OBSERVATIONS ON THE ACTION AND USES OF CROTON- CHLORAL HYDRATE.

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I HAVE the honour of directing attention to a new remedy, which serves to corroborate the theory I have propounded with respect to the action of hydrate of chloral.

When chlorine gas acts on aldehyde, croton-chloral is formed, as has been demonstrated by Dr. Krämer and Dr. Tinner. In order to avoid a mistake which is apt to be caused by the name, I must here remark that this body possesses no relation whatever to croton-oil, although its chemical constitution proves it to be the chlorated aldehyde of crotonic acid. Croton-chloral differs in its outward appearance from hydrate of chloral, by its being dissolved with difficulty in water, and by its crystallising small glittering tablets. Its action, though similar to that of hydrate of chloral, differs widely from the latter with regard to its physiological effects. Four *grammes*, or a drachm, of this substance, dissolved in water, and introduced into the stomach, produce in the course of from fifteen to twenty minutes a deep sleep, accompanied by anæsthesia of the head. Whilst the eyeball has lost its irritability, and the nervus trigeminus shows no reaction whatever on being irritated, the tone of the muscles remains unaltered.

I have experimented with this remedy on maniacs during an attack of mania. They remained quietly sitting on their chairs in a deep sleep, their pulse and respiration being unchanged for two whole hours together. If anæsthesia had reached so high a degree in consequence of the application of hydrate of chloral, the patients would have dropped from their chairs, and both their pulse and respiration would have been considerably retarded. I have seen croton-chloral acting in the same way on healthy individuals. In some cases of tic douloureux, the remarkable phenomenon is exhibited that pain ceases before sleep sets in. I am sorry to say, however, that this remedy acts only as a palliative in this dreadful disease. I nevertheless prefer its action to that of morphia, because it has effects as good as the latter remedy, without being so detrimental to the constitution in general. I have never observed any unfavourable effects of croton-chloral on the stomach or any other organ, although I have made frequent experiments with it.

The indications for the use of this remedy are to be found—1. In cases where hydrate of chloral is inapplicable on account of heart-disease; 2. In cases of neuralgia in the district of the nervus trigeminus; 3. In cases where very large doses of chloral are necessary to produce sleep. I there recommend the addition of croton-chloral to hydrate of chloral.

Whilst examining the difference between the action of hydrate of chloral and that of croton-chloral, I have discovered the remarkable fact that it is not the first, but the second, product of decomposition of the latter substance which is brought into action, on account of the first being too rapidly destroyed. Croton-chloral, when subjected to the influence of an alkali, first forms allyl-chloroform, a trichlorated body, which is rapidly decomposed into a bichlorated substance called bichloro-allylene. Now, both chloroform and trichlorated substances act, as I have shown, in their first stage on the brain, in the second on the spinal cord, and in the third on the heart. The retardation of respiration is to be explained by the agency of these substances on the last mentioned organ. Bichlorated substances act differently, as is proved by bichloride of ethylene. Even if the circulation of the blood in an animal have been stopped by this latter agent for one minute, life may be restored by artificial respiration, which is impossible whenever trichlorated substances have produced this effect, in which case the muscles of the heart remain paralysed. Well, in animals poisoned by croton-chloral to such a degree that both circulation and respiration are stopped entirely, artificial respiration is able to restore the action of the heart immediately, and the life of the animal may thus be saved. Bichloro-allylene, inhaled by the lungs, produces the same effect on animals as croton-chloral. We thus see these bichlorated substances acting on the brain, spinal cord, and medulla oblongata, but not on the heart, which explains the fact that both respiration and circulation remain unaltered

in man by a medicinal dose. It is a highly interesting fact, however, that under favourable conditions, we still are able to produce in animals the effects of the first product of decomposition of croton-chloral—i.e., of the trichlorated substance or of allyl-chloroform. In order to observe these effects, it is necessary to introduce immense doses of croton-chloral into the body, when paralysis of the heart actually does ensue.

CURRENT VIEWS

ON THE

TREATMENT OF STRICTURE OF THE URETHRA.

By WILLIAM S. SAVORY, F.R.S.,
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IN no affection which comes under the notice of the surgeon has so much mischief been done, in the way of treatment, as in stricture of the urethra; no instrument has, in practice, been so much abused as the bougie or catheter. An eminent and very skilful surgeon is reported to have said, in his blunt, forcible manner, that "more people are bungled out of their lives in the treatment of stricture than in any other way;" and another distinguished surgeon, not given to rash expression, affirmed that, "had bougies never been invented, it would, on the whole, have been a great gain to humanity." Whether one is prepared to endorse these opinions or not, the fact that they have been formed by observant and experienced men renders them worthy of serious attention. Nay, let anyone, who has seen much of surgery, recall what he has witnessed in the treatment of stricture by the employment of instruments, and reflect on the result. There is, in truth, a wide difference between actual practice and what is called the literature of the subject—a difference embarrassing to the practitioner and mischievous to the student. Records of rapid and complete cures of stricture by all sorts of mechanical means are abundant enough. Each of the very many plans of treatment which have been extolled is reported to be successful beyond any other—the one means by which the most obstinate strictures may be satisfactorily cured. But, if a surgeon of the largest experience were asked, how many cases of complete and permanent cure of bad organic stricture, by mechanical means of any sort, he could recall, what answer must he give? Is not the history of the subject strongly suggestive of another question? Are the views which have been, and are even still current of the pathology of stricture of the urethra sound ones? No doubt, in this case, practice has been worse than precept; but surely, even in what has been written on the subject, too mechanical a view has been taken of the nature of the disease. Of course, in our books, different forms of stricture are described as occurring either alone or in combination. We read of organic, inflammatory, spasmodic stricture, and so forth; but what is the impression left on the mind of anyone by the perusal of any treatise on the subject? That the treatment of stricture, in the great majority of cases, must be by mechanical means of some sort; by the employment of instruments. All other means, short of the use of instruments, are represented as exceptional or subordinate. But, in practice, matters have been, and still often are, far worse. Here, not unfrequently, the introduction of instruments constitutes the sole means of treatment, and, even when other measures are suggested, they are not often insisted on as of much importance.

This comes of the current view of the nature of stricture. Ask any advanced student or young surgeon, whose acquaintance with the subject of stricture is necessarily, for the most part, second-hand, what he understands by the term. He may, or may not, mention, in passing, the various causes of stricture; but it will soon become evident that his idea of stricture is simply a mechanical one; that stricture of the urethra is, to all intents and purposes, a constriction of a portion of the canal, due to thickening and contraction of a deposit in its walls, just as any tube or pipe may be blocked up, to a greater or less extent, by foreign matter; and the worst of this is, that it at once suggests to him the notion of treatment by purely mechanical means; that the obstruction must be overcome by force, either gradually and more cautiously by the repeated application of less force, or rapidly and violently by the sudden application of much force. Stricture of the urethra is too often regarded as a simple physical condition, to be treated only by physical means. But, even when a somewhat larger view is taken of the subject, when other means are employed, they are too often regarded as merely subsidiary or preparatory to the chief one—the dilata-

tion of the stricture by instruments. Nor can there be any doubt that this mischievous practice has been much fostered by the limited view which has been taken of the result of such treatment. When the passage is dilated forcibly by some instrument, of course the effect will be, for a time, marked by the increased size of the stream of urine. The improvement in this most obvious result is so striking, as to be at once accepted as success, and, for a time, it may be kept up by the repetition of the same or other mechanical means. But again, I would ask, what ultimately becomes of these cases? Is there a complete and permanent removal of all sensible obstruction, or a mere temporary improvement, with every probability of relapse? My experience of the treatment of stricture by instruments of various kinds is this—and I do not think it will be regarded as at all singular by impartial men:—I have seen very many cases of bad stricture—cases, I mean, where there has been obvious difficulty of micturition, or where only a small catheter—No. 3 or 4—could be introduced, treated by instruments of various kinds and in various ways; but I can hardly refer to any one in which the cure has been complete and permanent. Again, I have seen many cases with little or no symptom of stricture, but in which I have been informed that stricture of the worst possible kind existed. The evidence offered of this, however, has not been, to my mind, conclusive. I know very well what some will say to this—that my experience has been unfortunate. To this I reply, Does it not accord with that of others? Is it not the experience of most surgeons who have seen much of stricture?

The point I am anxious to impress is this: this plan—the usual plan—of treating stricture is founded on a misconception. Excluding from consideration now those cases of so-called stricture in which there exists no structural changes, even in the simplest cases of organic stricture, the condition is hardly ever, practically never, a purely physical one. With the structural change, there is associated an active morbid state, either some inflammation or congestion of the parts, and almost always more or less of spasm. The evidence of this seems plain to those who will read it.

How otherwise can one account for the varying size of the stream, even when permanently diminished? Very few instances of stricture occur in which, upon investigation, it does not appear that the stream of urine varies much in size; that, at certain times, and under favourable circumstances, the urine is passed much more easily and quickly than at others. This fluctuation in the size, and other characters of the stream, which is so very common in cases of stricture, cannot be due to any permanent alteration of the canal, but must be referred to some temporary cause which is capable of modifying its size and shape. Then the greater facility with which instruments may be introduced at some times, and under certain circumstances, than at others, irrespective of skill; and the speedy improvement which follows measures that can hardly, in so short a time, affect a permanent stricture—such as rest, warmth, aperients, and opium—can only be explained in the same way. And again, would an organic stricture, in a passive state, account for tenderness and disposition to bleed of the portion of the urethra which is the seat of the obstruction?

So, too, is evidence of the same kind offered by the effect of removing the pressure of the urine from behind, as by tapping the bladder. This is often at once followed by partial relaxation of the contracted portion of the canal. Beyond the immediate relief which is afforded by the operation, this appears to be the great advantage which is derived from puncturing the bladder or urethra behind in retention of urine from stricture. The urine is no longer impelled against the obstruction by straining, and the urethra is placed at rest. So also the evidence of the effect of enlarged prostate. Brodie remarks, "Where a simple chronic enlargement of the prostate gland supervenes on stricture of the urethra, the latter usually becomes less liable to spasm, and is more easily dilated, and altogether more tractable than it was before." Because now the pressure of the urine, which was a constant source of irritation, is intercepted by the prostate.

How otherwise can one explain the fact of complete retention of urine suddenly supervening upon stricture, with the fact, often verified after death, that the degree of organic contraction will not account for the degree of obstruction which exists?

Thus, it may be understood how the controversy about impermeable stricture has arisen. Much discussion has been expended on the question, whether such a condition as a really impermeable stricture ever occurs. That the canal of the urethra is ever so permanently closed and sealed up by organic stricture as to be altogether impervious to urine, may very well be denied; but no position can be less tenable than that taken by those who argue that, where urine can pass out, an instrument may be passed in. In some cases, indeed, neither urine nor instrument can pass; but, even if the former can somewhat freely escape, it by no means follows that the latter can be passed through. This is a very

different matter; for, setting aside other considerations, everything may depend upon the presence or absence of spasm or muscular contraction. There may be a passage for the urine, but it may be closed against an instrument.

Therefore, in the treatment of stricture, the employment of instruments should never be an early, only a late resource. Of course, I am not speaking now of retention of urine in consequence of stricture—that is another subject—but of the direct treatment of stricture of the urethra. In these cases, I submit that we should turn our attention more to the cause and less to the effect. The new matter, the lymph, which has been deposited or formed, and has subsequently contracted, causing stricture, is the result of a morbid process, which has either passed away, or which is still active, or lingers.

If such action be still present, what can be conceived more mischievous than irritation by an instrument of any kind? What can be more opposed to all that are recognised as sound principles of surgery? What would be done in an analogous case elsewhere? Would it not be deemed of the first importance to remove all causes of irritation—to obtain as far as possible absolute rest—to meet at once and directly the cause of the mischief, and assuredly not by rash meddling with the effect to aggravate the cause? Surely this is sound in reason, and I will venture to say supported by the results of the practice founded on it.

Foremost among the means at our command is rest—absolute rest in the recumbent posture. I do not pretend to say precisely how long this should be persevered in, or how strictly it should be enforced in every case; but I venture to assert that in the treatment of every case of stricture it should have a fair trial before any other means are resorted to. I do not think mere avoidance of business or confinement to the house sufficient; it should be continuous rest in the horizontal posture. Much good must be attributed to the effect of position. In short, the patient should be kept in bed, for this not only secures rest in the right position, but uniform warmth, which is a great point also. No doubt, many patients with stricture will object to this, just as surgeons neglect to enforce it, because its full value, and all the cost and trouble it saves, are not recognised, but I believe that, in proportion as experience is gained of the practice, so will it be esteemed—that those who carry it out most strictly and perseveringly will be best pleased with the result.

Beyond the all-important means of rest and recumbency, assisted by warmth in the form of baths or otherwise, there are remedies of great power in allaying irritability. Opium, which in these cases is so often valuable, as a rule perhaps is best administered, as laudanum, by the rectum; but this, or some other form of it, whether solid or liquid, may be given, when required, by the mouth, or morphia may be injected under the skin. Belladonna, too, or its alkaloid atropine, is sometimes useful, especially when locally applied; but, as a rule, I think they bear no comparison in efficacy to opium.

Active aperients also are often strikingly serviceable. The urine, which up to the period of their action had been obstinately retained, will sometimes pass freely during a brisk action of the bowels. The escape of urine, which is often simultaneous with the action of the bowels, and usually then most free, is a fact which should be always borne in mind. And it may be remarked that the success which attends the use of such remedies as those which have been indicated, is an additional proof, and a strong one, of the nature of the case. But, except in extreme and urgent cases, the use of drugs and of all active remedies may well be dispensed with, and reliance placed on simple measures of hygiene, such as wholesome food, warmth, and, above all, rest for a while even in bed.

But suppose a case in which all morbid action in the way of cause has passed by, and in which nothing remains but the effect, the thickening and contraction, what then? Now, in the first place, I say—and here, no doubt, I shall be at issue with many—that such a case in actual practice is, to state the least, extremely rare; that a troublesome stricture, due simply to a mechanical cause, apart from all morbid action of any kind, is to be regarded as a clinical curiosity; that this condition of things, which so often exists in idea, is hardly to be found in fact. But assuming it to be, even then I would repeat, still keep instruments in the background until other measures have been fairly tried. No one, I think, who has properly tested the value of these measures will be disposed to treat them with indifference in any case. Is it so anomalous an event for this new matter, this lymph, this outcome of inflammation, or of some kindred morbid process, to yield to time, when the parts thus for awhile hampered in their function are placed under conditions favourable to recovery; when causes provocative of still further mischief are carefully excluded? At all events, I would repeat the statement that these simple measures seldom if ever fail to effect improvement of the stricture. Of course one cannot pretend to measure in a given case how much, if any, active mischief may still be lingering there. In many cases it may be very little, in some possibly

even none. But this I can affirm: I cannot call to mind a case in which a stricture that gave trouble was not relieved by rest, recumbency, and the other means which have been mentioned. If this be so, should it not form an essential and primary part of the treatment of stricture? Should not the most be got out of these measures before we resort to instruments?

If it be urged that this would be waste of time, which may be saved by employing instruments at once, it may be replied that chiefly out of such mischievous practice have arisen such convictions as those to which I referred at the commencement of this paper. But then it will be argued that after all you come at last to instruments to accomplish the cure. Not always—very often not. I do not pretend that, under the sole influence of these measures, which are simply hygienic, all traces of stricture usually disappear; but the patient is very often, and sometimes speedily too, restored to such comfort, he is so relieved from every trouble, the stream of urine flows so naturally, that he is well satisfied with the result, and declines any further interference.

But by these means you have not thoroughly cured his stricture: you have given him only temporary relief, and his troubles may at any time return. That will in many cases depend on himself, and the care he takes of his health. No doubt in most cases of severe stricture these simple means are only palliative. Frequently, indeed, they will succeed so well that the patient himself is no longer conscious of anything amiss, yet the passage of an instrument will still detect an abnormal condition. But supposing you fall back on instruments, what then? How much more than this can you do? Dare you, in the majority of cases of severe stricture, promise by their aid a complete and permanent cure? Can you do more than restore a patient to a condition of comparative comfort, yet still with liability, under unfavourable circumstances, to relapse?

The sum of my argument, then, amounts to this: while I am not suggesting that instruments are never to be employed in the treatment of stricture, I venture to affirm that they have been, and still are, grossly abused; that an untold amount of mischief is perpetrated by their abuse; that the routine employment of instruments in the treatment of stricture is in the highest degree unscientific and improper; that, in a word, in the treatment of stricture, instruments of any kind should never be employed in any way except as a last resource.

REMARKS

ON

CASE OF INTESTINAL OBSTRUCTION FOLLOWING OVARIOTOMY.

By THOMAS KEITH, F.R.C.S., Edinburgh.

THE subject of the following history was my 160th case of ovariectomy. She was a young Irish lady in her 20th year, of a highly nervous temperament, thin, and fragile looking. For some months, she had suffered severely, and had been much pulled down by the disease. She had been tapped in February by Dr. Wallace of Parsonstown; and, when he sent her to me in the end of March last, she was already as large as before.

Ovariectomy was performed on the 1st of April, 1873. Dr. Zell of New York and other friends were present. There was nothing unusual in the operation. Omental and general parietal adhesions were separated, several catgut ligatures applied, and, as a good deal of blood had been lost during the breaking up of the semi-solid portion of the tumour, I did not follow my usual practice, in young subjects, of letting ten or twelve ounces of blood escape from the pedicle before securing it. In this case, both ovaries were diseased. The pedicle of the right was of fair length; that of the left was extremely short. Both were divided by the cautery, and, after waiting till all oozing had ceased, the wound was closed by silk sutures in the usual way. Ether was given during the hour and a half that the operation lasted. The weight of the broken down cyst-walls and contents was upwards of 40 lbs.

She did not do well from the beginning. From the afternoon of the operation, there was an amount of restlessness, such as I have never seen after ovariectomy. The pulse rose the first evening to 140, and continued so for the first five days; the temperature ranged from 101 degs. to 102 degs.; the limbs were rarely for a moment still; the nights were restless as the days, and were spent tumbling and tossing about. There were thirst and dry tongue; yet elimination went on well. There was an unusual amount of clear urine, and free perspiration from time to time. Flatus first passed twenty-six hours after the operation, and

continued to pass fairly well, yet the abdomen became distended, and, on the fourth day, there was much vomiting. On the fifth day, fluid was for the first time detected in Douglas's space, on examination by the rectum—for no vaginal examination was, in this case, at any time practicable. The fluid collected rapidly, and, on the seventh day, there was distinct fluctuation above the pubes; on the ninth, this reached to within an inch of the umbilicus. The wound, though much stretched, was healed, and the general condition improved. In the evening, as tension, which had hitherto been slight, had suddenly become great, and the patient was very uneasy, a small trocar was pushed by the rectum into Douglas's space, and, after six ounces of thick dark blood had flowed, the cannula became choked by clot, and was removed. This so far relieved the tension, that the patient had some hours' sleep. Next morning—the tenth day—a long syphon trocar was used, and thirty-six ounces of thick syrupy blood, not the least foetid, were removed by the rectum as before; then about three ounces of clot were sucked out by a powerful syringe, care being taken to admit no air. This gave much relief; the pulse fell to 100; the abdomen became soft; a large deep hollow being left above the pubes, very distinctly arched over by a roof of adherent intestine.

It was now that the anxieties of the case began; for it soon became evident that, as the roof of the hæmatocele fell inwards, a trap had been formed somewhere from folds of intestine adhering at angles; and, though the solid contents of the bowel moved downwards, or were moved by medicines, yet no air was allowed to pass. The fault seemed to be not far from the lower end of the colon.

The eleventh day after the operation was spent quietly, though scarcely any flatus escaped; and, by the twelfth, this had ceased altogether to pass outwards. Towards evening, the abdomen became distended, and windy pains were troublesome. On the thirteenth day, the bowels were freely moved by oil and an enema, without any relief to the flatulence. Douglas's space was again punctured, and ten ounces of bloody purulent matter, with broken down pieces of clot, were removed. The abdomen was raised by coils of distended intestine. On the fourteenth, the pulse was again up to 120; the distension was general and excessive; the rectum was dilated and empty; all attempts to pass a tube into the colon failed, as they had done before, and changes of position gave no relief. Enemata brought away some solid fæces, but never any gas followed. By evening pain was severe, and opiates were, as on previous days, necessary to moderate the increasing peristalsis.

On the morning of the fifteenth day, a little fluid was again felt in Douglas's space, and five ounces of thick pus were removed by the syringe. The cavity was then washed out with warm water, as the matter was a little foetid. This was the last of the hæmatocele. The distress and pain were this day great, and she was restless exceedingly. The distension was now enormous; the girth at the umbilicus was 52 inches, greater than before the removal of the tumour; the ensiform cartilage was turned upwards, and the stretched, though firm, cicatrix was almost at a right angle to the pubes. Prominent amongst the ever-restless coils of intestine, the colon—now as big as one's arm—from the thinness of the parietes, was easily traced upwards and across the epigastrium, but lost below and to the left of the umbilicus. By evening, the patient was much exhausted; the pulse was 140; and it was clear that the heart could not much longer bear the tension upon it. Late at night, my brother and Dr. Sidey gave me their help, and we agreed to make one more attempt to get a tube into the colon, and, should that fail, to open the abdomen up. At her request—for she disliked the smell of ether—chloroform was given. The hand was put into the rectum, and the fingers were with ease passed above the brim of the pelvis; but, farther than two inches beyond, no tube could be made to go. This took some time, and on turning the patient round to proceed with the proposed operation, respiration was found to have ceased, and it seemed for a time that we had to do with a death from chloroform. She slowly recovered, however; but the feebleness was so great, that such a severe proceeding as opening up the abdomen was not now to be thought of. The finest needle of an aspirator was then pushed into the transverse colon; it was put in several times, but seemed to be at once choked. I then punctured with a small trocar, and a very large quantity of air escaped through the cannula. Some nepenthe was injected under the skin, and she was left at midnight in a deep sleep; the pulse was then 140, though steadier since the relief given to the heart by the puncture.

Next day—the sixteenth after operation—there was less pain, and the abdomen was not so full. Some flatus passed in the forenoon, and several times during the day; pulse, 136; temperature, 101.8.

On the nineteenth day, much flatus passed, and the bowels were moved six times. She was nervous and feeble all day, with a tendency to faintness, and free stimulation was necessary. Towards evening, flatus again ceased to pass downwards, and the spasms became severe;