when sutured with silver wire. The method which I have of late adopted is a long curved incision through the extensor tendon just above the patella and prolonged downwards on each side to the line of articulation. The patella is then reflected downwards and the whole joint exposed. Even with this incision I have frequently found it necessary to make a vertical one of about an inch in length through the centre of the quadriceps extensor to thoroughly reach the upper extremities of the synovial pouches. But this does not materially damage the cicatrix, which in this incision is much wider than that obtained after the division of the ligamentum patellæ. The difficulty of satisfactorily clearing the crucial ligaments and the posterior aspect of the joint is not so great as one would at first imagine. When the semilunar cartilages have been removed, and they ought in all cases to be taken away if the synovial membrane is the part principally deceased, it is astonishing how easily by flexion and rotation the posterior part of the joint can be cleared of its synovial membrane. As regards the question of drainage for the knee, I hold very strongly that a tube ought to be brought through the popliteal space, so as to effectually drain the inter-condyloid notch. This is easily accomplished in the interval between the two popliteal nerves. A director having been made to project through this spot, it can be cut down upon from the back of the joint. A drainage tube, of which one end is left between the two condyles, is then drawn through. Two other drainage tubes are, in my experience, required, one in each pouch above the articulation, where a good deal of manipulation has been practised for the thorough removal of the synovial membrane. In reuniting the fibrous tissue, especially the quadriceps extensor tendon, a great number of sutures will be necessary, and the most effectual drainage will not, as a rule, come through the lateral bond of union, but will require separate incisions on each side leading into the empty pouches above the knee. Some surgeons have, I know, done without this system of drainage, and obtained successful results. This is not my experience, and time alone will show which is the better practice. If the ligaments have been destroyed and the joint feels movable in the lateral direction, an ivory peg should be driven up from each head of the tibia into the corresponding condyle of the femur, making a way for its passage with a bone drill.

In the ankle the difficulty of obtaining a satisfactory opening into the joint is very great. On the two occasions in which I have performed this operation four incisions have been found necessary in order that the tendons and ligaments might be avoided—one in front and one behind each malleolus. In this way the articulation could be thoroughly scraped out by passing the sharp spoon from one opening to the other, but no dissection and removal of the synovial membrane, such as that which is accomplished in the knee, could be performed. The articular surfaces could be sufficiently separated by traction to introduce the finger, and to obtain a good view of the whole of the interior of the joint. A pair of scissors also could be introduced to cut away the ragged tissue left by the sharp spoon. The patient in the first case in which I performed this operation has recovered with a very serviceable ankle. There is still one small and insignificant sinus (nine months after operation), but no swelling in the grooves around the malleoli. His joint is movable, although no attempt has been made to keep it so, and the limb is still supported on a knee-rest. The patient in the second case unfortunately caught scarlet fever, which was followed by albuminuria, and his parents have removed him from the hospital, so I am unable to tell what the sequel will be. The attack of scarlet fever has so far modified the result that one is unable to quote the case as an example of either success or failure.

The elbow-joint is very easily and effectually exposed by a transverse incision from the radio-ulnar articulation across the back of the olecranon, which is then sawn through and the parts separated. If more space is required, a vertical incision can be carried down from the external condyle over the head of the radius, making thereby a T-shaped wound. In the two cases in which I have performed this operation the disease was in an advanced condition, and a good deal of bone had to be removed with the sharp spoon. The olecranon was then united by strong silver wire to the ulna, and a drainage tube inserted on each side of the joint. No attempt was made to establish any movement, and both cases quickly terminated in healthy ankylosis. Here the possibility of obtaining a movable joint was sacrificed by not performing a regular excision, but as there was a strong family history of consumption in both, and the general health was feeble, I felt that the chances of success were greater after arthrectomy than after the old excision. To show the constitutional condition of these patients, I may add that one of them had previously suffered amputation of the thigh for disease of the knee. A well-ankylosed elbow produces also, to my mind, a more serviceable arm than an indifferent excision, and we cannot always count upon a good result after excision. It remains, of course, to be proved that the results of arthrectomy will always be as good as you have seen in these two cases.

Of the shoulder and wrist joints I cannot yet speak from personal experience.

The operation of arthrectomy is still in its infancy, but great hopes may be entertained that it will be adopted to cut short the disease in its early stage, and that, even when the case be one of an advanced nature, excision may be so far modified as to be limited to a removal only of the diseased parts.

ABSTRACT OF AN

Address

ON

COLD CONSIDERED AS A CAUSE OF DISEASE.

Delivered at the meeting of the Medico-Chirurgical Society, Nottingham, March 23rd, 1888,

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The doctrine stated.—Cold is considered to be the most common single cause of grave disease in this country, and in these latitudes is held to act both as a predisposing and an exciting cause, to produce inflammations and fevers, and to affect all or nearly all the viscera and tissues. It is held responsible especially for bronchitis, coryza, laryngitis, sore throat, pleurisy, pneumonia, pulmonary phthisis, rheumatism, Bright's disease, gastric catarrh, diarrhœa, peritonitis, paralysis from central or peripheral lesion, menstrual irregularity, cardiac diseases, even gout, and with more justification frost-bite, chilblains, and chapped hands. It is held that it may be applied at, near, or at a distance from, the part which becomes affected; that the locality of its application does not determine the seat of the resulting malady; that the extent, duration, and intensity of its primary action are not essential, and do not measure the severity of the disease. The theories framed in explanation of its action are not satisfactory, but until the fact is established that it acts as stated they are not essential. The laity hold somewhat similar views, but in a more crude and rigid form.

Its foundations stated.—These are mainly statistics, showing the greater prevalence of and mortality from certain diseases in the colder seasons of the year. For example, it is thus shown that pneumonia, bronchitis, and pleurisy are more fatal and prevalent in the colder quarters of the year, and less so in the warmer; that bronchitis and pneumonia are more fatal in the colder years; and that rheumatism is more prevalent in winter and spring than in summer and autumn. The geographical distribution of diseases scarcely adds much strength to the doctrine, but bronchitis and pneumonia are said to be more frequent in high latitudes and in exposed situations. Common experience is, however, generally accepted as sufficient evidence.

The doctrine criticised.—It ought to be proved as stringently as any other etiological doctrine, if it is to stand the test of practice, and if it has not been so established we ought to say so. It makes vast claims upon our belief, and thus invites criticism. Some of the chill diseases have already been removed from that group by competent authorities, and that removal has received a large amount of assent. These are such as have been clinically and pathologically defined, of which the etiology and pathogenesis have been made out by more exact methods. The best examples are tubercular phthisis and pneumonia; but to these I would add some forms of coryza, sore throat, bronchitis, rheumatism, and Bright's disease. The remaining "chill diseases" form a large and rather vague group, for which no very definite cause has been assigned, and which has not yet been divided into definite clinical units. The etiological question to be solved as to these has in consequence not been so stated as to admit of a definite reply. An apparent but needful digression is here intruded and apologised for. The stricter doctrines as to causes which consider them as necessary invariable antecedents—under constant conditions—are not easily applicable in medicine. If they were, we must at once say that these diseases are not caused by cold, although our conclusions become so much the less trustworthy in proportion as we use less stringent tests. The less exact, but still valuable, formula given by Sir Thos. Watson, by which diseases are imputed to certain frequently recurring antecedents with a degree of confidence measured by the uniformity of the conjunction and the rarity of the disjunction of the consecutive events, does not yield certain conclusions, and is, indeed, seldom used with due rigour. More commonly we refer a disease to some apparent outward change, usually held to be competent, such as exposure to cold, and attempt to explain the cases in which no result occurs by assuming a predisposition on the part of those who do suffer. Yet in some simple maladies in which it has been possible to put the question with definiteness the causes have been proved by invariable antecedence, by equality of effect, and by promptitude of action, just as in physical matters. So must the group of chill diseases be dealt with if equally positive conclusions are to be hoped for. They must be divided into defined clinical units, and the etiological question be separately put for each. At present, however, we are taught that the same cause may produce many and very different diseases, and *vice versâ*; or, in other words, that a cause is a variable and inconstant antecedent, and neither measures the intensity nor modifies the quality of the effect. It may be freely granted that where a state of unstable equilibrium exists it may be upset by any one of many diverse and often trivial causes, and that the effects are then determined less by the nature of the disturbing agent than by the kind of relations which subsist between the parts of the mechanism disturbed. Instances of this are common in medical practice. But these cases are not comparable with such diseases as are under consideration, in which we seek the causes of definite diseases in an averagely healthy person. Even where such a state of unstable equilibrium exists, we seek to learn that which brought it about, not so much that which disturbed it, for this may be replaced. We seek what may prevent a malady if it be avoided, or cure if it be removed. It may be as freely granted that disease processes, such as inflammation and fever, are caused by many and different agents; but it does not follow that the various forms of these generic processes are so indifferently caused. The statistical evidence is not a sufficient proof. It tells us not the causes of particular diseases, but some conditions of their action. On such grounds alone we might as well attribute typhus to cold as bronchitis and pneumonia. Moreover, in considering the statistics of disease we must take into account the great variations in nomenclature and diagnosis which exist and impair the value of the data. The topographical distribution of bronchitis and pneumonia in England and Wales, so far as it goes, is in conflict with the current doctrine; and the same may be said of the geographical distribution of rheumatism. Such statistics as exist and are used in support of the doctrine take insufficiently into account the contrary instances. These contrary instances which we see in the now frequent therapeutic employment of cold should also be more fully allowed for. The common experience and general acceptance of the doctrine both by the laity and the profession, although difficult to resist, do not amount to proof. The existing prepossessions are, indeed, pitfalls for the investigator, by giving a bias, which is especially shown by patients who continually state their inferences as facts. The common expression "a cold" begs the question, and yet is in almost universal use. My own experience, after nearly thirty years of rather special attention to this question, leads me to say that only in a very small minority of cases can the fact be established that a sufferer from a malady imputed to cold has been exposed to its influence in any unusual degree at an appropriate time prior to the commencement of the malady. So that I would extend Jürgensen's views about pneumonia to many forms of bronchial catarrh, pleurisy, rheumatism, and some other diseases. The cases in which after the action of severe cold serious disease has so promptly followed as to suggest a causal relation are too few in number to materially affect the argument. This doctrine, which refers to cold the production of so many serious and varied diseases of distant viscera, is difficult to reconcile with such notions of pathogeny as are derivable from the study of the simpler maladies, surgical affections, and experimentally produced diseases. These teach us that, conditions being constant, the effect follows the action of the cause with considerable constancy, and is measured by it; so that any variation of the causal agent or of the subject of its action is followed by a corresponding change in the resulting malady. This doctrine conflicts also with our notions of the pathogeny of inflammation as they are shown in triumphs of modern surgery. Conceiving of inflammation as the reaction of a living tissue to an injurious irritation, the rule seems constant that when the irritant is a simple mechanical or physical one the resulting inflammation is in direct proportion to the irritation, limited nearly in extent to the area irritated, as well as in time to a short period beyond that of the action of the irritant. The effects of cold might be expected to follow this rule, and do so as regards those effects about which there is no doubt. But the visceral diseases imputed to it do not; they much more resemble in their clinical aspect some of the infectious diseases. I am, however, inclined to the view that in some at least of these cold may be a factor in the production of the disease. I venture here to suggest that in these visceral inflammations there exists a real and specific distinction—whether clinically recognisable or not—between the forms of bronchitis caused by, say, cold, or by the pollen of certain grasses, or by an overdose of potassium iodide, or by the morbid virus of measles, and so on; and similarly between the pneumonias which are due to diverse causes. Thus I should expect to find almost as many species of inflammation of internal viscera as we now accept for the skin. Although it may not have equal weight with all persons, I attach some importance to a conflict which, to my mind, exists between this doctrine and the idea I have formed of the evolution of the faculty to inflame—in the sense of the power of living things to react against injuries. To me it seems that this faculty to protect by such reaction is more complete as against the more universally prevalent and long-existing injurious agents than against those of more recent and less constant action, and that organisms are thus better protected against mechanical and physical influences than against organic and organised ones.

Its injurious results are a widespread reduction of the average standard of health, brought about by habits which cultivate delicacy, while blindly incurring other and graver risks than those which it is sought to avoid; and a serious hindrance to the rational treatment of disease by abundant aeration, and to a sound hygienic practice.

Summary and conclusion.—I have tried fairly to state the doctrine, the grounds upon which is supposed to rest, its want of definiteness, as well as the weakness of the evidence put forward in its support; and I have ventured to suggest an alternative hypothesis, less indefinite and more open to proof or disproof. Should my views find favour, I should expect that advantages to public health and to both the theory and practice of medicine would result.

FIFTEEN CASES OF

TUMOURS OF THE BLADDER,

DIAGNOSED BY MEANS OF THE ELECTRO-ENDOSCOPIC CYSTOSCOPE.

BY DR. MAX NITZE.

IN the following lines I wish to direct the attention of my English *confrères* to the value of the electro-endoscopic mode of examination of the male urinary bladder, invented by me. I believe I could not have chosen a more suitable theme for that purpose than a short report of the bladder tumours diagnosed by me cystoscopically; for the diagnosis of these new formations offers the greatest difficulty, and in most cases it has been impossible till now to prove their existence with accuracy without digital exploration of the bladder. By the new method of cystoscopical examination the conditions have entirely changed. One look into the bladder, illuminated as if by daylight, is generally sufficient to afford means for forming an opinion of all