ON THE RELATION OF PARTIAL RETENTION OF URINE TO ITS DECOMPOSITION WITHIN THE BLADDER.

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THE opinion is, I believe, current, it is certainly general in our books and elsewhere, that inability to empty the bladder completely is a common cause of chronic inflammation of the viscus, because a portion of stagnant urine is retained until it is decomposed, and so becomes an irritant. In this way, by confining some of the urine until it becomes decomposed, it is stated that all forms of obstruction to its exit produce chronic cystitis. Now, is not this opinion that residual urine thus remains to decompose based on a misconception of the actual facts, or at all events of the cause, in such a case? Is it not assumed that the urine which remains behind is a fixed quantity, and therefore stagnant? Is it not overlooked that, although the bladder may be never emptied, yet that, inasmuch as a large portion of the urine it contains is frequently leaving it to be replaced by another portion which is continually entering it, the same portion of urine does not necessarily remain long enough in the bladder to become, of itself, the source of the mischief that ensues? Of course urine may remain long enough in the bladder to decompose, for no one needs to be reminded of the fetid urine reeking of ammonia in chronic cystitis; and it admits of demonstration that urine which enters the bladder in a healthy state is very rapidly changed by the disordered secretion of that organ. But this need not depend simply on retention, either total or partial. Micturition may be frequent, and the bladder may be frequently quite emptied, and yet the urine that flows may be putrid; or urine may be retained for a very long period, and yet when it escapes there may be no evidence of decomposition. In many cases, for example, where a person is confined to the recumbent position, the bladder is never able to expel the whole of its contents, and yet this state of things may continue for weeks without the residual urine showing any sign of decomposition; and in other cases, where it may even become necessary to draw off the urine in this enforced position of the patient, the bladder, in all probability, never becomes completely emptied, and yet, at least for a long while, the urine passes without change. Therefore, even if all reasoning on the matter be excluded, it cannot, in the face of these facts, be accepted that as a rule residual urine is to be regarded as the source of the mischief that is so often associated with it. No doubt the longer any given quantity of urine is retained in the bladder, when the causes of its decomposition are rife there, the farther will its decomposition proceed. But the duration of the retention of any given portion is by no means represented by the amount of residual urine after each act of micturition. The quantity of urine which enters and escapes from the bladder daily must rather be considered. Just as the water drawn off from a cistern gives no evidence of being stale or changed, although the reservoir is never emptied, because the whole of its contents are nevertheless rapidly renewed.

If healthy urine is passed into a bottle capable of holding, say, two pints, and the bottle as it becomes filled is from time to time only partially emptied, so that always about half a pint be left behind, the urine in the bottle, even after months, will still retain an acid reaction. It does not become ammoniacal or acquire the characters of the urine in chronic inflammation of the bladder. And in this experiment the circumstances, so far as ordinary decomposition of stale urine is concerned, are, on the whole, far more favourable to change; for although the temperature of the bottle may not be so high as that of the bladder, yet air is frequently and freely admitted, and of course it is always abundantly present. If the changes by which urine becomes ammoniacal are due only to long-continued partial retention, how is the negative result of this experiment to be explained?

Yet, although I venture to think that the prevailing opinion in this respect is not well founded, I am indeed very far from saying, or even intending to suggest, that

there is no evil in partial retention. On the contrary, it is clear enough that the presence of residual urine implies some delay in the escape of a portion of it, and therefore in a corresponding degree favours the advance of decomposition. But this, I need not remark, is a very different view from that which regards partial retention as a common starting-point of cystitis and the further evils which hence ensue. But then the fact remains that these two evils—residual urine, the result of partial retention, and chronic cystitis—are very commonly associated. Can this be explained? The direct and immediate cause of the decomposition of urine in the bladder, which enters it in a healthy state, is the presence and action of a morbid secretion of the mucous membrane. Practically the changes of urine in the bladder are dependent on the action of unhealthy mucus, and the degree of decomposition, although of course affected by the time the urine remains there, is yet chiefly determined by the character of the diseased secretion which mingles with it. Even under the conditions of health it is the mucus present in the urine which mainly determines the decomposition of urea, as the result of the simple experiment of filtering fresh urine shows, and when mucus is morbid it is far more destructive. Now, this disordered secretion must be the product of an unhealthy membrane; and while there is but little difficulty in understanding how, when the texture of its lining membrane is altered, the bladder is hampered in its action, and its full and complete contraction impeded, there is hardly less difficulty in believing that a full and complete contraction of it from time to time is an essential condition of a healthy mucous membrane, and of a due secretion and free escape of healthy mucus. Therefore, the conditions under which the bladder is only partially emptied are oftentimes the very conditions under which unhealthy, and therefore highly mischievous, mucus is brought in contact with the urine there.

ON A NEW TEST FOR ALBUMEN IN URINE.

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WHEN an albuminous urine is treated with a saturated solution of common salt, not the slightest reaction takes place; but if the brine be slightly acidulated with hydrochloric acid, the albumen is thrown down as a dense white cloud. This reaction constitutes a most delicate test for albumen in the urine. The best degree of acidulation for this purpose is obtained with about 5 per cent. of the dilute hydrochloric acid of the Pharmacopœia. A little more or a little less acid makes no appreciable difference in the sensitiveness of the test. Common salt dissolves in about two and a half times its weight of water at 60° F., and increase of temperature does not sensibly increase its solubility. The salt of commerce is always more or less dirty, and the solution requires filtration to fit it for use as a test. The salt solution should be fully saturated, otherwise the observer is apt to be led into error. In preparing the test with our common English measures the readiest plan is to mix a fluid ounce of dilute hydrochloric acid with a pint of water, and to saturate this with common salt, and filter. Dilute hydrochloric acid may be replaced by dilute sulphuric, dilute nitric, or dilute phosphoric acid. All these acids are of the same saturating strength in the British Pharmacopœia, and all of them yield with saturated salt solution an equally sensitive reagent for albumen. Even acetic acid may be used, but the delicacy of the test in that case is not quite so great as when it is prepared with one of the mineral acids. The method of applying the brine test is similar to that followed with nitric acid. A portion of the suspected urine is placed in a test-tube, the test-tube is then held very much aslant, and the salt solution is allowed to trickle along the sides of the tube to the bottom, so that it may form a distinct layer below the urine. If albumen be present, a white cloudy zone appears at the junction of the two fluids. Or the proceeding may be reversed. The salt solution may be first introduced into the test-tube, and then the urine added with the same precautions as before, so as to obtain two distinct layers, one above the other, in the test-tube. It is important to be aware that the precipitation of albumen by acidulated brine is not due to a true coagulation. In this respect the