tion, or the existence has been discovered of any suspicious history, I believe that scarcely any operation can be pointed to which is alike so simple, so safe, and so successful.

It is, however, important, even in the most favourable cases, that the operation should be performed with as much care and gentleness and caution as are possible; whereas, I fear, a little haste and roughness occasionally intrude into the management of these patients in hospitals. In all my own cases, I have, in the first place, made a point of using a perforated needle of small size, so that the outflow of fluid is slow, and time is given for the organs within the chest to accommodate themselves gradually to the changes of pressure. The occurrence of pulmonary oedema, as well as of albuminous expectoration, is probably due to the too sudden afflux of blood into vessels which have long been compressed, and whose walls have therefore lost, in part, those physical qualities which render them impervious under ordinary circumstances to the albuminous constituents of the blood. If, however, the lung be allowed to expand slowly, time is allowed for the vascular walls to recover their normal physical relations to the contained fluid. Nothing is gained, but the time of the operator, by rapidly evacuating the fluid through a trocar of considerable size, while it is likely to favour the dangers which have been mentioned, and which I need not repeat.

I have long adopted the plan, which is mentioned in the JOURNAL, of enveloping the chest, after the withdrawal of the fluid, with a broad band of plaster nine or ten inches wide; but I wrap it tightly round the entire chest, and so provide a much more secure and effectual support than is given by applying it to the affected side only. I believe this application to be most useful.

I am in the habit of stopping aspiration immediately on the occurrence of any troublesome cough; and I object to the removal of more than fifty or sixty ounces of fluid at the first operation.

The production of local anaesthesia, as suggested, is too frequently neglected, and a needless amount of pain and terror is thereby inflicted, especially in the case of female patients, who generally dread the operation very greatly; a predisposition to syncope is thus engendered. Most of the cases I have had under my own care have made complete and rapid recoveries. The only fatal case I have had was one in which, after a first puncture, a considerable quantity of pus was evacuated; and, on its rapid reaccumulation, the patient, who was a very nervous person, refused to submit to any further operation except under chloroform, which was accordingly administered, and a drainage-tube passed. She recovered from the operation, and appeared somewhat relieved, but she died rather suddenly a few days afterwards. On *post mortem* examination, the lung on the affected side was found compressed into a thin, hard, flat, tough and dark cake, which was firmly bound down to the upper and back part of the chest.

Another unpromising case was that of a cachectic looking man of middle age, in whom the course of the disease had been exceedingly latent, and who had never suffered, apparently, from any acute illness; on aspiration, we removed about fifty ounces of dark hæmorrhagic fluid, with great relief to the dyspnoea, which was the only symptom of which he complained. He made a fair recovery, and the compressed lung expanded, but we found the physical signs of consolidation (probably tubercular) of the upper lobe continuing after the aspiration. He returned some months after as an out-patient, complaining of cough, emaciation, and dyspnoea; but there was no evidence of any great reaccumulation of fluid; he continued to follow his occupation, and objected to come into the hospital again "until he felt worse". He has not appeared recently, and in all probability he has succumbed to the original pulmonary disease to which the pleuritic effusion must be regarded as secondary.

This case and another, the particulars of which I will immediately mention, lead me to call attention to the impropriety of evacuating by aspiration those pleuritic effusions which arise as secondary to tuberculosis of the lung and pleura. I believe it is unwise to interfere with these secondary effusions, save for the purpose of avoiding impending suffocation, when they are very considerable. The case to which I have just alluded was that of a young man who, twelve months before coming under observation, had had an attack of hæmoptysis, accompanied with other symptoms, which might be regarded as pointing to the existence of phthisis. These symptoms had disappeared, but he had now a considerable accumulation of fluid in the left pleura. He was aspirated, and between sixty and seventy ounces of fluid were withdrawn. The compressed lung re-expanded perfectly, and he made a quick and excellent recovery; but, within a month of his dismissal from the hospital, he reappeared with all the evidences of rapidly progressing phthisis. There can be little doubt that, in this case, the compression of the phthisical lung by the pleuritic effusion had arrested the progress of the tubercular disease, which, when the fluid was withdrawn, developed rapidly. I should always hesitate to recommend thoracen-

tesis for the removal of pleuritic effusion, where there had been any distinct and reliable history of the previous existence of symptoms of phthisis.

With regard to washing out the pleura after operation for empyema, and the best methods for doing it, and the best solutions to use, the whole subject is elaborately discussed by Dr. Fraentzel of Berlin, in his contribution on Diseases of the Pleura in Ziemssen's *Cyclopadia*, a translation of which will be found in the forthcoming volume of the Anglo-American edition of that work. Dr. Fraentzel describes in considerable detail a method of washing out the pleural cavity by means of two Nélaton's catheters, which he has found eminently successful. The description is too long to reproduce here; but, as the next volume is to appear in March, those who take an interest in this operation will be able to refer to it themselves.

ON DEFECTS IN THE DOMESTIC MEASUREMENT OF DOSES.

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THE scrupulous accuracy with which prescriptions are made up by the highly educated dispensers who are now entrusted with this duty, perhaps tends to make practitioners careless regarding the exact mode of administration of their drugs. For convenience in out-patient departments, the time honoured teaspoon or tablespoon is invariably recommended for the division of doses; and we console ourselves for our want of scientific precision by the belief that these domestic measurements usually correspond to certain definite standards laid down in our text-books. Sometimes, however, we find complaints made that our medicine will not last out the week; and, on inquiry, we find, that the patients have consumed nearly double the proper quantity during that time, although they have conscientiously endeavoured not to exceed the regulated number of tablespoonfuls. This happens so often, and is productive of so much waste and annoyance, that I have lately, with the kind assistance of Mr. Marriott, dispenser to the Western General Dispensary, made a careful inquiry into the capacity of our common domestic measures with the following results.

I will first consider drops which are in occasional use, and which are held in a vague way to be about equal to a minim, although the more educated student of pharmacy would probably tell us that, on an average, they are only equal to half that quantity. Of

Distilled water	...	...	56 drops represent	60 minims
Tinctura opii	...	...	113 "	60 "
Tinctura digitalis	...	...	114 "	60 "
Liquor morphiae hydrochlor.	...	...	100 "	60 "
Oxymel scillae	...	...	80 "	60 "
Syrupus papaveris	...	...	75 "	60 "
Glycerine	...	...	45 "	60 "
Spiritus atheris nitrosi	...	...	114 "	60 "
Tinctura camphorae comp.	...	...	112 "	60 "
Acidum sulphuricum dilutum	...	...	55 "	60 "
Oleum ricini	...	...	55 "	60 "
Spiritus terebinthinæ	...	...	110 "	60 "
Oleum anisi	...	...	100 "	60 "

The next point is to inquire in how far the size of the drop is influenced by the shape of the bottle in which the fluid is contained. The experiments just noted were all made by means of an ordinary beaked minim-measure, and I found barely a shade of difference between this and a small phial brought by a dispensary patient. But, on making comparative observations with tinctura opii, I obtained the following variations from my original estimate of 113 drops to 60 minims. Out of the ordinary surgery bottle, I obtained 83 drops to 60 minims, and from a large twelve-ounce bottle in the possession of a patient 93 drops became the equivalent; and, although these differences are considerable enough in themselves, the bottles in ordinary use differ but little from one another, and it will therefore be seen that the quality of the fluid is the main factor in regulating the relative size of the drops.

But drops after all are comparatively seldom used, and the universal engine for the division of doses is the tablespoon, which homely implement is always at hand, and is held to represent a certain fixed quantity. In a series of seven selected at random, and apparently differing very widely in shape and in size, I discovered the following remarkable variations. No. 1, contained six drachms; No. 2, six drachms; No. 3, eight drachms; No. 4, eight drachms; No. 5, eight drachms; No. 6, five and a half drachms; No. 7, five drachms.

It was found that a difference of from one to two drachms is made

by the spoon being only moderately or completely filled; and of any oily fluid, at least an additional drachm can be readily added. The result, therefore, that even the smallest tablespoon does not hold less than five drachms, and that the largest contains at least an ounce, must completely alter our notions of prescribing; for we can surely no longer pin our faith to the regulation quantity of half an ounce, which this domestic measure is usually believed to contain. The dessert-spoon was found to be equal to from four to six drachms; whilst four specimens of teaspoons were found to contain respectively, eighty minims, ninety minims, and two drachms. The wineglass must complete the series; and, in four experiments, No. 1 was found to hold three ounces and two drachms; No. 2, two ounces and six drachms; No. 3, three ounces; No. 4, two ounces and a half; and No. 5, two ounces and two drachms—thus exceeding pretty considerably in every case the regulation quantity of two ounces. So much, then, for the safety and accuracy of the modes of measurement most commonly employed.

It would clearly be difficult, if not impossible, to expect any reform in this direction to come from the overcrowded hospital out-patient rooms; but it is not too much to insist that, in the wards and in private practice, graduated measures should invariably be used as a matter of routine. We shall thus secure not only greater safety in dealing with poisonous drugs, but greater scientific accuracy in any therapeutical observations we may wish to make upon our patients. But table-spoons which hold an ounce, and wineglasses which contain more than three ounces, can hardly be said to fulfil either of these necessary requirements.

CLINICAL MEMORANDA.

PECULIAR ALBUMEN.

THE JOURNAL of February 12th contains a short paper by Dr. Haddon under the above heading. The supposed peculiarity in Dr. Haddon's case consists in the fact, that "adding one drop of nitric acid causes a little white cloud as it travels to the bottom, which soon clears up. Heat being applied, the urine remains clear, and does not change in any way." The urine is acid, and, when boiled before adding any nitric acid, or when an excess of nitric acid is added, a copious precipitate of albumen occurs.

There is nothing peculiar in this. It is a well known fact, mentioned in most text-books, that with a small quantity of nitric acid, as with other acids both mineral and vegetable, albumen forms a compound which is not coagulable by heat, but readily coagulable by an excess of nitric acid. It should be borne in mind that urine to which a drop of nitric acid has been added is not "the same urine" as that which is unmixed with the acid.

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AGREEING as I do with Dr. Haddon that the commonly employed tests for albumen are unsatisfactory, there is nothing, I think, peculiar in the behaviour of the albuminous urine referred to by him in the JOURNAL of February 12th. I have repeatedly found—and believe the phenomenon to be constant—that if to albuminous urine (be it acid or alkaline) nitric acid be added in so minute a quantity that the opalescence first formed disappears on agitation, no amount of heat will coagulate the albumen present, unless a further addition of acid be made.

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I HAVE recently been engaged in investigating some of the properties of albumen, with a view to the clearing up of some of its eccentricities in reaction, and to the discovery of a simple method for its quantitative estimation. The results I hope to publish in full; but I may here so far forestall them as to mention some reactions which may explain the peculiarities observed by Dr. Haddon.

The chemical behaviour of albumen is very greatly modified, and especially so in regard to the effects of heat, by the coexistence of various saline and other bodies in solution, and the proportions in which such bodies occur (*i.e.*, the state of concentration of the solution). I believe that very many of the peculiar reactions met with in albuminous fluids may be thus explained. I have now pure albuminous solution to which I have added various constituents, and have repeatedly noticed the remarkable differences in reaction which are thus obtained.

It was, I believe, first noticed by Dr. Bence Jones that albumen is not coagulable by heat when it exists in the presence of a certain small proportion of nitric acid. This was explained by Dr. Beale, on the

supposition that free phosphoric acid was set at liberty by the action of the nitric acid on the phosphates present, and that it was this which retained the albumen in solution: an explanation certainly incorrect, for the same thing takes place where no phosphates are present. The true explanation, I believe, has been given by Mr. G. S. Johnson, in a paper, an abstract of which appears in the BRITISH MEDICAL JOURNAL of last year; and it is, that a true compound of definite composition is formed by the union of albumen and nitric acid, which compound is not coagulable by heat alone. Similar compounds are formed with other acids. By operating in a manner differing from that adopted by Mr. Johnson, I also have shown to my own satisfaction that the acids form a series of compounds of definite composition with albumen. Every acid I have yet tried, whether organic or inorganic, possesses the property of retaining a certain proportion of albumen in solution on boiling. It is important to remember that the free acid ordinarily present in normal urine will do this: a fact, I believe, never pointed out before; and thus, in cases where the amount of albumen is small, heat alone is an extremely unreliable test.

One other point I will mention. It is constantly recommended, even in such standard works as that of Dr. William Roberts, that where a specimen of urine is alkaline, it should be acidulated by the addition of acetic acid before subjecting it to heat in testing for albumen. How such a mistake has originated I cannot imagine; certainly whoever will try it, as I have done again and again, and with every variety of specimen, will get no result but a negative one. The presence of free acetic acid, even in very small proportion, prevents the coagulation of albumen by heat completely, and it has this further disadvantage, that, unlike the mineral acids, it has no tendency to cause precipitation even when added in large excess.

The safest test for albumen is, I believe, in all cases the moderate addition of strong nitric acid in such a way as to form a distinct layer at the bottom of the test-tube. If it be added in very large amount, and uniformly distributed, re-solution takes place; and, if added in a very dilute state and in small proportion, no coagulation occurs even when afterwards heated. A further addition of acid will, however, still throw down the albumen.

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SURGICAL MEMORANDA.

REMOVAL OF CRANIAL BONES.

IN connection with the remarkable case of recovery after removal of large portions of the cranium, related by Dr. Hayes in the JOURNAL of December 25th, a reference to a somewhat similar case, in which the greater part of both parietal bones and a considerable portion of the occipital were removed, may be of interest.

In December 1843, the late Sir Alexander Morison related before the Society for Improving the Condition of the Insane the case of B. H., who, originally of weak intellect, ultimately became insane, and the inmate of a lunatic asylum. On one occasion, in the absence of his attendant, "he placed a long sitting-form before the fire, laid his body on the form, and the back of his head against the bars of the grate. He was found in this position in a state of insensibility." After the removal of large portions of scalp, bone, membranes, and brain, the patient recovered and lived for eleven years, no perceptible change in his mental condition having been caused by his extensive injuries. Further particulars of the case, together with two plates of the patient during life, and one of the macerated cranium, may be found in the appendix to Sir Alexander Morison's *Lectures on Insanity*, 5th edit., p. 473.

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THE TREATMENT OF EPISTAXIS.

IN the JOURNAL of February 5th, there appears a communication from Mr. Cox of Winchcombe regarding my method of treating epistaxis; and he contrasts it with his plan of injecting a solution of the perchloride of iron up the nostrils. He says that its advantages over the method used by me are, its easier application, and that "it is more likely to reach the source of hæmorrhage, and consequently would be more effectual". In regard to the first supposed advantage, surely it is not a very difficult operation to fill the nasal passages in the way I indicated. If we use a pair of ordinary dresser's forceps, it is a very easy thing to do. My method has the double advantage of fulfilling at once the part of a plug and a styptic; and I cannot see the force of the latter part of his statement. The great objection I would have to