tive culture has transformed a poisonous virus into a protective vaccine. In others, we may confidently hope that the same happy result will soon be reached; and, as we have already acquired the knowledge of certain specifics, it is not illogical to infer that if we can in one instance command the good results we see with mercury, in another with sulphur, and in a third with quinine, we may before long obtain such a knowledge of the various microbes which are the cause of communicable diseases, as may teach us how to destroy these organisms, or to arrest or mitigate their morbid influence, and so check, if not stamp out, some of the diseases which are now our most formidable difficulties. We may, then, congratulate mankind that the science of our own time has conferred, not only upon man, but on the whole animal kingdom, benefits equalling any which it owes to the lucky empiricism of past ages, or to the philosophic genius and marvellous industry of John Hunter. And we, his followers, may be encouraged by the conviction that we so cultivate our science and our art (devoted as they are to the public good) as to justify us in keeping our old motto,

"QUE PROSUNT OMNIBUS ARTES."

THE GULSTONIAN LECTURES,

ON

THE STERILITY OF WOMEN,

Delivered at the Royal College of Physicians, February 16th, 1888

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LECTURE I: PART I.—ITS 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 child. But the term is used with many other meanings, and I shall not make a definition, because I have not the right or the 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 semen properly. With a view to investigation, semen can be subjected to chemical and microscopical analysis; the depositing organ can be examined, and the conditions of the deposition can be ascertained. In women, the co-ordinate 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 subject for consideration, but one point in it cannot be passed over without some discussion, viz., its numerical amount. Much of our knowledge of the sterility of women consists in numerical statements with regard to amount under various circumstances, chiefly in marriages; and all such statements have a certain positive value for the physician, and still more for the political economist. But it is plain that, when inquiring into the amount of sterility due, not to unions or marriages, but to women, we must exclude what is due to the male. Some good 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," he says, "for physicians to assume that a man who is potent and 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 192 cases in which an examination of both husband and wife demonstrated that the former

was in fault in 33, or 17 per cent? Of this number, *Manningham* records 1 in 30; *Pajot* 7 in 80; *Moudot* 1 in 10; *Kehrer* 14 in 40; *Courty* 1 in 10; *Noeggerath* 8 in 14; and I have myself found that the male was deficient in 1 example in 8. The cause of sterility was: azoospermism, that is, want of spermatozoa, 31; aspermatisms, that is, the absence of semen entirely, 2. These facts show that the husband is at fault, according to Gross, in about one case out of every six." The matter is, however, still in a very insecure state, as may be shown by the statement of facts and considerations which must have important bearings on the question; but which, so far as I know, have been entirely neglected. Thus, it is assumed, by the examination of the male and the female, that we can decide whether one or the 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. 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 hereafter stated in these lectures, will show. In making the estimates of male sterility, no account is taken of the fact that the faulty condition of the 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 has been established, the fetus fading and dying prematurely from some inscrutable causes, or being monstrous, or perishing from disease implanted 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 likely 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. The plant, he adds, may be transplanted, or a branch with flowers and buds may be cut off and placed in hot water, and the pollen will be matured. Pollen, once mature, may be kept for weeks and 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 fertilised; 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 women, there is an universal profound belief, which no investigations have shaken, that in the human species the paramount source of sterility is in the female. I know of 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, are of importance and value in themselves, though very 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 the uterus and imperforate vagina—conditions so rare that, in the present 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, secondly, 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 well known. There is discharge of a highly developed decidua 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 conditions, and of such 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 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, when absolute, implies a failure to produce a viable child, while there may be evidence of conception—that is, of the commencement of the production of the 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 and unnatural production may be owing to ovuline imperfections derived from the male or 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 may arise from local or constitutional causes. In absolute sterility, and in sterility not absolute, there is no production of a viable child; no addition is made to the population, and all such sterility is sometimes, by economists, considered absolute; for, indeed, in the point of view of population it is so, but to me it appears 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 may produce one, or even several living children, but in number not according to her condition, and 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 this 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. A woman may be relatively sterile, from producing, according to her age, only a small number of children with ordinary intervals between the successive births, or from the number being rendered small by the extraordinary delay and loss of time between the successive births, or in other ways. All kinds of sterility may be congenital or acquired; it is therefore undesirable to use these terms as indicative of conditions. 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, in her, were congenital.

The amount of sterility in women, including the relative kind, is found by counting the number of productive and unproductive marriages of women within the reproductive age—say, generally, from fifteen to forty-five. Lever, giving no numerical details, says, five per cent. of married women are wholly unprolific. West found the proportion of sterile marriages, among his patients at St. Bartholomew's, to be one in every eight. Hedin, a Swedish minister, noticed in his parish of eight hundred souls, that one barren woman was not met with for every ten fertile women. Frank and Burdach roughly state that one marriage in five is unproductive, but they give no data. Simpson made an inquiry into the sterility of married women in Grangemouth and Bathgate. Of 210 marriages in Grangemouth, 183 had offspring, and 27 none; about one marriage in ten was without issue. Of the 27 unproductive marriages, all the subjects had lived in wedlock five years, and the female had been married before the age of forty-five. Of 402 marriages in Bathgate, 365 had offspring, and 37 none; one in eleven being unproductive. There were at the same time living in the village 122 wives and widows, and of these 102 were mothers, and 20 not mothers; or about one in six had no family. In all, there were 467 wives and widows; 410 had offspring, and 57 none; or about one marriage in eight was unproductive. Of these last 57, six had not been five years married, and other six were above forty-five when married. Subtracting these 12, we have 455 marriages, 410 being productive and 45 unproductive; or one in ten without issue. Simpson found that, among 495 marriages of British peers that lasted five years or more, the husband being under fifty-seven, 81 were unproductive; or one in six. Ansell found that, among 1,919 marriages of spinsters in the upper classes, the average age being twenty-five, and not counting as childless those who had merely still-born children, there were 152 without issue; or eight per cent.—nearly one in twelve. In this collection, all the parents survived the child-bearing age; and he considered that there was no further chance of child-bearing if the female was over forty-eight, and had had no child for two years; or if over forty-seven, and had had 'no

child for three years; or if under forty, and had had no child for ten years.

I have taken the registers of Edinburgh and Glasgow for 1855, and have ascertained 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 were not all pertaining to women married in that year, it may be assumed that, if the marriages are nearly the same in number for a few contiguous years, any of the first births in one year will give pretty accurately the fertility in contiguous years. From this fertility, sterility can be computed. In 1855, there were, in Edinburgh and Glasgow, 4,447 and 3,722 first living children, leaving 725 marriages sterile, or one in six. But in these figures are included 75 marriages which did not take place until after the women had passed the age of forty-four. This will damage the physiological value of the statement, as these 75 could not be expected to be fecund. Of women between the ages of fifteen and forty-four, there were married 4,372. Among women of the same age, 3,710 had first living children; 662 marriages were sterile, or one in six; in other words, 15 per cent. of all the marriages between fifteen and forty-four were sterile. This statement from Edinburgh and Glasgow has to be corrected for the dead-born; these not being counted. We have thus good statements of sterility, which do not differ much from one another; in St. Bartholomew's, one in eight; at Grangemouth, one in ten; at Bathgate, one in ten; among British peers, one in 6½; Ansell gives one in twelve among the upper classes; in Edinburgh and Glasgow, one in seven, excluding those who have borne dead children. The highest estimate (omitting the peers) 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, where a woman having a still-born child is held as fertile; and the women are amongst the best in the community, those living in easy circumstances, and making use of the protection of life-assurance. Were it otherwise, the estimate of sterility would, no doubt, be higher. We have thus estimates of sterility varying from one in seven to one in twelve; and considerable confidence may be had in the statement that about one in ten is the true amount.

I know of no estimate of those who are absolutely sterile, that is who do not conceive, or who if they do conceive, give birth not even to an abortion. But they are a large number in the better classes; for within the last five years there have consulted me, 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 of cases in existence, it gives no amount of estimated frequency among the married. The following table gives 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 to 8	9 to 13.	14 to 18.	19 to 23.	24 to 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	81	40	29	11	1		504

The economist makes many estimates, such as the deficiency of offspring in actual marriages, or the deficiency of actual births below what they might have been if all the women in the population had been married at the most favourable time for child-bearing. The solution of these and similar questions is an object of greater interest to statesmen than to physicians; they demand for their solution much calculation, and need not be entered on here. The degree or amount of relative sterility in average individuals varies, of course, according to the age of 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 in 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 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 weakness and inability but 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 exhaustion of the fertile energies, leaving the general bodily health vigorous, or as 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. In 1,767 fertile marriages of women of the age of twenty-five years, allowing ample time for the exhibition of fecundity, Ansell found 131 cases of one-child relative sterility; or one in every thirteen fertile marriages. The amount of this relative sterility may be approximated by comparing it with the average fertility of the same women in Ansell's table, which was nearly six; or, in other words, the relative sterility of these 131 only-child women was 655 children. Instead of having 131 children, they would have had 786, if they had even reached the average fertility of their 1636 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, judging them by other women.

DIET AT SEA.—The following instructions to superintendents have just been issued by the Marine Department of the Board of Trade: "Dietary Scales. The attention of the Board of Trade having been drawn to the increase of scurvy on board British ships since 1873, a report on the whole subject—'Sea Scurvy, Food Scales, Antiscorbutics'—has been recently prepared and forwarded to the local Marine Boards for their observations. The conclusions arrived at in this report were as follows: 1. That scurvy has been on the increase in British ships since 1873. 2. That limejuice, of itself, will not prevent scurvy, and that too much reliance is placed on it, to the neglect of varied food scales. 3. That limejuice, in connection with fresh or preserved meat and vegetables, may prevent scurvy. 4. That the dietary scale of ships should, therefore, include a fair proportion of fresh and preserved meats, as distinguished from salted meats. 5. That more fresh vegetables should be carried, notably raw potatoes. No satisfactory reason is given why fresh potatoes cannot be carried on board British ships. The allegation that they will not keep good on board ship, is clearly disproved everywhere else. 6. That it is not at present desirable to insert a statutory scale of diet in the articles of agreement with crews serving on long voyages, though it may possibly be necessary hereafter, unless the shipowners themselves move in the matter. The replies received from the local Marine Boards have confirmed these views, especially as regards the articles of diet referred to therein, and superintendents are therefore requested to take every opportunity of urging upon owners of vessels sailing on long voyages the necessity of supplying their crews with fresh potatoes, molasses, etc., and a larger supply of fresh or preserved meats, in lieu of salt beef and pork.—T. H. Farrer, secretary; Thomas Gray, assistant secretary."

VACCINATION INQUIRY.—The preliminary committee meeting was held in the Council Room, Exeter Hall, Strand, on Thursday, the 15th instant; Dr. C. R. Drysdale in the chair. Mr. Makuna stated that the object of the inquiry was to collect all available information on the subject, collate it, discuss it in conference meetings, and publish it in the *Transactions*. About three hundred and fifty members of the profession had answered to the questions in the circular, including eighty public vaccinators and medical officers of health, and several original investigators, as MM. Pasteur, Chauveau, Braidwood, Romanes, Klein, Fleming, Wydd, and others. There are twenty-five gentlemen who have, up to this time, consented to act on the committee: Dr. C. R. Drysdale, R. W. Batten, W. J. Collins, W. Easby, J. Greene, E. Gwynn, D. L. Haynes, W. K. Millican, R. Neale, A. Ransome, C. Renver, H. Tomkins, J. Tripe, C. F. Willoughby, G. Wyld, G. E. Yarrow, E. Haughton, P. M. Braidwood, C. Swaby-Smith, A. Cresswell Rich, T. E. P. Prydeaux, B. Maclehoose Duncan, Bernard O'Connor, G. Cordment, Charles E. Steele, and M. D. Makuna.

AN ADDRESS ON RENAL INADEQUACY.

Delivered before the Metropolitan Counties Branch (Hackney District), February 15th, 1883.

By ANDREW CLARK, M.D., LL.D.

Physician and Lecturer on Clinical Medicine, London Hospital; President of the Clinical Society.

GENTLEMEN,—After much, and indeed grave consideration, I have come to the conclusion that age is not to be gauged by years, but by feelings. I think that, so long as a man retains his receptivity, his power of adjusting himself to the ever varying environments of life, and his capacity to see, in the new things which the development of the ages brings before him, goodness, he may be saved from being brought within the category of "old" men. Now, I do not know that I have retained my receptivity, and I do not know that I quite adjust myself to all the varying environments of my life; but I do know that I retain, and have a very lively sense of the good things that the age is presenting to us; and on that account, if on no other, I will ask you to exempt me from being called old. I am struck particularly with the young men of this time. Looking back, I am afraid to say, but must say, thirty years, I notice a marked distinction between the young men of those days and the young men of these days—whether they be in consulting practice or in general practice. I notice, or I think I notice, a greater zeal, a greater industry, a greater frankness, a greater readiness to confess and remedy errors, a greater care in the expression of opinion; and, I think also, a less consideration of their own personality in their work. I admire the young men of the present time very much. But there are one or two things that I do not admire in them—that I quarrel with them for. I will only mention one thing to-night; it will be enough for to-night, and it will be the key-note of the subject which I am approaching. Most of the young men of the time declare that there is no such thing as functional disease, and that is one of my quarrels with them: for I think that all disease, in its primitive condition, is essentially functional. I think that, what people call structural or organic disease, is but an event in the history of disease; and that, long before the structural change appears, there has been going on, for we know not how long, changes in the physical condition, changes in the physical action or interaction of forces, various conditions in short, most powerful in relation to the actual state of living affairs, though invisible to the naked eye. I think that disease expresses itself in three different ways. First, we have disease expressing itself by what we call mere alteration of function, in which state, by no means at our command at the present time, can we discover the smallest change in the structure; and that is what I call functional disease. In the second place, disease expresses itself in visible changes of structure, which are temporary only, and not permanent. For instance, take hay-fever. I believe myself that hay-fever is preceded by molecular and chemical disturbances; and that, at a certain stage of these, there arises suddenly a swelling of the mucous membrane of the nose, together with the production of a viscid irritating secretion, which gives rise to sneezing and the other phenomena with which we are all familiar. And, lastly, there is a third aspect of disease, in which, with the preliminary physical or chemical changes, or both, with the transitory structural changes, there comes at last a permanent change: as, for example, cancer or tubercle. Take, for a moment, the case of cancer of the stomach. Years before cancer of the stomach appears, you will find in some of those people, that they are subject to pain or other distress. Somewhat later in the day, they are subject to recurring attacks of catarrh of the stomach, as it is called—states of the stomach in which the mucous membrane swells up, producing viscid irritating secretions, giving rise to what we call forms of indigestion; and then, after treatment, or independent of treatment, it subsides. This will go on for years, and by-and-by growth riots over development, and at last a permanent structural change appears; and we have what we call the third aspect of the way in which disease manifests itself. There is the first stage, and it is the first stage to-night to which I wish to allude, in which we have symptoms of disease without any visible alteration of structure discernible by any means at our command.

Every man present, I am sure knows as well as I do, multitudines