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.

By GRAILY HEWITT, M.D., F.R.C.P.,
Professor of Obstetric Medicine in University College.

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.,
Surgeon to the West London Hospital, etc.

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

which have been submitted to a crucial time-test. The matter stands thus: whatever operation we may select for the relief of a stricture, we shall fail to cure the patient; for, if he be really cured, he requires no after-treatment—prolonged for his life, be it observed. But, if no such treatment be pursued, we shall find out that the patient will infallibly have a return of his old complaint; thus proving that an operation, unless supported by a well regulated course of after-treatment for life, is only followed by temporary results. As a rule, there is no difficulty in restoring a contracted urethra to its normal calibre; but it is impossible to preserve the good we have achieved, unless we pass an instrument from time to time. We cannot cure; we can only remove troublesome effects, which will infallibly return unless we persist in our treatment. Pathologically speaking, no stricture is curable; but, practically, nearly all strictures are remediable. At whatever time after an operation we examine the urethra with a *bougie à boule*, we shall find that the pathological state is not removed, but only modified in some slight degree; for the soft velvety condition of the mucous membrane is replaced in the strictured part by a gritty unevenness of tissue, as felt by the *bougie à boule*.

A short time ago, I had an opportunity of examining, *post mortem*, the strictured urethra of a patient whose canal I had a year previously dilated up to a large size. Before I removed the parts, I passed a full-sized bougie into the bladder with ease, thus showing that the canal was still of a normal calibre; but, on laying open the bulb, the remains of a tunnel-stricture were very visible in the form of three longitudinal ridges of induration, each about one and a half inches long, and about the one-sixteenth of an inch in depth. These ridges did not disappear when the urethra was stretched laterally, and their yellowish appearance contrasted strongly with the healthy portions of the mucous membrane. Mr. Timothy Holmes records a case of death after the strictures had been forcibly ruptured, in which "the traces of the two strictures were quite perceptible in the urethra", although both admitted an ordinary-sized instrument. (See *Trans. Path. Society*, vol. xxi, p. 280.)

I have in my experience seen a few cases which would seem to prove that now and then the indurated lymph is wholly absorbed, and that the strictures were really and truly cured. But any deductions drawn from a few isolated cases must be regarded as worthless for practical purposes, as the following case will show, proving how unsafe it is to predict that there will be no relapse even under the most favourable conditions. A patient, whose strictured urethra I had restored by gradual dilatation to about its normal calibre, remained for two years without having any instrument passed, at the end of which time he came to report himself. The urethra was still as capacious as when I had last seen him, and the largest sized instrument could be passed with ease. Apparently, nothing could be more satisfactory; but, six months later, the patient presented himself, and said that contraction was beginning to return. I tried to pass the same sized bougie as I had done on a former occasion; but I could not, and had to use one two sizes smaller. One of the reasons why this patient had gone so long without any relapse was, that his urethra was naturally very capacious; and another was, that I had dilated his stricture up to an excessively large size, thus putting off the relapse for an unusually long period.

Of all the cases of operation for stricture that have come under my care, in which the operation was performed by myself or others, I have never yet met with one in which an examination with the *bougie à boule* did not detect some remains of the strictures, though years might have passed away after the operation. Nearly all the published cases of operation for stricture have been deprived of most of their value from the fact that the authors have given only the temporary effects of the operation, and not the trustworthy results which are to be seen a couple of years afterwards. On this subject, Mr. Cooper Forster made a most pithy and apposite remark in reference to a case of operation for stricture which was narrated at the Clinical Society. He said, "I do not care to see the patient now, but I should very much like to examine him two years hence." Herein lies the whole gist of the subject. The present state of the question is this: no surgeon has as yet been able to produce that evidence which would alone carry conviction; and so long as this is wanting, and we every day have cases of relapse after each and every kind of operation, we must come to the conclusion that no operation is desirable except in certain exceptional circumstances. Hence, therefore, as no operation can cure a patient, it is our duty never to recommend such a procedure unless it is absolutely called for, as we are not justified in endangering the safety of a patient's life, just to humour his whim, when we can achieve, by gradual dilatation, which is absolutely free from all danger, all that is open to us. The treatment of a stricture is a treatment for life-time, and he who fails to recognise this important truth deceives him-

self or his patient. Fifty years have now rolled away since the great Delpach wrote the following words, and no English surgeon has been able to reverse his judgment. "Quant à la coarctation elle-même, il est presque superflu de dire que nous avons bien vérifié combien cette affection est incurable, et ne peut être que soulagée passagèrement, et conserve une tendance invincible à se reproduire insensiblement. Il en est constamment ainsi, quels que soient les moyens par lesquels elle a été combattue. Ce serait abuser de la crédulité des malades et des médecins, et se jouer de la vérité, que de prétendre le contraire." (*Chirurgie Clinique*, vol. i, p. 273.)

ETHER AND CHLOROFORM.

By W. C. B. FIFIELD, M.D. Harvard, M.R.C.S. Eng., of Boston.

SOME one has been good enough to send me six copies of the BRITISH MEDICAL JOURNAL, of the date May 8th, 1875, containing a letter from Dr. Thomas Skinner of Liverpool, entitled Ether and Chloroform.

I had some idea of answering this letter, when I perceived that Dr. Skinner had answered it himself by saying that, out of hundreds whom he had witnessed administering chloroform in Edinburgh, London, Liverpool, and elsewhere, there are not six from whom he would take it. Could any statement be so severe as this? Could anything be more startling? Could any words give a more vivid idea of what a dangerous deadly agent is chloroform, or waken more quickly a vigorous public attempt on the part of the British public to forbid its use as an anæsthetic, when out of the hundreds or thousands of British surgeons who are daily employing it there are not six from whom Dr. Skinner would take it?

Surely, British wisdom was justified of her children when Dr. Sinclair told him (Dr. Skinner) that an American jury would deal very hard with him if a death occurred from his administration of chloroform, when even Dr. Skinner admits that he would not take chloroform from six of all the hundreds he has witnessed giving it.

An American jury would probably consider the matter in this light, viz., that whosoever administered chloroform by inhalation for relief from pain, knowing that sudden death may result from causes too numerous to mention, and even when no cause can be found for such death other than the agent employed, knowing also that another agent equally capable of producing such anæsthesia exists, which has been shown to be far more safe than chloroform, viz., sulphuric ether, shall be held criminally responsible for his temerity.

Leaving Dr. Skinner, I would ask to call attention to the various discussions and reports here in Paris on the subject of chloroform.

Firstly, Dr. Albert Bergeron has written a book, and a very good book it is, entitled *Chloroform in the Surgery of Children*. In this book, he seeks to prove exactly what Dr. Skinner has denied; and I heard Dr. St. Germain lecture about Dr. Bergeron's book at the Hôpital des Enfants Malades, and agree with Dr. Bergeron *in toto*, viz., that if chloroform can ever be given safely, it is to very young children. M. Bergeron is frankness itself. Speaking of cases where death occurs hours, or even days, after the administration of chloroform, he says: "But it is no longer the same in the series of facts, unhappily too numerous, where one sees death come abruptly, suddenly, the patient still on the operating-table, the surgeon having just finished, or even not having commenced his operation. These cases are not doubtful; it is truly to chloroform that they ought to be attributed. The discussion of them is difficult; and these are they which constitute the true necrology of chloroform, which the surgeons of Boston and Lyons have so often brought forward. Far be it from me to acquit chloroform with an adult."

M. Bergeron, however, meets with some ugly facts in regard to the perfect safety of chloroform in young children; notably, the five observations reported by M. Bouvier; but he escapes ingeniously from three or four of them. Hæmorrhages, relatively considerable, had taken place with three children; and the fourth has been recalled to life by artificial respiration, faradisation of the diaphragm, and methodical compression of the abdomen. This case was that of a child four years old. The fifth observation has been recorded at Berlin; it is that of a child, aged four years and three months. It was for a simple catheterism. Two or three minutes passed between the last inhalations of chloroform and the moment when the catheter was introduced; and during this interval the child breathed only pure air; the respiration and the pulse were carefully watched. It was at the moment that the catheter penetrated the bladder that respiration stopped; and, in spite of all efforts, the child died. It is said that the sleep had come