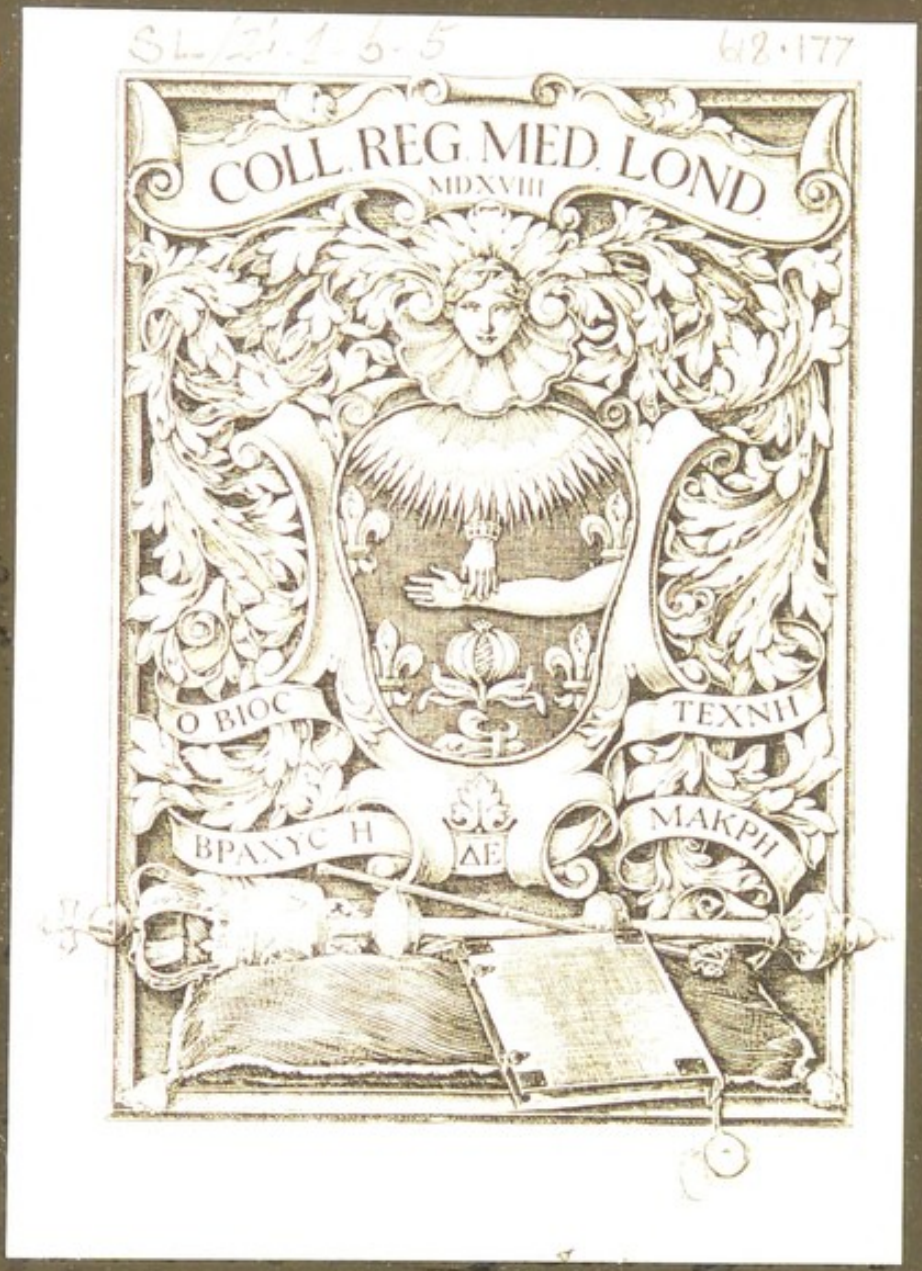
Clinical notes on sterility in women / by Graily Hewitt, M.D., F.R.C.P.



ON

STERILITY IN WOMAN

BY THE SAME AUTHOR.

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ON

STERILITY IN WOMAN

BEING THE

GULSTONIAN LECTURES

Delivered in the Royal College of Physicians in February, 1883

BY

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TO

THOMAS KEITH

ONCE MY SCHOOLFELLOW,

A

HERO OF SURGERY.

PREFACE.

THE following Lectures were prepared on short notice, and are now published as they were delivered.

They form a mere outline sketch of the parts of the subject of which they treat. Many of the paragraphs might, with advantage, be expanded into chapters, and the whole into a large volume.

Few subjects are more important or more interesting, none are more likely to attract general attention; and the author hopes that these considerations justify his devoting to sterility in woman those Gulstonian Lectures which he has had the honour of being asked to deliver.

As literary references do not form part of a lecture, they are not given here. But the author has taken care to provide, in scattered places, indications

sufficient to direct the reader to the chief literary sources of which he has availed himself.

He has pleasure in acknowledging assistance in the work from his friends, Dr. Champneys, Dr. W. Duncan Scott, and Mr. James Duncan Matthews.

71, Brook Street.

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LECTURE I.

NATURE AND AMOUNT.

STERILITY is generally considered to imply the condition of a woman who, under ordinary favourable circumstances for reproduction, does not bring forth a living and viable child. But the term is used with many other meanings, and I shall not state a definition, because I have no right or power to enforce adherence to it, and because, meantime, it is indispensable to have the word for various uses; and with the use of appropriate qualifying words ambiguity may be avoided.

Fecundity is a condition unique in gynæcology in this respect—namely, that it requires the combined matter and forces of two duly developed individuals to produce it. Sterility, therefore, may depend on error in one or in other, or in both.

The sterility of man, as compared with that of woman, is a simple matter. It depends on failure to produce semen, the production of semen more or less incomplete or imperfect, or of morbid semen (that is, semen conveying disease), or on failure to deposit the semen properly. With a view to investigation,

the semen can be subjected to chemical and microscopical analysis, and the depositing organ can be examined, and the conditions of deposition can be to a great extent ascertained. In woman the coördinate substances and functions are hidden and much more complex, and in her there are great organs and functions which have in the male no equivalent representative.

In the present lectures the sterility of man is not a point for consideration, but one point in it cannot be passed over without some discussion and estimation—namely, its numerical amount. Much of our knowledge of the sterility of women consists in numerical statements of amount under various circumstances, chiefly in marriages; and all such statements have a positive value for the physician, and still more for the political economist. But it is plain that, inquiring into the amount of sterility due not to unions but to women, we must exclude what is due to the male. Some notion of the amount of this latter sterility is therefore indispensable.

Several investigators have attempted the solution of the question in recent times; but I refer only to the new work of Gross on Male Sterility. "It is not at all uncommon (says he) for physicians to assume that a man who is potent, and who is able to ejaculate, is capable of procreating. As a result of the omission to examine the emitted fluid, and carefully to explore the male organs, little is known

of the relative frequency of sterility in the two sexes; and gynæcologists, with the exception of those mentioned below, do not appear to have made any contributions to the solution of this important subject. I have been able (he continues) to collect one hundred and ninety-two cases in which examination of both the husband and the wife demonstrated that the former was at fault in thirty-three, or in seventeen per cent. Of this number Manningham records one in thirty; Pajot seven in eighty; Mondot one in ten; Kehrer fourteen in forty; Courty one in ten; Noeggerath eight in fourteen; and I myself have found that the male was deficient in one example in eight. The cause of the sterility was azoospermism in thirty-one, and aspermatism in two. These facts show that the husband is at fault in about one case out of every six."

The matter is, however, still in a very insecure state, as may be shown by a statement of facts and considerations which must have important bearings on the question, but which have, so far as I know, been entirely neglected. Thus, it is assumed that, by examination of the male and female, we can decide whether one or other or both are at fault. Now, no doubt, impediments or complete barriers to reproductiveness may be found in individuals of either sex; but in the great majority of cases of sterility no impediment or barrier can be discovered by the most careful and minute investigation; and this is verified by comparative observations in animals and in plants,

wherein such inquiries can be carried to a completeness not attainable in the case of men and women. It is held that the man is not at fault if he duly ejaculates microscopically perfect semen, but this is certainly not a warranted conclusion, as facts in human and comparative physiology, to be hereinafter stated in these lectures, will plainly show. In making estimates of male sterility, no account is taken of the fact that the faulty condition of a man's semen may be only temporary. It is forgotten that sterility may be due to faults in the semen, even though conception has taken place and pregnancy been established; the fœtus fading and dying prematurely from inscrutable causes, or being monstrous and not viable, or perishing from disease implanted in it by the male. It is forgotten that both parents may be simultaneously at fault, and this with or without discoverable cause, generally without discoverable cause.

Speaking of the sterility induced by domestication and that of hybridity, Darwin remarks that in both the sterility occurs in various degrees, and in both the male element is most liable to be affected, but sometimes the female more than the male. In another place, speaking of the liability of plants to be affected in their fertility by slightly changed conditions, he says it is the more remarkable, as the pollen, when once in process of formation, is not easily injured; a plant, he adds, may be trans-

planted, or a branch with flower-buds be cut off and placed in water, and the pollen will be matured. Pollen also, when once mature, may be kept for weeks, or even months. The female organs are more sensitive, for Gärtner found that dicotyledonous plants, when carefully removed so that they did not in the least flag, could seldom be fertilized; this occurred even with potted plants if the roots had grown out of the hole at the bottom.

Whatever may be the causes of sterility in woman, there is a universally prevalent belief, which no investigations have shaken, that in the human species the paramount source of sterility is in the female. I know no scientific statement worthy of confidence as to the comparative influence of the two sexes. The data of Gross, which I have quoted, contributing as they do towards the settlement of this question, important and valuable in themselves, are yet far from substantiating the conclusion as to the amount of male sterility which he enunciates.

Of the sterility of women in whom, from gross and well-known causes, conception is impossible, these lectures take no account. Among such are cases of absence of uterus, and of imperforate vagina; conditions so rare that, in the present rough and imperfect state of our knowledge, they do not affect statements as to women generally.

In describing sterility it is common to qualify it as absolute or as relative. No author on human

sterility uses the term, without qualification, as including relative sterility. But when used without qualification it includes at least absolute sterility.

Absolute sterility, sometimes called congenital, including all cases where there is no child, no miscarriage, no abortion however early, comprises two sets; first, those where there is no conception, and, second, those where the impregnated ovum disappears in the tube or in the uterus without leading to what is recognisable as an early abortion. Some cases of women aborting every month are known; there is discharge of a highly developed decidua vera every four weeks, and there may be no trace of an ovum in it; and this monthly discharge is arrested by suspension of cohabitation: but there may be many abortions earlier than this without these indicating conditions, and of such practically nothing is known; they are classed along with those cases of absolute sterility where it is supposed that no conception takes place. In cases where there is no conception there may be no possibility of conception, from the failure of the ovary to prepare and mature an ovum. These varieties of absolute sterility are well illustrated and easily made out in the history of animals, and still more of plants.

Sterility, not absolute, implies the failure to produce a viable child, while there may be evidence of conception—that is, of the commencement of the production and growth of an embryo. A woman may be

sterile because the ovum perishes in utero, or becomes unnaturally developed, as in myxoma of the chorion and some monsters; and this premature death or unnatural production may be owing to ovuline imperfections derived from the male or from the female. A woman may be sterile because the womb does not afford to the ovum due accommodation, or nourishment, or neither; or because the womb ejects it prematurely from its cavity: and these unnatural conditions and events may arise from either local or constitutional causes.

In absolute sterility and in sterility not absolute there is no production of a viable child, no addition made to the population; and all such sterility is sometimes, especially by economists, considered absolute; for indeed, in the point of view of population, it is so. But it appears to me desirable to restrict the term absolute sterility to those cases where there is no evidence even of conception. Sterility indicates a larger group, including that of absolute sterility and all those other cases where no addition is made to the population.

There is another great department of sterility no less important than the kinds just mentioned, where a woman produces one or even several living children, but in number not according to her conditions of age and length of married life. This is called relative or acquired sterility. The gardener may have a plant, producing not a single flower, absolutely sterile; or

producing flowers, and setting seeds, but bringing none to maturity, or if to maturity not to perfection —a sterile plant which cannot continue its species; but he may also have a plant which produces flowers and matures perfect fruit, but in such small number as not to save it from the charge of sterility; and this is relative sterility. In woman it is often seen in cases of production of a single child—an only-child sterility, if such a seeming contradiction in terms can be permitted—of which we often hear. A woman may be relatively sterile from producing, according to her age, only a small number of children with ordinary intervals between successive births, or from the number being rendered small by the extraordinary delay or loss of time between successive births, and in other ways.

All kinds of sterility may be congenital or may be acquired. It is therefore improper to use these terms as indicative of distinctions. For instance, an absolutely sterile woman, one who never conceives, may be so not merely from congenital causes, but also from disease acquired in advanced life; or, again, a relatively sterile woman may be so, not from an acquired cause, but from conditions which were congenital in her.

The amount of sterility in women (excluding the relative kind) is found by counting the number of productive and of unproductive marriages of women

within the reproductive age, or from fifteen to forty-five. Lever, giving no numerical details, says that 5 per cent. of married women are wholly unprolific. West found the average of sterile marriages among his patients at St. Bartholomew's Hospital to be 1 in every 8·5. Hedin, a Swedish minister, noticed that in his parish of 800 souls one barren woman is not met with for ten fertile. Frank and Burdach roughly state that only one marriage in fifty is unproductive. Simpson made an inquiry into the sterility of married women in Grangemouth and Bathgate. Of 210 marriages in Grangemouth 182 had offspring; 27 had none; or about 1 marriage in 10 was without issue. Of the 27 unproductive marriages all the subjects had lived in wedlock upwards of five years, and in all the female had been married that period before she reached the age of forty-five. Of 402 marriages in Bathgate 365 had offspring; 37 had none; or about 1 marriage in 11 was unproductive. There were at the same time living in the village 122 relicts of marriages, and of these 102 were mothers; 20 were not mothers; or about 1 in 6 had no family. In all, of 467 wives and widows 410 had offspring; 57 had none; or about 1 marriage in 8 was unproductive. Of these last 57, 6 had not been five years married, and there were other 6 above the age of forty-five when married. If we subtract these 12 we have of 455 marriages 410 productive, 45 unproductive, or 1 in 10$\frac{1}{9}$ with-

out issue.* Simpson found that among 495 marriages of British peers which had lasted five years or more, and in which the husbands were under fifty-seven years of age, 81 were unproductive, or 1 in 6⅛. Ansell found that among 1,919 marriages of spinsters in the upper classes at an average age of twenty-five years, and not counting as childless those who had merely stillborn children, there were 152 without issue, or 8 per cent., or nearly 1 in 12. In this collection all the parents survived the childbearing age, and he considered that there was no further chance of childbearing if the female was

Over	48	and had had no child for		2	years.
„	47	„	„	3	„
„	46	„	„	4	„
„	45	„	„	6	„
„	44	„	„	8	„
Under	44	„	„	10	„

I have taken the registers of Edinburgh and Glasgow for 1855, and have found the number of first living children in that year. With this I compare the number of marriages in that year. It is evident that the number of first children only should be counted, for they indicate all the wives who are not sterile. If one living child is born to a marriage, that marriage is not sterile. Further, it is evident that, although the first births in 1855 will not all pertain to the women married in that year, it may be

* The figures are given from Simpson's paper, without attempt at correction.

assumed that if the marriages be nearly the same in number for a few contiguous years, the first births in one year will give the fertility very accurately of any of the contiguous years. From this fertility the sterility can be easily computed. Now in 1855 there were, in Edinburgh and Glasgow, 4,447 marriages, and 3,722 first deliveries of living children, leaving 725 marriages sterile, or 1 in 6·1. But in these figures are included 75 marriages which did not take place till after the women had passed forty-four years of age, and these will damage the physiological value of the statement, as these 75 women could not be expected to be fecund. Of women between the ages of fifteen and forty-four inclusive there were married 4,372; among women of the same ages 3,710 had first living children, leaving 662 marriages sterile, or 1 in 6·6. In other words, 15 per cent. of all the marriages between fifteen and forty-four years of age, as they occur in our population, are sterile. But this final estimate from the Edinburgh and Glasgow data has to be corrected for the dead born, these being not counted.

We have thus fairly good statements of the amount of sterility, which are not very different from one another :—

Patients in St. Bartholomew's Hospital .	1 in 8
Inhabitants of Grangemouth	1 „ 10
Inhabitants of Bathgate	1 „ 10
British peers	1 „ 6
Upper classes (Ansell)	1 „ 12
Inhabitants of Edinburgh and Glasgow . .	1 „ 7

Omitting that of British peers, the highest estimate is the last, and it is probably the only one in which living children are used, to the exclusion of dead, as the index of fecundity. Were dead children included there would be a great reduction—at least 4 per cent. The lowest estimate of sterility is that of Ansell. In it a woman having a stillborn child is held as fertile, and the women are the very best in the community, those living in easy circumstances and making use of the protection of life insurance ; were it otherwise, the estimate of sterility would no doubt be higher. We have thus estimates of sterility varying from 1 in 7 to 1 in 12, and may have considerable confidence in laying down 1 in 10 as very nearly the true amount of sterility of marriage in Great Britain.

I know no estimate of those who are absolutely sterile—that is, who do not conceive, or who, if they do conceive, give birth to not even an abortion. But there is a large number in the better classes, for within the last five years there have consulted me at my house, mostly on account of sterility, 504 absolutely sterile women, married between the ages of fifteen and forty-five, and of these, 337 were more than three years married. Though this shows a large number in existence, it gives no ground for an estimate of frequency among the married. The following tables give a classification of these 504 married and absolutely sterile women, according to age at marriage and number of years married :—

TABLE I.

Case-book Table of Sterility.

Age at marriage.	Years married.							Totals
	Und. 3.	4–8.	9–13.	14–18.	19–23.	24–28.	29	
15–19	12	19	15	4	7	2	1	60
20–24	70	66	37	24	13	9	—	219
25–29	47	51	20	8	8	—	—	134
30–34	26	20	8	4	1	—	—	59
35–39	6	13	4	—	—	—	—	23
40–45	6	3	—	—	—	—	—	9
Totals	167	172	84	40	29	11	1	504

It is certain that all populations are relatively sterile; and the economist makes many estimates, such as the deficiency of offspring of the actual marriages, or the deficiency of the actual births below what they might have been had all the women in the population been married at the most favourable time for child-bearing. The solution of these and similar questions is an object of greater interest to the statesman than to the physician. They demand, for their solution, much calculation, and need not be entered on here.

The degree or amount of relative sterility of the average individual varies, of course, according to the age at marriage, and it is not to be estimated by the deficiency below what is possible in child-bearing, but below the average amount of fertility of marriages at the various ages, or below what is not excessive, what can be done without injury to the average mother's health.

The average individual woman must be found and considered, for individuals vary extremely. It is not a rare observation, and I have one before me where the easy birth of a single child exhausted the fecundity of a healthy woman of twenty-five years of age at the time of the birth, and completely ruined her general health during the remaining child-bearing period of life. This woman was examined by many physicians, and all concurred in finding no cause of the weakness and inability but the child-bearing. On the other hand, Ansell records the case of a woman married at twenty-one, who in twenty-seven years gave birth to twenty-five children who all reached adult age, and the mother died of old age at eighty-eight.

Only-child fertility or one-child relative sterility occurs in two forms; as an exhaustion of the fecund energies leaving the general bodily health vigorous, or as an exhaustion of both sexual power and general constitutional strength. It is a relative sterility which is familiar to the public from its frequency and its importance in social respects. Ansell, in 1,767 fertile marriages, with a mean age at marriage of about twenty-five years, and allowing ample time for the exhibition of fecundity, as we have already stated (p. 10) found 131 cases of one-child relative sterility or 1 in every 13 fertile marriages. The degree of this relative sterility may be approximated by comparing

it with the average fertility of the same women, which was nearly 6; or, in other terms, the relative sterility of these 131 only-child fertile women was 655. Instead of having 131 children, they would have had 786 children if they had even reached the average fertility of their 1,636 sisters, and they would have had still more if they had reached a normal fertility instead of this average fertility, meaning by normal fertility what they might have had without injury to health, all as judged by women generally.

There are several tests of relative sterility secondary to that implied in the paramount question, How many did she bear? These subsidiary tests are based on the ascertained course of natural fertility, and show the deviations from this course of the relatively sterile. Inquiry made by these tests implies a knowledge of how many children a woman will naturally bear, or is likely to bear, and of the natural order of births. They are as follows:—

1. When after marriage did she begin the career of child-bearing? 2. How rapidly did the children follow one another? or, what was the interval between successive births? 3. When did child-bearing cease? or, what was the age at the birth of the last child? 4. How long was the child-bearing period of life? or, what was the interval between the beginning of the first pregnancy and the end of the last?

In studying population, these subsidiary matters are little regarded, for the statesman has direct interest only in the mutually related questions, How many are born? How many might have been born? What is the health of those born? The answers to these inquiries give him the actual relative sterility of the population, and in the case of a population this includes the absolute sterility. He may now attempt to increase or diminish the sterility of the people, not neglecting the health of the progeny so far as it is related to fertility; and this control he will effect chiefly by raising or lowering the age at marriage. On the other hand, the physician, having care of individuals, not of a people, and advising each from year to year of life, has his chief interest in these subsidiary matters, which the statesman may not utterly neglect, but may leave to the care of the medical philosopher.

The importance of the question, How soon after marriage does a woman bear her first child? is self-evident, and it will be found to be more a test of sterility than it appears at first sight to be. Whitehead, founding on the observation of 541 married women of the average age of twenty-two years, makes the average interval between marriage and the birth of a first child to be eleven months and a half. Sadler says that married females do not become fruitful, on the average, during the first year

of their nuptials, but nearly so. A great number of cases, he says, which he has collected, with a view of determining this point, gives three-fourths of them as producing their first child at the average of one year after marriage.

From the Edinburgh and Glasgow registers for 1855 I was able to make out this point in 3,722 cases. But in these extracts from the registers there are two sources of error, which prevent an exact comparison with the results of Ansell's more valuable table, for twins are excluded, being placed in the column of secundiparæ, not of primiparæ.

TABLE II.

Showing the Interval between Marriage and the Birth of a First Child.

Years married.	Number of births.	Years married.	Number of births.
Less	608	9	5
1	2,390	10	1
2	437	11	3
3	133	12	4
4	61	13	2
5	32	14	—
6	27	15	1
7	12	16	—
8	5	17	—
		18	1
Total			3,722

And, still more important, the great number of mothers whose children were stillborn is excluded. Now, twins affect specially young, immature, and quickly breeding mothers; their omission, therefore, from the column of primiparæ will tend to delay the estimated time of primiparity. Again, a similar

delay will result from the omission of women having dead children from the primiparous column, for such women, when they bear a first living child, which may be in reality a second, third or other child, will appear in the primiparous column with an overestimated and erroneous retardation of primiparity.

The Edinburgh and Glasgow table gives a mean interval of about seventeen months between the marriage and the birth of a living child. It shows that fecundity is not demonstrated by a living child in the majority of cases till a year of married life has passed; nearly two-thirds of the whole beginning their families in the course of the second year of marriage. It also shows that there is no ground for presumption of persistent sterility till the fourth year of married life is entered upon; for while of those three years married, and less than four, 133 bore a first living child, there were only 154 who did so in all the subsequent years taken together. Of the whole 3,722 only about one twenty-fourth part began bearing living children after four years of married life had elapsed.

Ansell's table includes first stillborn children, and is corrected for twins, and gives us the data in 6,035 cases. It is therefore better than the preceding, and better than any other of which I know regarding this point.

Ansell's table gives a mean interval of nearly sixteen months between marriage and the birth of

a child. The majority of the women in Ansell's table bore their first children before the first year of married life had elapsed—nearly seven-eighths before the expiry of two years of married life. It also shows that there is no good presumption of persistent sterility till the fourth year of married life is entered upon; for while of those three years married and less than four 421 bore a first child, there were only 292 who did so in all the subsequent years taken together. Of the whole 6,035, only about $\frac{1}{21}$st part began bearing children after the third year of married life, and only $\frac{1}{39}$th part after the fourth year.

TABLE III. (from Ansell).

Showing the Interval between Marriage and the Birth of First Children.

Year after marriage.	Number of first children.	Year after marriage.	Number of first children.
1	3,159	8	11
2	2,163	9	7
3	421	10	7
4	137	11	5
5	69	12	4
6	26	13	3
7	21	14	2
Total			6,035

It may therefore be held that married women delaying the commencement of fertility beyond sixteen months are already exhibiting a degree of relative sterility; and this conclusion is quite in keeping with the rest of our knowledge of this subject.

The second question proposed is, How rapidly do the children in a family follow one another? or, What is the interval between the births of successive children? Great authors on population used to hold that breeding women never exceeded, in rate of pro-

TABLE IV.

Showing the Average Duration of Marriage at Birth of each Successive Child, and the Average Interval between the Births of the Successive Children.

Number of children.	Number of mothers.	Duration of marriage in months.	Average interval between successive births.
1	3,722	17	—
2	2,893	38	19·0
3	2,534	64	21·3
4	1,982	90	22·5
5	1,543	115	23·0
6	1,221	137	22·8
7	848	162	23·1
8	641	181	22·6
9	425	203	22·5
10	222	225	22·5
11	152	235	21·4
12	61	246	20·5
13	34	263	20·2
14	11	281	20·1
15	6	280	18·7
16	2	336	21·0
17	2	252	14·8
18	1	252	14·0
19	1	204	10·7
Average			19·9

lificness, a child every two years; but, like many of the other suppositions on which Malthus and the rest based their principles, this has proved to be false. With our present knowledge, we can assert that Malthus erroneously endowed womankind with a degree of relative sterility; for women who breed do

so at an average rate of a child every eighteen months, or nearly so.

I here give a table compiled from the Edinburgh and Glasgow registers, which makes the average interval between successive children nearly twenty months. But this requires several corrections, which will, on the whole, tend greatly to reduce the amount. Twins are included, and counted as two

TABLE V. (constructed from Ansell's data).

Showing the Mean Time after Marriage of Successive Births, and the Average Interval between them.

Order of birth.	Mean time of birth after marriage.	Average interval between successive births.	Order of birth.	Mean time of birth after marriage.	Average interval between successive births.
Child.	Years.	Months.	Child.	Years.	Months.
1st	1·32	—	10th	16·33	20·0
2nd	3·02	18·0	11th	17·65	19·0
3rd	4·83	19·0	12th	18·85	19·0
4th	6·69	20·0	13th	19·87	18·0
5th	8·53	20·0	14th	20·71	18·0
6th	10·28	20·5	15th	21·41	17·0
7th	11·92	21·0	16th	22·01	16·5
8th	13·47	20·0	17th	22·54	16·0
9th	14·93	20·0	18th	23·02	15·0

children. But a source of greater error is the exclusion of dead children, whether one or more. This last error might not be grave, or even not an error at all, in the view of an economist such as Malthus; but reckoning for it would make his actual error comparatively much greater. Table IV., like Table V., made up from Ansell, is not correctly described as giving the average interval between births, but as

giving the average interval between the marriage and the birth of the child, divided by the number of children born, which is a near approximation to what is wanted.

Ansell's table does not require correction for twins or for dead-born children, and its value may be judged by the statement, indefinite though it is, that it is based on more than 25,000 observations. The average interval, as calculated from them, is eighteen months. Ansell's table may be studied, further, with a view to a statement of the average interval in those who have not excessive families, but families of natural or normal number. Now those mothers who have shown excessive intensity of fertility, either by a high number of births or by excessive rapidity so long as childbearing continued, are mixed up in each successive row of figures with those that are normal, or nearly so. Then, looking at the rows of figures of families varying from four to ten, which shows intervals of twenty to twenty-one months, we are safe in stating the interval for normal families as above twenty months, yet, probably, considerably under two years.

It may therefore be held that a married woman who, during childbearing life, does not have a child every twenty months is exhibiting relative sterility.

The third question is, When did childbearing cease? or, What was the age at the birth of the last

child? Now, it is the rule to confuse the childbearing period of life with the period during which a woman menstruates, and this is a great mistake. It is only a part of this that in married life is occupied by childbearing, except in rarest cases, such as have never come under my observation. When a woman begins childbearing she generally, under favourable circumstances, continues her career of fertility steadily till her last child is born.

The registers tell us when women actually begin to have children, and I have already made use of such information; but we have no data nearly sufficient to decide what is the average age of commencing or initial fertility; we may, however, be sure, from what we do know, that it is not the age of puberty or of commencing menstruation, and that it is not the age of nubility or age at which procreation is commenced with the greatest advantage to mother and progeny. It is evidence of good conduct in the race that we cannot get sufficient data, there being very few unions permitted in early life. The great mass of our women are, fortunately, married within the limits of nubility, or the marriageable age. Nevertheless, it is very desirable that we should find out what is the mean age of initial fertility.

Regarding the time of cessation of childbearing we have more exact information, and it shows well the distinction that must be made between the cessation of menstruation and the cessation of fecundity.

Menstruation ceases at from forty-five to fifty years of age, but childbearing ceases at an average age of thirty-eight. This cessation arises from no imperfection or decay of organs, that has been demonstrated, but it may be due to that nevertheless. It is highly probable that its main cause is a cessation of functional vigour or activity, for it is delayed in women who have begun their fertility late in life.

On the subject of the cessation of childbearing our best information is derived from Ansell, whose calculations are based on 4,899 observations, restricted to those in which both the father and mother survived the childbearing age of the latter, a point which was determined as regards each case in accordance with a scale already given (see page 10), whose chief governing rule is not to suppose a woman under forty-four years of age to have borne her last child until she has been for ten years barren. The quinquenniad 39–43 is that at which the largest number cease to bear children. Thirty-eight years is the mean age of mothers, married at the mean age of twenty-five, at the date of the birth of their last children in cases where childbearing was not prematurely terminated by the death of either parent.

The productive period begins earlier, and is protracted to a later age, in cases where the children are numerous than where they are few. This protraction is shown by the following table :—

TABLE VI. (from Ansell.)

Showing the Mean Age of Mothers at the Birth of their last Children in Families of different numbers.

Number in family.	Mean age of mothers.
1	31·08
2 or 3	34·21
4 or 5	37·04
6 or 7	39·21
8 or 9	40.61
10, 11, or 12	41·74
13, 14, or 15	42·83
16 or more	44·32

Women have, in their career, and with a view to our present subject, many stages in life. There is the age of puberty, or of commencing menstruation, and this is to be distinguished from the age of commencing fertility, regarding which we have no data adequate for a decision. But the age of commencing childbearing, though it may be identical with that of commencing menstruation in individual cases, is certainly not nearly so in the mass of women, being fortunately considerably delayed. Then, after the age of commencing childbearing comes the age of nubility or marriageable age, that at which a woman can enter on married life with the best chances of having a healthy and not excessive family. After the age of nubility comes the age of cessation of childbearing, which, as already said, is thirty-eight for women married at twenty-five years of age. A woman may bear children after this age, or even after the cessation of menstruation, but such cases are exceptional and rare. The last stage in their

career is usually the cessation of menstruation at an age of forty-five to fifty.

There is a mean age of puberty or of commencement of possible procreation, a still farther advanced mean of commencing procreation, a still farther advanced mean of nubility or fitness for procreation, a still farther advanced mean of cessation of procreation, and lastly comes the mean of cessation of menstruation and of possible procreation. Most of these stages of woman's life have their analogues in the female life of the lower animals which are best known to us, and some of them have analogues in the life-history of plants. There can be no doubt that they all have their coördinate physical states of the genital organs, and in this department there has been much successful anatomical investigation, especially as regards puberty, nubility, and the cessation of menstruation.

Writing regarding the age of cessation of child-bearing, Whitehead makes the following pertinent remarks: "The sum of the ages of the individuals (38) recorded in the preceding table, at the time of their last delivery, is 1,586, giving an average of 41·73 years; the average age of the same individuals, at the time of their last menstruation, is 47·54 years; so that a period of nearly six years is here indicated, during which, although the menstrual function continued to be more or less efficiently discharged, and the health good, aptitude for procreation did not

exist. They were all placed under equally favourable circumstances for the continuance of childbearing so far as regarded their matrimonial position. . . . . A like period of uterine quiescence," he adds, "is observed before childbearing begins."

The average cessation of childbearing is for all women no doubt between thirty-five and forty years of age, and a woman in whom this career ceases earlier shows relative sterility.

To the question, How long does childbearing continue? it is easy to give some answer; for if the average age at the commencement of childbearing is twenty-six years, and the mean age at termination is thirty-eight, the average duration of childbearing is twelve years. The duration of fertility will be the number of pregnancies multiplied by nine (months) added to the number of intervals multiplied by nine (months). It will vary therefore from a case of one-child sterility, with nine months of the childbearing period of life, to a case of ten-child fertility, with a childbearing period of life of 171 months, or about fourteen years; and to a case of twenty-child fertility, with very much less than thirty years of childbearing life; very much less, because women of this great and excessive prolificness do hurry their children into the world to get through the high number.

From Ansell's table of 4,899 married women,

whose ages at the birth of their last children were known, and where both parents survived the child-bearing age of the mother, I have constructed the following table to show the nearest figures I can

TABLE VII. (constructed from Ansell's data).

Showing the Average Age at Cessation of Childbearing in Families of different numbers, and the Time occupied in Childbearing, estimated at the rate of eighteen months for each child, in families of less than ten children: the mean age of mothers at commencement of childbearing being twenty-six years, and the parents both surviving the childbearing age of the mother according to the scale of Ansell (p. 10).

Number of family.	Number of cases.	Mean age of mothers.		Time occupied in childbearing.	
		Yrs.	Months.	Yrs.	Months.
1	244	30	6	1	6
2	401	32	11	3	0
3	425	34	5	4	6
4	485	35	10	6	0
5	565	36	11	7	6
6	494	38	0	9	0
7	490	39	0	10	6
8	467	39	8	12	0
9	387	40	6	13	6
10	312	40	10	14	10
11	239	41	1	15	1
12	170	41	7	15	7
13	115	42	5	16	5
14	43	41	10	15	10
15	34	42	8	16	8
16	10	43	6	17	6
17	10	43	5	17	5
18	6	44	7	18	7
19	1	45	0	19	0
20	1	45	0	19	

give to the actual lengths of childbearing life in families of different numbers. The commencement of childbearing at twenty-six years of age is, in all cases, assumed, because it really was very nearly the mean age in Ansell's collection.

Table VII. affords us further valuable information as to the duration of child-bearing in families which reach the normal limit of about ten, and we see that it is about fifteen years. A woman then may be regarded as relatively sterile who, married within the years of nubility (about 20–25), ceases to have children within fifteen years from the birth of her first child.

We must now try to answer the last and comprehensive question, How many children does a woman bear? without calculating it from the answers to previously considered questions. On the answer to this depends the settlement of the amount of relative sterility. It cannot be satisfactorily answered directly, on account of the paucity of data; but such answer as we can give is corroborated by the various subsidiary answers which we have just furnished. We shall not enter on subjects important politically, such as the numbers in actual families, the number to a marriage, &c., because these are foreign to our present inquiry.

In the district of St. George's-in-the-East the Statistical Society found, among the poorer classes, eighty mothers who had been married at ages varying from fifteen to nineteen, and who had lived in wedlock at least thirty-one years, or all the child-bearing period of life. These fertile wives had borne on an average 9·12 children. Considering the

undoubted existence of evident sources of error, all tending to unduly diminish the average amount of fertility, we may safely say, using the data of St. George's-in-the-East, that ten is about the average fertility of fertile marriages lasting during the whole childbearing period of life. The average age of marriage in England is twenty-five, and consequently the production should be less than ten, the women living in fruitful wedlock from twenty-five till the end of the childbearing period of life, not all the childbearing period.

The actual fertility of fertile marriages in England, if only nine in ten wives have living children, is, according to Farr, 5·2 ; but with a view to contrast with the data of St. George's-in-the-East and of Ansell this figure needs correction ; for in making it up, the condition of living in wedlock till the end of the childbearing period of life is omitted. If that condition were not omitted, there would of course be a large increase of fertility of wives in England. Ansell's collection includes 1,767 spinsters married to bachelors at a mean age of twenty-five, and living in fruitful wedlock till the end of childbearing, as calculated by a scale already given, and the production was 5·7 or nearly 6, a figure which I regard as indicating a less fertility than that of Englishwomen generally.

The fertile wives of England, without the condition of persistency in married life till the end of the child-

bearing period of life, bore 5·2 children. Ansell's mothers in the upper classes, married at a mean age of twenty-five, and living in wedlock till the child-bearing period of life was passed, bore on an average 6 children. The fertile wives of St. George's-in-the-East, a poor class, living in wedlock all the childbearing period of life, bore above 9 children. Each of these statements yields some corroboration of the others; and, keeping in view some further evidence, they seem to justify us in holding that a healthy woman, living in wedlock all her childbearing life, under the most favourable circumstances for natural procreation, should have a family of 10; or, women, under such circumstances, bearing fewer than 10 are relatively sterile, and the sterility is inversely as the number. Further evidence to the same effect is got by referring to the data derived from the registers of Edinburgh and Glasgow for 1855. There ("Fecundity," p. 125, 2nd ed.) I found that, in fertile wives married at various ages, there was a fertility of between seven and eight after the lapse of fifteen years of marriage, counting to the birth of the last child; and fifteen years is full allowance for average persistence in fertility. Now, as many women are married some years after the best period for commencing childbearing, we may, by making allowance for such delay, raise the number from between seven and eight to ten.

There are many women who bear families above ten in number, and it is desirable to devote to them further special consideration. Such families are on the whole abnormal, or excessive. For many an individual woman a family less than ten is excessive. We have, indeed, spoken of the occasional calamitous character of only-child fertility. But there is a mass of evidence tending to show that a family, in the average female rising above ten, begins to be excessive, and increasingly so as the figure increases. It may seem paradoxical to bring the consideration of excessive families into a lecture on sterility, but in the next lecture the paradoxical character of this proceeding will disappear.

The bearing of a first child is well known to be very dangerous, often fatal, to the mother. After this she comes into a period of childbearing which is the safest, and which continues while she has a natural or ordinary degree of fertility. The danger of primiparity is; for a fertile woman, inevitable, but the special danger of multiparity is incurred only when a family is excessive; and I hold this danger to be good evidence (along with other) of excessiveness. It is, at at the same time, to be kept in mind that danger has been demonstrated to rise with increasing elderliness; but elderliness of the mother is an essential element in a question of excessive family. I extract from my work on "Fecundity, &c.," the following table, whose composition is there stated:

TABLE VIII.

Showing a Comparative Percentage of Deaths in Successive Labours.

Number of pregnancy.	Number of mothers.	Number of deaths.	Percentage.	Or 1 in
1	3,722	254	6·82	15
2	2,893	60	2·07	48
3	2,534	64	2·52	39
4	1,982	39	1·97	51
5	1,543	31	2·01	49
6	1,221	28	2·29	43
7	848	16	1·88	53
8	641	15	2·34	42
9	425	13	3·06	32
10	222	9	4·05	24
11	152	5	3·28	30
12	61	1	1·64	61
13	34	4	11·77	8
14	11	—	—	—
15	6	1	16·66	6

It does not give actual mortalities, but only such mortalities as may be compared with one another with a view to making out the peril attending confinements of different numbers.

In the sequel I shall give further and varied evidence as to the excessiveness of families above ten. This evidence is based not on the danger to the mothers only, but on the nature of the production—that is, on the occurrence of twins, of weakly or malformed children, and of idiots.

LECTURE II.

THEORY OR CAUSATION.

In studying the theory, or inquiring into the causes of sterility in woman, it is advantageous to keep in mind the corresponding condition in plants and in animals; for in all living beings there is more or less similarity of the sexual organs and offices, and disturbance of function in one division will throw light on disturbance in another. On this subject I have made many, but only casual, observations, and have had the privilege of conversation with gardeners and breeders, classes of men in whom are found many of remarkable intelligence and acuteness of observation. But the great storehouse of facts and references on which I rely is Darwin's "Variation of Animals and Plants under Domestication."

Plants and some animals propagate otherwise than by sexual generation, but it is only the sterility arising from disturbance of the regular course and consequences of sexual conjunction that has a direct or nearly direct bearing on the present inquiry. The sterility of hybrids, which, considering the theory he is supporting, forms naturally the main study

of Darwin, is of comparatively little interest to us, and will not be hereafter referred to, but many of the principles of sterility find strong support in the special sterility of hybrids.

Viewing the subject generally, we may anticipate a great result by pointing out the paramount prevalence and potency of constitutional conditions as causes of sterility. Such are cold and heat, overfeeding and underfeeding, youth and old age, degradation of general health, confinement and domestication.

Local conditions occur in plants that are quite sufficient to account for or cause sterility. Such are contabescence of anthers, monstrous flowers, double flowers, seedless fruit. The local conditions are the result of the general or constitutional conditions of the individuals in which they occur; and they have their place rather in the results of sterility, or of the conditions producing sterility, than in the causes of sterility. They have their analogues in such abortions, dead fœtuses, unhealthy offspring, or monstrous products of animals as are believed to be often results of what may be called the sterile diathesis. The causes of sterility are causes of these imperfections, and for that reason they are referred to the sterile tendency. They do, indeed, constitute the sterility to be accounted for.

Thus, to wander into hybridism for confirmation, it is an observation of Gärtner that hybridism in plants, a great cause of sterility, produces also a strong tendency in flowers to become double.

In the vegetable kingdom every one has observed that source of sterility which may be, no doubt nearly truly, designated a degradation of general health. A plant covered with flowers is brought from a house where its fertility has been stimulated to the highest degree, and placed as an ornament in a sitting-room, where it remains till its charms are past, and the result is such an injury to its constitutional vigour that it is sterile, or nearly sterile, for one or for several subsequent seasons. Its fertility may never be restored, or only after several years of the medical care of a skilful gardener. The scarlet geraniums, which are brought from their healthy homes in full bloom to adorn the houses of inhabitants of densely populated cities, soon show the injurious influence of their new surroundings, however well they may be cared for; their flowers become less numerous, or are altogether wanting; then their leafage diminishes greatly in quantity, and their existence becomes a mere lingering. A rose-garden, lately in a suburban position near London, gets surrounded by the growing city, and gradually, as the buildings increase, the fertility of the roses diminishes; the garden becomes useless. Some of our finest forest-trees, and among them

some planes, grow beautifully in our squares, producing wood in even exaggerated quantity, and a clothing of leaves sufficient for ornament; but there is no wealth of leaves, and there is often no seed. In some cases an exception makes the rule more striking, as when a cherry-tree in the heart of the city of London lately produced flowers and matured its fruit, so far as maturity is indicated by beauty, size, and taste.

Practical gardeners attribute sexual injury to over-stimulation by manure, or what they call overfeeding. This ordinarily produces great growth of the tissues; and when this is restrained by judicious pruning, a large or excessive crop of flowers and subsequent fruit is forced out. In the language of Spencer, there is produced by overfeeding an excess of individuation, the restraint of which results in excess of genesis. The natural tendency of the overfeeding of plants is to produce a degree of relative sterility; and this may show itself in paucity of flowers, or it may show itself in the production of those double, or monstrous, or abortive flowers which are so much admired. The opposite result is produced by moderate or full feeding. Then, in mature plants, there is not great growth of tissues, but rather a production of fruit. Sometimes a plant, without assignable cause, but especially if underfed, has an exaggerated production, and is said to run to seed; and, from whatever it may arise, it in a reflex manner injures the plant,

which consequently becomes blighted and often dies. Excessive production here seems to take the place of sterility.

The following is an interesting illustration of the effect of overfeeding and of moderately feeding or underfeeding a vine, and it is important because it specifies a particular local condition or disease which is apparently the secondary cause of the infecundity of the overfed plants; and so it indicates a line of investigation which may with advantage be pursued in other examples of sterility. In a recent letter from Mr. Thomson, the well-known vine-cultivator, he writes:—"A circumstance has arisen in my own experience that I have never seen noticed in print. A vine called the Alnwick seedling, if grown vigorously in rich soil, fails to set its fruit even when aided. This failure is caused by the exudation from the female organ of a dewdrop of sap, which moistens the pollen, and it does not descend through the pistil and impregnate the ova. When the vine is grown in poor soil the dewdrop does not appear, and impregnation takes place; seeds are formed in perfection, but the pulp for which the grape is grown is almost absent. I know (he adds) no other grape affected in the same way or subject to the same influences."

Though there is no good account of the sterility of plants as regulated by age, yet the influence of age is well recognised. A young fruit-tree bears no fruit,

or very little, and that little imperfect, and the careful gardener does not permit it to bear much or even a little, believing that fruit-bearing injures growth and retards future copious fertility. The influence of old age and decay in fruit-bearing trees is also well known: the fruit is ill developed, and there is little of it.

"All know," says Spencer, "that a pear-tree continues to increase in size for years before it begins to bear, and that, producing but few pears at first, it is long before it fruits abundantly. A young mulberry, branching out luxuriantly season after season, but covered with nothing but leaves, at length blossoms sparingly, and sets some small and imperfect berries, which it drops while they are green; and it makes these futile attempts time after time before it succeeds in ripening any seeds. But these multiaxial plants, or aggregates of individuals, some of which continue to grow while others become arrested and transformed into seed-bearers, show us the relation less definitely than certain plants that are substantially, if not literally, uniaxial. Of these the cocoa-nut may be instanced. For some years it goes on shooting up without making any sign of becoming fertile, about the sixth year it flowers, but the flowers wither without result. In the seventh year it flowers and produces a few nuts, but these prove abortive, and drop. In the eighth year it ripens a moderate number of nuts, and afterwards increases the number,

until, in the tenth year, it comes into full bearing. Meanwhile, from the time of its first flowering, its growth begins to diminish, and goes on diminishing till the tenth year, when it ceases."

The evil influence of interbreeding is a subject too extensive to enter upon at any length. In plants it is corroborated by the well-known advantage of crossing of varieties. But it needs no confirmation, for there are self-impotent plants, plants more thoroughly fertilized by a nearly allied species than by pollen of their own species, and there are the wonders of dimorphism with sterility arising from union of individuals not only of the same species but of the same form. In the works of horticulturists is to be found ample evidence that interbreeding of plants tends to weakness, malformation, and sterility.

The influence of heat and of cold is, in plants, well illustrated by the failure of most alpine species to produce flowers and fruit in lowland gardens, and the same failure of lowland plants as they ascend the sides of mountains. A walk in the highlands will show the pines thriving on the hill-sides and well-covered with cones, but as greater altitudes are reached the trees are observed to become stunted and the fruit entirely to fail.

The abortion-like sterility of plants is illustrated by the bearing of double flowers, of flowers whose seeds do not ripen, or whose seeds though apparently perfect, are incapable of germination and growth.

In some of the cases of seedless fruit, and of fruit with few seeds, or with one seed, or with imperfect seed, we have also abortion and at the same time a fine illustration of the working, locally, of the opposition between individuation and genesis. The whole plant, as the vine or the pear-tree, may have the appearance of health, and its fruit alone is unnatural. The tissues of the fruit-capsule are enormously developed, while the seeds have disappeared, or are reduced to one or a small number. The luscious pear and the juicy grape are masses of hypertrophy or myxomatous-like degeneration while the seeds are the subject of extreme hypoplasia. Gardeners generally ascribe these results to overfeeding and overstimulation by manures and heat; but Darwin is more cautious, and in most cases does not analyse the causes farther than is implied in "unnatural conditions of life." No one, according to Lindley and Darwin, has produced double flowers by promoting the perfect health of the plant.

Before leaving vegetable physiology I would point out the frequent occurrence in plants of seeds which, though apparently perfect, will not germinate: they cannot be distinguished from their neighbours otherwise than by their incapacity for growing. The same failure to grow is often observed under closely similar circumstances in the eggs of the fowl and of other birds; they cannot be hatched, although no imperfection is discoverable in them. That there are

such ova in other animals and in woman is highly probable; but in them the completeness of the demonstration is unattainable.

Very little is known of the sterility of animals, and it is easily understood that reliable observations can only, with great difficulty, be made on them, especially in a state of Nature. Many authors, and latterly Darwin and his collaborators, have paid much attention to the great subject of the sterility of hybrid animals. Observations and experiments in this department are made chiefly on domestic animals, or wild animals in confinement, and each experiment has a high value. But the sterility of ordinary domestic animals has been little studied. In herds of fine heifers and cows, and in mares, it is occasionally exhibited; but I have no data as to its frequency, and in cattle, at least, observations are imperfect, the animal that, by sterility of one season, disappoints its owner, being forthwith fattened for the butcher.

It is a well-known belief among breeders, which may be historically traced to ancient times, that when the female of any kind is made to breed when very young, she does so at the expense of permanently preventing her own growth to perfection, and she will likely produce offspring that is not of the best quality. This failure is well illustrated in the case of the common fowl and of the turkey, the progeny of chickens and of turkeys one year old

being not the best of their kind, and specially difficult to rear. Fanciers breed these animals from a female two years and a male three years old. The occurrence of sterility in early and in elderly life is clearly seen, and its degree easily made out in pluriparous mammals, as the dog and pig, and in birds whose broods can be counted, and whose yearly production of eggs can be also numbered. This subject will be discussed fully when we come to consider pluriparity in woman.

Overfeeding, or the production of fatness or of obesity in the female, is well known to be hostile to fertility, to be an illustration of the opposition of individuation to genesis. By special feeding and fattening turkeys and common fowls the henwife arrests almost completely the production of eggs. They may also be made fewer by starving the birds, and not fewer only but also smaller. These birds when highly fed sometimes exhibit excessive productiveness, two eggs being laid daily, an instance of great intensity of fertility; but this is not regarded with favour, having, I am told by a turkey-fancier, an injurious influence, in their case, by delay of the commencement of laying in the season following that of the excessive production. The breeder of cattle prevents by careful management the fattening of the females.

In respect to feeding, comparisons are made between the relative sterility of wild animals and

the comparative fertility of domesticated or of confined animals of the same species, but the comparisons are not quite satisfactory, from the intermixture of the influences of food and of domestication or of confinement; and again, in the comparisons of animals fed on rich and on poor pasture, sufficient care is not taken to ensure that the compared animals are of the same breed and age. With this previous reflection I subjoin an interesting passage from Spencer's chapter on nutrition and genesis: "Clear proof," says he, "that abundant nutriment raises the rate of multiplication (and *vice versâ*) occurs among mammals. Compare the litters of the dog with the litters of the wolf and the fox. Whereas those of the one range in number from six to fourteen, the others contain respectively five or six or occasionally seven, and four or five, or rarely six. Again, the wild cat has four or five kittens, but the tame cat has five or six kittens two or three times a year. So, too, is it with the weasel tribe. The stoat has five young ones once a year. The ferret has two litters yearly, each containing from six to nine, and this notwithstanding that it is the larger of the two. Perhaps the most striking contrast is that between the wild and tame varieties of the pig. While the one produces, according to its age, from four to eight or ten young ones once a year, the other produces as many as seventeen in a litter; or, in other cases, will bring up five litters of ten each

in two years—a rate of reproduction that is unparalleled in animals of as large a size. And let us not omit to note that this excessive fertility occurs where there is the greatest inactivity—where there is plenty to eat and nothing to do. There is no less distinct evidence that among domesticated mammals themselves, the well-fed individuals are more prolific than the ill-fed individuals. On the high and comparatively infertile Cotswolds it is unusual for ewes to have twins, but they very commonly have twins in the adjacent rich valley of the Severn. Similarly, among the barren hills of the west of Scotland, two lambs will be born by about one ewe in twenty; whereas in England, something like one ewe in three will bear two lambs. Nay, in rich pastures, twins are more frequent than single births; and it occasionally happens that, after a genial autumn and consequent good grazing, a flock of ewes will next spring yield double their number of lambs—the triplets balancing the uniparæ. So direct is the relation, that I have heard a farmer assert his ability to foretell, from the high, medium, or low condition of an ewe in the autumn, whether she will next spring bear two, or one, or none."

An interesting department of the sterility of animals is that which results from confinement. This seems specially to affect what are vaguely designated the noble animals. Those which are sterile show great variations; some disdain to cohabit or have

lost sexual desire; others have increase of sexual appetite, and cohabit freely or excessively, but without impregnation resulting, or with the result very rarely following. Some, if impregnated, bring forth only abortions, or young which are dead-born, or, if alive, feeble and ill-formed. There is, for instance, as Shorthouse has pointed out, a common occurrence of cleft palate in the lion's cubs born in the Zoological Gardens.*

Among birds in confinement there are many good examples of change of sexual habits and of sterility. In some cases they have no eggs; or, if they produce, they have only comparatively few; or they may neglect the eggs when produced; or the eggs duly cared for may be incapable of being hatched. This last or abortional sterility, arising from imperfection of eggs as a result of confinement, is well proved by experiments made in France on the common fowl. When these birds were allowed considerable freedom, 20 per cent of the eggs failed to be hatched; when less freedom was allowed, 40 per cent. failed; when closely confined, 60 per cent. were not hatched.

The power of temperatures that are not according to an animal's nature to induce sterility is no doubt very great. Darwin mentions that Mr. Miller, a former superintendent of the Zoological Gardens, believed that the sterility of the carnivora there was

* See J. W. Ogle in *British and Foreign Medico-Chirurgical Review*, vol. xlix. p. 500, 1872.

increased by increase of exposure to air and cold. In winter, inadequately sheltered cows either cease to give milk or give it in diminished quantity. "And," says Spencer, "though giving milk is not the same as bearing a young one, yet, as milk is part of the material from which a young one is built up, it is part of the outlay for reproductive purposes, and diminution of it is a loss of reproductive power." Failure to maintain the cow's heat may entail such reduction in the supply of milk as to cause the death of the calf. Hard living, says Darwin, retards the period at which animals conceive, for it has been found disadvantageous in the northern highlands of Scotland to allow cows to bear calves before they are four years old. Roulin found that in the hot valleys of the equatorial Cordilleras sheep were not fully fecund.

The common fowl will not breed in Greenland or Northern Siberia. "In this country it is fed," says Spencer, "through the cold months; but, nevertheless, in midwinter it wholly leaves off laying, or lays very sparingly. And then we have the further evidence that if it lays sparingly, it does so only on condition that the heat, as well as the food, is artificially maintained. Hens lay in cold weather only when they are kept warm. To which fact may be added the kindred one, that when pigeons receive artificial heat they not only continue to hatch longer in autumn, but will recommence in spring sooner than they would otherwise do."

On the subject of the interbreeding of animals there is a vast body of opinion as well as of facts showing its power in producing monstrosity and its ally sterility. "If we were," says Darwin, "to pair brothers and sisters in the case of any pure animal which from any cause had the least tendency to sterility, the breed would assuredly be lost in a few generations." Elsewhere he shows that "long-continued close interbreeding between the nearest relations diminishes the constitutional vigour, size, and fertility of the offspring, and occasionally leads to malformations, but not necessarily to general deterioration of form or structure. This failure of fertility shows that the evil results of interbreeding are independent of the augmentation of morbid tendencies common to both parents, though this augmentation no doubt is often highly injurious. Our belief that evil follows from close interbreeding rests to a large extent on the experience of practical breeders, especially of those who have seen many animals of the kinds which can be propagated quickly, but it likewise rests on several carefully recorded experiments. With some animals close interbreeding may be carried on for a long period with impunity by the selection of the most vigorous and healthy individuals, but sooner or later evil follows. The evil, however, comes on so slowly and gradually that it easily escapes observation, but can be recognized by the almost instantaneous manner in which size, con-

stitutional vigour, and fertility are regained when animals that have long been interbred are crossed with a distinct family."

Regarding the very remarkable subject of sterility of sexual connexion with special individuals only, Darwin says:—"It is by no means rare to find certain males and females which will not breed together, though both are known to be perfectly fertile with other males and females. We have no reason to suppose that this is caused by these animals having been subjected to any change in their habits of life. . . . . The cause apparently lies in an innate sexual incompatibility of the pair which are matched. Several instances have been communicated to me by Mr. W. C. Spooner (well known for his essay on Cross-breeding), by Mr. Eyton of Eyton, by Mr. Wicksted, and other breeders, and especially by Mr. Waring of Chilsfield, in relation to horses, cattle, pigs, foxhounds, other dogs, and pigeons. In these cases, females which either previously or subsequently were proved to be fertile, failed to breed with certain males, with whom it was particularly desired to match them. A change in the constitution of the female may sometimes have occurred before she was put to the second male; but in other cases the explanation is hardly tenable, for a female known not to be barren has been unsuccessfully paired seven or eight times with the same male, likewise known to be perfectly fertile. With cart-mares, which some-

times will not breed with stallions of pure blood, but subsequently have bred with cart stallions, Mr. Spooner is inclined to attribute the failure to the lesser sexual power of the racehorse. But I have heard from the greatest breeder of racehorses at the present day, through Mr. Waring, that it frequently occurs with the mare to be put several times during one or two seasons to a particular stallion of acknowledged power, and yet prove barren, the mare afterwards breeding at once with some other horse. These facts are worth recording, as they show, like so many previous facts, on what slight constitutional differences the fertility of an animal often depends."

Before leaving the subjects of the causes of sterility of animals, I quote a passage from Darwin regarding the results of confinement. "Sufficient evidence," says he, "has now been advanced to prove that animals, when first confined, are eminently liable to suffer in their reproductive systems. We feel at first naturally inclined to attribute the result to loss of health, or at least to loss of vigour, but this view can hardly be admitted when we reflect how healthy, long-lived, and vigorous many animals are under captivity, such as parrots, and hawks when used for hawking, chetahs when used for hunting, and elephants. The reproductive organs themselves are not diseased, and the diseases from which animals in menageries usually perish are not those which in any way affect their fertility. No domestic animal is more subject

to disease than the sheep, yet it is remarkably prolific. The failure of animals to breed under confinement has been sometimes attributed exclusively to a failure in their sexual instincts ; this may occasionally come into play, but there is no obvious reason why this instinct should be specially liable to be affected with perfectly tamed animals, except, indeed, indirectly, through the reproductive system itself being disturbed. Moreover, numerous cases have been given of various animals which couple freely under confinement, but never conceive, or, if they conceive and produce young, these are fewer in number than is natural to the species. In the vegetable kingdom instinct of course can play no part, and we shall presently see (he says) that plants, when removed from their natural conditions, are affected in nearly the same manner as animals. Change of climate cannot be the cause of the loss of fertility, for, whilst many animals imported into Europe from extremely different climates breed freely, many others, when confined in their native land, are completely sterile. Change of food cannot be the chief cause, for ostriches, ducks, and many other animals, which must have undergone a great change in this respect, breed freely. Carnivorous birds, when confined, are extremely sterile, whilst most carnivorous mammals, except plantigrades, are moderately fertile. Nor can the amount of food be the cause, for a sufficient supply will certainly be given to valuable

animals, and there is no reason to suppose that much more food would be given to them than to our choice domestic productions which retain their full fertility. Lastly, we may infer, from the case of the elephant, chetah, various hawks, and of many animals which are allowed to lead an almost free life in their native land, that want of exercise is not the sole cause. It would appear that any change in the habits of life, whatever these habits may be, if great enough, tends to affect in an inexplicable manner the powers of reproduction. The result depends more on the constitution of the species than on the nature of the change; for certain whole groups are affected more than others; but exceptions always occur, for some species in the most fertile groups refuse to breed, and some in the most sterile groups breed freely. Those animals which usually breed freely under confinement rarely breed, as I was assured, in the Zoological Gardens, within a year or two after their first importation. When an animal which is generally sterile under confinement happens to breed, the young apparently do not inherit this power; for had this been the case, various quadrupeds and birds which are valuable for exhibition would have become common. Dr. Broca even affirms that many animals in the Jardin des Plantes, after having produced young for three or four successive generations, become sterile; but this may be the result of too close interbreeding. It is a remarkable circumstance that

many mammals and birds have produced hybrids under confinement quite as readily as, or even more readily than, they have procreated their own kind. Of this fact many instances have been given, and we are thus reminded of those plants which when cultivated refuse to be fertilized by their own pollen, but can easily be fertilized by that of a distinct species. Finally, we must conclude, limited as the conclusion is, that changed conditions of life have an especial power of acting injuriously on the reproductive system. The whole case is quite peculiar, for those organs, though not diseased, are thus rendered incapable of performing their proper functions, or perform them imperfectly."

Finally, Shorthouse has pointed out in mares the close alliance between sterility, abortion and that kind of excessive fertility which in this animal is demonstrated by twinning. I quote the examples which he gives in the *Sporting Times* for Dec. 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.
Thimblerig, 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.
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 inefficacious; 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 a table which I compiled from the data of Edinburgh and Glasgow

TABLE IX.

Showing the Variations of Sterility according to the Age at Marriage.

Age of wives at marriage . . .	15–19	20–24	25–29	30–34	35–39	40–44	45–49	50, &c.	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

in 1855. It is evident 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 first born living, excluding the dead from the reckoning, another 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. Closely similar results are 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 child-bearing; 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

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

the facts bearing on this. 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 quinquenniad after that of twenty to twenty-four.

In the quinquenniad preceding twenty I can give for each single year the increasing delay of child-bearing 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

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
No. of wives newly married. .	43	108	225	314
No. 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	57·84

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 till the next quinquenniad, 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 marriage 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, supported by a large body of evidence, alleging that the measurements increase with the age of the mother in a direct sort of proportion. Aristotle had considered the subject. "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 tenor of the whole evidence still very strongly indicates a concurrence of sterility, monstrosity, smallness and feebleness, and on that account I continue to 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., derived from

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

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 failure 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.*

* I would, in this connection, call attention to Ansell's ("Statistics

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-

of Families," p. 79) very remarkable Table VI. showing that, at all ages up to 45, first-born have a greater mortality than second-born, and third-born; that among the fourth-born, fifth-born, and sixth-born, mortality is greater than among second-born, and third-born; and that the mortality is still greater among seventh-born and later children. How remarkably this falls in with the doctrine of the text it is unnecessary to point out: imperfection, so far as shewn by failure to survive in first children; high degree of perfection in second and third (and possibly in fourth); increasing imperfection as the number of later children increases.

Excerpts Illustrative of Ansell's Table.

Age.	Numbers that survive each age out of 100,000 born alive.				
	1st child.	2nd child.	3rd child.	4th, 5th, and 6th children.	7th and later children.
Total births	104,016	102,005	101,549	101,738	102,085
Born alive.	100,000	100,000	100,000	100,000	100,000
5	87,678	88,359	88,901	87,358	84,956
10	85,599	86,194	86,307	85,261	82,536
15	83,592	84,489	84,657	83,532	81,309
20	80,902	82,157	81,857	80,778	78,567
25	77,708	78,842	78,521	77,791	75,472
30	74,885	75,398	74,996	74,684	72,230
35	71,824	72,333	72,017	71,492	69,581
40	68,282	68,932	68,719	68,274	66,832
45	65,778	65,251	65,504	65,377	63,510

uterine continuation of the previously otherwise conducted nutrition of the fœtus. Nursing is part of the reproductive process. The failure to rear may be the result of imperfection of the fœtus, 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 result of these two causes, but the potency of imperfect nursing is undoubted. It is a universally recognised rule in the selection of wet nurses, that very young or elderly mothers are to be avoided.

Imbeciles and idiots may be such 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 from accidental, causes acting during or after birth.

Among Mitchell's 443 idiots and imbeciles, 138 were first-born; among Wilbur'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 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

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

apparent." "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 collections as is given by Mitchell in regard to his. Down's data are given in Table XIV.

TABLE XIV. (constructed from Langdon Down's data).

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.

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.

Age	20–24	25–29	30–34	35–39	40–44	45–49
Percentage of all children . . .	22·62	39·99	23·61	14·76	5·15	0·58
Percentage of idiots	25·88	25·88	10·58	10·58	23·53	3·53

"The same eighty-five cases," continues Mitchell, "are used in Table XV. which were used in Table XIII. This table is read thus:—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 connexion 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 a prevalent opinion that the last born of a woman is specially liable to be a miscarriage, or a weak child, or 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 is some, though

imperfect, evidence 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 truly 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 the domestic animals." In addition to other strong evidence to a like effect, 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	22	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.

In Galton's statement of the actual infertility of heiresses there is observable a remarkable comparative paucity of male issue—a fact which goes, like many others, to confirm the ancient and still prevalent opinion that relative sterility or weakness of reproductive energy tends to the production of females rather than males. This department of the study of sterility I shall not enter on, the causes of the excess of males over females in all births being the subject of an extensive literature, and its relations being too numerous and complicated for advantageous discussion in this place. But I may state that I have long been impressed with a belief, in accordance with the chief pertinent facts, that the excess of female births is due to the prevalence of a degree of weakness of reproductive energy. Excess of female births is coincident with other evidences of sterility.

We have already given reason for believing that when a woman bears above ten of a family she shows an unnatural or excessive amount of fertility; and this belief is corroborated by the demonstration we now propose to give that excessive families occur chiefly in women who are married in the sterile ages or ages of weak reproductive energy characterized by absolute sterility and by morbid production, whether abortive, premature, or mature. At present we only consider the production of mature children, and we find the unnatural intensity of fertility in the young

shown by absolutely large number, that is, above ten; while in the elderly it is shown by rapidity of births or intensity of fertility, so long as it lasts; and we may here remark that it has been elsewhere proved that for such women as begin childbearing late in life there is a prolongation of the period of fertility beyond the average age of ceasing to bear, not a prolongation, as estimated from beginning to end, of actual childbearing.

That the fertile younger are more fertile than the fertile older is shown by the following table of data

TABLE XVII.

Showing the Fertility of Mothers Married at Different Ages.

Years elapsed since birth of first child.	Average number of children to each marriage formed at ages—			
	16–20.	21–25.	26–30.	31–35.
10	5·05	4·51	4·42	3·44
20	7·68	7·01	6·43	3·00
30	8·41	7·89	6·80	7·00
40	10·85	8·24	5·00	4·00

derived from St. George's-in-the-East. That the younger fertile have a longer perseverance in fertility than the fertile older is shown by Table XVIII. derived from my work on Fecundity. That the unnatural intensity of fertility in women bearing large families begins with the commencement of childbearing is shown by Table XIX. from Ansell, which demonstrates the rapidity, only up to the birth of the third child, in families of various

TABLE XVIII.

Showing the Amount of Continuance in Fertility of Wives Married at Various Ages, as shown within Twelve Months.

Age of mother at marriage	15–19	20–24	25–29	30–34	35–39	Total.
The number child-bearing in the fifth year of married life is 1 in .	2·6	2·7	4·1	4·9	10·5	3·2
The number child-bearing in the tenth year of married life is 1 in	3·2	4·0	5·9	8·7	—	4·4
The number child-bearing in the fifteenth year of married life is 1 in	4·6	6·8	18·2	37·4	—	8·0
The number child-bearing in the twentieth year of married life is 1 in.	8·5	14·6	129·8	—	—	16·3
The number child-bearing in the twenty-fifth year of married life is 1 in.	68·0	480·5	—	—	—	171·0

numbers. Up to the third birth the rapidity is twice as great in families of sixteen or more as in families not above three, and it is easily counted that while the small families came slowly, and the

TABLE XIX. (from Ansell.)

Showing Intensity of Fertility in Mothers of Families of Different Numbers.

In families consisting of the undermentioned numbers of children.	Interval between the marriage of the parents and the birth of the—		
	First child.	Second child	Third child.
	Years.	Years.	Years.
1, 2, or 3	1·78	4·84	7·38
4, 5, or 6	1·37	3·32	5·49
7, 8, or 9	1·18	2·82	4·68
10, 11, or 12	1·05	2·54	4·15
13, 14, or 15	1·06	2·40	3·81
16 or more	0.96	2·15	3·47

excessive families quickly, the families from seven to twelve came nearly at the average rate of one every eighteen months. That the unnatural rapidity of childbearing in excessive families continues throughout childbearing life is shown clearly by Tables IV. and V. In my table the quickest childbearing is every ten months, the family being nineteen in number. In Ansell's table the quickest is every fifteen months, the family being eighteen.

Lastly, we show, by a table framed from the Edinburgh and Glasgow data, that the wives beginning fertility at advanced periods of life have an unnatural intensity of fertility while it lasts, a greater intensity than that of women married and beginning to childbear at the best ages. The table reads thus:

TABLE XX.

Showing the Intensity of Fertility in Wives Mothers of Different Ages.

Duration of marriage.	Mother's age.						
	15–19	20–24	25–29	30–34	35–39	40–44	45–49
Under five years . .	1·128	1·519	1·825	1·844	1·827	1·698	1 200
Five years and under ten	2·500	3·190	3·750	4·048	4·085	3·792	4·000
Ten years and under fifteen	—	5·333	5·453	5·903	6·197	5·964	6·500
Fifteen years and under twenty . . .	—	—	6·000	—	7·914	7·993	8·435
Twenty years and under twenty-five .	—	—	—	7·000	9·396	9·718	10·528
Twenty-five years and under thirty . .	—	—	—	—	—	12·368	13·500
Thirty years . . .	—	—	—	—	—	—	13·000

To take the second row of figures—Fertile women five years married and under ten have, if they are

now from fifteen to nineteen years of age, 2·5 children; if now from twenty to twenty-four years of age, 3·19 children; if now from twenty-five to twenty-nine years of age, 3·75 children, and so on.

Multiparity is a term already well recognized as implying that the subjects of it have had two or more pregnancies and births; but a woman may bring forth two or more children at once, and to this condition we apply the term pluriparity. The most common degree of pluriparity is the production of twins, these occurring about once in every eighty pregnancies. Triplets and higher numbers are very much rarer, and the rarity increases with the number.

Chiari, Braun, and Spaeth have given some evidence that abortions are comparatively more frequent in plural than in ordinary pregnancies. M'Clintock, founding on large experience, shows that hydramnios is also commoner. Acephalous monsters are found only in plural pregnancies. Monstrosities of all kinds are commoner in plural than in ordinary pregnancies. There are more dead-born children in plural pregnancies. The children born alive in plural pregnancies are more difficult to rear. "The proportion," says Ansell, "of infants that are stillborn or die soon after birth is, in the case of males nearly five times, and in the case of females nearly four times, greater in multiple than in single births."

Subsequently we shall adduce evidence that pluri-

parity is specially associated with idiocy and imbecility of the children, and that it specially affects the sterile ages, or ages of weakness of reproduction. Excessive family—that is, above ten in number—specially affects the same ages, and is dangerous to the lives and injurious to the health of both mothers and children. Both have therefore an alliance with sterility.

In a case of quintuplets the mother's age was forty and the pregnancy the tenth. In 7 cases of quadruplets the age of the mother was given in 6, and the mean is twenty-seven; the number of pregnancy was given in 6, and the mean is nearly three. The ages were nineteen and twenty with first pregnancies; twenty-five with third pregnancy; thirty with number of pregnancy not stated; thirty-two with a fifth pregnancy, and thirty-five with a fourth pregnancy. In one case of second pregnancy the age of the mother was not given. From a great variety of sources I have collected 43 cases of triplets, and of these I give in the subjoined tables some account.

TABLE XXI.

Showing the Ages of Mothers in Forty Cases of Triplets.

Age of mother . .	19.	20.	23.	24.	25.	27.	28.	29.	30.
Number of cases .	1	3	1	2	4	2	2	1	6
Age of mother . .	31.	32.	33.	34.	35.	36.	37.	38.	44.
Number of cases .	1	1	1	1	6	2	2	3	1

In 40 cases the age of the mother is given, and the mean is thirty. In 41 cases the number of the preg-

TABLE XXII.

Showing the Number of Pregnancy in Forty-one Cases of Triplets.

Number of pregnancy	1	2	3	4	5	6	7	8	10	11	12
Number of triplets	8	8	12	2	2	2	3	1	1	1	1

nancy is given, and the mean is four. It is naturally expected that our best evidence should be derived from twins ; but while this is really so, we have, even in these cases, to deplore the inadequacy of the data in point of number. I have not at present sufficient time at my disposal to enter into the details of the production of twins, and for these I refer to my work on Fecundity It is there shown that the frequency of twins increases with the age of the mother and with the number of the pregnancy, the very early ages of the mothers and the first pregnancy forming exceptions to the rule.

In a paper by Arthur Mitchell, published in the *Medical Times and Gazette* (Nov. 15th, 1862), he shows that twins are peculiarly liable to be imbeciles or idiots. The conclusions of Mitchell's paper are so pertinent to the present subject that I quote them here at length. "1. Among imbeciles and idiots a much larger proportion is actually found to be twin-born than among the general community. 2. Among the relatives of imbeciles and idiots twinning is also found to be very frequent. 3. In families, when twinning is frequent, bodily deformities [of defect and of excess] likewise occur with frequency. 4. The whole history of twin births is exceptional, indicates

imperfect development and feeble organization in the product, and leads us to regard twinning in the human species as a departure from the physiological rule, and therefore injurious to all concerned. 5. When we pass from twins to triplets and quadruplets, everything we know regarding these latter gives support to the general conclusions in question."

Besides these accumulated dangers and disasters to the children produced in plural pregnancies, we know that plural pregnancy is dangerous and disastrous to the mothers. The trivial and graver disorders of pregnancy are more common in pluriparous than in uniparous women, and the disasters and deaths in childbirth and in childbed are also more numerous in the pluriparous than in the uniparous. Nothing can be better demonstrated than that woman is naturally or normally uniparous, and that pluriparity is an unnatural or abnormal condition connected with sterility by being observed in the sterile ages, or ages of weakness or imperfection of reproductive power. It does not imply the desirable productiveness of health and vigour, but the reverse.

Pluriparity in a population, then, is not an indication that its social condition is as it should be. It shows, according to its amount, that marriages take place too early or too late in life; and it may be predicated of such a population that it has a correspondingly large maternal and infantile mortality, and that the reared children are not of the finest.

While woman is normally or physiologically uniparous, like the mare and cow, many of the other domestic animals are normally or physiologically pluriparous, as the dog, the rabbit, and the sow; and the fertility of most birds is a sort of pluriparity.

In the uniparous animals, pluriparity is rare in various degrees in the different kinds; but the extreme rarity in some, as in the mare, may to some extent depend on the circumstance that, in general, only the finest specimens at the most suitable ages are allowed to exhibit their fertility. Little, indeed, is known about them with the exactness desiderated with a view to comparison with woman. Yet we may safely assert that, among breeders of horses and cattle, the production of twins is, with a view to their interest in both mother and offspring, not looked upon with favour.* The freemartin cow is a good example of monstrosity complicating twinning, and it is specially frequent when the co-twin is a male. In the sheep there is such a frequency of twins, and even of triplets, that there may be some hesitation in classing it with uniparous mammals.

In the pluriparous animals, on the other hand,

* Sampson ("Infecundity of Females Born Co-Twins with Males") says he has been "unsuccessful in tracing out any instance of a twin-mare or she-ass, born under the circumstance already pointed out, being reared to maturity. The mare, indeed, appears only in extremely rare cases to produce twins, and these twins are almost always endowed with such feeble powers of life as seldom to survive for any length of time after birth."

uniparity is uncommon, and pauciparity is an indication of reproductive weakness or imperfection, while a just degree of pluriparity is natural or physiological. It is remarked, says Spencer, by Buffon, that when a sow of less than a year old has young, the number of the litter is small, and its members are feeble and even imperfect.

The domestic hen, in its fertile career, admirably illustrates the rise and decline of pluriparity, and the variations are in accord with the great law of age which holds good in woman and in all living beings. Its first and its last productions are small in size, and are believed to be peculiarly liable to be addled or without yelk, or to be otherwise incapable of being hatched. In its first year, according to Geyelin, it produces only 15 or 20 eggs; in its second, 100 or more, up to 120; in its third year, from 120 to 135, and here the climax of fertility is reached; in its fourth year it produces from 100 to 115; in its fifth from 60 to 80; in its sixth, from 50 to 60; in its seventh, from 35 to 40; in its eighth, from 15 to 20; in its ninth, from 1 to 10. The fertility rises quickly to its summum in the third year of life, and more slowly fades to its disappearance in the tenth year of life.

In like manner the bitch and pig begin their fertile course with a small number, which year by year rapidly increases; and after a few years, whose number I cannot give, again decreases, till fecundity

disappears, the last production being often a premature or a dead fœtus. The pluriparous animal has its best young when its progeny is most numerous. The best young may be so described, as in pups, on account of their intelligence, docility, or special talents; or they may, as in a litter of pigs, be best because they are large and easily made to grow to great bulk or weight. In the case of the bitch, it is impossible to reduce to an exact statement the value of pluriparity, but it is, no doubt, very great; and while it is the case that when most in number are produced, there is also most in weight; the statement of weight of the pups gives no idea of their value. In a litter of pigs, the value of pluriparity is a simpler matter, being estimated almost entirely by weight and capability of rapid growth; and both may be very well stated in figures.

The uniparous mare has a foal which may be valued partly for bulk, especially if it is to do rough, heavy, work; but the bulk of a foal bred in the racing stud is a matter of comparatively little moment; and I daresay all will agree that the nobler the breed of horses, or the higher the qualities expected in them, so is bulk in the foal of less and less importance, and so also is pluriparity less and less desirable.

We have already used estimates of weight and length of single children as indications of fertility in woman; and if weight and length of twins were a test of paramount import, then twinning would,

correspondingly, connote fertility, as 12 lb. exceeds $6\frac{1}{2}$ lb. or 7 lb. But there are higher qualities than the combined weights and lengths, and it is these higher qualities that are deficient in twins. Weight and length are valued merely as indications of general health and full development of individuals; not of twins as compared with single children.

Pluriparity in uniparous animals is rare, and for its study great accumulation of instances is required; and knowledge regarding it in these animals is tardily gained. On the other hand, pluriparity in some common domestic animals is an every-day matter; and without any deliberate study its variations strike even the obtuse, a class often specially sensible of the pecuniary advantages of the higher degrees of pluriparity. It is the striking characters and advantages of high degrees of pluriparity in pluriparous animals that have led to the general adoption of the erroneous opinion that pluriparity even in the uniparous animals, as in woman, is an unqualified sign of fertility.

In pluriparous animals, especially in the common hen, the quick rise and more gradual decline of fecundity is plainly observed, the climax in the hen, as in other pluriparous animals, being marked by the highest number of annual production or in a single brood or litter. In woman there is the same kind of variation, but in her it is a decline from

occasional pluriparity to the production with due intervals of the best kind of single births; and the rise is back again to occasional pluriparity and hurry of births one after another.

In the common hen the rise to the climax occupies three years of life, and the more gradual decline occupies six years, according to Geyelin's data, already given. In woman the decline to the lowest, if we count roughly, from fifteen to twenty-five years of age, occupies ten years, and the more gradual rise, from twenty-five to forty-five, occupies twenty years. In the hen the rise is from 15 to 135, and the decline from 135 to 1. In woman the decline is from about 1·02 to 1, and the rise again to about 1·02. There can be little doubt that a similar rise and fall, or fall and rise, are to be found in the history of the fertility of other living things. The curve of this climax and anti-climax is not a part of a circle. Dr. Routh, in a valuable paper on "Procreative Power," published in the *London Journal of Medicine* for 1850, describes this curve, representing what he calls the inclination of procreative power, and thinks the circle is perhaps the nearest that could be selected; but the circle cannot be made to represent the figures on which he relies. He makes the age of greatest fecundity in woman twenty-six; and the climax and anti-climax may be partially indicated by the following figures, which he gives:— At fifteen years of age the figure is 22; at twenty it

is 82 ; at twenty-six it is 100 ; at thirty it is 92 ; at thirty-five it is 74 ; at forty it is 54 ; at forty-five it is 39.

In leaving the subject of twins, it is natural to refer to malformations and monstrosities as showing weakness or disorder of the reproductive powers; but on this point I have no good detailed evidence to adduce meantime. Yet it is well known that a great body of opinion is in favour of the view, and there are many facts pointing in the same direction. In the course of these Lectures I have frequently mentioned such opinions and facts, but the subject is well worthy of special study. Here I would only refer to the frequent combinations of idiocy and malformation, of idiocy and twins, of idiocy and premature or post-mature maternity, of malformation and twins, of interbreeding and malformation, of interbreeding and sterility, as combining to form an argument that may, if worked out, be found to be conclusive on this question.

Experiments in producing malformations and monstrosities in the common fowl have been very fruitful in results, and demand caution in judgment as to the potency of such influences as age of the mother. Especially interesting in this view is the recent discovery of Dareste that mere delay of incubation, in the case of the eggs of the common fowl, is a cause of malformation in the chick.

Excluding some remarks as to the influence of marriage in causing sterility in women, we have shown chiefly the influence of age in its production. Marking out by statistical evidence certain ages as peculiarly affected with sterility, we find at these same ages, in a proportion above the average, excessive families, pluriparity, weakly or idiotic or monstrous children; and not only in exaggerated proportion, but combined one with another. It is therefore reasonable to describe the sterile ages as ages of imperfect reproduction, and to associate or identify with sterility the conditions of excessive production, pluriparity, &c., which are demonstrated to have alliance with it. In other words, sterility, excessive families, and pluriparity are alternatives one of another, and almost certainly own the same general causes.

I know no cause of sterility or of its allies, excessive production, pluriparity, abortion, &c., that can be compared with age in extent and power. In discussing the cure of sterility, I shall allude to various minor causes, which may operate in individual cases, but have no extensive influence. But there are causes which probably have a great place in the production of this condition, whose action is only believed, not demonstrated. Such are bad general health, cold, and heat. The influence of bad general health is well observed in plants, but I know no good evidence of it in woman other than the testi-

mony of medical practitioners. The influence of cold and of heat on sterility has been much studied, and attempts have been made to get additional light on the matter by collecting observations of their influence on the age of commencement and cessation of menstruation. The subject divides itself into two portions: first, the influence of cold and heat on women breeding in their native lands; second, the same influence as exerted on women born in cold climates and transported to hot, or born in hot climates and transported to cold. But the data obtained are, in my opinion, quite insufficient for any reasoning being securely based. The hearsay evidence also requires scrutiny. We often hear, for example, of an Indian girl, say of eleven, bearing a child, and this is held as proof of early fecundity there. We rarely hear of the same occurrence in this country; and the reason of this alleged greater frequency in India may be, not earlier fecundity there, but earlier exposure of a large number of girls to the risk of becoming pregnant.

There are several important subjects, more or less closely bearing on our inquiry, which I pass by with mention only. Among these is the influence of cold and of heat on the commencement and stoppage of menstruation, an influence regarding which it is scarcely, by the statistical evidence, made probable that cold retards the appearance and hastens the stoppage, though many considerations support this

view. Another is the generally accredited influence of nursing in delaying the return of menstruation and the recurrence of pregnancy. Regarding these matters, Roberton has made valuable remarks, and collected many, though insufficient, observations. The great subject of interbreeding in its production of sterility I also pass over. The evidence regarding it is very bulky, and requires most careful sifting. In plants and animals the demonstration of this injurious influence of interbreeding in producing imperfection of offspring and sterility is copiously illustrated, and may be said to be well made out; but it is not so in the case of man. Yet, in the case of man, there is a most extensive, though not universal, consensus of intelligent opinion that interbreeding has the same general influence as in plants and animals; and to the entertainment of this view, the strong analogy of plants and animals lends powerful encouragement. The injurious influence in man, indeed, acts after birth, in special ways, other than by mere feebleness, for there is accumulating evidence that peculiar diseases, especially of the eyes, affect, by preference, the offspring of near relations.

"The evil consequences," says Darwin, "of long-continued close interbreeding are not so easily recognised as the good effects from crossing, for the deterioration is gradual. Nevertheless, it is the general opinion of those who have had most experience, especially with animals which propagate quickly, that

evil does inevitably follow sooner or later, but at different rates with different animals. No doubt a false belief may unduly prevail, like a superstition; yet it is difficult to suppose that so many acute and original observers have all been deceived at the expense of much cost and trouble. . . . . The loss of fertility, when it occurs, seems never to be absolute, but only relative to animals of the same blood; so that this sterility is, to a certain extent, analogous with that of self-impotent plants which cannot be fertilized by their own pollen, but are perfectly fertile with pollen of any other plant of the same species. The fact of infertility of this peculiar nature being one of the results of long-continued interbreeding, shows that interbreeding does not act merely by combining and augmenting various morbid tendencies common to both parents; for animals with such tendencies, if not at the time actually ill, can generally propagate their kind. Although offspring descended from the nearest blood relations are not necessarily deteriorated in structure, yet some authors believe that they are eminently liable to malformations; and this is not improbable, as everything which lessens the vital powers acts in this manner. Instances of this kind have been recorded in the case of pigs, bloodhounds, and some other animals." "In the case of man," he elsewhere remarks, "the question whether evil follows from close interbreeding will probably never be answered by direct evidence,

as he propagates his kind so slowly, and cannot be subjected to experiment; but the almost universal practice of all races at all times of avoiding closely related marriages is an argument of considerable weight, and whatever conclusion we arrive at in regard to the higher animals may be safely extended to man."

Leaving several minor or little-known causes of sterility to be mentioned in the next Lecture, I now turn to other matters in its history which throw light on its theory, and there are two worthy of great consideration. These are the well-known association of dysmenorrhœa with sterility, and the state of sexual appetite and sexual pleasure in sterile women.

Menstruation, when natural or healthy, is attended with no pain, and with little or no disturbance of general health. When there is pain or considerable disturbance of health, the condition is called dysmenorrhœa, and it is plain that the term covers a wide and ill-defined field of disorder and disease. It is with dysmenorrhœa, as thus vaguely defined, that sterility is prevalently believed to be very frequently associated; and there can, in my opinion, be no doubt of the truth of the general belief.

There is a kind of dysmenorrhœa regarding which I would enter into more details. It is called spasmodic, being regarded as a neurosis, characterized

by painful uterine spasms, which may be described as having no known object in view. It is often called mechanical or obstructive, terms implying a theory of its cause, and implying also that the spasms are, so to speak, intended for the expulsion of the menstrual fluid accumulating in the uterine cavity and distending it. There is no good evidence of the mechanical obstruction, nor of the accumulation of menstrual fluid, nor of the dilatation of the uterine cavity, nor of the use of the painful uterine contractions; and, as all admit the presence of these contractions or painful spasms, I call this kind of dysmenorrhœa spasmodic. It is a kind of dysmenorrhœa that is gradually, and I think justly, restricting to itself alone this term—the only real, positive, recognizable uterine dysmenorrhœa, or the dysmenorrhœa proper.

It is of this dysmenorrhœa proper that I am now to speak, and it is known by the following characters. It may occur at any time during the flow of menses, sometimes even before it begins; and, in cases of amenorrhœa, it may occur at the time of the menstrual molimen. In the very great majority of cases it occurs on the first or second day of the flow, and it is generally severer when the flow is scanty than when it is copious. The pain is constant, or in pangs, and the pangs may be more or less distinct; in other words, the intermissions of the pain may be more or less complete. The frequency of the

pangs varies, five to ten in the hour being common. The pain is rarely accompanied by bearing down, strangury, or tenesmus. It varies in severity, rising occasionally to the intensest agony, with cold sweats, vomiting, and other symptoms of prostration or collapse. Suffering from it, the patient often rolls about and groans, and the restlessness is not that of fever, but of griping pain. It may last only a few minutes, but generally it goes on for hours, the number of hours rarely exceeding four or five. It rarely returns during the current menstrual period. It is generally aggravated by marriage. In women who suffer from this disease, there is a super-sensitive condition of the interior of the body of the uterus, and, I think, especially of the internal os uteri, this condition being tested by the contact of a uterine probe or sound.

In making inquiries as to the connexion of this dysmenorrhœa proper with sterility, I have frequently, but not always, satisfied myself of the presence of all of these characters. Particularly, I have not classed with this dysmenorrhœa any case in which the severe pain lasted more than a day. In all inquiries as to pain, there is, owing to the indefiniteness of language, and the tendency of patients to exaggerate or make light of their troubles, extreme insecurity of statistical statements. I have tried to avoid being misled in 332 cases which I have, during the last five years, taken down in my

notes. These 332 cases were all absolutely sterile—that is, all women who had had an abortion or a child are excluded. Of these 332 married women, 159 suffered from spasmodic dysmenorrhœa, or nearly half. It is a most grave fault in my argument that I unfortunately cannot give the frequency of dysmenorrhœa among the fertile. But I can, meantime, only declare the importance of the omission, and express my belief, in accord with universal professional opinion, that, among the fertile, dysmenorrhœa is comparatively uncommon. The connexion of a neurosis of this kind with sterility cannot be unimportant, and I cannot leave the subject without expressing my belief of the association of it with abortion and miscarriage also.

Other mutually allied neurotic conditions demand full consideration—namely, sexual appetite or desire, and sexual pleasure or satisfaction of the appetite by coitus. In investigating the matter great difficulties are met with, from the delicate nature of the inquiry, the difficulty of making sure that the patient understands clearly what is the question to be answered, and the impossibility of finding words of well-defined meaning, or of the same meaning in different mouths. But these difficulties are not insuperable, and error is lessened by relying on a large number of concurring observations.

Sexual desire and pleasure have to be considered

separately, because, though they are naturally found combined in the same case, they are far from being invariably so. A woman, with healthy sexual organs, may have sexual desire and no pleasure, or even the reverse; and she may have no desire, and yet have pleasure. Although pregnancy and childbearing are natural consequences of sexual desire and pleasure, there is little or no connexion between the latter and the wish to bear children. The desire for offspring may be intense, while there is neither desire nor pleasure, and the desire to avoid pregnancy may be intense while there is desire and pleasure. Desire and pleasure may be excessive, furious, overpowering, without bringing the female into the class of maniacs; they may be temporary, healthy, and moderate; they may be absent or null. Instead of sexual desire there may be sexual aversion; and instead of sexual pleasure there may be only feelings of disturbance or pain. Instead of sexual desire there may be intense sexual antipathy, and instead of sexual pleasure there may be severe suffering, even agony, in coitus.

The variations of desire are chiefly on the positive side, greater or less. Desire may be absent. From the zero, or indifferent condition, there is, however, not rarely observed a rise into aversion or antipathy; and this, in married women, without any feeling regarding the husband other than affectionate. It is well known that desire may be fostered at special times by various stimulants of passion; but, apart from

such occasions, it may be increased or diminished or annihilated. This is a general belief, and I have frequently had spontaneous testimony of individuals to the same effect. The influence of society and its amusements, of diet, of special kinds of reading, of association with males, is well known and recognized in the increase of sexual desire; and the influence of the opposite conditions—of a truly ascetic life—is equally certain. Desire may, during the childbearing period of life, undergo great changes without any apparent cause; at one time, and it may be for years, being positive, at another time absent or negative.

Sexual pleasure must not be regarded as in all respects like sexual desire, and requires separate description. Its variations are chiefly on the positive side. It may be absent. Its variations on the negative side are, however, most remarkable. There may be slight or very great suffering, or the intensest agony; and this is often accompanied by more or less active involuntary local sphincteric resistance to penetration, called vaginismus. But the words pain and agony are here used in a quite extraordinary and misleading way. There is no pain, such as that of the infliction of a wound or contusion, or that of toothache or neuralgia. There may, indeed, be, in cases of diseased sexual organs, common pain of the kinds mentioned, caused by sexual congress; but of such pain we are not here speaking.

All kinds of pain or discomfort in coitus are often,

nowadays, classed as dyspareunia; but I think the word may be well restricted to the condition I am describing; or the condition may be called simple dyspareunia, and there is no common pain in simple dyspareunia. It has an analogue in disgust, but dyspareunia rises to far higher degrees than disgust. As sexual pleasure rises in intensity above all other kinds of pleasure, so dyspareunia reaches degrees exceeding those of the intensest disgust. The disgust of a child is often painfully intense, its resistance to tasting and swallowing involuntary and powerful, and often followed by vomiting the matter whether tasted or not; and as all this is not common pain in tasting and swallowing, so dyspareunia is not pain in sexual connexion. Sexual pleasure and dyspareunia differ from gustatory pleasure and disgust in this, that while the former are one in kind, and in all degrees excited by the same cause, the latter are various in kind, and elicited by different substances in each case. Pleasure, then, may vary from the intensest to mere indifference; and simple dyspareunia may rise from mere indifference to the highest degree, with sphincteric resistance to penetration, opisthotonos, and a state almost of insensibility.

It is to be remarked that, till sexual connexion occurs or is attempted, a woman does not know that she has simple dyspareunia; and it is only thereafter that she has dread of and pain from touching

the parts near the vaginal orifice. A dyspareunic woman suffers great, it may be intolerable, pain from examination by the physician. It is not sexual contact alone that produces the pain.

Pleasure is probably not directly increased by the causes of increase of desire, but the increase of desire is probably a cause of increase of pleasure, as hunger enhances the pleasure of taste. Pleasure is increased by continence, and diminished, or annulled, or converted into slight dyspareunia by over-indulgence. Sexual pleasure may vary without apparent cause, disappearing for short periods or for years, and reappearing with the same appearance of caprice. Pleasure is frequently absent at marriage, and gradually developed during the continuance of that state. If it is slight at marriage, then coitus will be painful, the common and not simple dyspareunic pain overpowering the pleasure and preventing it.

Describing the lower animals in this respect, we guess, by aid of analogy; but the analogy is so strong as to endow the guess with a high degree of assurance, reaching nearly to certainty. We may be sure that animals generally feel sexual desire, and that this sexual desire occurs normally or naturally only in connexion with fecundity. In many domestic, or otherwise well-known, animals, there is sexual desire only in the rutting season, and at other times not only an absence of sexual desire, but a positive sexual antipathy. A bitch not in heat

will angrily resist any attempt at sexual approach by the male, while quite ready for any other kind of play. Of sexual pleasure in female lower animals we know very little, but we may be sure it exists. Of its existence in males we have abundant evidence, and we may thence argue that it exists in females. Nothing is commoner in dogs than what may be called masturbation. This kind of sexual pleasure is generally believed to be increased by confinement, and the evidence afforded by zoological collections is held to be good.

I know nothing regarding the connexion of sexual pleasure in animals with fertility or sterility, but we have the testimony of Darwin to the presence, in animals that are confined, of sexual desire, sometimes in excessive degree, sexual indulgence being held as evidence of sexual desire, and the sexual excess is often connected with sterility. "Monkeys," says he, quoting from the Nine-Year Report from the Zoological Gardens, "are stated to unite most freely; but during this period, though many individuals were kept, there were only seven births." Elsewhere he says, that "although many of the felidæ breed readily in the Zoological Gardens, yet conception by no means always follows union. In the Nine-Year Report, various species are specified which were observed to couple seventy-three times, and no doubt this must have passed many times unnoticed; yet from the seventy-three unions only fifteen births

ensued." In many animals under confinement there is no coupling, nor any attempt at it; and this may be assumed to indicate absence of desire in female as well as male.

It is an almost universal opinion that in women desire and pleasure are in every case present, or are in every case called forth by the proper stimulants. The opinion is founded on experience, and it is, no doubt, nearly true; but the exceptions to the rule are numerous and important. It is also a popular opinion that desire and pleasure are essential elements in fecundity, and in cases of rape followed by pregnancy, that consequence has been made ground of defence against the charge. Great authors, among whom is Ambrose Paré, recommend the excitement by dalliance of great desire as a remedy of sterility.

I think it is very nearly certain that desire and pleasure in due or moderate degree are very important aids to, or predisposing causes of, fecundity, not on account of their own proper attractiveness, but on account of some connexion between them and the perfection of other parts of the complicated proceedings which result in fecundation. But this is only a firmly held opinion, for I can give no conclusive evidence or proof of it; and this absence of proof diminishes greatly the value of my observations on the absence of desire and pleasure in the sterile.

The want most acutely felt here is a knowledge of the state, in this respect, of the fertile. In producing evidence as to the sterile, I shall assume that sexual desire and pleasure are very rarely absent in the fertile.

Excess of sexual desire is probably unfavourable to fertility. It is recognised chiefly by excessive indulgence in sexual pleasure, and is observed in the weak and ill-conditioned, in imbeciles and idiots, as it is also in animals under confinement. Excessive indulgence in sexual pleasure is also probably unfavourable to fertility, or a cause of sterility; and it probably is specially influential in the young, as it may also be in prostitutes. In these circumstances the births of females are, on good grounds, believed to be far above the ordinary average, in proportion to males.

Masturbation in females is an unnatural and generally excessive indulgence in artificial sexual pleasure. It has always appeared to me to affect especially children and young women of weak mind. I have often been struck by the smallness or imperfect development of the external parts in young women who masturbate, and I have not rarely observed what appeared excessively high development of sexual desire in women who had imperfection or absence of internal genital organs. In one, dissection revealed the presence of ovaries and Fallopian tubes only. Some confirmation of these

views may be found in cases such as that of Campbell,* in which a woman addicted to masturbation had never menstruated, and had imperfectly developed genital organs; she had, however, also a dermoid cyst of the ovary. Aran† has a case, of what he describes as frightful excess of masturbation, in a young woman dying of phthisis, whose uterus and appendages were found to be very imperfectly developed. Kussmaul‡ mentions the concurrence of masturbation and nymphomania with imperfect development of the uterus and the genital organs; and Joulin§ refers to a case of Vaddington's where absence of uterus and exaggerated sexual appetite were combined.

Entire absence of desire and pleasure, or of one of them, or the presence of intense sexual antipathy and dyspareunia are not necessarily causes of sterility. It is not very rare for women to be pregnant and bear healthy children who aver in the distinctest manner not only absence of desire and pleasure, but presence of the opposite conditions. But the following statistics make it highly probable that absence of desire and pleasure and the presence of their opposites are powerful influences favourable to sterility. The statistics do not indicate what was occasionally found—namely, that desire was present

* "Memoir on Extra-uterine Gestation," p. 30.
† "Leçons Cliniques sur les Maladies de l'Utérus," p. 89.
‡ "Von dem Mangel, &c., der Gebärmutter," S. 74.
§ "Accouchements," p. 138.

while pleasure was absent; or in other and comparatively very rare cases, that desire was absent while pleasure was present. The cases observed were all in women absolutely sterile, of whom the great majority consulted me regarding the sterility. Among 191 sterile wives desire was absent in 39, or in about 1 in 4. Among 196 of the same sterile wives pleasure was absent in 62, or in about 1 in 3. The figures show that many sterile wives had desire but no pleasure. They do not show, what nevertheless is true, that some had pleasure who had no desire.

TABLE XXIII.

Case-book Table of Desire and Pleasure in Sterile Women.

Average age at marriage.	Number.	Desire.			Pleasure.		
		Present.	Absent.	No note.	Present.	Absent.	No note.
15–19 .	59	18	4	37	15	8	36
20–24 .	220	78	18	124	69	27	124
25–29 .	134	35	12	87	31	18	85
30–34 .	59	16	3	40	14	5	40
35–39 .	23	3	1	19	3	3	17
40–45 .	9	2	1	6	2	1	6

I have a strong impression, derived from all I know and have observed, which I may express theoretically, that while in healthy normal women there is abundance of sexual or reproductive energy for fertility and all its accompaniments, in many sterile, or relatively sterile, women, there is deficiency which may be exhibited in one or another, or in all the ordinary evidences of reproductive energy, and

that excess or deficiency in one department may be associated with deficiency or excess in another. It would seem that in women of deficient reproductive energy, excess in one department may be compensated by deficiency in another, and *vice versâ*, there being only a limited store of the original energy. In illustration, a remarkable class of cases may be cited, which I shall sufficiently describe by stating generally the chief points in one :—A robust healthy woman is married at eighteen ; she bears three children and has four miscarriages before she has passed twenty-three years of age. Up to the birth of her last child, and for five years subsequently, she experiences no sexual desire, and has no pleasure. Five years after her last pregnancy she almost suddenly comes to have intense desire and pleasure, but remains sterile for four additional years before she seeks a cure of her sterility. Fertility present, while desire and pleasure are absent: sterility present, while desire and pleasure are present.

LECTURE III.

PREVENTION AND CURE.

As in past ages, so also in modern, sterility has been an object of great interest, of study and of experiment. The acquisition of wealth has at all times stimulated the agriculturist, the gardener, and the breeder; and the desire of offspring has no less stimulated men and women. At no time has the subject had more importance than at present, for the growth of science and the love of daring speculation bring now on the field a class of men of trained intellect, who invade it, not to make money or secure offspring, but in search of knowledge. It is to such men that Nature opens her secrets, and the divulging of truth through them is the just pride of philosophy.

A true theory of sterility, even though it be lamentably incomplete, is of very great importance in medical practice. Thousands of women are seeking what they call cure, and their advisers should surely take care to know what they can offer in return for the confidence placed in them. According as medical men have their course illuminated by knowledge, so will they be wise in advising; and if increase of

knowledge, acting directly or by dispelling illusions, destroys faith in remedies, it may yet, in this negative way, add to the usefulness of the adviser. It has been said by Brodie of John Hunter that by teaching us when we are not to interfere with the ordinary course of events he has contributed more towards the advancement of the healing art than all the inventors of remedies who had gone before him.

The course and the details of the argument in these lectures point to a law or laws of sterility not yet clearly formulated; and it is to be expected that progress will be obtained from inquiries such as have been here described, as well as from investigations of the intimate state of the reproductive organs, including those parts of the nervous system which govern them or are governed by them.

Deficient reproductive energy or want of sexual vigour is a theory too vague to be satisfactory. It is only a general idea which loosely binds together, meantime at least, the items of knowledge we have acquired as to sterility. Of course it is a general idea to whose entertainment no known fact is hostile. But it is flimsy, like a ghost, and a fact might find it difficult to prove its steel; for, like a ghost, it might be cut by a sword without being destroyed or even damaged in the eyes of those who see it. Deficient reproductive energy is held to be shown by all the conditions which produce or which attend sterility in plants, animals, and man. In

woman it is shown by absolute sterility, by relative sterility, by excessive production, and by imperfect production, which may be abortion, or miscarriage, or morbid pregnancy, or children diseased or difficult to rear, or destined to peculiar diseases during extra-uterine life. Deficient reproductive energy cannot be regarded as a substantive disease with specific characters, course, and remedies. It is a constitutional condition, which, according to its cause, may affect a population or affect certain classes. Cold or heat may render a whole population sterile. Underfeeding or overfeeding, or premature or post-mature marriage, may cause sterility in certain classes within a population. Sterility, the result of deficient reproductive energy, is an imperfection which does not show itself by measurable, tangible, qualities such as a dwarf exhibits, but by absence of function, or a stunted or otherwise imperfect performance of function.

The consideration of the great causes of sterility, exhibited as they are in their results in populations and in classes of women, makes it almost certain that local causes, whether acting as impediments to conception or as unfavourable to pregnancy and to intra-uterine life, have very little scope for operation. These local causes have a clinical interest as affecting individuals; for they have not been supposed, far less shown, to have any connection, or even accidental association, with the great causes whose scope is wide

and certain. In the production of cancer of the womb there may be great operating causes, such as age and multiparity, and there may be minor local causes, such as the so-called ulceration of the cervix uteri and its injudicious treatment; and these minor causes, although doing little harm to a population or a class of women, may be of the highest practical importance to individuals.

In women, the chief and best demonstrated sources of, or attendants on, sterility are juvenility or prematurity, elderliness or post-maturity, dysmenorrhœa, and disorder of sexual appetite and pleasure. Of these the influence of age has been most fully shown, and it is that which is most under control with a view to prevention.

As in constitutional disease or epidemic fevers, so here, the good done by prevention immeasurably exceeds, or may immeasurably exceed, any possible good by cure; and this whether the good done is to a population or class or to an individual. The superiority of prevention is partly because the good is to a population or class, not to mere individuals.

Prevention is to be, in part, effected by avoidance of unions of immature women or of elderly women; in other words, by securing that women are married at the age of nubility, or best age of marriage, with a view to fertility and the rearing of healthy children and the safety of the mothers: and this age is fairly

well ascertained to be, for a population or mass of women, not under twenty and not above twenty-five.

In the breeding of domestic animals and of animals in confinement, man can interfere easily and without restraint, except from his own interests, but it is otherwise in woman. She enjoys liberty within wide limits, yet she is more or less subject to the restraint of social, moral, and religious law or custom. These restraints diminish the power of the medical adviser to guide; and, in general, he can do most good by diffusing knowledge as to the prognostics from marriage entered into under various conditions.

At present the law of England legitimizes marriage at a very early, a too early, age; and it, wisely no doubt, does not interfere with late marriages. "Without the sanction," says Major Graham, "of the laws of physiology, or of common sense, a girl may—but in the present day rarely does—marry at the age of twelve, a boy at the age of fourteen, under the existing laws of England; but the consent of parents and guardians is required in certain cases where either party has not attained the age of twenty-one; and the proportional number of either boys or girls who marry under the age of twenty is happily small. . . . The age," he adds, "of marriage cannot be directly fixed by laws; but legislation, by prescribing the minimum age of marriage, and the age of majority, does exercise a considerable influence

on good numbers of the people directly, and on all indirectly. It becomes the custom or the fashion not to marry below the age of majority. Thus in England about 9,000 young persons of the age of twenty and under twenty-one married in the year 1851; while about 139,000 married in the four years after they were of age, as it is called, or in the years of age, twenty-one to twenty-five. The age of majority is twenty-five years in France; and the age of twenty-five divided the minores from the majores in Roman law. The advanced age of majority, or of what becomes practically the lowest age of marriage, retards marriage indefinitely in many cases, and will probably be found, on investigation, to account, at least partially, for the comparatively small number of children to a marriage in France. By raising or depressing the age of majority, the Legislature then has the power to exercise considerable control over the population." These remarks of Major Graham are valuable in themselves, and indicate the view taken by a politician. The law of majority has no doubt great influence, and by it the State can modify the age at marriage to some extent; but the laws of love, of self-interest, and of social convenience are much more powerful.

The sterility of near relations, of interbreeding, or of breeding in and in as it is often called, is generally recognised, though far from well proved in man, and

forms what seems a contradiction in terms, an inherited sterility. It is believed to be shown not only by absolute sterility and its accompaniments, but also by the production of idiots and ill-formed children. Restraint by knowledge of these risks of intermarriage is no doubt a powerful preventive of sterility, but not so potent as it ought to be.

There is, as already pointed out, a sterility dependent on some inscrutable incompatibility of the parties, as in Augustus and Livia, Napoleon and Josephine. Cases like the following are not very rare, and I have actually observed them. A man marries successively three childless widows and has children by each of them. A woman is married successively, and within childbearing limits, to three men, and has children by only one of them. Such cases, if very rare, might carry little weight, but they are so common as to have occurred within the knowledge of most observant people. Sterility of this kind we cannot foresee and prevent; and religion, morals, and law combine to interdict the cure that might result from a change of husband. Unfortunately, however, among large classes—chiefly, I am told, in Wales and some parts of Scotland and of Switzerland—custom permits, and local morals do not interdict, a practice which produces many illustrations of this mutual incompatibility. The practice is called bundling, or keeping company, or hand-

fasting, and consists in parents or guardians permitting daughters to cohabit with an eligible man on the understanding that, if pregnancy ensues, the legal marriage tie is made. A woman proving sterile may be deserted by her follower, and gets another with whom the result is different. But it is not to be supposed that it is, always, with a view to fertility that the first one is followed or another one sought.

In ancient times much was known and taught regarding the avoidance of sterility, and most of it was in accord with what is still taught, but little was done with a view to the cure. The physiology of reproduction was little advanced, and its primary or elementary conditions were quite unknown. While certain winds were believed to cause sterility, and fecundation was supposed to be effected by an aura seminalis, we could not look for rationality in practice. Accordingly, such cures of sterility as were then practised appear to us ridiculous or fantastic.

In modern times the physiology of reproduction is comparatively far advanced, and the necessity of the physical conjunction of the male and female elements is especially recognized. But it may be doubted whether the cures of sterility are much more rational than those of the ancients, for the laws of sterility have been investigated with no great amount of success; and especially do we remain uncertain as to

the physiology of the conveyance of the spermatozoa to the Fallopian tubes.

During the last thirty years gynæcology has made great and rapid strides of substantial progress, and naturally sterility, as part of it, has swollen in bulk; but the growth of it has not been satisfactory, for it has not a sure foundation. While our general knowledge of sterility in woman has made little advance, and especially that part of it which might be turned to practical account, the curing of sterility has reached great dimensions. As in other departments of therapeutics, there has been a great failure of logic; the *post hoc* and the *propter hoc* have been confused —a coincidence has been regarded as a consequence. The credulity of patients and of doctors has been a basis for useless and often injurious practice.

It is scarcely an exaggeration to say that, in recent practical works on sterility, there is exhibited entire ignorance or entire neglect of the laws of fertility. Every woman from fifteen to forty-five is regarded as almost sure to breed. If she is sterile, a cure is at once set agoing; and if a child is not born, the failure is not debited to the nature of the case, but to the want of ingenuity in the doctor. A reputation for curing sterility is spoken of as if it were founded on substantial claims!

The prevalent methods of curing sterility are founded on an implied theory—that it in most cases

arises from impediments in the way of the spermatozoa reaching the ovum. Without sufficient evidence, strictures are assumed to exist, versions and flexions of the womb are held so to distort the interior passage as to prevent progress of the spermatozoa, cervical catarrh is believed to stop them by mechanical obstruction, or by chemically poisoning them; and for these real or imagined evils sterile women are made the subject of treatment. It is the theory of mechanical obstruction that by its simplicity and directness has possessed the profession and the public, and accordingly many operations and modifications of operations, and very many instruments, have been devised to do away with the obstruction. The theory has had real rational support, in the fact that dysmenorrhœa of a spasmodic kind does, as already shown, frequently accompany the sterility, and in the supposition that the same obstruction which causes sterility by impeding the entrance of semen causes also dysmenorrhœa by impeding the exit of menstrual blood, or *vice versâ*. It has had still more satisfactory support in the observation that the cure of the dysmenorrhœa does occasionally bring with it cure of the sterility. The very zeal with which the mechanical theory of sterility has been fostered, and its treatment in many ways pursued, has led to its present decadence, and there is now increased attention paid to other departments of fecundity than conception. Especially and justly, the difficulties of

naturally starting and healthily continuing pregnancy are brought prominently into view. The mechanical obstruction theory has begun to shrivel, because of the impression produced by the enormous, though inexactly ascertained, proportion of the failures of the attempts to cure founded on it. Even the ignorant sterile women could see that, if the theory of causation were true, there was an easy and plain theory of cure, and they could also see that the failure of the so-called cure was prejudicial to, if not destructive of, the theory. The importance of the difficulties of pregnancy now brought into prominence will, on account of its great reconditeness, be received with no enthusiasm, such as welcomed the obstruction theory, and the physicians who entertain it can offer no such brilliant prospects of cure to their confiding patients. It is, however, a decided step of progress in a subject of great practical importance.

It is in Germany that this department of sterility has been chiefly studied, and Grünewaldt of St. Petersburg is its best exponent. Recognising the importance of this work, I take the liberty of using it to show the great incompleteness of even the most advanced accounts of the subject. For Grünewaldt, sterility is truly never a disease, but a symptom of a disease. Nature has, he says, set no limits to female breeding other than the natural changes in the sexual organs that are observed in the senile state. Sterility is, for him, one of the most frequently

occurring disturbances of function caused by diseases of the female sexual organs. In these views, and in his whole work, it is implied that sterility depends on disease of the sexual organs, including chiefly endometritis, mesometritis, perimetritis, and parametritis. The difficulties of conception, he says, have only a slight importance compared with the disorders of the more important vital processes of pregnancy, and these disorders affect chiefly the tissues of the uterus.

It would involve a useless recapitulation of the substance of these lectures were I to set about showing how partial and imperfect is that theory of sterility which makes it depend on local disorder or disease, whether the disease impedes conception or interferes with the progress of pregnancy. Taken together, the obstruction theory and the theory of Grünewaldt do but cover a small part of sterility,which may be described as the part affecting scattered and sparse individuals, and giving thus its importance to these individuals, and to their advisers.

The obstruction theory and the theory of Grünewaldt make no room for that kind of prevention which we have described as of paramount importance. On the other hand, they open up great, indeed almost unlimited, fields for the activity of curers. But the failures of curers is so notorious and the curing of sterility has so bad an odour in the nostrils of many, probably of the majority, of the best in the

profession, that it is worth while to ask the question, Is sterility curable?

Before this question there comes another which is of great importance, Should sterility be cured, as it is called? That, in the interest of the community, it should be prevented, I have no doubt; but, in this department of the subject, statesmen and economists have taken much interest, and I shall not meddle in it. I am of opinion also that it should, if possible, be cured. Yet a good argument may be made out for not curing it, in many cases at least. For the laws of sterility show that if it is, what is called, cured, there is a risk of some of its alternatives or attendants—morbid pregnancy, abortion, miscarriage, weakly children, excessive family, death of the mother, and others. But the practitioner hopes, by appropriate cares, to conduct his patient and her offspring in safety through these perils; and we do not, meantime, feel disposed to cavil with this perhaps over-estimated view of his rational expectations.

It will be admitted that reputation, even with well-informed medical men, is not sufficient to prove the reality of a so-called cure, and we are constantly meeting with instances of exaggerated credulity in reported cures of young women married between twenty and twenty-five, and who had not lived three years in the married state; for it is common for such ardent young women to thus prematurely regard themselves as doomed to persistent sterility, and seek advice

with a view to averting their dreaded fate. But there can, I think, be no doubt that sterility is often cured, and such cases as the following do all but absolutely prove that cure is possible; and the sufficiency of the proof will not be controverted by any one if it is added that such cases, though rare, are sufficiently numerous to prevent by their number, apart from their other circumstances, the confusion of a coincidence with a consequence.

A. B——, married at twenty years of age, menstruated regularly since thirteen, had dysmenorrhœa most of her life, but not very severe; had never been pregnant. Had had no uterine treatment till now, when cervix was canalised by bougies in the usual way, twenty-two years after marriage. No known change was made in her conjugal or other habits. She became pregnant at once after the treatment, and had a living, healthy, child at forty-two years of age. Now, five years after the birth, pregnancy has not recurred.

C. D——, married at nineteen years of age, began to menstruate at thirteen, and was regular, with pain for a short time on the first day. After fifteen years of married life, had had no pregnancy. Had had much uterine treatment. Cervix canalised by bougies, and, for the first time, according to her. No change made in conjugal or other habits. On resumption of cohabitation, two months after the treatment, became pregnant, and had a healthy child at thirty-five

years of age. Since this birth three years have elapsed, and she has been thrice pregnant.

It is, however, desirable to go further than merely prove that cure is possible, that a cure has been effected; and I believe the most important means of curing sterility or relative sterility is improvement of the general health. In the case of plants, the value of digging about and dunging is well known, and so is the value of proper exposure to the sun, and so is the value, and, indeed, the necessity of good air, not the air of large cities; and the use of these, when previously withheld, is certainly curative of sterility in many kinds. The cure is sometimes, as in apple or pear trees removed from the shady side of a wall to a better exposure, accompanied by other changes in leafage and in growth of wood, which make better general health evident to the eye. But the cure may have no accompaniment of other signs of better general health, for some London trees which are sterile have a fine outward show of healthy vigour, and it can scarcely be doubted that return to a purer atmosphere would restore their fertility, though it could do little to improve their appearance. In the case of animals, a similar influence of general health may be noted. The starving of fowls diminishes or even arrests their fertility. We cannot doubt that the agouti, released from confinement and restored to its natural habitat, would produce healthy offspring

instead of dead and ill-formed; and that, similarly treated, the lioness would have cubs without cleft-palate.

In the case of woman, the restoration to, or improvement of, general health, involves such a variety of considerations as renders it very difficult of treatment, and the whole matter comes as much under the care of the general physician as of the gynæcologist. But it may be mentioned that special means have been recommended, and are much used, such as the waters and baths of Germany. These are of different kinds; and Schwalbach, Spa, Franzensbad, Ems, and Marienbad have great reputation. That they are often of some kind of service I have no doubt, just as I daresay that horse-riding, said to be recommended by Boerhaave against abortion, may also be sometimes valuable as a remedy of that tendency.

It may well be objected that general health is too vague a term, and that it would be better to profess ignorance than to ascribe to it such important and definite a result as sterility, and it will be justly asserted that the great mass of sterile women have the appearance of good health. The difficulty of the subject is well expressed by Darwin, in a passage I have quoted, treating of the causes of sterility in animals. After all, I think it best, in the present imperfect state of our knowledge, to group a large number of injurious ill-defined influences under the

head of general health, and to consider its improvement a means of cure. Although an animal sterile under confinement appears healthy, one cannot positively object to the statement that sterility is evidence that it is actually unhealthy, and the cure by restoration of freedom seems to confirm the view. Whatever may be the objections to the term "general health," everyone will recognize the importance of investigating the subject with a view to increasing our power over it; for it carries with it a strong influence not only towards the cure of simple sterility, but also towards the safety of the mother, the avoidance of morbid pregnancy, of miscarriage, of dead, ill-formed, and unhealthy children, and of excessive families.

Over-feeding, and the production of fat, are often spoken of as if they were identical; but this is plainly not the case, for many excessive feeders are not fat. What is the influence on sterility of over-feeding, or feeding by particular foods, without fattening, I do not know; but there are analogies which dispose the mind to suspect that influence may be thus exerted. Plants are habitually spoken of by gardeners as overfed by rich soils and manures, but they do not get fat. Mr. Thomson, recently showing me his tomato plants, pointed out some, set among strong manure, growing luxuriantly in wood and leaves, but producing little fruit: others, which had

been similarly placed, he had restored to due fruit-bearing, with diminished production of branches and leaves, by diminishing the contact of their pots with the rich manure. The growth of stems and leaves some may regard as the equivalent of fat in animals, but in that case stoppage of growth would be equivalent to resorption of fat, which would be driving analogy too far.

Although the injurious influence of fatness in women on fertility is universally admitted, it has not been altogether proved. But universal consent is strong evidence, and it is corroborated by all we know of the power of this same condition in the lower animals. Generally, young women before commencing to breed are fat, or at least plump. When they bear children they lose in weight by diminution of fat; again, as they cease to bear children, to resume the fat condition, the fat being now, however, differently disposed of in the body. The fat of the immature and of the post-mature is, within moderate limits, an indication of health. The fat of sterility is not an indication of health, but is, so far as I know, itself healthy, and indicates no active or positive disease. To obesity I only make allusion. I have known grossly fat women bear children; but facts about obesity are too few to justify its separation from the common exaggerated fatness of sterility here referred to.

Spencer makes a distinction between normal

plethora and abnormal plethora, as indicated by fat, and connects sterility only with the latter. I quote his ingenious remarks, not so much for the sake of giving his description of a distinction, the force of which I cannot see, as for the sake of stating his general argument regarding overfeeding or plethora as indicated by fatness. Medicine recognises no normal plethora. For physicians, plethora is always an abnormal condition, whether accompanied by much deposit of fat or not. "Many facts," says Spencer, "may be brought to prove that fatness is not accompanied by fertility, but by barrenness; and the inference drawn is that high feeding is unfavourable to genesis. . . . . There is a distinction between what may be called normal plethora and an abnormal plethora, liable to be confounded with it. The one is a mark of constitutional wealth; and this is the plethora which we have found to be associated with unusual fecundity. Abnormal plethora, which, as truly alleged, is accompanied by infecundity, is a superfluity of force-evolving materials, joined with either a positive or a relative deficiency of tissue-forming materials: the increased bulk indicating this state being really the bulk of so much inert or dead matter. Note, first, a few of the facts which show us that obesity implies physiological impoverishment. . . . . Neither in brutes nor men does it ordinarily occur either in youth or in that early maturity during which the vigour is the greatest and the digestion

the best; it does not habitually accompany the highest power of taking up nutritive materials. When fatness arises in the prime of life, whether from peculiarity of food or other circumstances, it is not the sign of an increased total vitality. . . . . Of like meaning is the fact that women who have had several children, and animals after they have gone on bearing young for some time, frequently become fat and lose their fecundity as they do this. In such cases, the fatness is not to be taken as the cause of the infecundity; but the constitutional exhaustion which the previous production of offspring has left shows itself at once in the failing fecundity and the commencing fatness."

The fatness of sterility is not apparently a matter of high or of low general health, and seems to be of a different origin from that fatness which comes on men and women at the great climacteric, and on the latter whether they have borne children or not. Whatever may be its natural history, it is known to be in some degree under the control of the physician. Not by medicine, but by diet and exercise, he can restrain its production or cause its removal. For success in removing fat, the co-operation of the patient is necessary, for on her part there is required change of habits and restraint of appetite. Little can be said regarding the cure of sterility by reduction of fat, but experience has furnished no reason to doubt the favourable influence generally expected from it.

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 fostering and indulgence the feelings may be, and not rarely are, produced or increased.

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 disappearence 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 an altogether natural one, for fevers are powerfully injurious to general health, and are known to affect powerfully 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 either 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 certain 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 part of the influence of this drinking should be ranked as mere overfeeding, whose injurious effects we have already spoken of. 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 disappearence 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 be 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 a practi-

tioner 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 facile 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 scrutinizes 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 gynæcologists 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 alleged cause has undoubtedly 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,

very many kinds of knives, many dilators, many expanders, many scissors, tents of various kinds, 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.

So long as 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 obstruction theorists to consider the passage of menses through the cervix alone. But they 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 passes 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 much exaggerated must be plain to every one 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 procidentia with cervical hypertrophy; cases in vesico-uterine fistula; 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 the semen passing the cervix are, in some mammals, increased in an extreme and often a curious manner, without any consequent obstruction. To this subject Kehrer 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 the 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. Any one familiar with the use of 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 wide, and that the same happens to the cervix; and while it is probable that such wide opening is not essential for fecundation, it must be held as facilitating it or rendering it more easy. 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 morbid painful and useless contractions 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 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 gynæcologists 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 other temporary novelties. 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 pre-

venting 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 gynæcology 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.

There can be no doubt that sterility is often a consequence of morbid conditions of the early embryo and of the fœtus; but these conditions are either quite unknown or very imperfectly understood, and no attempt has been made to adapt to them any special treatment.

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