AN ADDRESS ON CATHETER-FEVER.

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SOMEWHERE about the year 1850, one of the medical officers of Haslar Hospital (who, I dare say, will be kept in remembrance by my former messmate, Sir Joseph Fayrer), between fifty and sixty years of age, of nervous constitution, but apparently in robust health, requested me to examine his urine. Accordingly, I submitted it to as careful an examination as I was then competent to make, and all that I could find out against it was, that it was too great in quantity, too low in density, and too pale in colour. He then informed me that he had trouble with his bladder, that he meant to consult Sir Benjamin Brodie, and that he wished me to accompany him for that purpose to London. Sir Benjamin examined his patient; drew off a large quantity of urine from the bladder; told him he was suffering from simple enlargement of the prostate; prescribed the regular use of the catheter; and, with a few general cautions against a careless diet and exposure to cold, he quickly but kindly dismissed us both. We returned to Haslar. In about a week, the patient was free from local discomfort, and without complaint of his general health; but then he began to feel and to look ill. He complained of *malaise* and weakness, and of general pains. He lost his appetite; was tormented by thirst; had nausea; became feverish; took to his bed; got daily worse; and, notwithstanding the efforts of his colleagues, who could not agree as to the nature of his malady, he died in about three weeks from the beginning of his illness. No *post mortem* examination was allowed. The case here so imperfectly narrated made upon my mind an impression which has never been effaced; but, until about the year 1865, I saw no other case exactly resembling it.

In that year, I was summoned by Mr. Peter Marshall to visit a gentleman suffering from fever. Certainly he was in, what is called vaguely amongst our elder brethren, a typhoid state. He was between fifty and sixty years of age. He was lying on his back in bed, apparently in a state of great prostration. The face was faintly yellowish and mottled, the lips dry, the pupils dilated, and the breath foetid. The tongue was small, brownish red, dry, tremulous; there was complete anorexia. The bowels had been imperfectly relieved. The urine, habitually removed by the catheter, was low in density, acid, and deposited, on standing, a little muco-pus, and contained a small quantity of albumen. The heart's action was quick and frequent, the pulse small and compressible. The bases of both lungs were congested. The skin, subicteric, and for the most part rough and dry, was here and there, chiefly about the hands and feet, bedewed with watery sweat. The acuteness of all the special senses was blunted; and the patient, dull, heavy, and indifferent, could yet be roused to speak, and to answer clearly questions put to him. The temperature of the body at the time of examination in the afternoon was 103°. The story of this case resembled the story of the case first narrated. The patient, supposed to be healthy but suffering from an affection of the bladder, was, a few weeks before my visit, placed upon the daily use of the catheter, did well for about a week, then became ill, and fell suddenly downward into the condition in which I saw him. Neither Mr. Marshall nor I ventured to form a definite diagnosis of the nature of the patient's malady; but, remembering the case at Haslar, I suggested that the fever, which, we agreed, was not a specific fever, had originated out of the condition begotten by the entrance upon catheter-life. I saw the patient only once. The remedies proposed, the food, the alcohol, the quinine, the aperients, were all of no use to the patient, and he died within a week of our consultation. With great difficulty, permission was obtained to make a *post mortem* examination; and, although it was made with both care and interest, nothing definite was found outside the bladder, and nothing in it sufficient to necessitate or account for death. The prostate was enlarged, the bladder was dilated and thickened. Viewed from the inside, it was trabecular and slightly saccular; the mucous lining was congested, and, in part, eroded, and everywhere coated with a greyish-white stinking mucus.

There was nothing to be detected in the urethra or in the kidneys although they were carefully examined. I was accustomed to such examinations, and neither Mr. Marshall nor I could say anything better of the cause of death, than it was due to irritative fever.

The study of this case gave birth to the opinion (now a confirmed belief) that the entrance upon catheter-life occasionally gives rise to a pernicious fever, which, in the majority of instances, destroys life, and is sometimes, without the intervention of any sensible structural change, sufficient to account for death. Since 1866, every year has added to my experience of such cases; and, for over ten years at least, I have been in the habit of mentioning them to surgeons with whom I have had the privilege of consulting. From Sir James Paget, and especially from Sir Henry Thompson, whom I have often met in such cases, I have received at various times much important information; but as I have received from neither of these distinguished surgeons explanations completely in harmony with my own experience and views of such cases as the one just narrated, and as further information might be now in their possession, or in the possession of others, I ventured, upon a recent occasion, at the Clinical Society, to mention the subject in the way in which it has occupied my thoughts, and to invite from my surgical contemporaries their latest experiences and conclusions concerning it. The constitution of the Clinical Society did not permit abstract questions of this kind to be discussed, and my object was not to offer, but to receive, information. My remarks, finding a place in the medical journals, and being by them widely disseminated, have elicited from practical surgeons in all parts a variety of interesting and instructive communications concerning the nature and causes of the fever which occasionally follows casual or habitual catheterisation. Nevertheless, as the exact character of those remarks which I made has been, doubtless by my own fault, misapprehended, and as the subject is of such importance as not only to justify, but to require, the statements made respecting it should be accurate and clear, I gladly, as I have said, comply with the request conveyed to me from various quarters to re-open the subject. Now, sir, it is not my intention on the present occasion to narrate a series of cases and to build thereon a dissertation upon what, for the sake of provisional convenience, I have called catheter-fever. I have not, indeed, at my command the materials necessary for such an undertaking; and if I had, devoid as I feel myself to be of the enlightenment and strength which flow out of the surgical instincts begotten by long surgical experience, I would not attempt it. I have seen many cases of catheter-fever, but I have never had charge of one. I have visited my cases only occasionally, and in consultation with other practitioners; and almost always my colleagues have been too busy to keep and to furnish me with minute and continuous records. Indeed, from the present temper of the public mind, the fear which patients have of being made the subjects of experiment, and the demands so thoughtlessly and even cruelly made upon a practitioner's time, it has become increasingly difficult to keep clinical records of any case for oneself; and the public, in what I must call its vicious stupidity, is thus hindering us from helping it so well as, otherwise, we might have the power to do. But, although my records of individual cases of catheter-fever are thus, from the necessity of the case, incomplete, I venture to think that, both by the study of their salient characteristics and by the questioning of those in charge of them, I have learned at least enough of their nature and importance to justify me in making certain propositions, and in asking certain questions concerning them. The statements which I propose making about this catheter-fever will be most conveniently embodied in a short series of propositions. But, before submitting them to your consideration and criticism, it will be necessary, in order to obtain a clear and comprehensive understanding of the subject underlying them, to discover the origin and to follow the historical development of those ideas which have shaped the theories of surgeons and determined their lines of practice.

I have placed here a table, which, I regret to say, is imperfect. It is a table prepared by my assistant, Dr. Delépine, who has devoted to it most careful study and most conscientious care; and it has, on account of the deficiency of indices to books, occupied an amount of time and caused a degree of labour which you can scarcely imagine. I shall have it, for further purposes, completed. In the meantime, it will enable those who do not follow me closely in reading to determine for themselves the order and development of the ideas which have been promulgated concerning this subject. I shall not, in my retrospect, follow as minutely as is here followed the development of these ideas; but I shall take the leading ideas;

for, by holding to these leading ideas alone, I think we shall be able to survey the whole field.

In 1800 it was known, but not distinctly expressed, that surgical interference with the urethra and bladder was sometimes, and in certain circumstances, followed by an irritative fever. It was not, however, until 1810 that Moffat, as quoted by Velpeau, described a case of chronic stricture of the urethra, in which simple catheterism was followed by rigors, irregular fever, purulent arthritis, as it is called in France, and death. In 1832, ideas concerning the causal relationship of catheterism to consecutive fever first found form and expression in the writings and teachings of Brodie and Velpeau, and perhaps of Civiale. Brodie distinctly, and even emphatically, mentions the dangers of catheterism, and describes as occasionally occurring in consequence of it, paroxysms of irregular fever—like ague, and leading sometimes to prolonged debility, sometimes to continued fever, with rheumatic pains, and sometimes even to mania. He further says that, in such cases, death may follow, but he cites no case of its actual occurrence. Velpeau enters much more minutely into the nature and relations of the fever thus mentioned, and contributes several new ideas to the further development of the subject. He alleges that in some persons, perfectly healthy, not malarious, not exposed to its influences, even easy catheterism may develop a consecutive and continuous fever, and that this fever has five varieties. In the first it consists of a single paroxysm of fever, ending in *malaise* and debility, with recovery in a few days. The second consists of recurring paroxysms of fever, issuing in continued fever, and oftentimes fatal. The third consists of an inflammatory fever arising out of nephritis, phlebitis, and other local inflammation. The fourth consists of fever, associated with purulent arthritis. The fifth consists of a rapid succession of violent paroxysms of fever, speedily ending in collapse and in death. Velpeau then points out, and very clearly, that in the second and fifth varieties he has never found present at the necropsies any adequate structural cause of death, and in these cases he is disposed to regard the origin of the fever as caused by the re-absorption of vitiated urinary constituents. But on this, and indeed on similar points of pathogenesis, he is both obscure and vague. Civiale, whose great work is disfigured by passionate claims of priority in this matter (of which claims he furnishes no proof), and by the satirical invective which he launches against the juster and greater Velpeau, gives a full and admirable description of this catheter-fever; but, whilst admitting that the fever is due sometimes to the urethra, and sometimes to the bladder, and contending that the fever of the one differs essentially from the fever of the other, he almost angrily minimises the effects of surgical interference, and, it would seem as if with a judgment disturbed by emotion, contends that in most cases the fever has existed before the use of the catheter, or that it is due to nephritis.¹

But in the latter part of Civiale's work, marked by greater sobriety of language and a more judicial tone of argument, (page 612, ed. 1860), he distinctly qualifies these strong assertions, changes his point of view, and says that the cause of fever is vague and uncertain, and that, in speaking of its nature, we can only guess.

In 1858, Mr. Phillips contributed some further ideas concerning the conditions under which this fever is developed. Describing the fever, and in the main following the classification proposed by Velpeau (who, it may be remarked, is the source whence almost all subsequent writers have derived their inspiration, and sometimes, without acknowledgment, their ideas), he asserts, first, that the simplest as well as the severest catheterism, with the largest or the smallest instrument, may originate the fever; secondly (and this is the most important contribution, if it be a true one, which he has made), that, unless the affection of the urinary passage lies behind the bulb, the fever does not follow; and, third, that the predisposition to the fever does not lie in the state of the nervous system at all, but in the existence of certain diatheses and in chronic disorders of the general health.

In 1859, Marx, in a monograph of remarkable merit, reviewed the state of the question at his time, and contributed to its elucidation and development certain important facts and ideas, expressed with great clearness and used with judicial ability. He adopted a classification of the varieties of catheter-fever similar to that given by Velpeau; recognised uncomplicated cases issuing in death without any discernible structural lesion outside the bladder, quoting eight or nine that occurred in his own experience; asserted that it may occur in persons of perfect health; and that, in them as well as in

others not healthy, it may follow upon any state of the urinary organs requiring the passage of a catheter; and, finally, he declared with emphasis the fever to be positively uræmic, and ascribed its origin to insufficiency of the kidneys arising out of functional or of structural disease. In this work, perhaps for the first time, the causal relationship of catheterism to catheter-fever was most clearly and comprehensibly set forth, and a logical, coherent, and ingenious, if not accurate, theory of the genesis of the fever was propounded.

In 1867, the practical, and indeed, also, in some respects, the theoretical, aspects of this subject were greatly advanced by the publication of the now classical work of Sir Henry Thompson on the urinary organs. In this work, characterised by great force, method, clearness, precision, and knowledge, he mentions the perils of catheterism, gives instructions for averting them; notices the occurrence, in a few exceptional cases, of a low, irritative fever; and quotes Sir Benjamin Brodie to show that, in a few weeks, it might terminate in death. But, furthermore, he says (and here he differs from almost every one of his predecessors in this line of inquiry), that, in all such cases, there will be found old-standing pyelitis, with dilatation and marked degeneration of the renal structures, and that in no circumstances could such patients long survive. Looking at the unrivalled experience of this distinguished surgeon, and remembering how often I myself have consulted with him about cases such as this that I am now considering, I confess to a feeling of disappointment that he has not made time to give to the profession a more serious and adequate account of this important matter, which he must know better than almost any one else.

In 1867, the distinguished President of this Society, Sir Joseph Fayrer, gave an admirable account of the varieties of catheter-fever; declared, if I understand him aright, that the predisposition to it lay in the malarious state and its consequences, or else in incipient or advanced disease of the kidneys, or other parts of the urinary tract; that it began (and from him we have, for the first time, a clear and express statement on this point) in reflex disorder of the nervous system; that it was not toxæmic; and that catheterism alone, without injury or sensible irritation, was in these circumstances sufficient to originate the febrile phenomena and process. In 1868, Sir James Paget notices this fever, and makes some remarkable additions to our knowledge of it. He says that, when the urine is of low density and abundant; when the patient is gouty, dyspeptic, or otherwise chronically disordered; when, having a stricture, it becomes irritable, and weakens the health; or when, being old, something has occurred to cause temporary depression of the health, catheterism will be especially dangerous, and may bring about a fever, ending in death. He leans to the opinion, although he does not distinctly express it, that renal degeneration is the cause of the fever, but admits that, so far as his own experience goes, the cause is apparently inadequate. Lastly, he states that the mortality of such cases is from three to four per cent.; which, even with my grave views of the subject, is a much higher ratio than my own experience would have led me to expect.

In 1871, Mr. Banks, of Liverpool, describes the effects of simple catheterism, and classifies 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 other. In 1873 Malherbe, in his work on the fever of diseases of the urinary passages presents a fairly just account of the state of our knowledge of this subject at the time of publication, and he adds certain important cases of his own. These cases are illustrated by a number of interesting and instructive temperature-charts. He avers that the fever may arise without local irritation; describes it as uræmic; alleges that the predisposition to it is renal; and whilst admitting—nay, giving evidence of the admission—that in some cases, not a few, no structural lesions are found after death, he yet holds to the hypothesis that the origin of the fever is in renal imperfection. In 1877 Mr. Marcus Beck contributed to the further elucidation of this subject two important papers bearing upon them the marks of practical knowledge, accuracy of observation, and careful thought. Describing the fever in which death may occur in from nine to forty-eight hours (and not, as I found, noticing the longer fever of which I speak), he holds that the predisposition to it lies in chronic disorders of the health, in renal imperfections, and in age; that the exciting cause is prob-

¹ I owe to Dr. Matthews Duncan my best thanks for a letter, in which he tells me that he himself has listened to Velpeau setting forth the characters of this very catheter-fever to which I am calling attention.

ably mechanical irritation or lesion of the urethra, and that the fever is begotten through irritation of the spino-cerebral and sympathetic nerves, reflected upon the kidney, and bringing about structural or other insufficiency thereof. Beck strongly supports the hypothesis that the fever is uræmic, but plainly his views are based on the study of old worn-out hospital cases.

Now, without carrying it further, it will be seen from this rough historical retrospect that catheterism is occasionally followed by a fever which has received the various names of urethral fever, urethro-vesical fever, urinary fever, uræmic fever, catheter-fever, and so forth; that, in some cases, this fever is dependent upon, or associated with, purulent arthritis, ordinary pyæmia—what is known in England as surgical kidney, or interstitial nephritis—and that, in a small but still noticeable percentage of cases, no adequate structural cause of death has been found. Now, it was of this last variety of fever that I spoke upon a recent occasion at the Clinical Society, and it was of this variety alone that I made, or mean to make, the following propositions.

First, about middle life in men perfectly healthy, or with no discoverable evidence of disease, except, perhaps, and even then not always, a low density of urine, the commencement of the habitual use of the catheter is sometimes followed by fever of the remittent type, which often ends in death; and, for the fatal issue in such cases, no adequate structural explanation can be found.

Secondly, it is important that such a fever arising in the midst of apparent health from such a seemingly small cause, and leading so often (as it certainly does) to a fatal issue, should be well and widely known, lest death should take the friends of the patient by surprise, and arrangements necessary to the welfare of a family should be left unmade.

Thirdly, although it is well known that in persons affected with renal disease, or in chronic gout, or with grave disorders of the general health, the commencement of habitual catheterism is attended with peril to life from secondary fever, the fact that this fever may arise in what seems to be good health, and, without the mediation of any visible structural lesion, issue in death, is not well known, or at least well known only to a few, and has, I repeat, no adequate place in English surgical literature, or in the English surgical teaching of this time. Of course this knowledge will be found, as I have in a very imperfect way shown you, in special monographs, and papers; but those are the luxuries of the few, and, for the most part, the luxuries of specialists who work in that direction; but such knowledge should be, as I think, fully imported into all common text-books, and so made accessible to the whole body of the profession.

Fourthly, this fever is neither distinctly uræmic nor distinctly pyæmic; although having some of the characters of each, it has all the necessary characters of neither. Probably it begins in the nervous system. Probably the disturbance of the nervous system reacts, in the first instance, upon the general metabolism of the body; and, in the second instance, upon the secretory organs, beginning with the kidney. The effect upon the kidney may consist either in structural alterations of the kidney, invisible by the aid of our finest instruments of research, or, as seems to me much more probable, in alteration of the constitution of the blood, the dynamic condition of its constituents in the renal vessels essential to the elaborative action of the secretory cells thereof; and, lastly, the concurrence of these conditions may, and often is, enforced by septic re-absorption into the blood.

Fifthly, a more complete knowledge of this variety of fever and of the conditions of its origin, maintenance, and increase, may, at least we may hope, lead to a material diminution of its mortality; and, even now, by treating in a serious manner entrance upon catheter-life, by taking the precautions set forth by Sir Henry Thompson, by great temperance in the use of foods and stimulants, by rest, warmth, and by other general means upon which I shall not dwell, such mortality, I repeat, may be possibly considerably diminished.

Of these five propositions, the one at the present moment most open to attack is the fourth, wherein it is asserted that this fever is not distinctly and exclusively uræmic. For, in these days, it has come to pass that almost every writer of distinction adheres to the view of the uræmic origin and nature of catheter-fever, or the thing known under that and other names, and I am, as it were, left by myself very imperfectly armed to oppose it. I oppose it. I ground my opposition to the exclusively uræmic theory upon the fact that the phenomena of catheter-fever, not as they exist at a particular moment, but in their assemblage and in their progress together, are different from those of the ordinarily recognised uræmia. The

duration is at once longer and shorter—longer than that of acute uræmia, and wanting its headache, its perversions of sensation, its changes in the urine, its convulsions, its profound coma; shorter than that of chronic uræmia, wanting its neuralgias, its recurring headaches, its defects of sight, its itchings of the skin, its vomiting, its characteristic breath, its attacks of dyspnoea and palpitations, its painful nervousness, its low temperature. Furthermore, the urine of the catheter-fever—of this variety, at least, of catheter-fever—is always loaded with micro-organisms of various kinds; and, although it is deficient in urea, and contains more or less albumen, it deposits no tube-casts, and it is capable of amendment. Lastly, whilst chronic uræmia issues in death, catheter-fever may issue—sometimes does issue—in complete recovery. Sir Henry Thompson may remember that I saw with him a distinguished nobleman, not far from Oxford Street, who had to make an entrance upon catheter-life; and he very justly and wisely warned the relations that it was a serious procedure, and that he might suffer constitutional disturbance after the starting of this mode of life; and it happened that he did suffer this constitutional disturbance. It happened, indeed, that he had a mild variety of this fever which I am describing, and that it continued between a fortnight and three weeks. Eventually, with Sir Henry Thompson's help, he recovered; and I think I may say now—five, or six, or seven years afterwards—he is in good health at an advanced age. Except in its long duration, in its occasional rigors of great severity, and its exceptional clearness of mind, the phenomena of what is called endocardial fever resemble more nearly those of the fever which I have called for the moment catheter-fever than any other malady with which I am acquainted.

Two questions of a practical kind arise out of this study of the history of catheter-fever. Seeing that by almost universal assent the fever originates at least in a disturbance of the nervous system, and seeing, furthermore, that in the cases accessible to me, at least, there is no account of the fever following in cases where narcotics or anæsthetics have been used, may it not be that the fever is capable of being cut short by the administration, on entering upon habitual catheterism, of narcotic or anæsthetic remedies? I remember that my great master, Syme, in Edinburgh, with a reason which his instincts very often knew better than his understanding, invariably gave his patients, whom he had to catheterise, opium, frequently a grain or two from the very beginning, and I must also add that he was singularly free from catheter-accidents. The second question is this: assuming the presence of the fever, and seeing that quinine has signally failed in controlling it, what are the drugs to be employed on such occasions, and what is the sort of hygienic management to be followed, especially in respect of food and alcohol which are so variously used on such occasions, in order that the fever may be brought, if it be possible, to a successful ending?

Such, then, are the main conclusions which I have drawn for my fragmentary studies of this form of catheter-fever. I know that they are incomplete; I even fear that they may be inaccurate; but however this may be, I submit them to your consideration, and I console myself with the reflection that they may call forth the ripened experience of practical surgeons who, in this matter, furnished with a larger knowledge and a more practised judgment, may be able, not only to correct me where I am in error, and to confirm me in any small point where I may have caught the truth, but to supply us with the very knowledge which at this juncture we need and ask.

ST. MARY'S HOSPITAL.—In the new chapel of St. Mary's Hospital a striking feature is the handsome stained glass window to the memory of the late Miss Alys Hall, who was for some years the sister in charge of Manvers Ward. By her gentleness, amiability, and devoted zeal, she won the love and respect of all who knew her; and the staff—students, sisters, and nurses—have combined together to raise this tribute to her memory. Attached to the wall below the window is a brass, bearing this inscription, "In loving memory of Alys Hall, who died at her post as Sister of Manvers Ward, on the 9th of March, 1883. This window was put up by her fellow-workers in St. Mary's Hospital and Medical School."

A MEDICAL SOCIETY FOR ARMY MEDICAL OFFICERS.—A society, to which we wish every success, has been organised by the officers of the Army Medical Department at Woolwich for the discussion of medical and scientific subjects. The first meeting took place on Friday, the 14th inst., Sir James Hanbury, K.C.B., President, in the chair. Surgeon-Major Davy read a paper on the "Present System of Training Recruits," which was followed by a discussion.