Amongst these means, tapping appears to be the chief remedy; and believing that by its aid life may frequently be prolonged, and the success of ovariectomy not only not interfered with, but actually promoted, I beg to offer a few remarks on the advantages of this simple operation, which I trust may not be unacceptable to my professional brethren.

Medical literature supplies us with a few instances where tapping has sometimes arrested the disease for so long a time, that it has almost been regarded in the light of a cure. Three examples at least of this kind have come under my own notice; and my experience leads me to think that in cases judiciously selected the operation, if carefully performed, may be more frequently successful than is generally supposed.

In 1843, Mrs. G. of Salford consulted me respecting the propriety of the extirpation of a tumour, from which she had been suffering about three years. She was then thirty-three years of age, married, and had four children, the youngest two years old. In company with the late Mr. Thomas Slack, her surgeon, I made a careful examination. We found the abdomen much distended, the swelling caused apparently by fluid. As the patient was in a feeble state of health, we decided upon tapping, and drew off twenty quarts of a clear fluid of low specific gravity. The operation was followed by some constitutional disturbance, but in three weeks she seemed to have completely recovered. About twelve months afterwards, the abdominal swelling returned, and, at the end of two years from the former operation, the distension was so large that we had again recourse to the tapping, and removed twenty-two quarts of fluid. Her recovery was most favourable. At the end of a fortnight, she was able to resume her domestic duties, and remained in a satisfactory state of health until 1864, a period of nineteen years. Since that time, however, the swelling has gradually returned, but so slowly as to occasion little or no inconvenience, until the last few months; and, though the abdomen is considerably enlarged, she feels at present no necessity for operative interference. She is now in her sixtieth year, and to all appearance in better health than when she first consulted me twenty-six years ago.

In 1843, I was requested by Dr. G. C. Watson of Chester to visit with him Miss S., aged 25, residing in Liverpool, who was suffering from an ovarian tumour, the removal of which two gentlemen, who had made this disease a speciality, had recommended. A careful examination convinced us that the tumour was a unilocular cyst; and, as the patient appeared in good health, we advised tapping. The operation was performed by Dr. Watson with a medium-sized trochar, and between five and six quarts of a semi-transparent viscid fluid removed. The patient's recovery was rapid and complete; and, at the end of five years, when I last heard of her, there had been no return of the disease; and Dr. Watson informs me that from that time he has had no reason to suppose that she has not remained perfectly well.

In July 1867, Miss M., aged 25, asked my advice respecting an ovarian tumour that had been developing itself about three years, and which she had been urged to have removed. It appeared to consist mainly of fluid contained in a single cyst, and her health was not in the least impaired. I therefore recommended her to be tapped. Six quarts of clear viscid fluid were removed. She speedily recovered, and, on a careful examination made only a short time ago, I could detect no return of the swelling.

Such favourable results as the above are only to be expected when the disease consists of one or perhaps two cysts; but, as a fair proportion of ovarian tumours assume this character, the possibility of recovery after tapping ought not to be overlooked in their treatment. It must, however, be admitted that the beneficial effects of the operation are frequently only temporary, and therefore it ought not to be undertaken if likely to hinder the successful performance of ovariectomy.

Fortunately, this evil result is not often to be apprehended: on the contrary, one or two tapplings, especially when the cyst has obtained a considerable size, are usually succeeded by a marked improvement in the patient's health, and by a reduction of the original tumour, so that only a moderate incision through the abdominal parietes will be necessary for its extraction; both of which conditions are favourable to the success of the major operation.

Amongst the cases of ovariectomy that have come under my notice, several have been those of patients in comparatively good health, where the tumours were of recent formation, of moderate size, and free from adhesions, and the operation itself, therefore, of the simplest description; but the results have not been so satisfactory as in cases of longer standing, and of a more complicated character—acute peritonitis being usually the cause of death in the former, shock and its consequences in the latter. The tendency of the peritoneum, therefore, to inflammatory action seems to diminish with the duration of the disease, probably on account of the continued friction of this membrane against the surface of the cyst diminishing its sensitiveness.

Accordingly, in the earlier stages of the disease, whether the tumour consisted of single or of multilocular cysts, it has for some time been my custom to advise a postponement of ovariectomy; tapping the tumour once or twice, when it has become inconveniently large. Since adopting this plan, I have removed seven diseased ovaries, and in only one case was the operation fatal. Five of the patients had been tapped once, one twice, and one three times, the fatal case being among the first five. The recovery of those tapped only once was the most rapid, although in three of them the tumours were multilocular cysts. The two tapped more than once made favourable progress; but their complete restoration was not accomplished for several weeks, owing probably to the fluid in the cysts having become purulent after the second operation in one, and after the third in the other. Suppuration of the cyst I have not unfrequently found to be the consequence of repeated tapplings; and, therefore, it is a matter of doubt whether the operation should be undertaken more than once or twice, as its repetition may diminish the chance of successful ovariectomy. The experience of Mr. Spencer Wells is also favourable to a preliminary tapping; for he found that, whilst the average mortality in 135 cases, where the patients had never been tapped, was 27.40 per cent., where the patients had been tapped once, the mortality was only 25.64 per cent. (*Proceedings of the Royal Medical and Surgical Society, 1869*).

It is scarcely necessary to dwell on the advantages of tapping in clearing up any doubtful points in the diagnosis of abdominal tumours; for no one, I think, would resort to extirpation in such cases until he had first ascertained the nature and connections of the disease, for which purpose tapping has not unfrequently been found most useful.

In offering these remarks on the tapping of ovarian cysts, I must not omit to notice the mode of performing the operation, especially as, when ovariectomy was in its infancy, I first called attention to the dangers which attended its performance (*London Medical Gazette, 1843*).

It was then the custom to use a full sized trochar; the patient was seated on a chair, and, to ensure the complete expulsion of the contents of the cyst, the abdomen was compressed with towels by the operator's assistants. Additional force was also often applied by pressing the abdomen with the hands, whereby considerable violence was sometimes inflicted on the peritoneum and the contents of the abdomen, which, with the frequent entrance of air through the trochar into the cyst, often led either to inflammation of its walls or of the peritoneum.

The present mode of operation is comparatively free from these dangers. The patient is allowed to recline in bed or on a couch. A small sized trochar with a stop-cock is employed, to which is attached a long piece of elastic tubing for conveying the fluid to an utensil placed near the patient; which tubing, when used on the principle of the syphon, as recommended by my colleague, Dr. Roberts, excludes all air from the cyst. The fluid, by this means, is removed very slowly, and thus faintness is prevented, whilst the gradual contraction of the cyst and abdominal parietes renders it unnecessary either to press the abdomen with bandages or with the hands, and the risk of peritoneal inflammation is lessened. By attending to these precautions, tapping, which was formerly often accompanied with considerable danger, has become one of the simplest and safest operations in surgery.

HOSPITAL VERSUS HOME PRACTICE.

By J. MATTHEWS DUNCAN, M.D., Edinburgh.

[Continued from page 435 of last number.]

As in my former paper, devoted to a consideration of the value of the data adduced by the enemies of hospitals, I have not expressed my own opinion about the merits and demerits of these institutions; so, in this paper, I shall consider the reasoning (against hospitals) into which the abovementioned data are introduced, without entering on their good and bad qualities.

I must, preliminarily, express my disapproval of attaching to them the designations of "palaces" and "palatial structures." This is either a monstrous error of description, or it is a miserable specimen of grim and thoughtless irony.

A statistical argument is entirely, or almost entirely, based on figures. In the natural course, then, its conclusion demands and obtains unanimous assent, if there be no flaw in the reasoning. It is nearly like a mathematical demonstration. When, therefore, we find the conclusions of a statistical argument, such as that against hospitals, meeting with incredulity and opposition, we have to go back to the arguments in order to consider the data and the reasoning. The former I have already discussed; I now turn to the latter.

Before doing so, I may remark that students often find the conclu-

sion of an argument so outrageous, that they do not stop to scrutinise the data and the reasoning. They simply laugh at the conclusion and reject it. This process may very justly be adopted with Le Fort and Simpson's conclusion that, in the home practice of maternities, only 1 in 212 dies. An obstetrician, knowing that such favourable results are nowhere obtained, naturally says he cannot believe that they are now discovered among the poorest of large towns. When he turns to the details of the statistics, and finds such mortalities of childbed as 1 in 591, his rejection of the conclusion becomes derision of it. A like process of rejection of some of Sir James Simpson's surgical statistics is adopted by Mr. Holmes, who regards some of the successes recorded as being, in the eyes of a London surgeon, scarcely explainable, except by supposing a miracle. I shall give another example from Sir James Simpson, of an obstetrical conclusion adopted by himself (*Obstetric Works*, vol. ii, p. 544), in which he says the statistics "offer a kind of evidence, which is not less remarkable for its intelligibility and simplicity, than for its precision and certainty." I am sure that, had he only thought of it, he would have taken the vaunted statistics as a mere sample of incredible nonsense, masked by a solemn numerical statement. The statistics referred to are said to prove that, from 1660 to 1820, the mortality of childbed diminished from 1 in 44 to 1 in 107. It is needless to say that the statistics adduced are so imperfect in their nature as to prove nothing whatever; and that the statistical result in question gives such a magnificent view of the progress of the practice of midwifery, that it is at once rejected as beyond belief, without inquiry into the errors of the supposed proof. Would to God it were true; we might then hope soon to see childbed despoiled of all its dangers. We cannot even now tell what is the mortality of childbed in London. How Merriman and Simpson find out what it was in 1680 and onwards, when there was no registration of births, is an inquiry that might be amusing, certainly not instructive.

I shall give another example of a conclusion similarly deduced and to be rejected without scrutiny of data and reasoning. Sir James Simpson statistically demonstrates that first tappings of ovarian cysts are very dangerous—that 1 in 5 dies (*Obstetric Works*, vol. i, p. 266,) from the operation. Who that has any matured experience can believe this? Now, when it is known that Wells and Keith can perform ovariectomy with the same mortality, or less, who will think it worth while to inquire into the nature of the statistical data and reasoning which are vainly imagined to establish the paradox? (See *Lancet*, Feb. 28, 1857.)

The argument against hospitals is conducted as follows. The mortality of amputations and confinements is greater in hospitals than in home practice. The patients differ from one another in no important respect except the point of hospital or home residence; therefore, hospitals are murderous, and should be revolutionised or closed. If the argument here is all right, then the conclusion is fair. If the two first steps are valid, then the result is true. We do not doubt that the first step is valid, that the mortality in hospitals is greater than in home practice. This has long ago been often pointed out. But we refer to our former paper (*BRITISH MEDICAL JOURNAL*, October 23rd, 1869), for our reasons for rejecting Le Fort's and Simpson's evidence on this point.

The second step is, "The patients differ from one another in no important respect except the point of hospital or home residence." If this is not true, if this is not admitted or proved, then the conclusion against hospitals is not fair. The argument fails. Now this second step is neither proved nor admitted. So far is this from being the case, that the triumphant declaration of the conclusion against hospitals must be regarded as a burlesque upon reasoning.

Many differences between hospital and home patients have been pointed out. It is notorious that the health of towns is much worse than the health of country districts, in which are the homes of the patients now under discussion. It is notorious that many of the worst or most hopeless cases are sent from the country to the town hospitals. It is notorious that hospital surgeons are bolder in trying to rescue by operation than home practitioners. It is notorious that the most poor, wretched, unhealthy, patients find their way to hospitals in extraordinary numbers. These are a few differences between home and hospital practice. They are enough to invalidate the argument against hospitals. Even if they be not admitted, the difficult task of proving that they are not true remains for the enemies of hospitals as a preliminary to the acceptance of their argument against these institutions. It is incumbent on them to prove that residence is the only important difference between home and hospital patients. This they have not done, and, so far as I see, cannot do.

It is easy, and it may be instructive, to illustrate by example, the viciousness of the reasoning which is employed in the argument of Le Fort and Simpson. The mortality of childbed in Edinburgh, as got from the Registrar-General's returns, is about 1 in 160. In the rural districts

of Scotland, it is about 1 in 200. The patients are in both regions in the same conditions, except that in Edinburgh there is a great aggregation of experienced and learned obstetricians; in the rural districts, practitioners are much segregated. The obstetricians of Edinburgh are, therefore, more dangerous to their patients than those of the country. They should be separated from one another, and deprived of their learning and experience. Now the error in this argument lies in the arbitrary assumption that the two sets of cases considered differ only in regard to the medical attendance. In like manner the error in the argument against hospitals lies in the arbitrary assumption that hospital cases differ from home cases only as regards residence, in the hospital or at home.

There is another error in reasoning on the subject under discussion, into which Le Fort and Simpson have fallen. They take the data of all hospitals—good, bad, and indifferent—and use the results of the combined data to damage the character of every hospital. For example, they say that maternity hospitals have a mortality of 1 in 29. To this I reply, that no good maternity hospital has so high a mortality, and that such should be spared the disgrace of being placed in connection with hospitals which have. To judge fairly of an hospital, it is only necessary that it should show a large enough and long enough trial to justify a judgment. The Dublin Hospital has been in existence more than one hundred years. There have been more than 190,000 deliveries in it, and the mortality has been 1 in 72. (*Dublin Quarterly Journal of Med. Science*, vol. xlvii, 1869, p. 293). Is it proper, generous, or fair, to couple the fair name of this institution with a disgraceful mortality of 1 in 29? to malign maternity hospitals as a whole, because some are abominable? No doubt 1 in 29 is the mortality of maternity hospitals as a whole. But what has that to do with the question, whether a maternity hospital, if well conducted, is a valuable institution or not; whether it yields better or worse results than home practice? As a help in settling these important questions, it is grossly misleading. The statement of a mortality of 1 in 29 as the mortality of maternity hospitals is a slander on such hospitals as that of Dublin. The Dublin Hospital can show, for a period of seven years, and above 16,000 cases, a mortality of less than 1 in 100—a mortality probably nearly as small as that of the best contemporaneous practitioners in Dublin, and perhaps still more remarkably small if the character of the cases in the hospital be kept in mind. These last appear to me to be indisputably the kind of data fitted to be the basis of a judgment regarding the value of maternity hospitals.

What would be thought of a surgeon, wishing to estimate the value of ovariectomy, who took as his data the mortality of all the cases of ovariectomy he could find, done by any surgeon, or by any method? He would not be listened to. In his data would be found the experience of many men who never succeeded at all, everyone of whose cases proved fatal. Such a collection would tell the actual mortality of ovariectomy, how many it had slain, how many it had saved. But it would tell nothing, nor help to tell, as to the value of ovariectomy. Such statistics might, if Le Fort and Simpson's reasoning were imitated, lead to an argument against ovariectomy, as too dangerous to be ever justifiable. The proper data for judging of the value of ovariectomy are those from the large and long experiences of Wells and Keith. Judged by the Le Fort and Simpson argument, it might be condemned as murderous; judged justly, it is commended as a triumph of modern surgery.

It has long been well known that the mortality in hospitals, from all sources, is greater than in home practice. Farr in this country, and several distinguished foreign authors, have strongly insisted on this. It has been generally regarded as in a considerable degree depending on hospital arrangements being imperfect. While there can be, in my opinion, no doubt as to the injurious influence of bad hospital arrangements, or even those in common use, there is equally no doubt that they are only one of several causes of high mortality in hospitals. To how great an extent this injurious influence operates, no one knows; and certainly the evidence of Le Fort and Simpson is so little trustworthy and so ill argued as to contribute nothing towards the solution of the difficulty.

Some hospitals are extremely bad. It is related, for example, that Dupuytren lost every patient on whom he performed amputation in the Hotel Dieu, during an occupation of Paris. The Clinical Maternity Hospital of St. Petersburg is said to have a mortality of 1 in 11. Of such institutions there can be no hesitancy in condemnation. They should be revolutionised or abolished.

Some hospitals are extremely bad at certain times. This is generally the result of overcrowding. Pyæmia, erysipelas, hospital gangrene, flourish. Such hospitals should be shut for a time, and the overcrowding or other discovered cause of the evil days be absolutely prevented in future.

Some hospitals cannot be shown to be in a marked way prejudicial to the health or recovery of their inmates. For example, the great hos-

pital for lying-in women in Dublin has frequently and for series of years had a mortality probably nearly as small as that of the practice of the best practitioners in Dublin. Such hospitals should be imitated and admired. The causes of their diminished healthiness at certain times should be investigated, and, if discovered, removed in future. It must be remembered that private practitioners, as well as hospitals, have probably bad times in their practices.

All hospitals which I have seen are very imperfect. They should be improved. Medical men should combine to discover the causes of their imperfection, and the requisite remedies. To adopt notions as to the necessary insalubrity of hospitals, and the salubrity and advantages of cottages, would, in my opinion, be the adoption of mere whims, leading to a reckless and injurious mode of proceeding. It would lead to the destruction of invaluable institutions without securing compensating advantages of any kind. If cottages are shown to have, on the whole, advantages over hospitals, then I am sure the profession will adopt them, and energetically set about to overcome the great and easily seen disadvantages which the cottage system would necessarily entail.

I lately visited the magnificent Hospital of Leeds. The wards seemed to me overcrowded. Whether this be so or not, the managers can, if they find the hospital unhealthy, improve it to almost any extent, by thinning out beds from the various wards. Till all this is done, and a great deal more, they need not think of pulling down the house and building cottages.

CLINICAL MEMORANDA.

[Under this head, we shall publish from time to time, as materials accumulate, short records of remarkable cases in practice which are sufficiently rare, interesting, or instructive, to deserve record, but do not call for lengthened statement or comment. Brevity and point should be the valuable characteristics of cases forwarded for this column.]

ACTION OF BROMIDE OF POTASSIUM.

By J. LOCKHART CLARKE, M.D., F.R.S.

IN the JOURNAL of October 16th is published an account of some peculiar feelings experienced by a patient from the effects of considerable doses of bromide of potassium. I have recently witnessed somewhat similar but still more remarkable effects from only two half-drachm doses of this medicine. The patient called on me the morning after he had taken them, and surprised me by the peculiar expression of his countenance. He was perfectly rational, but declared that he was wholly unfit for any kind of occupation. At my request, he wrote me the following account of the feelings which he experienced. I have known him several years, and have reason to rely on the accuracy of his statements. He was not epileptic.

"I took one dose at 11 A.M., and the other at 5.30 P.M. the same day. I had tea at about 6.30, and went to Bayswater to spend the evening with some friends. I took nothing to eat or drink after tea, with the exception of half a pint of beer when I left my friends' house at about 11 o'clock.

"Before I left my friends, I told them that I had *dreamt* everything they said to me the night before. They noticed that I looked *queer*, and would have given me credit for being slightly '*screwed*', if I had not spent the evening with them; and they made me promise to send them a line by post to say I had arrived at home safely.

"On waking up the next morning, I could not remember anything that had happened to me the night before, and asked my brother what day it was, the month of the year, etc. I began to feel alarmed, as I could not collect my ideas in the slightest; so thought I would look in my pocket, and see if my letters would help me. I was astonished to find that I had written a letter the night before to my friends in Bayswater, saying I had arrived at home safely, in spite of my dream coming so true. I next found some notes of my sensations written on the back of a letter, and then a fair copy of the same, written on another piece of paper. I have destroyed both of these; but, to the best of my recollection, they were as follows.

"*'12 o'clock, Midnight.* Doses at 11 and 5.30. Ate nothing since 6.30."

"*'Sensations.* I feel as if everything that I feel, see, or think, had been experienced before, as in a dream. This delusion is so strong, that I could swear I dreamt it all."

"I came down and saw you directly after breakfast, and then walked

to my office in Westminster; but found I was in such a confused state I could not get on with my work, so walked up to Regent Street, and had my head shampooed. After that, I called at my married sister's house, and she at once noticed that I was looking queer; and, as if frightened, said, 'Well, you do look a wretch. What on earth have you been doing with yourself?' I began to tell her, but had only got half way in my account when I cried. After a *good cry* and two glasses of sherry, I felt better. My sister and her husband, thinking the fresh air and a little mild excitement would do me good, took me down to the Crystal Palace. About five o'clock in the afternoon, I suddenly remembered having come home on an omnibus the night before, and gradually recollected many things that had happened, but cannot even now distinctly remember how I got from Victoria Station home, or what I did on my arrival. I may mention that I now remember thinking that I must be under the influence of Mesmerism, and saying to myself, 'Well, anybody who does not believe in Mesmerism after this must be a fool.'

"I ate a good dinner after my return from the Palace, and felt I was quite myself again before going to bed."

DANGEROUS ATTACK OF ERYSIPELAS, OCCASIONED BY A GLASS EYE.

By J. M. WINN, M.D., etc., Senior Physician to St. George's and St. James's Dispensary; late Resident Physician to Sussex House Lunatic Asylum; etc.

ON the 26th of July last, I was requested to see, in consultation, Miss —, aged 25, who had been suffering since the 22nd from what was supposed to be a severe attack of idiopathic erysipelas of the face. I found her in an extremely low condition. The forehead and upper part of the face were much swollen, and of a palish red hue, indicating inflammation of an asthenic character. The pulse was feeble and fast; the tongue red and rather dry; the bowels inclined to be relaxed. On closely questioning the friends of the patient, I ascertained that she had been wearing, for a month past, an ill-fitting glass eye, which had given her considerable uneasiness; and that she had herself frequently, but ineffectually, attempted to remove it. Suspecting that this might be the cause of the malady, I suggested its removal. As soon as this was effected—and it was extracted without any difficulty—an ounce and a half of pus escaped from the orbit, clearly showing the source of the mischief. She was ordered to take quinine, beef-tea, and wine, every four hours.

July 27th. She had rallied slightly; but the bowels were much relaxed, and she complained of pain in her head. She was ordered to continue the quinine, and to take chalk mixture for the diarrhoea.

July 28th. The diarrhoea was unabated. She was apparently sinking; but the pulse was tolerably firm. She was ordered to omit the quinine, and take two drachms of liquor ammoniæ acetatis, with ten minims of tincture of opium, in an ounce of water, every four hours; and to substitute brandy for the wine.

July 29th. She had again rallied, and the bowels were less relaxed. The erysipelas had left the forehead and upper part of the face, and had extended to the lips, throat, and chest. The catamenia, which had been irregular of late, appeared to-day, but very scantily. The quinine was omitted; and tincture of sesquichloride of iron was ordered, in ten-minim doses, every four hours.

July 30th. She was much exhausted by a return of the diarrhoea. The throat was swollen to such an extent that she could only swallow the smallest quantity of liquid, and that with extreme difficulty. She was ordered to omit the iron mixture, and take ten minims of dilute nitric acid and fifteen minims of tincture of opium, in water, every four hours.

July 31st. The erysipelatous inflammation had subsided on the chest; but the redness and tension of the skin around the throat were so great, that deglutition was almost impossible. In spite of these unfavourable symptoms, the pulse was stronger, and the diarrhoea had ceased. A linseed-meal poultice was applied to the throat. She was ordered to continue the nitric acid mixture; and, if totally unable to swallow, to have an enema of beef-tea, with brandy and laudanum, every four hours.

August 1st. After the application of the poultice, she was able to swallow better; and the enemata were not required. She was ordered to continue the nitric acid mixture.

August 4th. An abscess had formed beneath the chin. On puncturing it with a lancet, a considerable quantity of pus escaped.

August 5th. She was able to sit up. She had a good appetite, and was going on satisfactorily in every respect.

This case speaks for itself, and shows the great care that is required in adjusting such an apparently simple appliance as a glass eye.