marked, it is true, than was the case four years ago, and less easily to be mistaken for the speech of her companion case, but still belonging, with her, to a type which we have not met with under other conditions.

As the nervous sequelæ of small-pox have received very little attention in English medical literature, we could be sure they were very uncommon in practice, but thought it worth while to turn for a moment to the experience of some of the chief small-pox hospitals of London. The records at Stockwell were consulted by Dr. Myers, and showed in the last fourteen years about 1,600 deaths from small-pox, but in only three of these is any nervous injury mentioned; and in these it was slight cerebral meningitis, with no symptoms that could clearly be distinguished, during life, in the patient's state of high fever and delirium. The clinical accounts of the last 2,000 cases were looked through without finding any records of nervous sequelæ. At Homerton, the records showed nothing; but from the cases at Deptford, the Junior House Officer, Mr. Struthers, was kind enough to tell us of two cases in the last eighteen months, in which the speech had become unintelligible. In the first, it so remained for three weeks and then recovered, but concurrent delirium tremens lessened the possibility of drawing any accurate conclusions; the second case died early, before detailed record had been made, and there was no post-mortem examination of the nervous system. From Highgate, Mr. Goude has been good enough to send us word that the records throw no light upon the subject; so we have thought we might be excused for drawing up a brief summary of seventeen cases we find elsewhere, previously printed, mostly in Germany and France; only one, so far as we know at present, in England. There is a striking group of eight cases, in which symptoms closely analogous to those of the patients we have brought forward to-night have been carefully observed. Westphal originally, in 1871, after an epidemic of small-pox in Berlin, brought such cases into notice by a brilliant paper, which raised a temporary interest in the matter in France as well as Germany, where Gabler, by his comprehensive essay in 1880, on the nervous sequelæ of fevers in general, had prepared the way. The accuracy with which Westphal's description of the speech applies to the present cases is not a little remarkable.

This is not the time or place to attempt to discuss the pathology of the affection, even if there were a sufficient basis of facts for such an attempt. It may just be remarked, however, that there is nothing in the symptoms incompatible with the hypothesis of minute scattered lesions, chiefly cerebral; and that it should occur in small-pox much more frequently, apparently, than in other fevers—for six out of the eight most closely analogous cases in our table are of small-pox—suggests minute hæmorrhages as a possible origin.

The clinical picture is not that of the commoner types of disseminated sclerosis. There is sudden onset; there is no increase of tremor; no nystagmus. The peculiar laboured speech, with each syllable "squeezed out," as Westphal phrases it, is different from the more ordinary forms in disseminated sclerosis. And what is more important as a characteristic and a distinction is, that the prognosis is better. In these cases, there has been slow improvement during six and four years, and the improvement seems likely to continue. In a third of the analogous cases we tabulate, there has been recovery; in nearly two-thirds, improvement; and in the single case of death out of the seventeen, the prevailing and fatal malady was tuberculosis.

Note.—Since writing the above, we have been glad to see that the same subject has been very ably handled by Professor Jaccoud, in three clinical lectures published under the date of this year. After reviewing the greater part of the previously recorded cases which we have tabulated, he supplies a very good example in his own practice of the same kind of affection of speech after small-pox, and another after pneumonia; and shows his appreciation of the earliest and most valuable evidence of Professor Westphal, to which we have briefly alluded. To the general condition he is content to give the name of "false" disseminated sclerosis—false, in as much as it tends nearly always to recovery, though the recovery may be very slow, as it has been in the cases we have recorded. He has not been able to find any *post mortem* evidence on the pathology of the affection, beyond what we have noticed—namely, the single case under Ebstein; and agrees in admitting that, on that matter, there is not at present enough material for a hypothesis.

AMBULANCE CLASSES FOR RAILWAY OFFICIALS.—The project of forming a Special Ambulance Class for Midland Railway Guards, under the auspices of the St. John Ambulance Association, was mooted at a meeting, held at Derby on Saturday last.

PELVIC TUMOUR COMPLICATING PREGNANCY.

By P. HORROCKS, M.D., B.Sc., M.R.C.P.,

Assistant Obstetrician to, and Demonstrator of Practical Obstetrics and Gynecology at, Guy's Hospital.

IN the BRITISH MEDICAL JOURNAL of March 20th, Mr. John W. Taylor suggests that the case which I published on March 6th, as one of Fibro-myoma complicating Pregnancy, might have been chronic distension of the Fallopian tube.

I should like to point out the differential diagnosis between these two conditions; and I will not lay any stress upon the fact that I have had ample opportunities for examining the case, both in the hospital and in the out-patient department, whilst Mr. Taylor has never seen the case.

I will take simply my own account of the facts of the case, given in the JOURNAL, March 6th, from which Mr. Taylor has drawn his conclusion, and I will show from them, even to his satisfaction, that the case is much more likely to be a fibro-myoma than distension of the Fallopian tube.

First. The substantive complaint of the patient was profuse losses at the monthly periods, accompanied by severe pain. Now, it is a well-known fact, that menorrhagia is almost constant in fibro-myoma of the uterus, whereas, even on Mr. Taylor's own admission, it is not a very common symptom of tubal occlusion and distension. Hence this symptom alone renders it highly probable that it is a fibro-myoma.

Secondly. The sound passed three inches and a half, proving that the uterus was considerably enlarged. This, again, is a common condition in the case of uterine myomata, and one which is much less common in distension of the Fallopian tube.

Thirdly. When the uterus had become pregnant, so far as to reach midway between the pubes and the umbilicus, the pelvic tumour itself had not been raised to any appreciable extent, but was lifted up by manipulation, so as to rest in the iliac fossa. Now, had it been a distended Fallopian tube, either it would have been raised up by the growing uterus, supposing it to be movable and free from adhesions, or it could not have been dislodged, and would have probably caused a miscarriage, had it been fixed by surrounding inflammation.

Fourthly. After the confinement, the tumour underwent evident involution, a well recognised phenomenon in the case of uterine myomata, and, to say the least, an unproved process in distension of the Fallopian tube.

Fifthly. The patient had severe pain at her monthly periods, and, when in the hospital, had pain in passing motions; otherwise, there was no pain. Mr. Taylor says that chronic distension of the Fallopian tube "is generally accompanied by pain, increased before or during menstruation." Therefore, if mine were such a case, it would have exceptional symptoms, or lack of symptoms, unless it were a chronic and stationary hydrosalpinx. The menorrhagia, the uterine enlargement, the behaviour of the tumour during pregnancy and under manipulation, the involution during the *post partum* period, and the absence of intermenstrual pain, appear to me to justify the diagnosis of uterine fibro-myoma.

In this differential diagnosis, I have used only the facts contained in my clinical account, which Mr. Taylor had before him; but I feel sure that, had he been able to examine the case carefully, as I was, he would have come to the same conclusion as myself in regard to it. I am far from denying what Mr. Taylor asserts at the end of his paper, namely, "that chronic hydrosalpinx, both by signs and by symptoms, may occasionally closely simulate nodular myoma of the uterus, and need differential diagnosis from that affection;" but I think I have made this differential diagnosis in my case.

REMARKS ON COLLOID DEGENERATION OF THE SKIN.

By ROBERT LIVEING, M.D., F.R.C.P.,

Physician to the Skin Department of the Middlesex Hospital.

THREE cases of a rare degeneration of the skin have come under my observation during the last few years, to which I desire to call the attention of those who are interested in dermatology. Unfortunately, all three cases occurred in private practice; and I have been unable to exhibit them, or even to make a microscopic examination; therefore all my remarks will be simply clinical.

I will preface these remarks by saying that a similar disease or degeneration has been referred to by Wagner, under the name col-

loid milium, and by Besnier as "colloid degeneration of the skin." Duhring says "the disease is characterised by numerous disseminated, small, pin-head sized, discrete, rounded, flat or slightly raised lesions, of a pale or bright lemon colour. They are shining and translucent, and have the appearance of being yellowish vesicles. Their appearance, however, is deceptive, for they are of firm or solid consistence. When pricked with a needle, or opened sufficiently deep to cause bleeding, a whitish, or yellowish, transparent gelatinous substance may be expressed." He also says that it resembles xanthoma, but the lesions differ in being bright and translucent. With this latter remark, I entirely agree. In each of the three cases that I have seen, the first glance led me to believe that I had before me a case of xanthoma, and suggested to me the name colloid xanthoma. A careful examination, however, satisfied me that the disease was not xanthoma, or at all events, that, if the little growths were originally xanthomatous, they had undergone, and were undergoing, remarkable changes from their original condition, and quite unlike those usually seen in xanthoma.

The first case I met with was in a young woman; of this case, I lost sight. The second was in a man; the disease attacked his face and neck, and ran its course in about a year; he at last quite recovered. The third instance was in a girl of about 16, in whom the face, neck, and arms were attacked. The following brief description, taken from my notes of one of the cases, will apply pretty nearly to the other two. The little growths are scattered about the face, neck, and upper arms, and many of them are undergoing metamorphosis; there are, however, one or two very perfect ones on the back of the neck. They consist of small, slightly raised, yellowish tumours, varying in size from a large pin's head to a split-pea, somewhat flat, of solid or semi-solid structure, but from being translucent, they look as if they contained fluid; minute vessels are seen round the margin of some of the larger ones; the appearance of these reminds one of rodent ulcer in its earliest stage, though the resemblance is in appearance only. These little tumours undergo change by the formation of a central depression, so that many are umbilicated, the depression gradually becoming a shallow crater-like excavation; and, lastly, they inflame, scab, and dry up, leaving a mark, but not a defined scar. The changes which they undergo remind one of those seen in molluscum contagiosum, but the general appearance is not very like that disease, and could not be mistaken for it.

It would be interesting to determine the two following points with regard to this disease: (1) where, and under what circumstances, the disease originates, whether from previously healthy skin; or (2) whether the degeneration may not occur in more than one disease of the skin, such as, for example, milium, xanthoma, and molluscum contagiosum.

A MODIFICATION OF FEHLING'S SOLUTION FOR TESTING FOR, AND ESTIMATING SUGAR IN URINE.

By F. CRESSWELL, L.R.C.P.LOND., M.R.C.S.,
Late House-Physician, St. Bartholomew's Hospital.

I HAVE always found that the chief inconvenience in using Fehling's solution for estimating sugar, was that it would not keep for above a few weeks, at the outside, and in hot weather it frequently became untrustworthy in a much shorter time. This trouble is owing to the readiness with which the sodium and potassium tartrate undergoes decomposition. I have therefore tried to dispense with the use of this salt, and I think have succeeded thoroughly in the following manner. After trying several organic substances which were not prone to decomposition in an alkaline solution, I have found that glycerine answers best. It completely prevents the precipitation of copper sulphate by caustic potash or soda, and the solution will keep for an indefinite time in an open vessel, in fact, until the caustic alkali becomes converted into carbonate, and in a stoppered bottle seems quite permanent.

I find that grape-sugar reduces somewhat less copper from the glycerine solution than from the tartrate solution, and that uric acid will scarcely attack it, whereas it was one of the chief causes of fallacy in Fehling's test.

The best method of preparing the solution is as follows. Take about 35 grammes of copper sulphate, and dissolve it in 200 cubic centimetres of glycerine and 100 cubic centimetres of water; then add 80 grammes of sodium-hydrate dissolved in 400 cubic centimetres of water, and boil the whole for fifteen minutes. This is necessary, as all the specimens of glycerine which I have met with contain a small

quantity of some substance capable, at a boiling heat, of reducing an alkaline solution of copper. After boiling, the solution is made up to 1 litre with distilled water, and allowed to stand until it is clear. It must be standardised by a solution of grape-sugar of known strength for accurate determinations, and is sure, if made as above, to require diluting. If it be only required for rough clinical purposes, the above quantities may be diluted to 1250 cubic centimetres; 10 cubic centimetres will then be approximately equal to 5 centigrammes of sugar.

The process is carried on in exactly the same way as the well known method of Fehling; 10 cubic centimetres of the copper solution are mixed with 50 cubic centimetres of water, and boiled in a small flask, and the solution of glucose (which should not contain more than one per cent.) is slowly added from a burette, the contents of the flask being kept steadily boiling until the original blue colour of the solution has entirely disappeared; the quantity of solution used must then have contained 5 centigrammes of sugar. Diabetic urine usually requires diluting to one-fifth or one-tenth, in order to reduce the sugar below one per cent. For example, 10 cubic centimetres of a diabetic urine were diluted to 100 cubic centimetres, and transferred to a burette; 10 cubic centimetres of the copper solution require for complete decolorisation 26 cubic centimetres of this diluted urine; 2.6 cubic centimetres of the original urine, therefore, contain 5 centigrammes of sugar, or 100 cubic centimetres would contain 1.92 grammes, or 1 ounce (437.5 grains) would contain 8.4 grains of sugar. In the quantitative examination of urine for sugar, a few drops of the copper solution are mixed with about 5 cubic centimetres of water and boiled, about ten drops of urine are added, when, if sugar be present, on again boiling for a few seconds, the characteristic brick-red hydrated suboxide of copper will fall.

I hope the above will prove especially useful to country practitioners, who seldom require to test for or estimate sugar, and who, when they do, by using the above solution, will escape the annoyance of finding their testing solution decomposed and useless. I have had some of the above solution in an ill-stoppered bottle for between four and five years, without any signs of decomposition.

OBSTETRIC MEMORANDA.

THE ADVANTAGE OF STRAIGHTENING THE UTERUS IN CASES OF HÆMORRHAGE.

Mrs. G., aged 39, had adherent placenta in her three previous confinements, necessitating peeling off from the uterine surface, but made excellent recoveries, with no pyrexia, or undue subsequent hæmorrhage. This last time, however, the placenta was, after some delay and difficulty, removed by expression and traction combined, and she made a good recovery, excepting that there was slight persistent hæmorrhage, which, at the end of six weeks, and after she had begun to move about, became so copious that she was obliged to resume the recumbent posture, and an examination became necessary. The uterus was found large, flabby, and ante flexed, with the os patulous and the fundus tender, and through the anterior wall something was felt hard and nodular; but, as it was just a month since she had ceased to suckle the baby, it was thought that perhaps this fresh accession of hæmorrhage might be due to the return of the catamenia; and, beyond freely manipulating the uterus, and pressing the fundus well up, with injunctions to remain lying on the back as much as possible, nothing further was done. The same evening, however, the nurse brought me a substance she had passed about an hour or so after my departure, which would seem to have been one of the cotyledons of the placenta, that had been retained, and which, although loosened, had been unable to escape, owing to the ante flexion. The placenta was examined when removed, and seemed entire; but it may have been one of the irregular and lobulated sort, in which it is often difficult to be sure that there may not have been a lobule left. The hæmorrhage immediately ceased on the expulsion of the substance, and the patient was soon all right again, the catamenia subsequently being quite regular and natural.

It ought to be mentioned that this patient had, eighteen months previously, and after suppression of the menses for several periods, a severe flooding, which may have been due to an abortion, although the medical gentleman in attendance failed to find any ovum extruded. At any rate, there was very obstinate and prolonged menorrhagia, especially during the menstrual periods, which was only subdued after long confinement to bed, with the rectification of an ante flexion by a pessary, cold irrigation, sitz-baths, etc., together with iron, ergot, and strychnine. Is it possible that the substance above referred to may have been the remains of this abortion that had maintained its