gout. These cases were certain; they occurred where there had been no infection, and they were not themselves infectious.

Prostatic disease or enlargement was very frequent in those who were gouty; but it could not be said to be in any way peculiar to them, and it possessed, when found in gouty persons, no features that were characteristic of gouty disease. There was an affection of the penis which, in the few cases the lecturer had seen, had occurred in persons who were gouty. It consisted of a fibrous thickening and hardening of the corpus cavernosum, and appeared to depend on changes very similar to those that took place in the thickening and contraction of the palmar fascia, which had been described in the previous lecture. It was seen in men who were generally past 55 years of age. The sheath of the crus or the septum presented a tough rigid plate, almost as hard as a piece of imbedded cartilage, varying considerably both in its size and its shape in different cases. Sometimes it was not more than two or three lines across, in others it had an extent of more than half an inch, and was placed either on the dorsum or at the side of the corpus cavernosum, or in the septum. The affection was very chronic. In some instances, these plates had gradually increased in extent, in others they had slowly disappeared. They led to no harm, and were little affected by treatment. They were, however, not rarely a source of apprehension to those in whom they occurred, and who feared them as the first signs of cancer; in other instances, they gave trouble by leading to distortion of the crus during erection; but they did no other harm. The last of the minor signs of gout that would be referred to were painful and persistent erections at night. They were found in old men who had the gouty constitution in its incomplete form, and in whose urine there was frequently an excess of lithic acid. In such cases, the bromides of potassium and ammonium had been very useful.

ON DR. COPEMAN'S NOVEL TREATMENT OF OBSTINATE VOMITING IN PREGNANCY.

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I HAVE read the valuable practical paper by my friend Dr. Copeman of Norwich on the subject of the treatment of obstinate sickness in pregnancy, in the BRITISH MEDICAL JOURNAL for May 15th, with much interest. Dr. Copeman has described a process which he has, he states, accidentally discovered; viz., the artificial dilatation of the os uteri by the fingers as a cure for obstinate sickness in pregnancy. His results are remarkable, and, stated as they are by a gentleman of his known experience and accuracy, they are very important. Dr. Copeman, to use his own words, does not "attempt to explain the *modus operandi* of the treatment suggested"; and I desire to offer what I consider to be the true explanation.

In the year 1871, I read a paper before the Obstetrical Society (see *Transactions*, vol. xiii) on the subject of this vomiting in pregnancy. I there enunciated the theory, which I supported by facts and observations, that obstinate vomiting, and indeed ordinary vomiting, in pregnancy are due to a flexed condition of the uterus, the compression of the tissues of the uterus at the seat of the flexion constituting the irritation which gives rise to the vomiting. My view was strongly criticised at the time, and, indeed, Dr. McClintock of Dublin was at the pains shortly afterwards to write and publish a paper, expressly directed to the abolishing of my theory. I have been content to wait until professional opinion was more ripe for discussing the matter calmly, believing firmly that my view expressed four years ago is in all essential particulars correct.

Dr. Copeman's clinical contribution of three cases has a strong and direct bearing on the above question, and the cases offer to my mind strong confirmation of the truth of my original statement. The three cases occurred respectively at six months, two months, and eight months, and in each case the vomiting at once ceased on dilatation of the os with the finger. In the second case, Dr. Copeman says the uterus was "anteverted". He gives no account of its condition in the other two cases, so far as flexion or version is concerned. Now, it is my belief that all three cases were alike; that there was, or had been, acute flexion in each case, and that the dilatation operation of Dr. Copeman effected good and removed the vomiting by reason of its also relieving the cramped confined condition of the cervix.

On the supposition (which I make as regards two, but which is a fact in one of the cases, according to Dr. Copeman) that there was flexion in all three, the os must have been far back, and, in order to dilate it, it must have been pulled forwards. The dilatation would and must necessarily imply a righting of the os and lower segment of the uterus,

and a consequent unbending of the organ; for I need hardly remark that to draw the os forwards would of necessity tend to tilt the fundus upwards. The uterus, as a whole, is on a pivot; direct pressure on or dragging on one extremity of it will affect the other extremity, and thus the process of dilatation, involving as it does the dragging of the os forwards, would practically aid in the placing of the whole organ in its proper position.

It is customary with obstetric authors to speak of the gravid uterus as being naturally anteverted in the first part of pregnancy. This is a statement which requires important qualification. There are degrees of anteversion. It is one thing for the body of the uterus to be rather easily felt by the touch through the anterior wall of the vagina, as it undoubtedly is in ordinary cases, but it is another for the roof of the vagina to be actually depressed by the abnormal descent of the enlarged body of the uterus when it is anteflexed. In the latter case, the os is always further back than usual, and, in marked cases, the body of the uterus is for the time completely jammed in the pelvis. It is under these latter circumstances that obstinate vomiting most commonly occurs. Retroflexion is equally operative in a quite analogous way; but I say nothing further on that subject at present, as it does not seem likely that either of Dr. Copeman's cases belonged to that category.

But it may be said, How do you explain the cases in which the vomiting persists as late as the eighth month, which was the fact in Dr. Copeman's third case? The answer is, that, when there has been an acute flexion in the early part of the pregnancy: as the uterus enlarges (if abortion do not occur), the flexion is in most cases abolished, and the effect of this is, that the sickness generally disappears under such circumstances. But the tissues of the uterus at the seat of the flexion are sometimes left in a diseased state, being stiffened and unduly resistant, and thus the irritation is kept up.

Dr. Copeman's treatment would undoubtedly tend to remove this stiffening and constraint. He himself says, in his paper, "I wondered whether the relief to the vomiting so urgent and threatening to her life could have been effected by my having dilated the os uteri, and thus removed any undue tension which might be producing sympathetic irritation." Undoubtedly, there was undue tension; this tension was, I consider, situated at or near the internal os uteri, which is the situation of flexion under ordinary circumstances; and Dr. Copeman's procedure acted precisely in the way he conjectures. I submit to his careful judgment, and that of others accustomed to consider such problems, whether my explanation of the utility of his process is not the sound one.

I have been accustomed to treat cases of obstinate sickness in pregnancy by elevating the body of the uterus, and I have found that the same immediate good result follows as was observed in Dr. Copeman's cases; viz., the cessation of the sickness; but I am quite prepared to hear that traction of the os uteri forwards will produce a like effect. Both procedures have the same result: the liberation of the tissues of the uterus at the internal os uteri from their cramped compressed condition.

There are other details, but at present I forbear to say more on the subject.

IS STRICTURE OF THE URETHRA CURABLE?

By W. F. TEEVAN, B.A., F.R.C.S.,
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UPON the answer to this most pertinent question depends our treatment; for, if there be no evidence to prove that we can remove the pathological conditions incidental to a stricture, then one of the chief inducements to operate for its relief no longer exists, and palliative measures assume an importance which they would not otherwise present. How comes it to pass that there is no subject in the whole range of surgery which elicits such varied and contradictory opinions regarding its treatment as stricture of the urethra? It is not difficult to find the reasons why. The contradictory statements as to the treatment of stricture of the urethra arise from two separate causes. After every operation for stricture, there are certain effects to be seen which are temporary, uncertain, and often misleading. At a later period, we observe other results, which are permanent and worthy of acceptance. The first kind are usually published in profusion; of the second we see scarce anything. Then, again, by some extraordinary combination of circumstances, we find that deaths after operations for stricture are rarely published. It thus appears that we are deprived of the most valuable evidence regarding the treatment of stricture of the urethra. So far as I know, no surgeon has been able to place before the profession a dozen cases of stricture which he has cured by operation, and