fitful, little circumstances causing it to be greatly diminished; for a few hours, and then suddenly increased. If the uric acid is directly formed in the renal cells, it is natural to suppose that much of it would escape the action of the hippurates contained in the fluid portion of the urine, and especially if that fluid is in an acid condition; and experiments seem to confirm this idea.

I will here give you the results which I have obtained.

First Set.

The average of four days' urine passed at 12 noon; breakfast at 9 A.M.; specific gravity 1015; diet normal.

5 fluid ounces gave of uric acid 0.63 grains.

After benzoate had been taken 34 hours, and 95 grains taken in repeated doses.

5 fluid ounces gave of uric acid 0.126 grains.

After omitting benzoate 76 hours.

5 fluid ounces gave of uric acid 0.665 grains.

So that, in these experiments, the uric acid appeared to be much diminished by the influence of the benzoate.

Second Set.

Urine at 11 P.M.; dinner at 7 P.M.; no drug taken.

5 fluid ounces gave of uric acid 0.34 grains.

Urine at 11 P.M., after 55 grains of benzoate administered in divided doses.

5 fluid ounces gave of uric acid 0.03 grain.

So that, as far as these experiments go, the uric acid was reduced to one-tenth of its original amount by the action of the benzoate.

Third Set.

Benzoate of soda in 30-grain doses, three times a day. Urine from 11 A.M. to 11 P.M., after having taken benzoate for 12 hours.

Total quantity = 39 fluid ounces.

Uric acid in the 12 hours = 3.90 grains.

Urine before taking benzoate, from 11 A.M. to 11 P.M.

Quantity = 31 fluid ounces. Uric acid = 4.96 grains.

So that the quantity of uric acid was much diminished by the benzoate.

Fourth Set.

Benzoate of soda given in 20-grain doses three times a day. Urine passed from 11 A.M. to 2 P.M., after 60 grains had been taken. Quantity, 5½ fluid ounces; specific gravity 1020; evaporated to 5 fluid ounces.

Total uric acid = 0.17 grain.

Urine passed from 11 A.M. to 2 P.M., after 120 grains of benzoate had been taken. Quantity = 9½ fluid ounces; specific gravity 1016; evaporated to 5 fluid ounces.

Total uric acid = 0.57 grain.

When no benzoate had been taken for 27 hours. Urine from 11 A.M. to 2 P.M. Quantity = 8 fluid ounces; specific gravity 1018; evaporated to 5 fluid ounces.

Total uric acid = 1.00 grain.

When no benzoate had been taken for 51 hours. Urine from 11 A.M. to 2 P.M. Quantity = 12 fluid ounces; evaporated to 5 fluid ounces.

Total uric acid = 1.25 grains.

When no benzoate had been taken for 75 hours. Urine from 11 A.M. to 2 P.M. Quantity = 10 fluid ounces; evaporated to 5 fluid ounces.

Total uric acid = 1 grain.

In this last set of experiments, during the administration of the benzoate, the uric acid was diminished in a remarkable degree.

The results of the preceding observations made with the alkaline benzoates differ from those obtained by myself in 1842, and also from those of Kerner, in 1858, on which occasions benzoic acid itself was employed. Nor is it difficult to explain the discrepancy, for we know that benzoic acid, given by itself, increases the acidity of the urine, as is well shown when it is administered in cases of phosphatic urine; and, under these circumstances, it can possess little or no power of changing any urate it may meet with, in the renal cells or other parts of the urinary tract. I am still engaged in clinically investigating the value of the benzoates and hippurates, both in cases of gout and of gravel and calculus; and I hope ere long to bring the further results before the profession. I can confidently affirm that I have already obtained great advantage in the treatment of these diseases from their employment.

I frequently give the benzoic acid in the form of benzoate of sodium; but, if I wish at the same time to increase the quantity of the urinary excretion, then I give the benzoate of potassium or of lithium; and, if there be an abnormal acidity of the urine, some alkaline citrate.

Seeing that herbivorous animals excrete hippuric acid in a greater or lesser quantity according to the character of the food upon which they are fed, I cannot help thinking that some article of diet might be devised for those who suffer from the diseases above-mentioned, which might, at least to a great extent, keep in check the tendency to form and deposit uric acid.

THE GULSTONIAN LECTURES,

OR

THE STERILITY OF WOMEN.

Delivered at the Royal College of Physicians, February 1883.

By J. MATTHEWS DUNCAN, M.D., LL.D., ETC.,

Physician-Accoucheur and Lecturer on Midwifery at St. Bartholomew's Hospital.

LECTURE III., PART II.—ITS PREVENTION AND CURE.

THE regulation of desire and pleasure cannot be passed over without some remarks. Of the moral condition of those in whom these feelings are absent, or in whom they are in excess, I shall say nothing; and this silence is not because the moral condition is either unimportant or without influence on bodily health and on sterility, but because there is little that requires to be said. The healthy performance of the function of childbearing is surely connected with a well-regulated condition of desire and pleasure; and a well-regulated condition is not a reduction to a minimum or total absence, neither is it excess. I have already said that both desire and pleasure may be, and not rarely are, entirely absent; and it is my opinion, founded partly on the distinct testimony or concurrence of married women who are examples of the evil, that an education, injudiciously ascetic, as it may be called, sometimes produces this deficiency, which is a source of much disappointment and disaster in married life; and this view is corroborated by what is quite certain—namely, that by indulgence the feelings may be, and not rarely are, produced or increased. Writing on sterility, Ambrose Paré gives directions how to increase desire with a view to conception. Equally important is excess of desire and pleasure, and its reduction within moderate limits is equally advantageous. Religion, morals, bodily health, and childbearing all combine to exalt the value and importance of moderation, and to show the evils of absence or of excess. The influence of separation of married people, or of living without cohabitation for a long time—a period, say, of several months—is very widely recognised; and it is probably dependent on the increase of desire and pleasure in those who have little of either, and on the restoration of them in those who have been rendered nearly impotent by excess. This power of separation has appeared to me to be far more frequently operative in women who have had a family than in those who are absolutely sterile; and remarkable examples are not rare.

I have heard and read of, but have not personally witnessed, the disappearance of sterility after recovery from a fever; and this result is ascribed to the prolonged separation caused by the illness. The explanation may be correct, but it does not appear to be the natural one, for fevers are powerfully injurious to general health, and are known to disorder the ovarian and uterine functions.

I have already spoken of sterility as caused by marriage, especially in the young, and we know the sterility of prostitutes and the sterility of confined animals who couple freely or excessively; and it is probable that all these infertilities may have a bond of union in their being due to excessive desire and pleasure, or to excessive sexual indulgence, or to both combined.

In animals, especially in cows and mares, the semen is described as being not rarely expelled from the vagina soon after coitus; and this failure to retain is said to be, in some cases, owing to the animal not being duly in heat. Attempts are made to cause retention by dashing cold water over the buttocks and external parts. A like failure to retain the semen is frequently complained of by women, who describe it as coming away immediately after coitus, and without leaving the horizontal position, or only on getting up. In either case, women often attribute sterility to this failure of retention, and seek a cure of it with a view to fertility. Further, I have repeatedly been distinctly informed by careful women who habitually have this disagreeable imperfection, that conception has

followed the rare occasions on which they have, as they noticed at the time, retained the semen. That this non-retention is often only partial is made probable by the occurrence of pregnancies in women who describe themselves as invariably suffering from it. It is rarely complained of except by the sterile, and, I believe, it is rare among the fertile. I have also a very strong impression, which I have no data to corroborate statistically, that it is especially common among those sterile women who have not sexual pleasure. I know nothing that modifies this condition, but believe that the production of sexual pleasure may have favourable influence. It probably depends on the failure of the timely dilatation of the cervix uteri, and, perhaps, of the uterine openings of the tubes so as to admit the semen, and on the failure of the simultaneous production of a condition of increased temporary negative abdominal pressure, or of that adspiratory action of the abdomen, which numerous old and recent authors invoke to explain the mechanism of fecundation; or it may depend on the failure of both of these conditions of ordinary successful coitus. Before leaving the subject, I must add that the facts as to this profluvium seminis are not of the highest degree of security; for, so far, at least, as I am concerned, they are not more than the statements of intelligent wives. They are probably quite accurate, as they are certainly given in good faith; but it is possible that mucous discharges or glandular secretion through the ducts of Cowper or Duverney may be, in some cases, mistaken for semen.

The immoderately great consumption of alcoholic drinks by women, without their necessarily ever reaching the stage of drunkenness, is so common and so potent a cause of disorder and disease, that it requires special mention. It is possible that much of the influence of this drinking might be justly ranked as part of mere overfeeding, whose injurious effects we have already spoken of; but this is far from certain. Indeed, while I am unable to give any strong evidence of the specially injurious action of alcohol, considered as an article of diet, I am much disposed to this view, being led to it by the good results in practice which I believe justly attributable to desisting from the use of it. The instances on which I rely are cases in which I have, by physical examination and other modes of inquiry, been unable to discover any evidence of disease of the internal genital organs. It would not make the conclusion more assured to enumerate cases which are not in number or other circumstances sufficient for a demonstration; but I may mention the leading features of one which could not but strike the most careless observer. This patient was brought to me to be cured of sterility; and, as some prolonged treatment was expected, she proposed to reside near me for a time. She was between twenty and thirty years of age, and had been several years living in fruitless marriage, absolute sterility. On two occasions, with at least two years of interval, I declared my inability to do anything against the sterility by local means, because I could discover no disorder or disease of the womb or its appendages. Having some suspicion of too liberal use of alcoholic drinks, I recommended teetotalism. After the lapse of a few years, the patient, now a happy mother, was again brought to me on account of some trifling complaint, and I was told as follows. Her drinking habits having increased, she was induced to go into seclusion with rigid surveillance, and in this she lived for about a year without any kind of alcoholic drink. When she came home again, she had lost much flesh, but was in good health, and she maintained what were now teetotal habits. She immediately became pregnant, and pregnancy recurred. Such cases are not singular, and induce a belief in a special hostility of alcoholic drinking to fertility.

But alcoholic drinking has, in addition to the general or constitutional disorder which it produces, well ascertained power, in certain cases, to induce disease of the internal genital organs. That which is most easily and distinctly made out is chronic ovaritis. It often comes and goes in the presence or absence of the cause. When it is present, sterility is not always a result, but frequently so, and its cure is often followed by the disappearance of the sterility.

We have, lastly, to consider the power of various local and chiefly uterine diseases and disorders, which have too much engrossed the attention of the profession hitherto. As I have already remarked, there can be no rational doubt that these local affections have a very limited scope of action; are, indeed, quite subordinate to the great causes of sterility affecting populations or classes. That they should have been the chief study of practitioners, as distinguished from statesmen or medical officers of health, is not only natural but in a sense just; for the practitioner cares not for the population or the class, but for the individual. If he is to do any good to the individual, it is by discovering something amiss, and providing a

remedy, that he must work. And where is the practitioner first to look for a special cause of sterility if not in the essential organs of generation? Here he finds several diseases, only in recent years the subject of scientific investigation, so-called ulcerations, displacements, strictures, subinvolution, and others, upon which he easily founds a theory, generally a mechanical one, of the sterility which he at once proceeds to attempt to cure. If he fails to cure, that does not discourage him; for, in the present state of therapeutics, the reputation of remedies is founded more upon faith than upon evidence.

The wisest practitioner is he who, giving due weight to all items of knowledge acquired in regard to a disease or an unnatural condition, sets limits to his faith or his expectations, and scrutinises the evidence on which a treatment is based, and this all the more severely if a certain result of the treatment is gain to himself.

Spasmodic dysmenorrhœa is the most striking morbid condition connected with sterility. It has its seat in the womb or its neighbourhood, and it is by most gynecologists regarded as a purely local affection, having as its cause obstruction to the passage of the menstrual blood from the womb into the vagina by local or general congenital contraction of the canal of the cervix uteri. The nature of the affection, and the place it occupies in the theory of sterility, make me believe it to be a local affection in only a very limited sense—only in the same sense as irregular action of the heart or of the bronchi is a local affection. Its frequency, apart from numerous other considerations, is enough to make the pathologist hesitate to accept an alleged deformity of the cervix uteri as its cause. Besides, when the very rare alleged cause has really presented itself in rare cases of real pin-point os uteri, dysmenorrhœa has not been always present; in my practice, it has been always absent.

When evidence is led in favour of the obstruction theory of dysmenorrhœa, the argument from the success of treatment by enlargement of the passage is generally held to be irresistible, and its force to be, if that is possible, increased by the cure of sterility, which often accompanies the cure of the dysmenorrhœa, or, at least, follows the enlargement of the passage. The frequent success in curing or relieving dysmenorrhœa by this treatment, and the occasional success in curing sterility, are not matters of doubt. I have, indeed, no hesitation in saying that while many other pieces of advice are of great value in the treatment of the associated conditions of dysmenorrhœa and sterility, and in the treatment of them when not associated, this is the only medical interference that approaches in dignity to a cure. By this means, and chiefly by this alone, have cures such as concern us here been effected. In attestation of this utility we may cite the very great number of much vaunted means by which the object is effected, by very many kinds of knives, many dilators, many expanders, many scissors, by tents of various kinds, by bougies of various shapes, all of them, when put into use, producing enlargement of a part or of the whole of the passage through the cervix uteri.

For those who deny the existence of contraction it is not necessary to say a word further against the explanation of cure by mere enlargement. For them, that is certainly not the explanation. And it is easy to frame theories of the cure of dysmenorrhœa by enlargement of the passages, which may have the great superiority over that founded on obstruction, that they may also explain the cure of the associated sterility. Now, though the very simple cervical obstruction theory has been held sufficient to account for the sterility as well as for the dysmenorrhœa, it is plainly in this respect impotent.

While it is doubtful whether any menstrual blood is regularly passed through the internal extremities of the tubes into the uterus, it may justly be held sufficient by the dysmenorrhœal obstruction theorists to consider the passage of menses through the cervix alone. But they, of course, extend their theory of causation and cure to sterility, and here it is semen whose passage has to be studied, not menstrual fluid, and the cervix is not the only narrowed part of the semen's route, for it must pass not only through the cervix, but also through the Fallopian tubes. And if the seminal obstruction theorists find impediments in the cervix with its comparatively considerable dimensions, such as to allow their knives or scissors to work, what will they say of the closed capillary channel of the internal extremity of a tube? Their cure of sterility merely enlarges a passage where there was no apparent mechanical obstruction, and leaves untouched a passage where there is apparent entire impermeability.

That the obstruction theory (in all except its absolutely certain applications, as in imperforate hymen—cases to which we here make no reference) is excessively exaggerated, must be plain to everyone

who regards the almost innumerable cases of fecundation in extraordinary circumstances—cases without penetration; cases of impregnation in peculiar conditions, through the rectum or through the urethra; cases in advanced uterine cancer; cases in proidentia with great cervical hypertrophy; cases in extreme distortion by fibroids; and others. In this matter the appeal to comparative anatomy is most instructive, and the argument from it very evident. The apparent mechanical difficulties in the way of semen passing the cervix are, in some mammals, increased in an extreme and often a curious manner, without any consequent obstruction. To this matter Kehler and Lott have paid particular attention, and have shown that the apparent mechanical difficulties affect the construction of the male organ in its relation to the female passage as well as the female passage itself.

To me it appears theoretically reasonable to connect dysmenorrhœa and sterility with rigidity of the cervix, and the opinion that it is so is confirmed by its being actually discovered in most cases. Anyone familiar with the use of increasingly sized bougies in dilating the cervix must recognise the greater force required in dysmenorrhœal than in healthy women, and the increase of painfulness of the process as the force used, slight though it is, increases. The overcoming of this rigidity by temporary dilatation, not the overcoming of a mechanical obstruction, seems to me in some mysterious way to exert a generally beneficial influence on that part of the process of fecundation in which the uterus is implicated during insemination. For it may be held as almost certain that, during the natural sexual orgasm in coitus, the internal ends of the tubes, which we almost never see but as absolutely closed passages, are temporarily opened inside, and that the same happens to the cervix; and while it is probable that such wide opening of the cervix is not essential for fecundation, it must be held as facilitating it or rendering it more probable. Besides, this opening is an indication that the whole nervous arrangements, as well as the physical organs, are co-operating to produce the object in view. The opening here pointed out has, in its natural or healthy performance, and in the obstacle from rigidity, close analogy with similar processes going on during the premonitory and first stages of labour; and the dysmenorrhœal pains have analogy in the irregular painful and useless contractions and pains of these stages of labour, and of the hours immediately following delivery.

No other disease—local, or presumably local—has such importance in the theory of sterility as spasmodic dysmenorrhœa. This great place is established by the frequent association of the two conditions, and by the probable connection of the dysmenorrhœal neurosis with profluvium seminis, with disorder of sexual desire and pleasure, and with other derangements of the sexual orgasm of coitus. But dysmenorrhœa has its place confirmed in a unique way, for its cure is universally admitted to be a distinct and direct step towards the cure of sterility, and this can be said of no other local condition.

During recent times, no disease has more engaged the attention of gynecologists than the catarrh and peculiar changes of the cervix uteri connected with it, known generally by the name of ulceration of the neck of the womb. To it, even when in a very slight form, has been ascribed a very great pathological importance, and the Croonian Lectures of West seem to have had less effect in bringing the profession to a just judgment of its comparative insignificance than the overshadowing influence of some other temporary novelty. Among other evils which this very prevalent disease has been alleged to produce is sterility; but there is not a tittle of evidence that it has any special influence in preventing conception; and we have, for guidance as to this matter, our best help in the fact that conception and natural pregnancy are extremely common during its continuance. Among twenty-six cases observed by Grünewaldt, with a view to the study of the changes of the cervix uteri in the first month of pregnancy, he found only eleven with a quite healthy state of the cervical mucous membrane. Six had papillary and nine catarrhal ulceration, which no doubt existed before conception.

Almost identical statements may be truly made regarding versions and flexions, and I do not repeat them. But in this department of gynecology increase of knowledge not only tends to diminish importance, but to show that the great mass of versions and flexions are conditions of simple health.

The importance of those diseases which prevent the commencement of uterine pregnancy, or render such commencement improbable or difficult, needs only to be mentioned. To Grünewaldt we owe a careful statement of the extent and potency of this class of diseases, and for them he, as already said, vindicates a morbid superiority over those conditions which prevent conception.

The diseases and disorders of the genital organs, whether they act in preventing conception, in preventing uterine pregnancy, or in interfering with its natural healthy progress, are operative, in individual cases, and demand the most careful study of the practical physician, for it is chiefly through his power over them that he can hope to cure sterility. That in the early stages of the study of these diseases their influence should be exaggerated is natural. At all times there can be no doubt their study and treatment will be most important, not only on their own account, but with a view to the improvement of the general health. In the case of those local diseases which may be proved to have no special influence in diminishing fertility, their removal, by increasing the general health, will help towards a removal of sterility.

ABSTRACT OF LECTURES ON PHYSIOLOGICAL DISCOVERY.

Delivered at the Royal Institution of Great Britain.

By JOHN G. MCKENDRICK, M.D., LL.D., F.R.S.E., F.R.C.P.E.,
Professor of Physiology in the University of Glasgow, and Fullerian
Professor of Physiology, Royal Institution.

LECTURE III.—RESPIRATION: RELATION OF THE ORGANISM TO THE AIR BREATHED: EXTERNAL BREATHING.

AFTER giving a short account of the function of respiration, as at present understood, the lecturer pointed out the importance of recognising it as really consisting of two processes, namely, the respiratory exchanges of the organism, and the respiratory exchanges of the tissues. The older writers had no clear ideas as to even the necessity of respiration. Hippocrates dimly recognised that, during breathing, a *spiritus* was communicated to the body. Galen and his followers held that the air passed bodily into the lungs, and, reaching the left ventricle of the heart, cooled the blood, and generated *spiritus*. Similar opinions were expressed by Descartes (1596-1650). Van Helmont (1577-1664) observed how impure the air became during respiration, and hints that the gas sylvestre may be given off from the lungs. About the middle of the sixteenth century, Malpighi (1628-1694) investigated the structure of the lungs, and described the capillary plexus on the air-cells, long known afterwards as the *rete mirabile Malpighi*. Then came the celebrated physical experiments of Robert Boyle (1627-1691), in which he experimented on animals with the air-pump. Boyle was the first to show: (1) the true mechanism of inspiration, comparing the thorax to a bellows enclosing the lungs, so that, when the bellows expanded, the greater "spring" of the air on the inner surface of the lungs dilated them; (2) that "depuration" of the blood in the lungs is one of the principal uses of respiration; and (3) that fishes respired by the air dissolved in water. Boyle's simple explanation of inspiration was for long not accepted, and two errors prevailed, namely, that the lungs actively expanded, and that air existed in the pleural cavity (Willis, 1622-1675; Malpighi; and Erasmus Darwin, 1731-1802).

These notions were thoroughly exploded by Borelli (1608-1679), who, in his great work, *De Motu Animalium*, Props. 82 and 83, practically stated the views presently held. Borelli was the first (Prop. 81) to make an estimate of the amount of air in an inspiration. The lecturer referred to a work by Swammerdam (1637-1680), dated 1667, and entitled *Tractatus Physico-Anatomico-Medicus de Respiratione usque Pulmonum*, containing, at pp. 20-21, a description of an experiment in which he immersed in a vessel of water a dog having a long tube inserted in the trachea, and he observed the rise and fall of the level of the water during respiration. The lecturer referred to the experiments of Sanctorius (1561-1636), who detailed, in his work *Ars de Statica Medicinæ*, probably the first attempts to examine the phenomena of nutrition by a process of weighing. Sanctorius constructed a kind of balance, by which he weighed himself repeatedly, and observed what he gained by food, and what he lost by excretion.

The lecturer then described the celebrated experiment of Robert Hooke (1635-1703), performed before the Royal Society in October 1667, in which he kept a dog alive by artificial respiration; and showed that it was the fresh air, and not the movements of the chest, that renewed the contractions of the heart. This was a repetition of an experiment of Vesalius, detailed in his work published 1543.