

from the lowest stratum would at the second stratum be joined by the bubbles formed here, and at the third stratum by a third set of bubbles, and at the hundredth stratum there would be a set of bubbles together generated in a hundred strata. Thus, in the hundredth stratum, there would be a hundred times as many bubbles as in the lowest stratum; and, if the process went on uninterruptedly, these relative numbers would be constantly present. The phenomenon is best demonstrated on champagne; and the regular followers of science could not resist even my explanation derived from so plausible a material. Now, if Dr. Jones will try the experiment upon a bottle, the last glass will quite convince him of the correctness of my explanation. It requires repeated examination to find that the many bubbles in the upper strata are not all generated there, but have only risen to that position from lower spheres. Dr. Jones is evidently under a misconception when he assumes the drachm of urine not to be thoroughly mixed; for, on the addition of liquor to the drachm of urine, the liquor must sink to the bottom, as being of the highest specific gravity;* and the urine must rise, and permit all subsequent portions of liquor to pass through its substance. And then the tube is inverted, and, should any urine be left unmixed, it would have to go to the top again; hereby, and by the rapid evolution of gas, the fluid is brought into such a commotion that it must be most intimately mixed. Indeed, if neither the different specific gravities nor the commotion caused by the gas did effect an intimate mixture, the mere act of pouring ten, or twenty, or more parts of the liquor down a long tube, to one part of urine, would speedily effect a mixture; and, if that mixture was not effected in the manner I have described, it could not be effected at all, because, if the urine could ever be at the top of a column of the liquor, it must remain there, in virtue of its lower specific gravity, and could not on any account descend in the manner described by Dr. Jones.

I have made only a limited number of analyses by Dr. Davy's method, but never found any inconvenience from the mercury, such as Dr. Jones asserts that he has met with. A friend of mine, who has made many hundred analyses in this way, never observed the mercury being driven violently out and scattered about, and cannot account for this singular mishap to Dr. Jones, which caused him to adopt his first modification.

I hope Dr. Jones will continue his researches with improved methods. He has rare opportunities, and, being a pathologist, might turn them to good account. The fact of such a man applying himself to chemical analysis is an illustration of the turn which medicine is taking. We have had a great deal of pure chemistry: what we want now is chemistry applied. Applied to medicine, chemistry can only be by the profession itself, otherwise there will be no end of conflicts. If, then, the older members of the profession do not find application to quantitative analysis quite easy, they must not be astonished at it, for it took their junior colleagues years of practical study in the laboratory, and not a little reading and headwork, to attain the little which they can call their own. That ground it is their duty carefully to weed and cultivate, in order to come up to the expectations of those well versed in science, who assert that, in a short time, no physician would be able to give an opinion unless he be a microscopist and a chemist.

CASE OF FEMORAL HERNIA.

By EDWARD A. BROWN, Esq., Surgeon, Eastwood.

At 12 o'clock on the night of Friday, June 27th, I was summoned to attend Mrs. G., a near neighbour residing at Eastwood, suffering from strangulated femoral hernia, brought on by sudden and unusual exertion. The symptoms were well marked and severe. The pulse was irritable, about 90. There were anxiety of countenance, vomiting of bile and mucus, and distressing pain referred over the entire abdomen. She was placed in the usual position, with the thigh of the affected side rolled inwards towards the opposite one; and the taxis was applied in the direction downwards, backwards, and upwards, for twenty minutes, ineffectually. An ordinary stimulating enema was then ordered; effervescent medicine, with hydrocyanic acid and an opiate, were prescribed; and the case was left till the following morning.

* 1034, according to the "Dublin Pharmacopœia". The "London Pharmacopœia" gives no determined specific gravity of its preparation. The liquor, obtained from Mr. Morson, of Southampton Row, I found to have the specific gravity of 1.043.

June 28th, 8 A.M. There was no improvement, though the symptoms were somewhat masked by the anodyne. There had been no evacuation by the bowels. She had obstinate vomiting, increasing pain, anxious and distressed countenance. The taxis was again employed, with the use of warm bath, unsuccessfully, for twenty-five minutes, this being as long as the tenderness of the sac would admit of. Continue medicines.

2 P.M. Reduction was again ineffectually attempted.

8 P.M. There was no improvement; the symptoms were becoming aggravated. The taxis was used half an hour, under the influence of chloroform, unsuccessfully. The patient was left for the night.

June 29th, 8 A.M. There was constant stercoraceous vomiting, excessive pain and dragging all over the abdomen, eructation, hiccough, and tympanitis. The bowels had not acted; the pulse was hard, wiry, 140. It being now high time to afford relief if possible, the operation was proposed, and acceded to.

There being only an old woman and myself present, the inconvenience of giving chloroform and operating can readily be conceived, especially in a cramped up chamber, and with bad light. The patient, moreover, being naturally weakly, and further reduced by suffering, it was not wise to overdo the administration of chloroform; the consequence was, that she occasionally came to, which delayed the steps of the operation. Making the usual incisions, and dividing the several coverings down to the sac, I proceeded at once to open it, numerous adhesions around quite preventing it from being returned. I found the neck tightly embraced; the bowel was of a chocolate colour, and the vessels circulating on its surface were greatly congested. On dividing upwards, and very slightly inwards, a portion of Poupart's ligament, the return of the hernia could not be effected; and the stricture was evidently caused by Gimbernat's ligament. The bowel was also punctured with a grooved needle, and the flatus expelled to afford relief, but without effect. On carefully exploring the opening with the finger-end, distinct pulsation could be felt, from an irregular distribution of the obturator artery surrounding the inner part of the neck of the sac; and, being indisposed to risk its division, having oiled the forefinger, I gradually dilated the opening, and was pleased to find the method quite successful, the bowel being returned without much difficulty.

I ordered an enema of warm gruel, a dose of castor oil, and a mild mercurial purge. The bowels were freely acted on. Her symptoms were materially relieved, though peritonitis supervened: this, however, fortunately gave way readily to treatment, and everything progressed to a favourable issue.

November 3rd. Not the slightest inconvenience has arisen since the operation; and she speaks of the affected side being stronger and more comfortable than the opposite one. I have not the least doubt of the obliteration of the weakened aperture. She has for a considerable time been taking her ordinary food, and follows her ordinary avocation, being quite well. One of Huxley's elastic abdominal belts, with air-pad, has been supplied, and affords great support and comfort.

REMARKS. The process of mechanical dilatation acted in this case most favourably.

1. The risk of wounding the obturator artery was removed.

2. The part itself was not so weakened subsequently as by division with the knife.

3. Although, if too roughly handled, the gut might be bruised, in the absence of this, inflammatory action may take place around the neck of the sac, causing effusion of lymph and a repair of the otherwise weak points of the parietes. In the present case, I fully believe this has actually occurred.

THE TREATMENT OF ACUTE RHEUMATISM.

By HENRY WILLIAM FULLER, M.D. Cantab, F.R.C.P., Physician to St. George's Hospital.

DR. INMAN's article on the treatment of acute rheumatism, inserted in our JOURNAL on October 24th, demands a passing notice. Its quotations are incorrect, its statements of a strangely perplexing nature, and its conclusions at variance with the result of observations in London, Liverpool, and elsewhere.

And, firstly, as to Dr. Inman's quotations, or rather the statements and opinions he puts into my mouth. "Dr. Fuller," he says, "gives as the average duration of his cases *five weeks*." How far this quotation is consistent with accuracy may be