At Moorfields, from eight to twelve patients are anaesthetised during the few hours of each day devoted to operations: thus the time consumed by each administration is of some, although secondary, importance. A slow anaesthetic is hardly admissible. Ether has been used there since August in a large number of cases. No difficulty has been found in speedily producing perfect muscular quiescence. The time necessary for complete narcotism is about the same as with chloroform—certainly not longer.

A marked and invariable feature has been the absence of all disposition to syncope. There have been no sudden failures of pulse, with pallid lips, even under the most lavish use of the anaesthetic; on the contrary, complete muscular relaxation was generally associated with a flushed face and full pulse. A folded towel enclosing a piece of sponge has been the only inhaler used. Only when this is soaked with anhydrous ether is a vapour of adequate concentration obtained. The presence of even small percentages of water reduces the volatility of ether very considerably, and prevents a satisfactory result.

Without disparagement to less simple devices for administering a vapour of known strength, it may be asserted that the folded towel possesses great practical convenience, and has yet to be proved dangerous. By no other method can a vapour of adequate concentration be so readily obtained. Not only is there no danger in this lavish use of the anaesthetic, but narcotism is produced more quickly, and with less cerebral excitement, than is apt to result from too weak a dose at the outset. If the latter fault be committed, the patient becomes noisy and violent, and the vessels of his face and head become turgid—a condition unfavourable to many operations. On the other hand, by an adequate dose, narcotism, without excitement, is produced, and can then be easily maintained for an indefinite period by a less concentrated vapour.

The sole objection to this mode of administration is the overpowering sense of impending suffocation, apt to be produced by the first contact of the vapour with the air-passages. While my experience accords with that of Dr. Joy Jeffries as to the necessity for a strong dose at the outset, I do not believe with him that a forcible administration, in spite of the patient's struggles, is a desirable or necessary practice. By a little management in the case of adults, it may generally be dispensed with. They often take it without struggling, when forewarned that a few deep inhalations resolutely taken dispel the sense of choking. When their voluntary efforts fail to overcome the spasm of glottis provoked by the vapour, it is best to relieve them by momentarily raising the towel from the face, and allowing one full inspiration of unmixed air before panic takes possession of them. By this manœuvre the spasm instantly disappears, and does not return when a concentrated vapour is again inhaled. It should be remembered that the struggling occurs before the ether has had time to affect the brain, and the cause of it—the irritation of the air-passages—disappears at the very commencement of anaesthesia, after one or two full inhalations of vapour. Occasionally, no doubt, an adult is entirely wanting in self-command, and fails to co-operate in the administration. A little force may then be needed, but should not be prolonged more than a few seconds. After a few inhalations of strong vapour, a breath or two of unmixed air should be given, and the inhalation then resumed, probably without resistance from the patient.

Children must be narcotised forcibly, but their struggles are neither so violent nor so distressing to bystanders as those of adults. Ether does not blister even after prolonged contact with the skin, and in this respect is altogether different from chloroform and methylene.

The recovery from a properly conducted etherisation is usually characterised by a quiet, rather profound, slumber. Even patients known to be intemperate alcohol-drinkers do not recover more noisily than from chloroform.

After severe operations under chloroform, most surgeons have had to lament instances of death from obstinate chloroform vomiting, in which the shock of the operation is prolonged and intensified by incessant nausea, and the stomach continues, day after day, to reject food, until the patient dies exhausted. Mr. Curling records an instance after lumbar colotomy. A similar case occurred in my hands: they are by no means very rare. Operations implicating the pelvic and abdominal viscera appear to be especially liable to this accident; and it would not be inopportune were obstetricians now to give the result of their experience on this point, in cases demanding deeper and more prolonged chloroform narcotism than is usual in normal labour.

More than a few months' experience is of course required to determine whether ether ever exerts this formidable effect on the stomach. Thus far, no such instance of it has occurred in my hands, or has come to my knowledge. In a recent case of ovariectomy, my house-surgeon, Mr. Sargent, maintained etherisation for nearly three-quarters of an hour without inducing retching. The intestines never even came

into view during the operation: there was no subsequent vomiting. In a tedious operation of lumbar colotomy performed by me last month, the narcotism lasted more than half-an-hour. The patient vomited once immediately on leaving the theatre, but had no subsequent nausea. On the following day he was able to retain liquid food in his stomach, and has since made a good recovery from the operation.

Thus far my impression is, that ether is less dangerous to life than chloroform, on account of its inaptitude to produce cardiac syncope; that its administration is rarely if ever followed by obstinate and dangerous nausea; that it narcotises as quickly and as certainly as chloroform, and with as complete muscular quiescence; and that recovery from it is generally quiet and satisfactory.

THE MORBID EFFECTS OF ALCOHOL.

By W. H. DICKINSON, M.D. Cantab., F.R.C.P.,

Senior Assistant-Physician and Lecturer on Pathology, St. George's Hospital; Physician to the Hospital for Sick Children; Corresponding Member of the Academy of Medicine, New York; etc.

As the leader in last week's JOURNAL, upon the morbid effects of alcohol, is pointedly directed against myself, I claim the right of reply. The writer, who apparently did not hear the paper, has commented in a manner which, even on the showing of the abstract, appears to me to be unjustifiable. He has misrepresented the purport of the argument and disregarded the stated limits of the inquiry. In comparing traders in liquor with persons otherwise employed and not ostensibly drunken, I was careful to state that the inference that those who got liquor in the way of business for nothing took more of it than those who had to buy it, related to the general average. Among the 149 traders in drink may have been a teetotaler; some of the contrasted class may have been intemperate in spite of the care used to exclude such. But it can scarcely be doubted that the average of imbibition was greater on the side of gratuitous drink, and sufficiently greater to give preponderance in the class where it was current to the diseases which drink engenders. The writer of the article admits the notorious inebriety of traders in liquor, but "demurs to the assumption that the morbid appearances found in the dead bodies of all hospital patients who have not been engaged in the liquor-traffic are certainly not the result of alcoholism"; and describes as fallacious "the assumption that, excepting known drunkards, temperance may be taken for granted among the entire population." He is welcome to demur to such fallacies; they do not belong to me. As to persons not dealing with liquor or ostensibly drunken, my assumption is not that their diseases are invariably unconnected with drink, but that they are less often connected with drink than are the diseases of those who do so trade and are thus admittedly inebriate. The question is not between drink and no drink, or even between drunkenness and no drunkenness, but between more and less. All I assume is, that an average poor man who drinks at others' cost will take more than another who drinks at his own. If the writer question this, and deny that there is more intemperance on one side than the other, let me ask why it is that with the drink-traders cirrhosis of the liver is more than twice as frequent as with the contrasted class. If there be no excess of drink on one side, the fundamental proposition of my paper falls to the ground. If the excess exist where I have placed it, and be such as to tell upon health, then not my conclusions, but his criticisms, must be described as "misleading and mischievous."

The article then proceeds to inform us that "an ordinary *post mortem* record as to the appearance of the kidney affords no indication of the actual amount of disease. A kidney, which on a superficial examination might pass for healthy, may reveal to a skilled microscopic observer extensive and advanced degeneration." Are we to infer from this that it is of no use looking at the kidney except through a microscope? Bright made a few useful observations with the naked eye, an instrument, indeed, which is by no means without value in the detection of "extensive and advanced" renal changes. It needs no microscope to determine whether the kidney be twice or half its proper weight, or whether the surface be granular or the section that of a parsnip. The unassisted but practised eye, which though it does not scrutinise disjointed bricks comprehends the building, is a surer test for potential renal disease than the fragmentary microscopic examination which only is habitually practicable. And because we have not evidence of both kinds, shall we reject the more important?

The writer is kind enough to suggest total abstinents as a standard of comparison—an idea sufficiently obvious to have occurred to myself. The number is to be five hundred. Will he place me under still further obligation by producing them? a process which he himself admits

to be difficult. He must sternly exclude all broken drunkards from the immaculate array. An inquiry on this basis, however, though commended as likely to be instructive to myself, can convey no information to the writer of the article, since his far reaching knowledge enables him to forecast the results. Liberal with what has cost him nothing, he gives them to us. He has "no hesitation in declaring that they would show such a large preponderance of albuminuria amongst the tipplers, as would lead to the irresistible conclusion that alcohol in excess is a most powerful agent in the production of kidney-disease". He is among the prophets. Happy he to whom the results of an investigation are so manifest before it is begun that he has no hesitation in declaring them!—an economy of labour desirable in busy times.

The writer objects to mechanical principles. In considering the organs in relation to stomach-absorption, I had to take them in some order. I might have placed them alphabetically, or according to their distance from the centre of the earth, but it seemed more natural to refer to them according to their position in the path of the influence in question. I argue nothing from this. It is sufficiently obvious that, if any alcohol be altered in the liver, it cannot get unaltered to the lung, and that what escapes with the breath does not afterwards enter the general circulation. These simple considerations, mechanical though they be, are enough to guide arrangement, and have been thus used by Trousseau. My statements, however, were not based upon any considerations whatever beyond simple inspection. I went by what was seen, not by what might have been expected. If the liver displayed a more unequal distribution of disease than the kidney, the matter was one not of theory but of observation. I necessarily excluded all considerations connected with physiology as apart from the design and basis of the inquiry. I limited my business to the exposures of the *post mortem* knife—appearances rather than theories, results rather than modes of action; not that I ignored the uses of physiological inquiry, but because I kept to the results of dissection, which it was the sole object of my paper to display. "The pathologist", says the article, "who ignores physiological considerations..... is not likely to throw much light upon renal pathology." I will rather say, the pathologist who in matters of simple observation does not ignore everything else, is likely to allow what he sees to be warped by what he expects, and to fall into grievous error.

I will refer to one other criticism—not that it needs an answer, but in evidence of the animus of the article. In the paper, the portal and hepatic veins, the vena cava, and the pulmonary artery, were not considered, for the obvious reason that they were not examined. The paper was avowedly based upon *post mortem* examinations made without special object, and, it is scarcely necessary to add, without habitual examination of these structures; yet the eager critic affects to infer that, because no disease of these vessels was described, therefore none existed; and proceeds to argue accordingly against what he calls my theory. How often, let me ask him, has he seen, in a *post mortem* examination made without special object, the vena cava, the veins of the liver, and the arteries of the lung, opened up? Considering the basis of the paper, the criticism is inappropriate, and, like the general style of the article, captious rather than judicial. The whole article, indeed, savours of the bar rather than the bench, and speaks with the impassioned voice of a combatant, rather than in the balanced tones which should proceed from the serene sky of editorial wisdom.

ON SUBCUTANEOUS ORTHOPÆDY VERSUS KNEE EXCISION FOR ANKYLOSIS.

By W. J. LITTLE, M.D.,

Formerly Physician to the London Hospital, etc.

IN the JOURNAL of October 26th last, Mr. James Adams, surgeon to the London Hospital, has done me the honour of quoting my opinion, expressed in an article on Orthopædic Surgery in *Holmes's System of Surgery*, respecting the value of subcutaneous osteotomy applied to the relief of rectangular knee ankylosis, in the manner since described by my son, L. S. Little, in the *Medico-Chirurgical Transactions*, vol. liv, 1871, compared with resection at the knee in the manner in which it has been, in my opinion, too frequently practised.

Mr. J. Adams's neat and successful operation has two great drawbacks—viz., the subsequent occurrence of several weeks' suppuration, and the loss of two inches in length of the limb, owing to removal of a wedge-shaped piece of bone. Subcutaneous osteotomy at the knee, as practised by my son in the only recorded case of the kind, was, on the contrary, followed by no suppuration and by no similar shortening of the limb, inasmuch as no removal of bone takes place in subcutaneous osteotomy.

Notwithstanding my personal regard for Mr. Adams, and his having

been one of my pupils, I cannot avoid characterising the removal of a wedge-shaped piece of bone, and consequent resort to a considerable open wound, as a retrograde step in surgery—a proceeding stigmatised years ago by me, when performed on less defensible grounds by Meyer, for the relief of severe knock-knee.

It is a practice opposed to the "conservative" surgery of the present day, one of the greatest triumphs of which has been the substitution of subcutaneous operations whenever practicable, without after-suppuration, for operation with a large wound, ablation of bone, and almost certain after-suppuration. Mr. J. Adams has in this case shown himself a good operator, but apparently wanting in confidence as a pathologist in one of the restorative processes of the economy. I entertain no doubt that, when selecting the method of partial resection rather than adopting subcutaneous osteotomy, he was apprehensive that by the latter method he might have greater difficulty in keeping the severed bones in suitable apposition for the purposes of future use of the limb. But experience of ankylosed joints shows that in the reparative processes which ensue when periosteal and bony structures are involved, an abundance of plastic materials, prone to bony consolidation, is thrown out, owing to which the limb, whether it be straightened without wound, with the help of anaesthesia and manual gentle violence, or whether subcutaneous osteotomy be resorted to, a continuous stable, more or less straight, more or less firmly united and normally shaped, femur and tibia is the result.

CLINICAL MEMORANDA.

THE TREATMENT OF STRICTURE.

IN the JOURNAL of November 9th, Mr. Furneaux Jordan describes a new method of treating retention of urine and impassable stricture, *apropos* of which I would make a few remarks. I have no intention of claiming a priority for any one else in the operation described, nor yet of disparaging the new method, which, if it can be performed as easily and safely as the description leads one to imagine, deserves the name of "improved" in addition to new. My reason for writing is simply to put in a claim for trial for an old method, before any operative proceedings are thought of, whether bistoury or trocar be the instrument used. A glance at the diagrams given with Mr. Jordan's lecture will show that the difficulty in passing a catheter from before backwards, through such a stricture as is there figured, arises from the certainty of the end of the instrument catching in the pouch which surrounds the stricture; or, to use Mr. Jordan's apt simile, its getting under, over, or at the sides of—in fact, outside—the narrow end of the funnel. Now the prevention of this is easily effected by what is generally known as "Hutton's railway catheter", or, rather, by a slight modification thereof. That commonly used consists of a No. 2 flexible catheter, cut off at the eye, and a piece of catgut instead of stylet, which, on the end of the catheter reaching the stricture, is pushed on into the bladder, and then serves as a railway for the catheter to run on. If, instead of a No. 2, we take a No. 10, cut smoothly across at the eye, and pass it down to the stricture, it is evident that its end will go into and fill up the circular pouch, and the centre of the stricture will project into the catheter somewhat like the os uteri into a vagina speculum. The next thing to be done is to inject a small quantity of oil into this tunnel, and then proceed to use the smaller instrument determined on, which may be a piece of catgut, a filiform bougie, or a No. 1 catheter. By the above method I lately succeeded in a case when I had almost given up hopes; and I believe it may be of use in some other difficult cases, so I think it right to ask for it a trial.

P. J. MOLONY, B.A., M.B., etc.

Waterbeach, Cambridge, November 9th, 1872.

SYPHILITIC INFECTION BY A LOZENGE.

WHEN attending the clinique of M. Hardy at the Hôpital St. Louis, in Paris, I saw a case of syphilis of a similar character to that mentioned by Dr. Drysdale, of infection from smoking a cigar. The subject was a child of about five years of age, who was brought to the hospital with a cutaneous eruption of an unmistakable syphilitic character, combined with other constitutional and local signs, the exact nature of which I now forget. M. Hardy was at first unable to trace the infective cause. He at length discovered that the child's brother was the subject of constitutional syphilis, and that some time previously he had, before the young patient presented any symptoms, given his sister a lozenge to suck, which had been partially dissolved in his mouth, and had thus infected the child. I remember nothing more of the con-