THE Mitchell Banks Memorial Lecture.

DELIVERED ON NOVEMBER 3RD,

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WILLIAM MITCHELL BANKS: HIS LIFE AND WORK.

I FEEL I cannot commence my address without alluding to a circumstance the mere mention of which brings with it a sense of the deepest regret. I refer to the death, on February 28th last, of the first William Mitchell Banks Memorial lecturer—Reginald Harrison, for many years a leading surgeon of this city. I remember the warm feeling of brotherly kindness towards his old friend and former colleague which Harrison expressed when he told me he was about to deliver the first of this long series of lectures to which members of the profession will be annually invited to listen in the generations—perhaps even in the centuries—to come.

At that time he mourned for the friend whom he was about to commemorate, as now we mourn the loss of them both. As a citizen, a surgeon, a Mason, a man, and a friend Reginald Harrison was equally admirable. By his death we lost a delightful and genial companion; by his life he has for ever impressed upon our memory the ideality of a generous nature and a noble mind.

When shortly after the sudden death of Sir William Mitchell Banks at Aix-la-Chapelle in August, 1904, it was resolved at a large and variously representative meeting in this city, presided over by the Lord Mayor, to institute a memorial of their much beloved and regretted fellow citizen, and that this memorial should in part take the form of a lectureship to be permanently identified with his name, it was intended that the lecture should be on some subject connected with the study or practice of surgery in which he had been interested. And when in February last I was honoured by the Council of the University of Liverpool to deliver the fourth of the William Mitchell Banks memorial lectures, I felt it a privilege to be able to accept the invitation.

Mitchell Banks must have been known to the profession in all civilized countries as a brilliant and forcible writer, and as a pioneer in certain surgical procedures; whilst we in England also knew him as an exponent of sound and enlightened views on medical education, as an able teacher of anatomy, as well as of surgery, and as a man of decided literary and general culture.

For eight years he was a member of the Council of the Royal College of Surgeons of England, and for some part of that time our allotted places were adjacent to one another. Thus I got to know him better than otherwise I might have done. As a councillor he showed great sagacity, force and decision of character, and singleness and straightforwardness of purpose. He was an impressive and easy speaker, with a persuasive and pleasing manner, and he had a keen sense of duty, which he discharged fearlessly and conscientiously.

On three subjects he was very emphatic: (1) That Members of the College ought not to have votes for the Council; (2) that the number of physiologists on the roll of the Royal Society was unfairly in excess of the number of anatomists and of the surgeons; and (3) that academic robes and ceremonial add to the dignity and impressiveness of official functions.

As a member of the staff of the Royal Infirmary and of the teaching body of University College, Liverpool, and also as having been the (first) representative of the Victoria University on the General Medical Council, Banks was able to speak with great authority on matters connected with the curriculum of medical studies and on medical education in general; while as the President of the Lancashire and Cheshire Branch of the British Medical Association, as Secretary and then President of the Liverpool Medical Institution, and as the first President (in 1886) of the Biological Society of Liverpool, he had been brought into close relations with a large number of

provincial medical men, and was therefore able to approach a variety of subjects which from time to time came before the Council of the Royal College of Surgeons with an extensive and intimate knowledge as to how far they concerned or in what manner they affected the general practitioners throughout the country.

His opinions on these questions were acknowledged by us to be of much value.

His labours in connexion with the new Royal Infirmary, with the origin of University College and its relations, first with the Victoria University and afterwards with the University of Liverpool, were, I believe, appreciatively referred to by his old friend and colleague, Reginald Harrison, in the first Banks Memorial Lecture three years ago. His connexion as President of the Surgical Section of the British Medical Association, and his having been chosen to give the address on surgery at the Montreal Meeting¹ in 1897, show the high regard in which he was held as a surgeon.

BANKS ON EDUCATION AND LITERATURE.

Two of Banks's general addresses—namely, "The Gentle Doctor" and "Physic and Letters," give an excellent insight into his character.

In the former he compares the doctor of the seventeenth century with him of the present day, as illustrated by the novelist, the dramatist, and the painter of those two periods respectively. His word picture of his old teacher, Warburton Begbie, Physician to the Edinburgh Infirmary, whom, as he tells us, he always thought of as the "Gentle Doctor," shows Banks to us as a confirmed hero worshipper; and convinces us that years of surgical experience did not case-harden him to suffering, but had rather the opposite result, and that, in his own words, the more blood he shed, the more readily were the tears brought into his eyes.

"Physic and Letters" was the title he gave to the annual oration which he delivered at the Medical Society in 1893. After saying of himself, "I am vain enough to hope that I have done something for the furtherance of medical education," he went on to consider what professional training is best suited to make a youth a well-educated doctor, and then to inquire into the position of the said doctor as a well-educated man.

He regretted that the study and practice of medicine are so exacting that those engaged in them have little leisure, and too often grow to have even less inclination, to give time and thought to general literature. Although many of the departments of professional studies are most entrancing, yet they give, he said, no play to the imagination. He points out that the antidote, or as the old apothecaries would have said, "the corrective and adjuvant," for this state of things is "a good book by the fireside at night."

He made some very trenchant and condemnatory criticisms on the private preparatory schools of the small provincial towns of England, at which many youths who enter the medical profession receive their early education; and I regret to say that the strictures Banks passed in 1893 seem, from reports I receive, to be true of a great deal of the preparatory education in this country at the present time.

I am told that many boys are so miserably prepared that it is difficult to bring them into line with the ordinary work at a public school.

It is to be regretted, as Banks said, that more attention is not paid to the science of pedagogy, and that the business of teaching the teacher how to teach does not receive the same amount of care in England as it does in Germany.

His remarks on the undue prominence given to athletics are temperate and discreet, but admonishing. He ridicules the idea of a boy being encouraged to regard the life of a mere athlete as the highest to which a human being with an intellect can aspire, and he adds, "It seems nowadays to be forgotten that after all the *corpus sanum* is merely the fleshy tabernacle of the *mens sana*." Indeed the whole of his remarks on this subject are such that they might have inspired the comments on "flannelled fools" made by Kipling some years later. And Banks, be it remembered, was himself a lover of and a participator in games and sport.

Banks drew a happy comparison between choosing books and choosing friends. Then he passed on to mention Lord

Beaconsfield's celebrated speech in Manchester in 1872, in which he substituted the words *Sanitas sanitatum, omnia Sanitas*, for the words of King Solomon, *Vanitas vanitatum, omnia vanitas*, and said how the speech was contemptuously stigmatized at the time as advocating a "policy of sewage." Next he quoted from the speech of the Marquis of Salisbury delivered twenty years later, when pleading for the Radcliffe Infirmary at Oxford in 1892.

Lord Beaconsfield had declared that to his mind the great social question, which should engage the attention of statesmen, is the health of the people, because it refers to all those subjects which can advance the comfort and happiness of man.

Lord Salisbury spoke of the important rôle which researches into the infinitely small—the bacilli—were destined to play in the following generation upon the course of medical science, and said, "The investigation of these creatures which have been revealed to us by new methods of research and by singularly patient labour, and upon which the lives of millions of human beings depend, will figure larger in the scientific field than any other object of study, and these are the special domain and privilege of medicine."

These were striking remarks for Banks to quote in 1893, but the sagacity and prescience of the speakers, one speaking in 1872, and the other in 1892, are even more conspicuous now that the predictions have been verified to the fullest degree. To-day there are not only medical officers of health all over the country; but various commissions are sitting to deal with tuberculosis, sleeping sickness, and other aspects of disease; there is the scheme for the physical examination of children in primary schools; and there are reports periodically made by medical officers on intricate scientific investigations conducted under the authority of the Local Government Board, as well as of the Privy Council.

It is obvious that the aid of the medical profession is being more and more sought than ever before, by the governing authorities, to assist in the work and objects of municipalities and in the public business of the State.

Looking to the splendid results of the last few years by the research workers in connexion with Malta fever, malarial affections, yellow fever, and other tropical diseases, Lord Salisbury might, with a near approximation to truth, have said, "and these are the special domain and privilege of that scientific and beneficent institution which is occupied for the improvement of human and animal life, and which is known all the world over as the 'Liverpool School of Tropical Medicine.'"

HIS METHOD OF INTELLECTUAL INQUIRY.

Little thought is given by medical writers and investigators to the nature of the intellectual methods of inquiry they employ. Many seem unmindful as to whether they are reasoning by one method or by the other. Banks we find used almost exclusively the inductive process. He felt that facts were more important than ideas, that facts, therefore, should be put before ideas, and not ideas before facts; that we must trust to observation and experiment, and work upwards from the knowledge gained through our senses to general laws, not downwards from general principles through idea after idea to the facts, which we too often endeavour to make agree with our theories.

He seemed to have had a poor opinion of the syllogistic or deductive method of inquiry in medical investigations, and to have thought that all our knowledge—at least, in its beginnings—must be built upon facts.

His view that there is no room for the use of the imagination in the philosophic study of medicine will not, I am sure, meet with general acceptance. There have, indeed, been some great men in the profession whose method of inquiry has been purely deductive; and without imagination this method cannot be employed. It is certain that the magnificent discovery of Harvey, and, to go outside our profession, the splendid generalizations of Newton, could not have been made by observation and experiments exclusively. An inordinate regard for experiments, and an undue accumulation of facts, may outstrip knowledge and impede progress by overtaxing the memory and perplexing the judgement, unless the imagination is exercised

in the co-ordination, generalization, and ordering of the facts. When the imagination comes to the assistance of the understanding, the intellect should be stimulated not controlled by it. As Aristotle well said, "Reason should have over the imagination that commandment which a magistrate hath over a free citizen."

But though Banks considered, erroneously as I believe, that there is no scope for the imagination in our scientific work, he urged the students and practitioners of medicine to make poetry the ally of their science, so as to prevent the faculty of imagination from languishing or atrophying from want of use.

ASSOCIATION OF IMAGINATION WITH SCIENCE.

The ancients, as we know, conjoined music and medicine in Apollo, because, as Bacon said, "the office of medicine is but to tune this curious harp of man's body, and to reduce it to harmony." The music of the lute and the harp were used as therapeutic agents by physicians of olden time. With a different object, and for the beneficial influence it has over the doctor, rather than his patients, Banks recommended the habit of reading poetry. By the study of Shakespeare and Byron, Scott and Tennyson, every man "should be able to draw some sun into his soul," he thought.

There is, I think, a far closer association of poetry with philosophy, of imagination with science, than is commonly realized. Just as men eminent in letters, and great warriors, so great poets and great philosophers and great scientists have flourished at much the same periods. Certainly the greatest poets have lived in warlike and stirring times, when the emotions, the feelings, and the imagination of men have been roused to their greatest height and sounded to their profoundest depths. Within the span of a single generation are included Aristides and Marathon, Themistocles and Salamis, Xerxes and Plataea, together with Pindar and the three greatest of the Greek tragic poets. During the Peloponnesian War Athens, though just entering upon the decline of her political pre-eminence, was distinguished by the greatest activity in all intellectual movements; and Plato, of royal descent, was receiving the education befitting Athenians of his rank, including both gymnastic and musical culture. Although later, Plato was to share with Aristotle the first place among the Philosophers of Antiquity, he devoted himself to poetry, and composed poetry, before he took up philosophy. Indeed, it has been considered doubtful whether, regarding the dramatic setting, the consummate art, and the poetic elevation of his dialogues, we ought not to admire in Plato the artist and the poet even more than the philosopher. Later Philip of Macedon roused Demosthenes to defend Greek freedom against Macedonian military despotism, and inspired the greatest orator of antiquity to pronounce his immortal Philippics and Olynthiacs. In Egypt and in Persia, in Greece and in Rome, the periods most renowned for arms were most distinguished for learning. Alexander the Great studied philosophy with Aristotle. Julius Caesar was the great rival in eloquence of Cicero, and within their lifetime were born Virgil and Horace, who soon were to write verses which made them poets for all the ages afterwards.

In modern times, and in England alone, whether we take the history of the sixteenth, seventeenth, or eighteenth century, we find that the spirit of philosophy has had a concurrence, or a near sequence, with that of poetry.

When Harvey was preparing himself for his great discovery, when Bacon was exposing the errors, and discrediting the methods of the schoolmen, and teaching the direct way to advance the natural sciences, Spenser and Marlowe, Shakespeare and Ben Jonson, disregarding the old scholastic and theological subjects, were holding the mirror up to Nature, and by their lofty flights of imagination, and their poetic conceptions, were stimulating the minds of men to inquire into the unseen, to question the unknown, and to crave for the ideal; instead of resting content with the superstitions and dogmas, the prejudices and credulities of mediaevalism.

Milton the great poet of English liberty, Dryden the critical and the imaginative, and Samuel Butler, who wrote the first part of *Hudibras* "in the time of the late wars," were but a few years the seniors of Boyle and Newton.

Wycherley and Congreve, Pope and Young and Gay, were the contemporaries of, or nearly contemporary with, Halley the astronomer, Woodward the geologist, and John Locke the physician and philosopher. The mighty Shakespeare, the transcendent Milton, and the imaginative writings of those other great poets I have referred to, must have given a turn to every form of intellectual life in their own and the succeeding times, and must have served to quicken philosophers and scientists—not even excepting the brilliant Newton—to more creative work in natural philosophy, and to wider and deeper speculative musings in metaphysics.

BANKS AS A TEACHER OF ANATOMY.

No survey of Banks's work could have the slightest pretence to completeness without a reference to him as a teacher of anatomy. I have, of course, no personal knowledge of his capabilities in this way, but from the accounts I have read, and also from what I have learnt in conversations with an old pupil of Banks, I infer that few of the eminent anatomists of this country have taught their subject better than he did. Mr. Frank Steele—the former pupil of Banks whom I refer to, the son of the late Dr. Steele of Liverpool—who made several of the drawings for Banks's lectures, speaks of him as a fluent lecturer, an admirable demonstrator, a very skilful and rapid dissector, and an excellent draughtsman. Mr. Rushton Parker has told us that Banks had the faculty of putting things in a clear and attractive light, with abundance of ready wit and entertaining humour; that his lectures were most highly valued by generations of pupils, and that his aptitude for teaching anatomy so as to make it acceptable and useful to students was admitted to be exceptionally great.

He was, I understand, the real founder of the Liverpool School of Anatomy, and the first Professor of Anatomy in University College, Liverpool; and as Emeritus Professor of Anatomy took his seat as a member of the medical faculty of the University of Liverpool on the inauguration of the university in 1903. But besides his distinction as an anatomical teacher, Banks did research work in embryology.

He gained the gold medal given by the Faculty of Medicine for his thesis on the Wolffian bodies when he graduated M.D. of the University of Edinburgh in 1864. When the age of the author and the condition of the study of embryology at that time in this country, are taken into account, this thesis must be regarded as a piece of work of truly remarkable excellence. Banks was barely 22 years old, and although it is probable he obtained some suggestions and advice from Goodsir, Allen Thomson, and Turner (now Sir William Turner, F.R.S., Principal of the University of Edinburgh), three eminent anatomists with whom he had studied, yet very little attention had been given to embryology by British anatomists up to that time, and there was no English treatise whatever on foetal remains in relation to the generative organs, or on the development of those organs.

For information Banks had to look to either French or German books, and to such reviews and translations of them as he could find. He digested, collated, and criticized with considerable acumen the observations of Valentin, Bischoff, Rathke, Kobelt, and Kölliker. But he did not content himself by merely studying the results of the investigations of others; he made independent research. It was from original dissections that he formed his own opinions, judged the work of others, and gave expression to his assent or dissent to their conclusions. He did this with a courage, an honest frankness, but at the same time with a modest confidence in what he was writing, that are quite surprising in so youthful a critic.

If at times he rejected a conclusion for the unphilosophical reason that it was "*inconceivable*"; and if in one or two instances he supported an opinion on the unreliable ground that "*he could see nothing against it*," these were the exceptions to his usual method of argument.

His statements as to the formation of the globus major, and as to the ducts of the Wolffian body, of the genital cord, and the Müllerian ducts, are based on observations made with great industry and patience by means of a continuous succession of sections through the whole embryo.

His independence of judgement, and his terse and luminous sentences, give an earnest of that determination and discrimination with which in later life he took certain previously untrodden paths of surgical practice, notwithstanding discouraging and hostile criticism.

As a proof of the value set upon this treatise by one exceptionally well able to judge, I can refer to the fact that Professor Allen Thomson quoted some of Banks's conclusions in the article on embryology in the ninth edition of *Quain's Anatomy*, published in 1882.

Within the last twenty or thirty years embryology has made enormous strides, so that it is not a matter of surprise that some of the conclusions arrived at by Banks are not regarded as correct at the present day. Others, however, are strictly in accord with the latest knowledge. The great advances in embryology are due to the invention and perfecting of the microtome, and to the introduction of the graphic method of reconstruction, and the method of reconstruction by wax plates. With their aid, the genito-urinary organs, like most of the other organs of the body, have been restudied, and a great deal of information regarding them has been added. But in reference to Banks's work in this difficult field of investigation, I will quote from a recent letter written by Professor Peter Thompson, the chief of the Anatomical Department of King's College, London. He writes:

What I find most interesting about Banks's paper is where he says: "I have repeated their observations (that is, Thiersch, 1852, and Kölliker, 1861) many times on sheep from 2 to 4 in. in length, by hardening them in spirit and making sections through the whole embryo, from the commencement of the genital cord continuously down to the sinus urogenitalis." This was indeed pioneer work in the domain of embryology, since the first microtome which at all corresponded in principle to those in use at the present day is apparently the one described by Him in 1870.

Banks's difficulties were great, because the serial sections such as we can obtain to-day were out of the question in 1864 with the instruments then available.

BANKS AS A SURGEON.

There are three surgical subjects specially associated with Banks's name. They are *Cancer of the Breast*, *Radical Cure of Hernia*, and *Urethral Fever*.

Cancer of the Breast.

In thinking what I should make the topic of this lecture, I at one time decided upon "*Cancer of the Breast*; its treatment, and the scientific grounds upon which the surgical treatment is based." It seems to me the subject of all others to which Banks himself would have given the preference. But as I contributed a paper this year to the International Congress of Surgeons which met in Brussels in September last, and at which cancer occupied the greater part of the time of the various sections, and as "*The Diagnosis and Treatment of Cancer of the Breast*" was one of the special subjects of discussion at the Sheffield meeting of the British Medical Association in July last; and as, moreover, the Executive Committee of the Imperial Cancer Research have just brought out at very great cost a large volume, embodying three years' work by Dr. Bashford and his able coadjutors, which will supply an abundance of material for study for some time to come, I decided not to inflict upon you an address on cancer in any of its regular or heteroclitic aspects. I cannot, however, omit to allude to the important work done by Banks in relation to mammary cancer; and more especially because the opener of the discussion at Brussels on this particular branch of cancer—beyond the mention of Banks's name with many others in footnotes in connexion with statistics—made no reference whatever to Banks's important place in the history of the operative treatment of this disease. At Sheffield also no mention was made to Mitchell Banks's work except by one speaker, who referred to him to disagree with him about the removal of the pectoral muscles.

The modern history of the operative treatment of cancer of the breast and of Banks's connexion with it is very noteworthy, as it affords an excellent opportunity of studying the working of different philosophic methods in medical science, and I will therefore briefly recall it. From the first, with commendable candour, he explained that his views were in large part due to the paper by

Charles Moore, published in the year 1867.² Moore and his colleagues, especially Campbell de Morgan, were propounders of the theory of the local origin of cancer, and to them the sufferers from cancer are indebted for the advocacy and practice of extensive removal at an early stage of the disease.

Banks wrote:³

In 1867, Moore of the Middlesex Hospital, whose experience in that institution was great, wrote what is now an almost historic paper on "Inadequate Operations in Cancer," which set a great many minds thinking, inasmuch as it was a revolt against constituted authority. It made me think, too, so that I became deeply interested in the question.

Like Banks, Christopher Heath⁴ also tells in a clinical lecture, published in 1871, that it was Moore's paper which led him to perform as a routine practice much more extensive operations for mammary cancer than had been customary with surgeons previously. And amongst English-speaking surgeons Samuel Gross⁵ of Philadelphia made a notable move in 1880 in favour of the free removal of breast cancers, and pushed the cause with earnestness and enthusiasm. Gross also ascribed his own movement to the teaching contained in Moore's article, and which he alluded to as "*a remarkable paper*." And Gross was quite right, for Moore's paper is a very thoughtful and philosophical treatise, and an admirable example of a shrewd and correct induction from clinical experience leading to a most important improvement in surgical treatment.

It should be read by those extremists in our profession who think no progress in knowledge concerning medicine, surgery, or the ancillary sciences can be made except by experiments on animals; and who by their exaggerated language, and by ignoring or denying the many discoveries which have been made by careful observers at the bedside and in the *post-mortem* room, conduce to and increase the bitter resentment and hostility against all who practise these knowledge-giving and necessary experiments.

Moore proved by induction the correctness of the theory of the local origin of cancer. His conclusions and reasons are entirely in conformity with recent investigations on the minute anatomy and pathology of the lymphatics. He first briefly described the other chief theories of the origin of cancer, namely, the (1) constitutional, (2) textural or organic, and (3) the regional. The question as to which of these several theories is correct, Moore stated, can only be decided by accurate and repeated observations of the mode of recurrence. He pointed out that there are two elements of uncertainty in the question: uncertainty, namely, whether all the area invaded by the primary disease was removed by the operation; and the uncertainty respecting the possible duration of the *inactive* life in a fragment of cancer severed in the operation from the primary tumour, and left in the tissues.

If from a series of operations an imputation upon their completeness is justified, then, so far as those cases go, it is superfluous, he thought, to invoke either the constitutional, the textural, or the regional theory. He stated his opinion that the difficulties of completely extirpating cancer are much underrated by those who hesitate to suspect the perfectness of their operations; that there is too often reason, without calling in the evidence of the microscope, to expect the return of the disease.

His directions for operative treatment were that not only the whole mamma, but skin, lymphatics, fat, and when the disease even approaches the pectoral muscle, part of that muscle, too, should be taken away. The axillary glands, when diseased, should be cleared away in one mass with the breast, without dividing the intervening lymphatics. The flaps of skin should be undercut, so as to remove any undetected prolongations of the disease, and chloride of zinc should be applied to parts not capable of being removed.

With reference to the spreading of cancer, he said:

Its continuity with the first tumour was traceable over half the chest, the pleura, and the glands from the neck to the groin, or the inguinal region, and possibly also to the liver.

How possible, the interesting and important investigations of Sampson Handley have made very clear.

The reading of Moore's paper stirred Banks to adopt the routine removal of the whole of the fatty and lymphatic contents of the axilla, and of the skin covering the mammary gland, as well as very wide resection

of the fascia connected with the muscles upon which the mammary gland rests. He arrived at his own conclusions through witnessing many imperfect operations, and by observing the frequency of early recurrences, and the situations and tissues in which these recurrences appeared. He brought no fresh microscopical, anatomical, physiological, or chemical information to bear; but his conviction as to what the proper operative treatment should be was entirely an induction from clinical experience.

And he was induced to adopt this important change in practice because of the greater immunity from recurrent disease which he expected from it, and quite apart from the advances in aseptic and antiseptic surgery, although, happily, these advances gave courage to surgeons in the performance of the more extensive operations.

Banks read his first paper, which he styled, "A Plea for the more Free Removal of Cancerous Growths," before the Lancashire and Cheshire Branch of the British Medical Association in 1877, and in it he suggestively asked, "Have you ever imagined what the results would be if all cancers were thoroughly excised when they were no bigger than peas?" His next paper was published in 1883, and was read at Worcester and also at the Liverpool Medical Institution in the same year. He gave it the title, "Free Removal of Mammary Cancer, with Extirpation of the Axillary Glands as a Necessary Accompaniment."

Four years later, in 1887, he brought the same subject before the Harveian Society of London, and reported the results of 82 operations.

In 1900 he delivered the Lettsomian Lectures on "Practical Observations on Cancer of the Breast," and published the analyses of his results in 213 cases of operation.

The meeting of the Harveian Society listened to his paper without endorsing his views. Prominent London surgeons questioned whether the complete operation recommended by Banks and Gross afforded better results than the incomplete. One very distinguished surgeon among them thought that patients lived longest who had never been operated upon, and that it was a question for consideration *whether a local excretion of cancer did not render patients less liable to constitutional disease*.

Banks was very much cast down, he tells us, after this debate, but he returned to Liverpool resolved on continuing what he felt sure was the right line of treatment, but which he had so unsuccessfully come to London to advocate. He would, of course, listen to no argument in favour of a return on his part to the dangerous treatment, as he had learnt to know it to be, of excising the cancer tumour and leaving behind a remnant of the breast.

But the opposition to wide operations did not end with the discussion at the meeting of the Harveian Society. It was followed up in a work published shortly afterwards, in which the practice of removing the whole breast as a routine operation for mammary cancer was condemned; as also was the removal of glands which are not felt to be enlarged through the external textures; and disapproval was expressed of Kocher's operation for removal of the tongue.

It was considered that evidence pointed to the fact that large and sweeping operations which include not merely the free removal of the primary disease, but the dissection of the glands—whether in cases of tongue, vulva, penis, lower lip, or breast—were successful, not because the surrounding tissues were widely removed and the lymphatic ducts and glands taken away, but because the operations were practised in favourable cases.

If we seek for the reason why the wide operations proposed by Moore, and urged by Mitchell Banks and Gross, were opposed by able and experienced London surgeons, it is to be found in the difference in the philosophic views as to the origin of cancer held by those who proposed and those who opposed them. Whilst Moore and his school regarded cancer as being at first a local disease, the others held the view that cancer is primarily a blood or constitutional affection.

And if a blood disease from the outset, what was there to hope from these extensive removals of tissues? How could the excision of a local tumour eradicate a malady of the whole system? How could recurrence be prevented until the whole constitution was cured of its pernicious tendency? Hence it was thought to be "a question for

consideration whether a local excretion of cancer did not render patients less liable to constitutional disease"! I again quote these words because they give so emphatically the stamp and motto of the blood theory of the origin of cancer, to the opposition against extensive excision of local cancer.

But between this debate at the Harveian Society and the delivery by Banks of the Lettsomian Lectures in 1900 a good deal of water had ebbed and flowed at the mouth of the Mersey, and other barks had floated on the tide, carrying even larger sails than any that had been in view before.

It was now Banks's turn to cry, "Hold! Enough!" Not only had the constitutional theory of the origin of cancer receded before the strong and widely-rolling wave which placed the microbic theory for many consecutive years in the foreground, but in 1889 and 1892 papers which made considerable additions to our knowledge of the minute anatomy of the lymphatics of the breast and pectoral fascia were published by Heidenhain in Langenbeck's *Archiv*, vol. xxxix (1889), and by Harold Stiles in the *Edinburgh Medical Journal* in 1892 respectively. Both Heidenhain and Stiles agreed in believing that the entire mammary gland is not removed by the operations which had been usually practised, a fact about which anatomists, being also surgeons who had witnessed operations for breast cancer some years ago, must have been quite sure.

Both Heidenhain and Stiles gave expression also to their views as to the mode of extension, and the causes of local recurrence of cancer after amputation of the breast.

In 1894 Halsted of Baltimore, influenced by the work of Heidenhain and Stiles—twenty-seven years after the publication of Moore's paper and seventeen years after the first paper by Banks—published⁶ the results of some very extensive resections which he had been led to perform, and gave a description of the steps of his operation illustrated by plates. The consequence was that a revision of the practice of performing limited operations was deemed to be necessary by those who had previously resisted the more extensive excisions. Thus a reform which had failed to be effected by the correct induction from clinical and pathological observations of Moore, De Morgan and Banks, and others, was brought about by the practice of Halsted, based as it was on a deduction both premises of which were imperfect—namely, that the disease spreads in the course of the lymphatic current and by an embolic process.

Although Heidenhain had not altogether overlooked the process of permeation and the tendril-like growth of lines of cancer cells along the fine lymphatic vessels, he had failed to appreciate its significance as the principal agent in dissemination. It was left for Sampson Handley, now a successor of Moore and De Morgan on the staff of the Middlesex Hospital, to find out, in the course of his investigations during the last three or four years in the cancer research laboratory of that hospital, that permeation—or, in other words, the growth of the cancer cells in the lumen of the fine lymphatics—takes place almost as easily against as with the lymph stream, and extends centrifugally from the primary tumour, in an ever-widening circle and in the plane of the principal lymphatic plexus—that is to say, along the deep fascia.

"According to the embolic theory" of Heidenhain and Stiles, "which four years ago met with general acceptance, the force of the blood and lymph streams was the principal agent in dissemination, the cancer cells themselves being so much driftwood" (Sampson Handley).⁸

Halsted's operation was based on the embolic theory, and Halsted's operation based on this theory, was what converted the opponents of Banks. It is intelligible how an operation based on an erroneous theory can be at once too extensive and too limited, and this is the case with the method of Halsted.

Moore had written: "It must be considered that operations are not adequate merely because they have been large"; and Handley points out that an operation may be very extensive and yet futile if it is based upon false pathological premises. Handley's careful investigations have shown that the very extensive removal of skin practised in the Halsted operation is unnecessary; and, on the other hand, that a sufficiently wide removal of the

deep fascia, and the requisite undermining of the skin flaps form no part of Halsted's technique.

And, in regard to the removal of muscle, Handley does not consider it dangerous to leave the uppermost part of the great pectoral, provided it be well retracted upwards, while the costo-coracoid membrane and the sub-clavian glands are being removed. The removal of the pectoralis minor, he thinks, is a mere matter of convenience to the operator; but as the serratus magnus is a far greater source of danger, he advises that at least a superficial layer of this muscle should be taken away, and in advanced cases the removal of the whole thickness of it together with the overlying breast is requisite. This teaching, based on microscopical pathology, is almost identical with that put forward by Moore and Banks on clinical grounds.

In the course of the thirty-three years since Moore wrote, the surgical pendulum had swung from one extreme to the other, and in so doing, Banks thought it had got somewhat off the line. And so in his Lettsomian Lectures⁹ he, with the courtesy which softens controversy, but with the candour and straightforwardness which command respect, commented on the work which had lately been done by others. He desired to dissent absolutely from Dr. Halsted's view that the entire pectoralis major needs to be removed, that the pectoralis minor should be divided, and that the supra-clavicular region should be invariably cleared out. He refused to recognize that the rule ought to be "operate on the neck in every case," and he opposed to that apophthegm his own experience to the effect "that when cancer has laid hold of the supra-clavicular glands it has got such a grip as to be past extirpation." Banks supported his views by his operative results, which, though equalled, have not been surpassed by others.

It is evident, reading between the lines, that Banks was smarting under a sense of wrong done to him, for he says:¹⁰

In justice to myself, and to the memory of my late friend Gross, I think it rather hard that a certain group of surgeons should date the origin of the modern operative treatment of cancer from the microscopic researches of Heidenhain and Stiles, and the labours of Halsted and themselves.

If this opinion be not now unanimously held by surgeons, I cannot doubt that it will be by posterity, who will be able to take a dispassionate view of the history of the treatment of mammary cancer from the date of Moore's paper in 1867 to Banks's Lettsomian Lectures in 1900.

Perhaps Banks was too optimistic if he expected to be a prophet in his own land and among his own people. For it has not infrequently been noticed that if a suggestion come from across the seas, and especially if it be made in Germany, it is much more likely to "catch on" than if it emanates from one of our own people.

But I have the satisfaction of referring to one of his English commentators, at least, who takes a fair and correct view of Banks's rightful claims.

Jacobson in *The Operations of Surgery*¹¹ says:

The tendency nowadays to talk and write as if the origin of the modern operative treatment of cancer of the breast dated to the labours of Heidenhain, Stiles, and Halsted, ignores most unfairly the work of others who should not be forgotten. And one name at least—that of an English surgeon—rises pre-eminently as a worker in this field. I refer to Sir W. M. Banks. For twenty-three years this surgeon, with unflinching earnestness, and characteristic vigour and terseness of expression, has in many places urged the need of more extensive operations in this disease.

And then Jacobson adds: "If honour is to be given where it is due, a fair share of it must justly fall to him."

I have neither time nor intention to introduce a description of what my own practice has been, and is, in the surgical treatment of breast cancer. As a member of the staff of the Middlesex Hospital, I followed Moore and De Morgan; but I should like to mention that for many years I have been in the habit of removing the fascia over the serratus magnus, and often a layer, and on occasions, in places, the whole thickness of the muscle; and that as a teacher at my hospital and an examiner at the University of London and the Royal Colleges, I was in the habit of pointing out to the students and candidates that a good half of the mammary gland was in contact with the serratus magnus, and only the rest with the pectoralis major; whereas their answers to questions led one to

believe that the general impression they had got was that the mamma rested entirely on the great pectoral muscle.

I think that too much importance is just now being attached to clearing the axilla from the apex downwards, instead of from the base upwards. The essential precaution is to avoid cutting across the lymphatics or leaving behind any glands or fatty tissue.

Nor do I believe in the danger of making a direct cut into, or even of removing a slice from, a cancer tumour, for the purpose of diagnosis, before the operation is commenced, provided the wound is stuffed with a little aseptic gauze and covered with collodion previous to proceeding with the excision, and the knife used for the incision is laid aside. The danger lies in inadvertently dividing affected infiltrated tissues, which was so frequently done in the days of inadequate operative measures.

Radical Cure of Hernia.

Banks was an early advocate of the so-called radical cure of hernia, or, as he preferred to call it, "the curative treatment of hernia by operation."

His first paper on the subject was read before the Surgical Section of the British Medical Association at Worcester in 1882. Subsequent communications from his pen followed, and are to be found in the *Medical Times and Gazette*, July, 1884; and in the *BRITISH MEDICAL JOURNAL* of December 10th, 1887, and November 11th, 1893.

He took exception to the term "radical cure" as being misleading, because it implies, what is far from being the truth, that a patient upon whom the operation was performed need never again wear a truss, nor ever again be in danger of his hernia coming down. He had come to the opinion that only a few patients after the operation were able to do without a light truss; and with commendable candour and straightforwardness he reported that rather more than 13 per cent. of his own operations were complete failures, and rather more than another 16 per cent. were only partial successes. He had operated upon all sorts of cases—on the young, middle-aged, and old.

He recommended the operation in children only when it was impossible to get proper attention given to the wearing of a truss.

He recognized the dangers of operating on ruptures of very great size, but he undertook such cases when the sufferers were rendered quite unfitted for work of any kind, and were willing to run the risks of the operation.

Banks's operation has an established place in surgical textbooks under the name of "the method of Banks." The method has the merit of great simplicity, and he adhered to it in his own practice, notwithstanding the objections urged against it by other surgeons, on the ground of insecurity.

In the discussion at the Newcastle meeting in 1893¹² Mr. Godlee is reported to have said:

Every one should recognize the fact in every case of hernia, whether strangulated or not, he might expect that by performing Banks's operation he would effect an efficient radical cure.

With rare exceptions Banks's operation is the one which I have invariably used, though I have not followed him in the employment of silver sutures.

Urethral Fever following Catheterism.

Before Banks was 29 years of age his name was brought prominently before the profession by a paper which he published in the *Edinburgh Medical Journal* in June, 1871, on Urethral Fever following Catheterism. The foundation of this article was the knowledge derived from actual cases which had been under observation in the Liverpool Royal Infirmary, but before coming to his conclusions he took pains to inform himself as to what had been found out and made known on the matter by others. The article is written with the clearness, terseness, and succinctness which characterize his other writings. Banks considered the disease from the anatomical, pathological, and clinical standpoints; but the particular case which formed the text of his paper was that of a young apparently healthy man who had a narrow urethral stricture, and who after the passing of a bougie was seized with pain, rigors, retching, rapid pulse, convulsions, and coma, and died within six and a half hours of the use of the instrument.

In the discussion in December, 1883, at the Medical Society of London "On Catheter Fever," Sir Andrew Clark, who opened the debate, referred to Banks's paper in the following words:

In 1871, Banks of Liverpool described the effects of simple catheterism, and classified them into three varieties: First, rigor with malaise, recovering speedily; second, rigor with malaise and prostration, followed in a few days by fever and death; and third, cases of shock, producing death within sometimes a single day. Admitting that renal disease predisposes to such attacks, he denies that they are due to suppression of urine, and he ascribes them to shocks of greater or of less severity propagated through the sympathetic nervous system. Here the nervous theory begins to preponderate over the others.

Banks divided the cases of feverish symptoms following surgical interference with the urethra into two groups—namely, those indicating a pyaemic or septicæmic state, and "due to an altered and poisonous condition of the blood acting secondarily upon the nervous system," and those "indicating the existence of shock, where the nervous system is primarily affected, and by its reaction upon the blood and vascular system produces the outward signs of fever, or at any rate of pyrexia."

In a recently published lecture on coli-bacilluria, the writer, referring to the debate at the Medical Society, says that it is a national gratification to know that interest in the subject of urinary infection was first aroused by Sir Andrew Clark's paper; and also that the name "bacteriuria" was invented by an Englishman, Sir William Roberts, when in 1881 he described a condition in which the sediment of fresh urine was found on microscopical examination to consist largely of rod-shaped organisms. It was left, however, for much more recent workers to point out that the growth of organisms in the urinary passages was capable of producing signs of systemic poisoning.

Cases of fever following the use of the catheter or sound, and such as are now often ascribed to the poisonous action of the *Bacilli coli*, had been described under various names many years before 1883, and Banks had given the references to several such. Twenty years before the discussion referred to, in which, besides Sir Andrew Clark, Sir Henry Thompson, Sir Joseph Fayer, Sir William (then Mr.) Savory, Mr. Berkeley Hill, Mr. Reginald Harrison, and others had taken part, there had been at the same society "a considerable discussion" (October 26th, 1863) on the same subject. But, owing to the fact that the *Transactions of the Medical Society of London* were discontinued to be published in 1862 and did not again appear till 1872, the only account of this discussion I can find is a very brief one in the *Lancet* of October 31st, 1863 (p. 510). Nevertheless, it had not escaped the diligence of Banks when preparing his own paper.

Although Sir Andrew Clark in 1883 endeavoured to focus attention on one form (but not the septically-infectious) of urinary fever which he designated catheter fever, it is difficult in the face of the writings of Velpeau in 1847, of Civiale, of Heyfelder in 1857, M. Phillips in 1858, and Marx in 1859, all of which had been published prior to Banks's article; as well as of the work on *Fever of Diseases of the Urinary Passages*, by Malherbe, which appeared two years after Banks published his article, and ten years before the debate of 1883—it is difficult to see how we can as Englishmen take the flattering unction to ourselves that interest in the subject of urinary infection was first aroused by the discussion at the Medical Society in 1883.

Notwithstanding the interest attaching to the history of urinary, including catheter, fever, a question of more importance at the present moment is the causal relationship of coli-bacilluria to the various classes of cases formerly described under the general term "urinary fever."

A perusal of many published cases shows that the urine has been found loaded with the *Bacilli coli* in patients with symptoms and histories exactly coinciding with those of urinary fever in its several forms. But it is not on that account conclusive that the presence of these bacilli in the urine has been the cause of the attacks in all the cases in which fever and rigors have occurred after simple catheterism; or after the passage of an instrument in persons with prostatic hypertrophy, or stricture, or other conditions demanding the use of the catheter or

sound; or after other disturbances to the urethra when no instrument has been passed.

The facts at present known want sifting and checking, by closer clinical observations, by bacteriological examination of the urine both before and after the use of any instrument upon the urethra, and by laboratory experiments, before we are in a position to frame a general principle.

The *Bacillus coli* is credited with the power of causing in some persons the most violent and acute infection, attended with rigors and pyrexia oscillating between 101° F. and 105° F. or higher; in others, a chronic infection with symptoms varying from a few that are slight and unimportant, to others of considerable severity; while in other persons no symptoms suggestive of impaired health in any way have been present.

In some cases severe pain, in others only slight pain or a little burning along the urethra has been complained of; some have had frequency of micturition and a large output of urine, in others the urine for a time has been scanty; in some there has been an intolerable, or at least distressing, irritation, while the only symptom noticed in others has been an offensive fishy or sulphuretted hydrogen smell with the urine.

In some cases one kidney, in others both, and in a third set the spleen is said to have been enlarged. Influenza, malaria, tuberculosis, gout, rheumatism, duodenal ulcer, typhoid fever, and pernicious anaemia, are some of the chameleon-like aspects which coli-bacilluria is said to have assumed. In other cases cystitis, ureteritis, pyelitis, enuresis, dysentery, mucous colitis, chronic undulating fever, and otherwise unexplained attacks of pyuria are reported to have been produced by the colon bacillus.

Whilst these micro-organisms may be present in large numbers without causing symptoms and without being accompanied by any other abnormal contents in the urine; there may, on the other hand, be gross suppuration, or a turbidity of varying degree, and haematuria may or may not complicate the urinary conditions.

It is a very remarkable fact that the *Bacilli coli* should be capable of stirring up in some persons, and on some occasions only in the same person, such varying and severe and even alarming symptoms; while in other persons, or at other times in the same person, no symptoms whatever are excited in response to precisely the same stimulus.

Nor can one close one's eyes to certain marked differences between the classical features of urinary fever, on the one hand, and those of acute and chronic infection of the urinary tract described as due to *Bacillus coli*, on the other hand. For instance, I am not aware of any case of sudden death from coli-bacilluria, like those tragic events following the use of the catheter, such as Velpeau, Bérard (quoted by Velpeau), Heyfelder of St. Petersburg, Henry Smith, Sir Joseph Fayrer, and Banks, and others have reported. Secondly, urinary fever has been met with chiefly in men, very rarely in women; and in adults, not in children. As Banks says:

It seems highly probable that this sensitiveness of the urethra is heightened after the accession of puberty, as I do not know of an instance of death, nor indeed of subsequent shock or fever, following the mere examination of a child for stone, which is an operation sometimes involving a good deal of rough handling of the urethra.

And although Savory remarked in 1883 at the Medical Society that it was not very uncommon for great shock to follow the introduction of a catheter upon the young and healthy, or the sounding of young children for stone, I think the general experience of surgeons accords with that of Banks.

But bacterial infection of the urinary passages in young¹³ children—in infants indeed under 18 or even 12 months of age—is said to be quite frequent. Many varieties of micro-organisms have been detected in the urine, but the *Bacillus coli* is by far the most common. Coli-bacilluria is much more frequent in female children than in male. In women, according to Dr. Dudgeon, the *Bacillus coli* multiplies in the urine, giving rise to bacilluria, and sometimes to pyuria, and every now and then provokes pyrexia. Some women have many attacks of fever without any definite ascertained cause to explain them; but it is the custom now to attribute to the *Bacillus coli* those acute and subacute infections of the urinary tract coming on without any apparent cause. In men such cases are

rarer, and it is usual to obtain in them a definite history of an abnormal state of the urine.

The so-called pyelitis and pyelonephritis of pregnancy are also now usually ascribed to coli-bacilluria, but on insufficient grounds. The *Bacillus coli* is not infrequently found in the urine of pregnant women with complete absence of any symptoms pointing to an abnormal state of the urine; and there is "conclusive evidence that the organism which is regarded as the common cause of acute infection of the urinary tract in pregnant women may be present in the urine in pregnancy, and during the puerperium, without producing any acute symptoms" (Dudgeon).

Again, how, on the bacteriuria theory, can we explain single attacks of transient rigors and pyrexia?

An elderly man, in good health but for epithelioma of the glans penis, and without any obstruction to the outflow of urine, is suddenly seized with a prolonged rigor, faintness, and sickness, at the moment when during micturition the growth opened into the urethra and urine escaped through the resulting fistula. In a few hours he was quite well again, and though urine afterwards always escaped through the abnormal opening whenever he passed water, yet he never again had a similar seizure or any other form of urinary fever.

A man, aged 65, keeping his bed during convalescence after the radical cure of hydrocele, one day had a seizure of shivering and a temperature of 101° F., after passing a bougie for an old stricture. After a few hours he was quite recovered from the attack. He continued the use of the bougie every other day, but he had no further rigor or pyrexia.

Are such transient, though severe, attacks due to *Bacilli coli*, or are they—as Banks, Savory, and Clark would have said—reflex nervous effects? Other important questions need to be answered.

1. How are we to explain the absence of rigors, pyrexia, malaise, etc., in cases in which the urine is known to be teeming with countless thousands of the bacilli?

2. Considering the pertinacity, the almost incurability, of coli-bacilluria, how is it that a lifelong use of the catheter excites only one attack, or even it may be no attack at all, in the subject of the bacillurial affection?

3. If it is only, or specially, in persons with coli-bacilluria—with or without any indication of change in the urine other than the microscopic evidence of the bacilli—that urinary fever occurs after urethral instrumentation, would it not be well to exclude the use of instruments in every case where it is possible to do so, until after a bacteriological examination of the urine has been made?

In the cases in which the use of instruments is followed by cystitis and alkaline urine, the micro-organism introduced is of the *Proteus* type, or some form other than the *Bacillus coli*; and with our present antiseptic precautions such cases are becoming very rare. But with the *Bacillus coli* it is different; there are so many channels other than the urethra by which it may gain access to the urinary tract. Moreover, urinary infection from *Bacillus proteus* is often rapidly cured by urinary antiseptic drugs, with or without lavage of the bladder.

4. As irrigation of the bladder, anticolon-bacillus serum, the vaccine treatment, urinary antiseptics, etc., all seem to be useless in chronic and subacute cases of coli-bacilluria; and as spontaneous recovery is known to occur, is it worth the patient's while to undergo any form of treatment for chronic and subacute cases of coli-bacilluria? Some most acute cases are reported to have been much benefited by the serum treatment so far as the general health was concerned, though the *Bacillus coli* could still be cultivated from the urine.

5. When the kidney is the seat of diffused minute abscesses attributable to the *Bacillus coli*, is the best treatment nephrectomy, which, though successful in some, has been in other cases rapidly fatal? or should a renal exploratory operation terminate with the simple exposure to view of a kidney in such a state, and the wound then closed—a procedure which has been followed by complete recovery?¹⁴ or should the case be left uninterrupted by any surgical operation, as experience in other cases of good results from non-interference indicates?

6. How are we to determine the precise limits of the infection in any particular case of coli-bacilluria? That

is, as to whether pyelitis, ureteritis, and cystitis exist separately or together.

There are many other points connected with colibacilluria which require to be considered before any accurate generalizations can be arrived at, and before we can introduce into the present discord of facts the harmony as yet not understood.

I have touched on the fringe of the relationship between colibacilluria and urinary fever, because of Banks's article on Urethral Fever following Catheterism. Like cancer, bacteriuria and urinary fever might well have occupied the whole hour. Either, indeed, would furnish material enough for two or three lectures.

I learn from the Calendar of your University that the William Mitchell Banks Memorial lecturer is expected to deliver a lecture, or course of lectures, on some one subject. I hope I have not committed any serious breach of the regulations if instead I have touched upon several subjects in one lecture.

By doing so I have sought to pay tribute and do justice to the professional worth and work of Sir William Mitchell Banks himself, and at the same time, and in that way, to do honour to the medical faculty and to the whole university of which he was so distinguished a member.

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An Address

ON

THE VALUE OF CHROMOCYSTOSCOPY IN THE DIAGNOSIS OF RENAL DISEASE.

DELIVERED AT THE ANNUAL MEETING OF THE SOUTH
WALES AND MONMOUTHSHIRE BRANCH OF THE
BRITISH MEDICAL ASSOCIATION.

By W. F. BROOK, F.R.C.S.,

SENIOR SURGEON TO THE SWANSEA GENERAL HOSPITAL; PRESIDENT
OF THE BRANCH.

GENTLEMEN,—It is the established custom of our Branch that every President upon his induction to the chair should read a short address dealing with some matter in which he has especially interested himself, and I am going to ask you to bear with me for a few minutes while I endeavour to bring to your notice a point in connexion with renal surgery which in my opinion has not received the attention which it deserves, namely, chromocystoscopy.

The introduction into the blood current of pigments which are excreted by the kidneys, and the observation by means of the cystoscope of their elimination by the ureters, as an aid to the diagnosis of renal disease is a method of investigation which is by no means new. Considering, however, its simplicity, and the amount of information it affords, and the vital importance of that information, it is a matter of surprise to me that it has not before now been generally adopted as a routine clinical method. Methylene blue was the substance first used, but it was found in practice to give uncertain results, since a large but variable quantity was converted into and secreted as a colourless product. In 1903 two German surgeons, Voelcker and Joseph, drew attention to the value of this method of investigation, and it is from a paper of theirs, published in that year in the *Berliner klinische Wochenschrift*, that I now largely quote. They used sulphindigotate of soda (so-called indigocarmine), which was employed many years ago by Heidenhain in his investigations of renal secretion.

Heidenhain's researches were probably familiar to all of us in our student days. It will be remembered that when

a solution (or, to be more correct, a watery emulsion) of sulphindigotate was injected into a vein it was practically wholly recoverable in the urine. Moreover, that when the urinary flow had been arrested by section of the cord below the medulla, and the animal killed at a suitable time after the injection and during the process of excretion, upon the kidneys being examined particles of pigment were found in the renal epithelium and tubules, but none in the Malpighian bodies. In other words, the pigment particles were eliminated solely by the actively secreting structures of the kidney—to wit, the epithelium of the tubuli uriniferi—and not at all by the filtering apparatus, the glomeruli and Malpighian capsules. Sulphindigotate of soda is absolutely harmless, is entirely excreted unchanged, appearing in the bladder in from twenty to thirty minutes after being injected, and is mostly eliminated in about two hours. About a drachm of a 4 per cent. solution freshly prepared with sterilized water is injected into a muscle. The solution itself must not be boiled or it becomes gelatinous. It is as well to give a diuretic or a large quantity of water shortly before the examination, as otherwise the commencement of excretion may be much delayed. If general anaesthesia is not being employed the urethra and neck of the bladder are prepared in the usual way with cocaine. The examination is conducted in urine if this is clear and has not been too deeply stained by pigment already excreted, otherwise in boracic lotion or sterilized water. Upon cystoscopying a healthy bladder about half an hour after the injection the phenomena observed are as beautiful as they are striking. Jets of blue urine are seen to issue alternately from each ureteral orifice spreading out into a thin cloud towards the base of the bladder. The more distended the bladder the finer and more forcible are the jets.

In a bladder containing only a few ounces the jets are shorter and thicker.

Clinically, it may be contended that chromocystoscopy does not take the place of separately collecting the urine from the two ureters for examination, whether by cystoscopic catheterization of the ureters or by separation by means of Luys's instruments. It is, however, much simpler and more quickly conducted than either, and almost always renders either quite unnecessary. Moreover, it is free from the objections and fallacies from which these suffer. Thus catheterization is not only a difficult and lengthy proceeding, but it is also open to the objection that it may carry infection from the bladder into the ureter. Furthermore, the catheter does not necessarily collect all the urine from a ureter, jets having been observed to escape by its side; also the catheter may become blocked by a blood clot or pus, which has happened to myself.

The use of the separator, too, is not satisfactory without a cystoscopic examination, since bladder disease—for example, ulceration or cystitis—may be one-sided and account for pus in the urine drawn from that side of the bladder which would be erroneously attributed to the corresponding kidney. Then, again, in the presence of an enlarged prostate both catheterization and separation are generally impossible, while in many conditions the ureteral openings may be very difficult or impossible to find—for example, in ulceration, cystitis, or in the presence of growths. Here chromocystoscopy is applicable, and even though one or both of the ureteral orifices be completely hidden, by comparing the coloured jets it may be possible to say this or that kidney is or is not doing its full share of work.

For the last four years I have been making use of chromocystoscopy in my practice, and by way of illustrating my remarks I have selected a few cases, mostly recent, in which I have found the method of help not only in the estimation of the working capacity of a diseased kidney and of its fellow, but also in the diagnosis of renal disease and the differential diagnosis of renal from other abdominal tumours. I think it will be of greater interest to you that I should go into these few cases somewhat fully, than that I should merely tabulate a much larger number. The first case I cite I came across only a few weeks ago. It was a case of a large right-sided hydronephrotic kidney which varied in size in a young woman under the care of Dr. Cameron of Swansea. Now the treatment of hydronephrosis, unless it is seen in its early stages and is due to mobility of the kidney and kinking of the ureter, when nephropexy or