment of an advanced school of therapeutics, from whose labours signal benefits may not only be anticipated for medicine, but have already been conferred upon it. To Dr. Lauder Brunton, for example, belongs the great merit of conceiving accurately the therapeutic action of the nitrite of amyl from its physiological properties, and thereby of adding an useful remedy to our armamentarium. And so, also, to Dr. Fraser we are indebted for the elaborate investigations regarding the Calabar bean, which have resulted in a demonstration of its therapeutic value. Care must, however, be taken that the results of scientific inquiry and those of patient, oftentimes laborious, observation in the field of practical medicine running counter, do not interfere with the ultimate grand object of our profession—the healing of the sick.

We are not entitled to withhold remedies because we do not understand their exact nature, nor the minute changes they produce in the animal economy. Cæsterlen has well expressed the attitude which, as practitioners of medicine, we are called upon to assume. "The patient requires our aid, and we must decide for or against the employment of a particular medicine, and upon the manner in which to employ it. If we possess sufficient experience, knowledge of the subject, and practical tact, we shall no doubt be able to do all for that patient which circumstances permit."

Let each one of us be fully persuaded in his own mind. While deeply interested in, and much instructed by, the experiments performed by a Committee of this Association, regarding the use of mercury, for example, I remain as thoroughly convinced as ever that the much abused drug in question exerts a powerful action on the function of the liver, and is to be trusted as a most efficient remedy in controlling not a few of its disorders.

I regard cold as a powerful antiphlogistic, and its external application, already briefly referred to, as a remedy of unquestionable value in the treatment of hyperpyrexia; but my own observation, and the fullest attention I have been able to give to the recorded observations of others, have convinced me that the real reason for the present abandonment of a remedy of superior power—to wit, bloodletting—does not lie alone in the advance of scientific pathology. "The thinking man", writes one of the most philosophical of living physicians, Dr. Stokes, "finds it hard to believe that the fathers of British Medicine were always in error, or that they were bad observers and mistaken practitioners. They, indeed, have rested from their labours, but their works remain; and he who reads the writings of Sydenham, of Haygarth, and of Fothergill; of Heberden and Fordyce, of Gregory, Cullen, Alison, Cheyne, or Graves; must have a very inapprehensive mind if he fail to discover that there were giants in those days; and that the advocacy of such ideas only indicates a state of mind not consonant with the modesty of science."

AN ADDRESS

DELIVERED AT THE OPENING OF

THE SECTION OF OBSTETRIC MEDICINE,

At the Annual Meeting of the British Medical Association,
in Edinburgh, August 1875.

By J. MATTHEWS DUNCAN, M.D.,
President of the Section.

CURSORY NOTES ON OVARIAN PATHOLOGY.

THE application of physical methods to the investigation of the diseases of the genital organs, brought to light, first, the most superficial or most easily observed of uterine lesions—the ulceration, as it was called, of the cervix; and it was inevitable that to this great importance should be attached, an importance far beyond what it really possesses. This importance has now, for at least a decade, been rapidly declining; and while it is admitted that the lesion is very significant, and forms an element of the most common disease of women, it is now well known that it has been not only exaggerated in importance, but misinterpreted as to its relation to the discharge; and that the treatment, by applying lunar caustic through the speculum to the visible ulceration, once so very widely used, is only rarely a good or successful method. Great improvements have recently been made in the theory and treatment of diseases of the neck of the womb; but we can only say that great and admirable attempts have been made to improve in a similar manner the theory and treatment of the analogous diseases of the body of the organ. Endometritis, even, is very inadequately described; and the attainment of this is, or should be, our first step to knowledge of disease of the uterine body. The influence of displacements and their relation to uterine congestion are not settled matters, after all the voluminous writing which has been zealously devoted to them.

Advancing knowledge of gynecology brings more and more into view the cardinal importance of the ovaries: organs which have, from the earliest times of true anatomy and physiology, naturally attracted a great share of medical attention, and whose extensive pathological influence in the minor ailments peculiar to women Tilt and many others have long tried to demonstrate.

It has been very generally supposed that the ovaries are the *fons et origo* of all the sexual functions, reliance being placed on such evidence as is afforded by the history of their growth and activity and atrophy, contemporaneously with the rise, progress, and decline of menstruation and of childbearing; on the results of disorganising disease of both of them; on the results of their accidental absence, or of their removal, as in Pott's well known case. I do not know that this view of the ovarian influence has been entertained in recent times by professed physiologists; but, at any rate, it has extensively prevailed among practitioners, and has only lately been rudely shaken by the observations of ovariologists as to the continuance of menstruation and the occasional persistence of sexual appetite, or its increase even after complete extirpation of both ovaries. I think that of this occasional result of double ovariectomies there can be no doubt, although care must be taken that, in this human vivisectional experiment, all the conditions are truly fulfilled, the whole of both ovaries being removed, and not merely portions of them, or, instead of them, mere parovarian cysts. In antithesis to this argument from removal of the ovaries, we have evidence to the same effect from their occasional functional activity, as observed by Grohe in the newly born, without any evidence of sexual appetite or possibility of sexual potency.

This dethronement of the ovaries from the supreme position in all departments of sexual activity is, however, as yet, not complete; and there is, no doubt, much truth in the view described as being held by practitioners. The rather vague results of the experimental investigations of Eckhard, Obernier, Spiegelberg, and others, have been greatly added to by the further inquiries of many, including Oser and Schlesinger, Goltz and Freusberg. The researches of the two last-named seem to show that there is in the lumbar spine a nervous centre for the sexual function. The phenomena of erection of the penis and seminal emission are so easily made the basis of this experimental investigation that, as a matter of course, males have been used for it, but it is almost certain that the physiological results are true of the female also. These investigations afford, in various ways, possible explanations of the persistence of sexual appetite and activity after the destruction or removal of the organs in which undoubtedly commences the production of those germs for whose growth and development the whole of the complex generative function is arranged.

These physiological questions, purely scientific or merely theoretical though they may appear, are far from really being so; for already operations have been many times performed whose justification lies in the belief that the removal of the ovaries is the annihilation of all or some of the sexual activities. I allude to spaying, or what is called normal ovariectomy by Battey, who, as well as Thomas and Sabine, have done it with what is perhaps justly called success. The proper attitude in regard to it is to defer judgment till more is known about its theory and practice. The diseases against which its performance is recommended, such as nymphomania, menorrhagia, dysmenorrhoea, are not so dangerous to life as the great ovarian cystoma for which the very dangerous ovariectomy is now an operation everywhere admitted to be at least theoretically justifiable. Spaying must be able to produce better statistics of success than ovariectomy—indeed, a nearly uniform success—to be theoretically equally justifiable, for the diseases against which it is used are not nearly so dangerous as ovarian cystoma.

It will be observed that I introduce with care the words "theoretically" and "practically" when I speak of an operation being justifiable; and it is well worth while to point out the difference between being theoretically justifiable and being practically justifiable; and I am particularly interested in this important distinction, because I have been gravely misrepresented by one or two excellent professional brethren as having changed my mind as to ovariectomy. I have made no such change. It is they who have not properly appreciated the difference between theoretically justifiable and practically justifiable: a difference which explains any apparent, but not real, change in my opinion. My remarks will apply to spaying as well as to ovariectomy; but, to save time, I shall refer to ovariectomy only, which has a history convenient for the purpose.

Ovariectomy is theoretically justified, because the possible success of the operation far outweighs the danger of it and the dangers of the disease combined. But theoretical justification is not a sufficient sanction of its performance in any particular case: is not a practical justification. For that you must have other conditions satisfied, and an operator who may be presumed to have the needful surgical skill, or who has demonstrated that he has it by the statistics of his success. When I opposed the operation, I was, as I am now, living in the midst of surgical ability, and I knew of many ovariectomies; but they were, with scarcely an exception, failures, and failures often under the most terrible circumstances; disaster after disaster. The only justifiable practical cry was that of Syme, Miller, Spence, and myself, for mercy on the poor sufferers prematurely deprived of life. The operation was then and there practically unjustifiable. Sufficient trials had been made, and there was no success, or almost none. Who could justly do anything else than oppose ovariectomy there and then? There are at present, even after Spencer Wells has for a long time flourished, many districts, even countries, where ovariectomy, now happily justified theoretically by the whole world, is still practically unjustifiable, and accordingly little, if at all, resorted to. In such districts or countries, the good physician must say to his patient: "Go to some great ovariectomist. If you can't do that, stay at home and die. The operation is not practically justifiable, or not practically justified by success, here." Fortunately for Edinburgh, there appeared among us a true surgical genius. Long before Thomas Keith had achieved his unparalleled success, he had done enough to make ovariectomy here and now not merely theoretically, but also practically justified. Indeed, since he took up the scalpel, we have never heard a word among us against ovariectomy; and as this silence is logically proper, so was the opposition not only logical, but urgently called for.

I hope I have said enough to show the difference between the theoretical and practical justification of an operation; and I may be allowed to add a word as to the rarity of real surgical talent. I do so with the conviction that there are plenty of young surgeons in whom it may be cultivated; and that, in order to its wider development, encouragement, and ripening, we need not merely invaluable theory and systematic teaching, but also invaluable practical teaching, on the basis of the deficiency of present theory and the still persisting necessary distinction between theory and practice. It is very sad to have to say that operations have in practice to be condemned which are theoretically justifiable and supremely desirable. But it may come to pass that spaying gets recognition as an operation justified both theoretically and practically. It is not so yet.

We may anticipate the time when surgical inquiries shall have been advanced so far as to reveal the conditions essential for success in operations such as ovariectomy. At present, they are not known. We know that one operator has a great amount of success, while another has not; and to a very great extent we do not know the cause of the difference between the two. There is no reason to think that the cause of this difference between operators is beyond our powers of dis-

covery; and it is rational and natural to suppose that, when the discovery is made, it will enhance even the success of those previously most successful. Past attempts to discover this cause of difference have been limited to matters of form of clamps, or kind of ligatures, or dealing with the pedicle, or amount of spicing, or method of draining, or mode of after-treatment, and these are all truly important; but the causes of the differences are as yet not demonstrated. When they are so, then great operations will be rendered practically justifiable over a much wider field than at present. Yet, after all, there will never, within reasonable limits of time, be such progress as to reduce surgeons to a dead level. There will always be room for genius and for zealous care.

The subject of ovaritis, acute and chronic, has recently, through the labours of Rokitsansky and Slavjansky, made some progress; the former describing follicular and parenchymatous inflammation; the latter, parenchymatous and interstitial: a nomenclature which would seem to indicate a greater difference in their views than really exists. But, unfortunately, we have no means as yet of using this knowledge in practice. Our signs and symptoms have to be immensely improved before such a result can be attained. Rindfleisch, indeed, from the pathologist's point of view, and Churchill from the practitioner's, have well pointed out that we cannot tell where ovarian irritation, as it is called, ends, and inflammation begins. But, although this is true, we have now the authority of most of those who have devoted themselves to gynecology in this country for saying that we can, in a very great majority of cases of ovaritis, and especially of chronic ovaritis, and still more especially when adhesions are absent, diagnose, by bimanual examination and otherwise, the existence of the disease, with a degree of assurance that, at one time at least, Scanzoni was disposed to dispute. This diagnosis is a matter of very great moment to the practitioner, not only on its own account, for that is a matter of course, but also on account of the value of its discovery as a contraindication—to which Nélaton is said to have attached importance—of various other gynecological operations. There are few kinds of cases more frequent occasions for correction of treatment than those that occur under this category. How often is a pessary intolerable because an ovaritis has been neglected! How often is an uterine catarrh inveterate, or a displacement supposed to be painful, or a metritis supposed to be peculiar, when there is an explanation in the neglect of an ovaritis!

Symptoms and physical examination, especially the bimanual, are our reliance for diagnostic purposes; and, in the bimanual examination, a chief point is the size and consistence of the organ. An enlarged, and consequently a heavy and probably a descended ovary, can generally be felt; while a healthy one, or an ovary merely irritated, may escape the most expert bimanual examiner. The size and consistence of the organ were chief points in Boivin and Dugès's practical classification of kinds of ovaritis, and none better has been proposed. But size and consistence, although often well made out, always escape the examiner in two very important classes of cases: those where the ovary is surrounded by adhesions, and those where it has undergone cirrhotic atrophy. After all, I have no doubt that our improved modern knowledge of ovaritis is one of the greatest steps of progress of gynecology; and it is a blessed one, for the disease, although often obstinate and liable to return, is also often distinctly amenable to treatment. Further aid in diagnosis is offered to us by the rectal examination pushed even as far as Simon has recently recommended, or the examination *per vesicam* recently recommended and practised by Noeggerath. But rectal and vesical examination in such cases are not often required.

One of the most striking advances in gynecology in my day has been in our knowledge of hæmatocele, which I think is generally called by the name uterine to distinguish its origin. But it is well known that some eminent pathologists, and among them the great Virchow, bring in new inflammatory productions in the pelvis as the source of the bleeding; and, as no disease leads more frequently to such developments than ovaritis, so we find Klebs connecting this great disease with ovarian inflammation. The matter is of great importance with a view to the proper appreciation of such cases, and consequently their proper management. Upon the question I shall not further enter here, merely remarking that I cannot agree with Klebs in, as a rule, connecting retrouterine hæmatocele with any kind of ovarian disease. I speak of the great majority of cases; for I have myself seen in life, and dissected after death, cases in which disease, especially cystic degeneration, of the ovary was the cause of large pelvic hæmatocele. I believe that the majority of hæmatoceles are uterine, and not directly connected with ovarian pathology.

There is, alas, far more of darkness than of light in the field of gynecology, and I take this opportunity of stating a conviction I entertain which is founded on considerable experience. We all know how

far from uncommon are those simple follicular dropsies of the ovary, seeing them in autopsies as very thin-walled cysts, often less than an orange. Such cysts are, I am sure, the source of much difficulty in practice, and a retrospective diagnosis of them may be formed in this way. A distinct ovarian swelling is found larger than any ovaritis ever grows, clearly diagnosable as an enlarged ovary; but the diagnosis cannot be made more exact. The commencement of an ovarian dropsy is dreaded. After a time, the tumour disappears. Frequently its disappearance is accompanied by adhesive perimetritis. Now, what has happened in these cases? Of course, it may be said that it is a case of mere ignorance, or that the cysts were parovarian; but, to this latter explanation, there is, for me, the great objection that the cases occur more frequently by far than to admit of their being justly so explained. We must suppose, therefore, the bursting of the not infrequent small follicular dropsies. I would further add that the bursting of simple parovarian cysts does not appear to me to be followed by perimetritis, or accompanied by it, so generally in the case of the disease of which I have been speaking.

In connection with parovarian cysts, a great step of good progress in practice has been made, and progress also in pathology. Such cysts used generally to be described as never reaching a great size, but this is not true. They do certainly attain enormous dimensions, so as to be in competition in this respect with true ovarian cystic disease. After death, or when they are removed by gastrotomy, they may, at least frequently, be easily distinguished by characters which Bantock has recently described; chief among them are the non-implication of the ovary, the almost invariable unilocularity (not surgical, but pathological or absolute unilocularity), and the easy separation of the peritoneum from the internal coat of the cyst-wall. During life, they are also, at least occasionally, diagnosable by examination of the fluid withdrawn by tapping from examples of them which have never been inflamed. Then we find the fluid to be limpid or very slightly opaline, of very low specific gravity, containing little or no albumen, and having in solution only a little saline matter, chiefly chloride of potassium and of sodium. Such cysts have been long known and partially described. Even yet they are imperfectly described; but it is interesting to note a kind of identification of them in those ovarian cysts which Boinet found to be most readily cured by the once famous iodine injection. Only the important fact is now known that many at least of such cases are far better cured by mere tapping than by that combined with iodine-injection; and the practitioner should always keep in mind these propitious circumstances when he meets with cases of this kind. The subject of parovarian cysts, when fully made out, will be still further interesting and fruitful in gynecology; for already we may presume that the numerous kinds and examples of cures of ovarian dropsy, without resort to ovariectomy, which have been described by many, including the highest authorities, were merely examples of delusion arising from imperfect knowledge. We know no one example of the cure, otherwise than by the operation of Ephraim McDowell, of an ovarian dropsy properly so-called; not one, however many may be found described, or whoever may be the describer. Cures by one or more tapplings, cures by medicines, cures by spontaneous rupture, cures by advancing pregnancy, have been, if not mere egregious mistakes, almost certainly cures of parovarian cysts whose history, as already known, quite accords with and explains such erroneous allegations. These remarks are based chiefly on observations of parovarian cysts that have never been inflamed. The study of inflamed cysts is still in a very backward state. Their pathology and treatment are matters of great difficulty. It must also be kept in mind that simple inflammatory serous collections take place in the peritoneum, and may be mistaken for parovarian cysts.

Ovarian cystoma is the great disease of the ovaries, and improved ovariectomy is the greatest recent step in therapeutics. It is probably to the inspiring influence of the beneficence of this operation that we owe the great amount of scientific exertion that is being made in the field of ovarian anatomy, physiology, and pathology; and already the student may catch glimpses of justly expected practical fruits of this scientific zeal which will enormously extend the utility of the practitioner.

The anatomy and physiology of the ovary, closely connected as they are, have recently received many additions, and more may still be expected from the application to them of the newest methods of investigation. First among these researches comes the histological anatomy of the organ, and here we find the most recent views of Waldeyer receiving, besides verification, addition also and correction by the excellent work of Foulis. Like Waldeyer, this author, beginning with the normal ovary, pushes his studies into the more difficult and more vexed inquiries as to the origin and growth of the multiple ovarian cyst. These points have by many, and frequently, been supposed to be settled; but it is a vain thought, as any one may find who tries to

unravel the divisions and subdivisions of recent pathological writers, and then peruses the most recent inquiries and setting forth of views by such authors as Malassez and Foulis.

To notice the whole of the work that is being carried on in connection with this department, is far beyond my present intention. I shall merely, in conclusion, make brief reference to the difficult questions in connection with the malignancy of ovarian cystoma. The uterus, although far from being an isolated organ, is generally regarded, among others by Walshe, as remarkable for the slowness or rarity of the spreading of cancerous disease from it as a centre, the slowness or rarity of what is now called the infection of neighbouring or distant parts. There is, on the other hand, no organ in the body which is actually more isolated than the ovary, whether in health or disease, at least so long as it has no adhesions; yet many pathologists, especially Klebs, who cites various authors, have attracted attention to cases of apparent infection, especially of the peritoneum, having malignant ovarian cystoma for their source. The diagnosis, especially in the earlier stages of growth, of even large malignant ovarian cystomata, is often a matter of great difficulty, and the examination of the fluids drawn off by tapping is anxiously made with this view, but, as yet, without completely satisfactory results. In fact, we have, in all departments of ovarian diagnosis, more to admire in the zeal and diligence of histologists, in regard to the fluids, than in the exactness and reliability of the practical results they can show. In connection with burst and unburst ovarian cystoma, it is well known that chronic peritonitis of great extent is often set up and long maintained; but the examination of the fluids has led Foulis to the belief that in the bursting of malignant ovarian cysts we have a new kind of evil, an explanation of the infection of other parts, and especially of the peritoneum, with malignant disease. While the diffuse chronic peritonitis is explained, to some extent, by a rude induction of analogies, the diffusion of cancer is less understood. Foulis believes he can identify in the fluids of certain ovarian cysts a peculiar richly proliferating epithelial cell which he regards as the seed sown broad-cast, when a malignant cyst bursts, over the peritoneum, finding in any part of this large surface a suitable soil for its further development into cancerous nodules, such as are seen in the ordinary malignant peritonitis, as it is sometimes called. To the discussion of this subject, Thornton and Lawson Tait have contributed, and we hope that, as a final result, the truth will be elicited. We shall then have ovariectomists instructed and guided as to the special urgency and claims of a class of cysts that may be equally important and urgent with those which are in a condition of suppuration.

I might still further enlarge on the present state of ovarian pathology, had I not already overpassed the proper limit of time. Many diseases and many researches I have not even hinted at, and the silence is not to be attributed to neglect or to an estimate of inferior importance.

In opening the section with these remarks, I congratulate you on the promise of interest that is offered by the list of papers to be read, and I can assure you that everything which contributes to increase, improvement, or illustration of our knowledge, will be received with the respectful attention which it ought to command.

AN ADDRESS

DELIVERED AT THE OPENING OF

THE SECTION OF PUBLIC MEDICINE.

*At the Annual Meeting of the British Medical Association,
in Edinburgh, August 1875.*

BY THE RIGHT HON. LYON PLAYFAIR, M.P., C.B., F.R.S.,
President of the Section.

I HAVE recently delivered so many speeches and addresses on public health that you will not expect me to do more than give some practical remarks on the recent efforts of the legislature to improve the health of the people. The present session of Parliament has had under consideration several important sanitary measures. Modern sanitary legislation dates from the year 1846, when Parliament carried out many of the recommendations of the Health of Towns Commission by a large and comprehensive Act to promote the health of communities. Since that year, Act after Act has been supplemented on the main one, as the necessities for increased powers arose. And, at last, sanitary laws became so cumbrous and complicated that neither experts in law, nor experts in hygiene, could master the accumulated legislation on public health. A Royal Commission sat a few years ago, under the presidency of Sir Charles Adderley, and recommended a consolidation of sanitary law. The president of that Commission prepared, not without much labour, a Consolidating Bill, upon which the present Government