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-