3. *Reptiles*.—In so far as the eye can discover, or as chemical analysis has succeeded in making out, the urinary excretion of ophidian and saurian reptiles is identical with, or most closely resembles that of birds. I have carefully examined that of several pythons, boas, cobras, and the common English snake; also of various saurians, as the Australian monitor, etc. Urea is said to be absent, and, with one exception—viz., the common green snake—I have failed to find it. Whether it exists, in traces, in such excreta, as a rule, is at present a moot point. In the chelonian reptiles, the excretion differs from that of ophidians and saurians in that it is almost liquid, usually consisting of a clear watery fluid, containing opaque white flakes of urates. These latter vary much in quantity, depending, as I believe, on the length of time which has elapsed since food was last taken, these animals, as is well known, often fasting for a long period; they differ, also, in amount in different chelonians. In the land-tortoises, for example, I have found much more uric acid in the form of these white masses, than in the terrapins or water-tortoises, although the latter are carnivorous, whilst the former are herbivorous animals.

4 and 5.—*Batrachians and Fish*.—With regard to these classes of animals but little information has as yet been obtained.

6. *Invertebrata*.—Lastly, one word as to the nature of the urinary excretion in the invertebrates. With the exception of animals belonging to the class arachnida, as the scorpion and spider, which excrete guanine ($C_5H_5N_5O_3$), a substance found in guano and probably derived from uric acid, and closely related to it in composition, all the invertebrata throw out uric acid or urate of ammonium. It is from Dr. John Davy, whose name is associated with many physiological researches, that most of our knowledge of the excretion of invertebrate animals is derived. One interesting observation which he makes on the subject of the excretion of caterpillars and moths is this: "The urine of insects in their earlier or their larval state would appear to differ considerably from that of the same insects in their imago or perfect form. Thus, whilst, in the latter, it was found to consist chiefly of urate of ammonium, in the former, urate of ammonium was sparingly detected or not at all; what seemed to be hippuric acid being more abundant." We shall find during the course of these lectures that these facts are not without significance.

Having now given a short sketch of the character and composition of the urinary excretion of the different classes which compose the animal kingdom, we are naturally confronted by the question: Why this difference in the excretion of nitrogen; why in some animals does it chiefly take the form of urea, in others that of uric acid? The supporters of the first view have attempted many explanations; the favourite one being, that it depends on the greater or less activity of the function of respiration. This, I believe, originated with Liebig, and it is a view much insisted on by chemists, for it must be remembered that the uric acid, under the influence of oxidating agents, readily breaks up into urea and other products; thus, a slight oxidating cause splits it up into urea, allantoin and oxalic acid, all of which substances are found at times in the animal economy. Liebig pointed to the fact that mammals, having a high temperature and active respiratory function, throw out but little uric acid and a large quantity of urea; whereas reptiles, with a low temperature and a correspondingly low respiration, throw out their nitrogen chiefly as uric acid. This view was at first sight most plausible, but unfortunately it was founded on limited data; it was, in fact, a most partial view, for we have only to turn to the large class of birds for its refutation. Here we see animals with the highest temperature, and a respiration correspondingly active, eliminating their nitrogen in exactly the same form as the cold-blooded reptiles.

If other facts were wanting to show the error of this, at first sight captivating view, the recent experiments, set out in the *Comptes Rendus* of December 1881, would be sufficient. M. Cazeueneuve, in a paper termed "Sur l'Excrétion de l'Acide Urique chez les Oiseaux," describes how he kept a sparrow-hawk in a cage, and fed it on flesh, the bird throwing out a certain quantity of uric acid and urea in the twenty-four hours; he then put it under the influence of a large increase of oxygen, and even kept it for twelve hours in an atmosphere of pure gas. In no instance, however, was the quantity of uric acid diminished or the urea increased, but the ratio between the two remained the same as before. One can hardly help looking upon this fact as conclusive against the idea that urea and uric acid are excreted by animals in proportion to the activity of their respiratory function. Lastly, we may look at the excretion in invertebrate animals, whose temperature is high, while no urea is found in their nitrogenised excreta.

The nature of the food taken has been thought by many to have a powerful influence on the excretion of uric acid, but it needs very little consideration to show the inaccuracy of this idea; for ophidian reptiles, as the python and boa, which live exclusively on animal food; and grain-eating birds, such as the canary and others, whose food consists entirely of seeds; excrete the same nitrogenised products; in fact, it is difficult to separate the urinary excretion of the one class from that of the other, as I have already stated, both consisting of uric acid in combination with some ammoniacal compound. Again, if we compare a toad with a lizard, the little influence of the character of the food is at once strikingly shown; both animals live on flies, yet the urine of the toad is clear and watery, and contains no appreciable uric acid, whereas that of the lizard resembles cream, and consists mainly of urate of ammonium. The excretion of uric acid and of urea, also, is, doubtless, much influenced by the amount of food taken, whether in the same ratio has not yet been determined.

Some physiologists look upon the spleen as the producer of uric acid; others regard, some the liver, some the lymphatic glands, and some the cartilaginous tissues, as the originators of this principle; but there is one fact which must not be lost sight of, viz., that, whether an animal throws out all its nitrogen in the form of uric acid, or in that of urea, it, equally in each case, possesses a spleen, a liver, lymphatic glands, and cartilaginous tissues.

Of course, if we accept the second theory, and regard the kidneys as the producers of uric acid, the difficulty of the question is at once solved; we have only to regard the kidney as containing different cells; some, perhaps, for the formation of urea; some, at least, for the production of uric acid, and to hold that the number of the latter cells, compared with the other excreting cells of the kidney, differs in different classes of animals. The amount of respiration in any animal would then be of little moment with reference to the excretion of uric acid; nor would the nature of the food, provided only that the blood contained sufficient pabulum fitted for its supply. Possibly, however, many of us already see difficulties ahead, which have to be resolved before such a view can win acceptance.

[To be continued.]

THE GULSTONIAN LECTURES, ON THE STERILITY OF WOMEN.

Delivered at the Royal College of Physicians, February 1883.

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

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LECTURE II, PART II.—ITS THEORY OR CAUSATION.

FINALLY, Shorthouse has pointed out in mares the close alliance between sterility, abortion, and that kind of excessive fertility which is demonstrated by twinning. I quote the examples which he gives in the *Sporting Times* for December 12th, 1874; and, as adding to the force of the evidence, it is to be remembered that, in the mare, twinning is a far rarer event than in woman and the cow: in these it occurs about once in 80 pregnancies; in the mare it is said to be only once in 400:

Miserrima, barren in 1855, 1858, 1867, 1870, and 1871; slipped foal in 1856, 1859, and 1863; had dead twins in 1860 and 1862. Caricature, barren in 1852, 1854, 1855, 1861, 1867, and 1871; had twins in 1856 and 1863; slipped foal in 1866.

Legerdemain, barren in 1852, 1859, 1864, and 1866; slipped foal in 1849; slipped twins in 1856, 1860, and 1862.

Crystal, barren in 1858, 1860, and 1865; in 1866 slipped twins.

Slander, barren in 1851, 1854, 1864, 1865, and 1866; slipped twins in 1857.

Thimblorig, barren to two horses in 1867; slipped twins in 1869.

Zoe, barren in 1865, 1866, 1867, 1868, 1869, 1870, and 1871; slipped foal in 1860.

No. 1, barren in 1865 and 1868; slipped foal in 1867.

No. 5, barren in 1856, 1858, 1860, 1864, and 1866; slipped foals in 1862 and 1868.

No. 7, barren in 1857 and 1860; had twins in 1858.

No. 8, barren in 1867; had twins in 1861.

No. 9, barren in 1858, 1860, 1864, and 1867; had twins in 1868.

No. 10, barren in 1858, 1860, and 1864; had twins in 1861.

TABLE X.—Showing the Initial Fecundity of Women of Different Ages within the First Two Years of Marriage.

Ages of wives newly married...	15-19	20-24	25-29	30-34	35-39	40-44	45-49	50-54	55-59	60-64	65-69	Total.
Number of wives newly married ...	700	1835	1120	402	205	110	46	20	6	2	1	4447
Number of wives mothers bearing in 1855, and within two years of marriage ...	306	1661	849	253	84	17	2	...	...	...	...	3172
Proportion of latter to former is 1 in ...	2.3	1.1	1.3	1.5	2.4	6.4	23.0	...	...	...	...	1.4
Or percentage ...	43.71	90.51	75.80	62.93	40.97	15.45	4.35	...	...	...	...	71.33

No. 11, barren in 1856, 1863, and 1864; slipped foals in 1859 and 1865.

I know no observations worth quoting as to the special sterility of male lower animals, and the subject requires much further investigation. It is not quite a new subject, for it is popularly believed that certain stallions are often inefficient; and accordingly breeders, in their advertisements, take care to add to the other qualifications of a named horse that he is a "sure getter."

In woman, sterility varies in amount according to the age at marriage. This is shown by the table which I compiled from the data of Edinburgh and Glasgow in 1855. (See Table IX.) It is evident

TABLE IX.—Showing the Variations of Sterility according to the Age at Marriage.

Ages of wives at marriage ...	15-19	20-24	25-29	30-34	35-39	40-44	45-49	50-54	Total
Number of wives ...	700	1835	1120	402	205	110	46	29	4447
First children ...	649	1905	809	251	96	10	2	...	3722
Sterile wives ...	51	...	311	151	109	100	44	29	725
Percentage sterile ...	7.3	...	27.7	37.5	53.2	90.9	95.6	100.0	16.3
Proportion sterile: 1 in ...	13.72	...	3.60	2.66	1.88	1.10	1.05	1.00	6.13

that this table gives only an approach to the truth, for in its second column there is an excess of children over marriages that cannot have been. Incongruity of this kind is not only accounted for, but to be expected, from the manner in which the table is made up. The numbers of marriages in Edinburgh and Glasgow in 1855, at different ages of the wives, are compared with the numbers of first living children born in the same year to wives married at the same ages in that year or previously, and the number of sterile wives is got by subtracting the latter figures from the former. The comparison is of the first births of one year with the marriages of the same year, while they were mostly the result of the marriages of the former year; and the table is consequently imperfect. It must be remembered that this table, like the others from the same source, gives the title of first children to the firstborn living, excluding the dead from the reckoning—another manifest source of error. But there can be no doubt, I think, of the conclusion as to age which is derivable from it—that women married under twenty years of age have much more sterility than women married from twenty to twenty-four inclusive; and that the sterility of marriages before twenty is less than the sterility of marriages after twenty-four; and that, of marriages after twenty-four, the sterility increases with the age at marriage. A nearly similar conclusion is derivable from the *Statistics of Providence* published by Snow.

The relative sterility of women at different ages is in part shown by their slowness to become mothers, or the length of interval between marriage and childbearing; and this is found to tally with the sterility according to age, which I have just stated. I give another Edinburgh and Glasgow table, embodying the facts bearing on this. (See Table X.) Those married below twenty years of age were longer in married life before becoming mothers than those married between twenty and twenty-four, inclusive. These latter showed the highest fecundity and quickness to commence bearing children. Those, again, married after twenty-four, were slower than their predecessors; and the slowness increased with every additional quinquennial after that of twenty to twenty-four.

In the quinquennial preceding twenty, I can give for each single year the increasing delay of childbearing as age decreased. Table XI, from the Edinburgh and Glasgow data, shows this relative sterility of early ages.

At this point of the inquiry as to the influence of age, I interpolate an argument as to the influence of marriage or cohabitation in causing sterility. Although it seems, at first sight, absurd to rank marriage among the causes of sterility, yet the conclusion that it is so, at least in the very young, appears to be inevitable: for, if the women married under twenty are more sterile than those married at twenty to twenty-four, and are also more relatively sterile, so far as

delay of childbearing shows this quality, then, if the marriages of the very young—that is, of those under twenty—had been delayed

TABLE XI.—Showing the Initial Fecundity of Women under Twenty Years of Age within the First Two Years of Marriage.

Ages of wives newly married ...	16	17	18	19
Number of wives newly married ...	43	108	225	314
Number of wives mothers within two years of marriage ...	4	27	98	177
Proportion of latter to former is 1 in ...	10.7	4.0	2.3	1.8
Proportion after correction for immaturity is 1 in ...	7.7	3.3	2.1	1.7
Or percentage ...	12.90	30.00	46.44	57.94

till the next quinquennial, they would in greater numbers have shown fertility, and shown it also more quickly. Now, as the only difference known between those of twenty to twenty-four, and those younger, is age at marriage, we may reasonably conclude that premature marriage was the cause of the sterility. There may be some analogous injurious influence of too long delayed marriages upon the elderly, and the delay of commencing childbearing may point to it; but we cannot say of them, as we do of the youngest married, that, if they had still further delayed marriage, they would have had more chance of being mothers. Some further reference to this evil influence of marriage, and attempt at explanation, will be found in the discussion on sexual pleasure.

I might here adduce evidence of the influence of age which is found in the weight and length of the children produced, the length and weight rising with the age of the mother to its climax in the children born to mothers between the ages of twenty and twenty-nine inclusive, and then again falling as the age of the mother increases above twenty-nine. This is a matter tested by too delicate variations of length and weight to be, as yet, quite relied on; and great authorities have, indeed, contested its truth—Hecker, for instance, alleging that the measurements increase with the age of the mother in a direct sort of proportion. But I have Aristotle on my side. "Premature conjunctions," says he, "produce imperfect offspring, females rather than males, and these feeble in make and short in stature. That this happens in the human race," he adds, "as well as other animals, is visible in the puny inhabitants of countries where early marriages prevail." The general tenour of the evidence is, indeed, very strong in showing a concurrence of sterility, monstrosity, feebleness, and smallness; and, on that account, I still hold that diminished length and weight of children accompany the diminished fertility of the premature and post-mature women.

It is matter of regret that we can present no demonstration of the influence of age on fecundity founded on the frequency of abortions and of ill-formed children. But we approach near to such evidence, and may guess what it would yield, when we present the facts, scanty though they be, as to rearing of children and as to idiocy. Table XII,

TABLE XII.—Showing the Mortality of Children Born in Marriages formed at Different Ages.

Years elapsed since birth of first child.	Mortality per cent. of the children born to marriages formed at ages—			
	16-20	21-25	26-30	31-35
10	36.87	37.09	37.89	35.48
20	47.44	43.10	44.36	16.67
30	53.03	43.89	48.53	64.29
40	63.12	57.14	68.00	50.00

derived from the data obtained by the Statistical Society in St. George's-in-the-East, is the only body of facts as to the rearing of children born of mothers of different ages that I know of. It shows a diminished amount of rearing of children of the sterile ages. The sterility or weakness of reproduction by mothers of sixteen to twenty years of age is shown by the failures in rearing; and increased failures in rearing appear again as the sterile ages above twenty-five

are entered on, the failures to rear increasing with the age of the mothers, just as sterility increases at the same ages.

We suppose that, from the time of their birth, the children of these observations were tended with the same care, or desire of the mothers to act fairly by them; and that we must look to some cause of the failure to rear in the reproductive arrangements. Now, here we include the nourishment of the child among the reproductive processes; while in our other studies of sterility, we stop at its birth, or, if we proceed further, we consider only conditions presumably already established, or commenced at the time of birth, such as idiocy. The child is naturally fed upon its mother's milk, and the feeding is an extra-uterine continuation of the previously otherwise conducted nutrition of the foetus. Nursing is part of the reproductive process. The failure to rear may be a result of imperfection of the foetus, now a child, or it may be the result of the imperfection of the mother as a nurse. I know no method of disentangling the results of these two causes, but the potency of imperfect nursing is undoubted. It is an universally recognised rule, in the selection of wet nurses, that very young, or elderly mothers are to be avoided.

Imbeciles and idiots may be so from original or innate causes, sometimes called developmental, or from injury or other accidental causes. The undoubted frequency of accidents at birth, or other injuries, as causing imbecility and idiocy introduces an element which should be subtracted, with a view to the ascertainment of the influence of the mother's age in the production of the mental weakness; but, although in individual cases the two kinds, the developmental and accidental, may with much assurance be distinguished, I know no way of doing so in the statistics to be adduced. Authors on this subject, especially Little, attach great importance to the resuscitation of the stillborn as an accidental cause of idiocy, and it may be so; but I am disposed to attribute the necessity for resuscitation partly to the feebleness of the imbecile child produced. Among Langdon Down's 2,000 cases, 400, or 20 per cent., were born in a state of suspended animation, and 40 per cent. of these 400 were first children. At all events, it will not be disputed that the great majority of idiots and imbeciles are so from innate or developmental, not accidental, causes acting during or after birth.

Among Mitchell's 443 idiots and imbeciles, 138 were first-born; among Wilbar's 675 there were 191 first-born; among 100 of Beach's, 20; among 2,000 of Down's, 480. Or among 3,218, 829, or about 26 per cent., were first-born, and presumptively born of young mothers.

"Among 443 idiots and imbeciles consecutively examined," says Mitchell, "I found 138 first-born, or 31.1 per cent.; and 89 last-born, or 20.1 per cent. When it was known, however, that almost every sixth idiot in Scotland was illegitimate (663 idiots and imbeciles giving 108 illegitimate, or 17.1 per cent.), it was thought that an element of disturbance was probably thus introduced into the foregoing figures which might affect their value. The great majority of illegitimate children are known to be first-born and only children; while not a few of them are last-born, though the last of a small number of pregnancies—say of two or three. It was, therefore, thought desirable that a fresh series of observations should be made, excluding the illegitimate, and dealing only with those born in marriage. It was also thought well to confine these observations to those cases in which not more than one idiot occurred in a family, and in which the idiocy was noticed very soon after birth—that is, in which it was probably congenital. Further, no cases were accepted but those in which the mothers at the time of the inquiry had passed the age of childbearing, though some of them, I think, were widows before that age was reached. All these restrictions made it difficult to obtain a large series of observations, and

TABLE XIII (from Arthur Mitchell).—Showing the Comparative Frequency of Births of Idiots, and of all Births, in First and Subsequent Pregnancies.

Number of Pregnancy.	Percentage of all Births.	Percentage of Idiot Births.
First	22.8	33.0
Second	17.7	18.8
Third	15.5	17.6
Fourth	12.1	2.4
Fifth	9.4	2.4
Sixth	7.4	2.4
Seventh	5.2	7.0
Eighth	3.9	3.5
Ninth	2.6	2.4
Tenth	1.3	7.0
Eleventh	.9	3.5

account for their number not exceeding 85—44 males and 41 females.

I sent my results in detail to Dr. Matthews Duncan, who kindly drew up for me the two tables embodying the facts in a way which makes their teaching apparent. (See Table XIII.) This table is read in this way: Of all the children born in Edinburgh and Glasgow in 1855, 22.8 per cent. were first pregnancies; while of the 85 idiots, 33 per cent. were first pregnancies, and so on. What the table appears to teach is briefly this—that idiocy is more likely to occur among first and latest (seventh to eleventh) pregnancies than among others. This is substantially the same thing as was taught by the first inquiry, which included 443 cases, and in which all that was asked was whether the patient was first-born or last-born."

Similar evidence is derivable from the data given by Langdon Down, but in regard to them, we have not the same assurance of the circumstances of the collection as is given by Mitchell in regard to his. Down's data are given in Table XIV.

TABLE XIV (from Langdon Down).—Showing the Comparative Frequency of Births of Idiots, and of all Births, in First and Subsequent Pregnancies.

Number of Pregnancy.	Percentage of all Births.	Percentage of Idiot Births.
First	22.8	24
Second	17.7	14
Fourth	12.1	9
Fifth	9.4	5
Sixth	7.4	7
Seventh	5.2	10
Eighth	3.9	2
Ninth	2.6	9
Tenth	1.3	2
Eleventh	.9	2
Twelfth	.4	1
Thirteenth	.2	3
Fourteenth	.06	1

Fortunately, Mitchell gives the age of the mother at the time of the birth of the idiot, and the result is very striking. Down does not give the age of the mother in his collection, but, considering the excess of primiparity, and the very large proportional number of pregnancies of high figure among them, we can have no doubt they would yield a like result.

"The same eighty-five cases," continues Mitchell, "are used in Table XV which were used in Table XIII. This table is read thus:

TABLE XV (from Arthur Mitchell).—Showing a Comparative Percentage of the Children Born at Different Ages of Mothers to all Children Born, and of the Idiots Born at Different Ages of Mothers to all Idiots Born.

	20-24	25-29	30-34	35-39	40-44	45-49
Percentage of all children	22.82	30.09	23.61	14.76	5.15	0.58
Percentage of Idiots	25.88	25.88	10.58	10.58	23.53	3.53

Of all the children born in Edinburgh and Glasgow in 1855, 22.6 per cent. were born of mothers whose ages were from twenty to twenty-four years; while, of the eighty-five idiots, 25.8 per cent. were born of mothers of corresponding ages, and so on. What we learn from the table is this: that mothers under twenty-four years of age, and above thirty-five, are those more specially liable to have idiocy in their children."

Several times I have been told by men of experience that an old bitch often ends her career of breeding by a dead and premature pup. Whitehead regards those pregnancies which occur near the termination of the fruitful period in women as being the most commonly unsuccessful; and Arthur Mitchell has connected the occurrence of idiocy in a child with the circumstance of its being the last born of its mother. "That in the mother," he remarks, "which leads to the miscarriage may lead also to the idiocy, and the only connection may be one through a common cause. It frequently happens," he adds, "that between the birth of the idiot and that of the child which precedes or follows, an interval occurs which is much longer than usual, or that, after the birth of the idiot, permanent sterility appears." Again, when the idiot is born eighteen or twenty-four months after the preceding child, but when, for six or seven years thereafter, no impregnation occurs, he thought there was reason to suspect that the imperfection in reproductive power, which showed itself in the idiot, had merely another and fuller expression in the subsequent barrenness. And so also when permanent sterility follows. In many cases, indications of barrenness preceded the birth of the idiot, and became permanent thereafter.

We have alluded to prevalent opinions that the lastborn of a woman is specially liable to be a miscarriage, or a weak child, an idiot, and female rather than male, and have shown that these opinions have considerable support from facts. We have also spoken of the only-child sterility, the mothers being in Ansell's

collection at the high mean age of thirty-one. Now, in addition, there are some, though imperfect, evidences that such children, especially if female, are not merely illustrations of one-child fertility, or only-child sterility, but are also the last of their race. They represent a family's last effort at continuation of its line. Girls in such a position are often heiresses, though not certainly single children, and this circumstance has enabled Galton to follow up their history and to show their infertility. I know several remarkable cases of single children of this kind, feeble, rich, childless, the last of their race; but a collection of cases forms stronger evidence than any scattered good examples. Speaking of marriages of heiresses as peculiarly unprolific, Galton remarks: "We might, indeed, have expected that an heiress, who is the sole issue of a marriage, would not be so fertile as a woman who has many brothers and sisters. Comparative infertility (he adds) must be hereditary in the same way as other physical attributes, and I am assured it is so in the case of domestic animals." In addition to other strong evidence of the same kind, Galton found, in a partial search through the peerage, a total of fourteen heiress-marriages among seventy peers, resulting, he says, in eight instances of absolute sterility, and in two instances of only one son. "I tried the question from another side," he continues, "by taking the marriages of the last peers and comparing the numbers of the children when the mother was an heiress with those when she was not. I took precautions to exclude from the latter all cases where the mother was a co-heiress, or the father an only son. Also, since heiresses are not so very common, I sometimes went back two or three generations for an instance of an heiress marriage. In this way I took fifty cases of each. I give them below, having first doubled the actual results, in order to turn them into percentages:—

TABLE XVI (from Galton).—Showing the Infertility of Heiresses.

Number of Sons to each marriage.	One Hundred Marriages of each description.	
	Number of Cases in which the Mother was an Heiress.	Number of Cases in which the Mother was not an Heiress.
0	23	2
1	16	10
2	22	14
3	22	34
4	10	20
5	6	8
6	2	8
7	0	4
Above	0	0
—	100	100

"I find that among the wives of peers, 100 who are heiresses have 208 sons and 206 daughters, 100 who are not heiresses have 336 sons and 284 daughters. The latter shows how exceedingly precarious must be the line of a descent from an heiress. . . . One fifth of the heiresses have no male children at all; a full third have not more than one child; three-fifths have not more than two."

DONATIONS AND BEQUESTS.—"An Old Friend" has given £100 additional, to the Westminster Hospital.—Mr. T. A. Denny has given £100 to Miss Mary Wardell's Convalescent Home for Scarlet Fever.—"F. T. B." and "M. T. D." have each given £50 towards finishing the building of the Bexhill Seaside Branch of the Metropolitan Convalescent Institution.—Mr. George John Eyre, of Hove, bequeathed £200 each to the Eastbourne Convalescent Institution, the Earlswood Asylum for Idiots, the Hospital for Nervous Diseases, Paralysis, and Epilepsy, the Royal Hospital for Incurables, the Sussex County Hospital at Brighton, and the Brighton and Hove Dispensary.—The Lincoln County Hospital has received £100 under the will of Mr. Charles Doughty.—Mrs. Atkinson, widow of Mr. William Atkinson, has given £2,000 to University College Hospital, for the endowment of a bed in memory of her husband.—Mr. Joseph Shuttleworth bequeathed £1,000 to the Lincoln General Dispensary.—Mr. James Symes, of Winterborne-Abbas, bequeathed £100 to the Dorset County Hospital, Dorchester.—The Grocers' Company have given £100 to University College Hospital, and £100 to the North-Eastern Hospital for Children.—The Dowager Countess of Craven has given £50 to Miss Mary Wardell's Convalescent Home for Scarlet Fever.—Mr. F. A. Hamilton has given £50 towards the building fund of the Bexhill Seaside Branch of the Metropolitan Convalescent Institution.

LECTURES ON THE COMPARATIVE PHYSIOLOGY OF MENSTRUATION.

By ALFRED WILTSHIRE, M.D., F.R.C.P. LOND.,

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LECTURE II.

(Continued from page 448.)

RACIBORSKI regards woman and the quadrumana probably as the only females who have complete exfoliation of the mucous membrane of the uterus after each conception, and considers that the commencement of this important process is repeated at each ovulation. He says, also (p. 18), that he has seen in the Jardin des Plantes, at Paris, monkeys who were abundantly regular every month for five or six days.

Mr. Darwin incidentally refers to the existence of the periodical function in the females of the lower animals thus (*Animals and Plants under Domestication*, vol. I, pp. 33-4): "Several years ago I saw, confined in the Zoological Gardens of London, a female hybrid, from an English dog and jackal, which even in this, the first generation, was so sterile that, as I was assured by her keeper, she did not fully exhibit her proper periods," and again (*Ibid.*, vol. II, p. 132): "Elephants . . . both male and female, have their proper periodical seasons."

Fleming (*Veterinary Obstetrics*, p. 55) says: "The rutting, heat, œstrum, or venereal œstrum of animals is analogous to menstruation in woman, and marks the period of maturation in the ovarian ova or ovum, according to species. This condition is intermittent or periodic, not continuous; and is characterised by a peculiar systemic excitement, that usually continues for a somewhat definite period in the two sexes. In the male and female, but especially in the latter, the generative organs at this period become more or less turgid and sensitive, and the uro-genital secretions are increased. In the female, there is a determination of blood to the ovaries, and changes take place in these which have already been described. The excitation of the generative apparatus reacts on the whole system, and produces a kind of fever or irritability in the animal; its sensibility is increased; the appetite is more or less in abeyance, or capricious, and usually there is thirst. . . . Attempts at micturition in the female are frequent, but only a small quantity of urine is passed; the mucous membrane of the vagina is injected; and, with solipeds, there are oft-repeated movements of the clitoris and vulva, and an opaque white secretion, or even emissions of blood are ejected spasmodically, *per vulvam*." In a foot-note, Fleming says: "Kaiser, in the *Magazin für die Gesamte Thierheilkunde* for 1859, mentions a mare, twenty-four years of age, which, every three weeks, had a sanguineous emission from the vulva; this discharge ceased towards the middle of pregnancy, but returned after parturition. I have frequently witnessed the periodic discharge from mares either streaked with blood, or blood-tinted;" and then, resuming the text, "In other animals, this ejection sometimes consists of a viscid red-tinted or sanguinolent fluid. It has a special and powerful odour, which attracts the males. . . . The uterine mucous membrane is also very congested, and there is poured out on its surface a fluid containing epithelial *débris*, mucus, corpuscles, and blood-globules."

"The existence in the lower animals of what is analogous to the menstrual discharge in the human female, has frequently been denied, but without any reason or proof. A discharge of blood from the sexual organs of woman announces the advent of puberty; and its coincidence with the maturity and escape of the ovarian ovule, as well as its periodical appearance until the termination of fertility, establishes between this phenomenon and the heat or 'rut' (œstrum) of animals a very close analogy. Kahleis, Fuschs, Spinola, Numann, and others, have observed this in the cow, and have also noted that the discharge occurs regularly at intervals of nineteen or twenty days when the animal is not giving milk or in calf. The hæmorrhagic flow appears two or three days after the commencement of 'rutting,' and when this is most intense. The amount of blood does not exceed one or two ounces, and the coagulated clot