THE HUNTERIAN ORATION,

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II.

I MUST now obey the other direction in the trust deed, and endeavour to express something as to the "merits, in comparative anatomy, physiology and surgery," of John Hunter. The direction seems a happy one, for Hunter based his surgery upon physiology, and his physiology upon comparative anatomy. What can more strikingly illustrate this than his greatest improvement in practical surgery?—the abolition of amputation for popliteal aneurysm, and the practice of tying the artery in a sound part at a distance from the seat of disease. This has already saved thousands of human lives; and it has been well said that if Hunter had done nothing else, "on this account alone he would have a right to be classed among the principal benefactors of mankind."

One who, perhaps more than any other of our contemporaries, resembles Hunter in completing the union of thought with action, the wisdom of the philosopher with the skill of the surgeon—Sir James Paget—says: "It was really a splendid achievement; and its utility is not half told by counting the thousands of lives that it has saved. Its higher value is that it still abides as a great testimony of the power of the scientific mind in surgery. I think it has done more than any other of Hunter's works to make, not only surgeons, but surgery, scientific." And observe how comparative anatomy and physiology led to surgery—how thought and experiment led the way for action. It was probably without any idea of surgery that Hunter was first led to investigate the phenomena of the annual growth and shedding of the antlers of the stag or buck. But he did so, and tied the artery which supplies the growing antler and the soft covering called the "velvet," which conveys the vessels supplying the material of growth. Pulsation in the vessels in the velvet ceased, and the antler soon became colder. A week afterwards the vessels were again pulsating, the velvet was warm, and the antler growing. The buck was killed, the tied artery found to be obliterated, but the circulation was carried on by vessels above and below the ligature, ordinarily very small, but enlarged under the new conditions.

Now, I am well aware that some modern misanthropical zoophiles have said that Hunter had no right to make this experiment, that human morbid anatomy had taught him that when the current of blood ceased to flow into an aneurysmal sac, the blood clotted and was absorbed. But it is quite indisputable that Hunter was doubtful whether, after the supply of blood to a part had been cut off by obliterating the main arterial trunk, the circulation would be kept up by other vessels, or the part would die; and it is certain that his experiment on the deer removed his doubt, for, as Professor Owen tells us, that there was a coachman in St. George's Hospital with popliteal aneurysm, who had consented to amputation; but Hunter thought that if the anastomosing vessels in the man would carry on the circulation after the obliteration of the femoral artery, as they did in the antler of the buck after obliteration of the carotid, he could cure the aneurysm and save the limb. Professor Owen emphatically says that Hunter explained to his assistant and pupils the results which he believed would follow a repetition on the man of his experiment on the deer. And, just as he predicted, there was the same stopping of pulsation, the same cooling of the part from which the supply of blood was cut off, the same return of natural warmth, and in six weeks the man walked away cured. This account Professor Owen tells me was given to him by Mr. Clift.

In this and two subsequent cases artery and vein were both tied; but in his fourth case, Hunter tied the artery only, not the vein. This was in the year 1787. The patient was then 37 years old. He lived till he was over 86, and died in 1837. At his death, Mr. Wormald obtained the limb from his widow, and found the femoral vein pervious, the artery obliterated, the tortuous anastomosing vessels as you see them in this specimen which Mr. Wormald presented to our museum, and the aneurysm represented by the small calcareous body not larger than a filbert.

This association of surgery, physiology, and comparative anatomy is manifest in the leading idea or plan of Hunter's museum, which is to show each step from the most simple conditions in which life

can be traced upwards to man himself. We are told that, observing that in the advanced stages of successful incubation eggs did not putrefy, he was first led "to recognise life as a power, and organisation as the mechanism by which life operates." Observing some snails drowning, and noticing the effects produced upon lizards if they were brought too early in winter out of their lurking-places, he ascertained some of the facts "which guide and encourage us in our attempts to restore suspended animation." Here we see the man who both observes and thinks. "For one person who can think," says Buckle, "there are at least a hundred who can observe; an accurate observer is no doubt rare, but an accurate thinker is far rarer." Can any one look up to this portrait of Hunter, by Reynolds, and not agree with Lavater, who, when he saw it, said, "This man thinks for himself?" Hunter used to say that "he delighted in thinking;" and the great artist who is said to have peculiarly excelled in painting the mind of his sitters, has caught Hunter in the attitude of meditation. In that absorbed and upward gaze "from earth to heaven," "as imagination bodies forth the forms of things unknown," we can trace the hoped-for explanation of some of the mysterious phenomena of nature by the application of some great leading principle, the elucidation of some physiological problem, the unravelling of some as yet unfolded revelation. Home and others, who knew Hunter, have said that this portrait gives a very faithful representation of his countenance, person, and manner. While painting the portrait, Reynolds induced Hunter to have a cast taken from his face. From this cast, which has been preserved in our museum, and is here before you, Flaxman's marble was sculptured. He and Chantrey studied both cast and portrait, for the College is fortunate in possessing two busts of our great master, as well as the full-length sitting figure in the museum by Weekes. And we have a second portrait, by Sir Nathaniel Holland, which was said by Mr. Clift, and by Sir John Dorat, Hunter's last surviving pupil, to be better as a mere likeness than the idealised portrait by Reynolds. Perhaps Reynolds's portrait is more suggestive of the man who thought, and Flaxman's bust of the man who observed, experimented and acted.

Though Hunter loved to think, he followed Bacon in insisting on observation and experiment as the only foundation of true science. "If you check experiment, you stop discovery," is one of his aphorisms. And he once wrote to a friend, "I think your solution is just. But why think? Why not try the experiment? Repeat all the experiments as soon as you receive this, and they will give you the solution." Thus, in Hunter, we find the mind which investigates the laws of disease, and the hand which improves the art which cures disease; not only philosopher and pathologist, but surgeon—as rare a combination as that of a profound jurist and an eloquent advocate—an agricultural chemist and a farmer—an astronomer and a pilot. We have a combination of Faraday investigating the laws of electricity and magnetism, and Swan or Edison applying the knowledge in the electric lighting of towns.

It is unnecessary to repeat now, what Mr. Wormald proved in this place twenty-five years ago, that Hunter was well aware of the possibility of curing aneurysm by pressure on the artery, as well as by tying it—in this, as in so many other instances, anticipating recent improvements in practice supposed to be new. Just as we find that modern views of phlebitis and pyæmia had been advanced by Hunter; and Burdon Sanderson, in his lectures on inflammation, acknowledging that "we find ourselves once more coming back to the notions, which at one time were thought obsolete, of the great father and founder of physiological pathology;" so we may be certain that Hunter foresaw much of that progress in abdominal surgery in which it has been my own pride and pleasure to assist. In 1762, William Hunter distinctly suggested that it might be advisable to expose an ovarian cyst by a small incision—"tap the bag and draw it out." In 1785, John Hunter said, "I cannot see any reason why we should not make an opening into the abdomen and extract the cyst itself. Why should not a woman suffer spaying without danger as well as other animals do?" The influence of Hunter's teaching upon John Bell, of Bell upon McDowell, and the history of ovariectomy since McDowell's first operation, is a theme which I have treated at length in this theatre and elsewhere; and however strongly tempted to enlarge upon it, and upon the more recent extensions of peritoneal surgery—the removal of uterine tumours, of the spleen, of one kidney, of hydatids, of gall-stones, of omental and mesenteric tumours, of fibro-plastic and fatty tumours of various origin from the abdomen, of the entire uterus, or of part of the uterus with the fetus of an extra-uterine foetation, of the pylorus, and of portions of diseased intestine—all operations which have been performed with increasing success; however tempted, I say,

I regret very much that I have not time to do more than point to the preparations on the table, which show how our younger hospital surgeons are joining in the most recent advances of abdominal surgery. Mr. Treves's specimens show how perfectly union between two portions of intestine may be effected after the removal of an intervening diseased portion, and the three large gall-stones removed from the gall-bladder of a woman in the Samaritan Hospital by Mr. Meredith are gratifying evidences of boldness and skill. Both Mr. Treves and Mr. Meredith, at my suggestion, were desirous of experimenting on some of the lower animals as to the best mode of uniting divided edges or surfaces of peritoneum. But the trouble and delay of the present licensing system has hitherto restricted their operations to men and women.

I wish I could say more of this, but I am compelled to devote the very few minutes allotted to me to a hasty sketch of what we hope may be gained in the not very distant future by combined association for the advancement of medicine, in its higher sense, by research.

And first let me say with how much pleasure I can state, that the Association lately founded under the auspices of all the leading men in our profession in the United Kingdom has already begun useful work. On the part of the Association, Mr. Watson Cheyne visited Dr. Koch at Berlin, and Professor Toussaint at Toulouse, and has since carried on investigations, the results of which enable him to explain their opposite statements with regard to the micro-organisms associated with tubercle. Mr. Cheyne has also made experiments with reference to the specific nature of tubercle, which tend to confirm the view of the specific nature of tubercle first promulgated by Villemin, and so strongly supported by Koch's observations. Mr. Cheyne's further observations lead him to the conclusion that the bacilli of tubercle multiply by preference in the epithelium of the alveoli of the lungs, and lead to inflammatory exudation in the walls of the alveoli. According to the number and rapidity of growth of the bacilli in the alveoli, we have the two conditions of fibroid phthisis or caseous pneumonia, which by many have been looked on as different processes. In this view is also explained the difference in the effects produced by these organisms in man and rodents. Rodents, when inoculated subcutaneously, always develop general acute tuberculosis. That disease is extremely rare in man when compared with the frequency of pulmonary tuberculosis, because in man the bacilli are not inoculated, but are received into the bronchial tubes by inhalation, and their entrance into the circulation is prevented in the first instance by the inflammatory changes which occur around the alveoli in which the bacilli grow. If man were inoculated as we inoculate rodents, all analogy would lead to the conclusion that acute tuberculosis would probably be developed. Mr. Cheyne is carrying on these researches, the animals experimented on being kept under exceptionally favourable hygienic conditions, and it is not too much to hope that they may lead to a successful mode of treating phthisis. However strongly tempted to say more on this immensely important subject, I must leave it for a future Hunterian Orator, and pass from tubercle and its bacilli to other diseases, more commonly classed as contagious or infective.

Of all the discoveries of modern times, perhaps the most important is that of the dependence of fermentation and putrefaction upon microscopic organisms; and of a number of communicable diseases upon specific microbes—a convenient word to include the micrococcus, the bacterium, the bacillus, the vibrio,—microzoa, or microphytes, whichever they may be.

At Cambridge, eighteen years ago, I attempted to show the relation between the work of Davaine and Pasteur, and the causes of excessive mortality after surgical operations. Allow me to read one sentence from my address in 1864, partly to allude to advances gained since that year, and partly to point out some lines of future research. I said: "Applying the knowledge for which we are indebted to Pasteur of the presence in the atmosphere of organic germs, which will grow, develop, and multiply, under favourable conditions, it is easy to understand that some germs find their most appropriate nutriment in the secretions from wounds, or in pus, and that they so modify it as to convert it into a poison when absorbed—or that the germs, after development, multiplication, and death, may form a putrid infecting matter—or that they may enter the blood and develop themselves, effecting in the process deadly changes in the circulating fluid."

In the history of this discovery, we find Davaine discovering bacteria in the blood of animals suffering from charbon. Then, having studied Pasteur's researches on butyric fermentation, Davaine found that he could propagate a fatal disease, not only by a purulent virus, but by a drop of infected blood. It was left for Pasteur to separate

and identify the microbe, to propagate it through successive generations, and to arrive at a general law that a whole series of contagious diseases could be produced artificially, and that the microbe which was the cause of each disease could be so modified by successive cultivation, some with, some without access of oxygen, as to be rendered almost inert—nay, more, even to protect the recipient for a time from a second invasion, and secure immunity to the offspring of infected mothers.

I must not go back to the history of vaccination as a protective against small-pox, nor remind you that Jenner was a pupil of John Hunter, nor refer to many of the letters which passed between them; but I may notice a resolution of the Council of this College, carried sixty years ago: "not to inoculate small-pox, but to pursue, and to the utmost of our power promote, the practice of vaccination." We persevere in this course. We require every candidate for our diploma to produce proof of practical knowledge of vaccination. We support the law which protects the many from the danger to which a few ignorant opponents of compulsory vaccination would expose the whole population. And as we glory in the life-saving work of our countryman, Jenner, so we welcome the teaching of the illustrious Frenchman, Pasteur, and his extension of the protective influence of vaccination to other diseases—every year adding to the list of new vaccines which protect man and animals against virulent diseases.

When I first thought of the subjects for this oration, I had a very exaggerated idea of what it might be possible to do in sixty minutes. I hoped that (in addition to what I must say of our deceased brethren, and of John Hunter himself), of recent additions to our museum, and of the new pathological catalogue, I might be able to indulge in a review of the progress of modern surgery, and perhaps compare the present state of medical practice in London with that in Hunter's time, or even to take a hasty review of the progress of the nation since his death, and refer to the rapid increase of population and wealth, the discovery of steam, the influence of railroads and steamships, the use of gas, of the electric telegraph, the spread of education, the effects of newspapers and cheap literature, of reforms in our laws, improvements in our habits, and many other things which tend to make a people wiser, happier, and better. I even thought it might be possible to sketch very rapidly the share which the medical art, in its larger and wider sense—not only as curing, but as preventing disease—has had in assisting national progress, and to prove that neither medicine or surgery has lagged behind the general advance. I hoped I might be able to prove beyond dispute that, since active sanitary work has been undertaken in this country, death-rates have fallen very greatly; and fallen most in those places—the great towns—where sanitation has been most active.

I was very anxious to show how the knowledge gained by the statistical work begun by Dr. Farr, and since carried on by Dr. Ogle, at the General Register Office, had led to sanitary legislation; and how sanitary work has been followed by a lower general death-rate and smaller mortality in single forms of disease, as in typhoid fever, as well as after wounds, injuries, and surgical operations. I wished also to show how statistics lead to the saving of life by throwing light on the natural history of disease, on the prevalence of various zymotic diseases at different seasons, and on the indisputable proofs that small-pox has declined considerably with the extended use of vaccination; that it is false to attribute that decline to general sanitation, exclusive of vaccination; and, what is a more novel or less generally known fact, that we have statistical proof that the preservative effects of vaccination wear out more rapidly and surely than the preservative effects of small-pox itself. I imagined, also, that I might be able to sketch what the nation might gain if State medicine were really administered by a well-organised department of the Government; if politicians of both parties could be roused from their indifference to social or domestic legislation, and give some small share of their attention to the health-interests of the people—to their food, drink, occupations, house accommodation, care of infants, prevention of infective diseases, local sanitary administration, and many details of sanitary reform—such as a permission or encouragement of cremation, as a substitute for the present mode of burying the dead.

My intentions, sir, may have been good, but their fulfilment is impossible; and, in conclusion, I can only refer to the prospect which expands before us as we are shown that more than thirty destructive diseases—including tubercle and typhoid fever, ague and yellow fever, scarlatina, diphtheria, erysipelas, syphilis, and septicaemia in man; in the lower animals, splenic fever, fowl-cholera, cattle plague, glanders, hydrophobia—all depend upon specific microbes. In many of these thirty diseases, the specific microbe has been identified beyond dispute. In some, the process of attenua-

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