the brain, nor was the time which had elapsed since the accident by any means so long as is usual in cases of this nature, nor had rigors or sweats announced the formation of matter. The fatal result also was unexpectedly sudden; for, though the operation was undertaken without any great expectation of benefit, it was not thought that he was actually in the act of death. It will be seen that the pus was not reached, and that it was situated, for the greater portion, in a remote part of the brain.

ST. MARY'S HOSPITAL.

TWO CASES OF VESICO-VAGINAL FISTULA.

Under the care of I. B. BROWN, Esq.

CASE I. Rachel K., aged 22, was admitted, under Mr. Brown's care, on October 15th. She was delivered of her first child on March 25th; the labour was allowed to continue three days without interference, after which she was delivered of a still-born male child. Her urine ran away from her immediately after the labour, and had continued to do so down to her admission. She was examined at the Leicester Infirmary a month after her parturition, and her impression is that some operation was performed, but whether cauterisation or of what nature she could not say. It did not produce any benefit. The fistulous orifice was about the size of a sixpenny piece, and was situated in the upper wall of the vagina, about half an inch in front of the os uteri. After the usual preliminary treatment, Mr. Brown operated on October 27th, after Bozemann's plan. The opening was brought together transversely, and six metallic sutures were used. An elastic male catheter was secured in the bladder, and removed on November 2nd. She was then allowed to pass her water voluntarily during the day, and the catheter worn only at night. The bowels were kept confined by opium till November 5th, when they were opened for the first time. On November 6th, the sutures and shield were removed, and the wound was found to be united firmly in the whole of its extent.

CASE II. Charlotte H., aged 27, was admitted on the same day as the last patient. She was a native of the same village, and had been under the same medical treatment. She was brought to bed of her third child in November 1856, and was three days and nights in labour. She was then delivered by craniotomy. Seven days afterwards, she first perceived her urine running away. No treatment was adopted. She again became pregnant, and was delivered in May of a living child. The labour lasted three days this time also. The symptoms have not been worse since the last confinement.

The fistula was of small size, situated close to the os uteri, and appearing to involve a small portion of it. The operation was performed also on October 27th. On paring the edges of the opening, it was seen that though the fistula had encroached upon the mucous membrane of the os, it had not penetrated into the cavity of the uterus. The wound was brought together longitudinally with four silver sutures. The same treatment was adopted in this as in the former case, and equally successfully. On removing the shield and sutures on November 6th, the wound was found firmly united by a linear cicatrix.

Mr. Brown observed, in reference to these cases, that he did not attach the same importance as Dr. Bozemann was inclined to do to the direction in which the wound is sewn together. That gentleman always contrives that the line of suture shall run transversely across the vagina; but from Mr. Brown's experience, it appears that longitudinal wounds are just as likely to unite favourably. Mr. Brown also observed that he had been led to discontinue the use of the flexible metallic catheter usually employed in these cases, and to use instead an elastic male catheter. The former he had found apt to excite catarrh of the bladder, which seemed less prone to follow upon the wearing of the elastic instrument. He also pointed out as a favourable sign of union, a healthy purulent discharge from the vagina (combined with an absence of escape of urine). This is so common in cases which are to do favourably, that it is now usually looked for by the nurses as a signal of success in these operations.

KING'S COLLEGE HOSPITAL.

SILVER SUTURES.

THE recent improvement in sutures imported from America, has been adopted more extensively at King's College than at any other of our London hospitals. Here they are used, at present, for every purpose, so that, as we are informed, silk is only employed accidentally when the wire is not at hand. They

are in much favour with all the staff of the hospital; and, as we are informed by the house-surgeon, Mr. Liddon, able to answer all purposes as well as silk, with the further very great advantage, that they produce hardly any appreciable irritation. We saw a remarkable proof of the contrast in this respect between the metallic and the vegetable suture, in the case of a boy with fissured palate, in which both kinds had been used. Several days (about six) had elapsed since the operation, in which two silver wires had been used and one piece of silk. The silver sutures were still in their place, and no trace of any action could be seen in their neighbourhood, while the silk suture had required removal on the fourth day, and some ulceration still existed in its track.

With respect to the method of passing and securing the silver wire in cases of fissured palate, Mr. Fergusson's plan is as follows. A long silk suture is passed through the right side of the fissure, and a shorter one through the other. To the end of the latter, the silver suture is attached. This string is then passed through the loop of the former, and then by withdrawing the double-thread, the silver wire is drawn into place. The two ends being twisted together, no further clamp is requisite. The only drawback yet discovered to the wire sutures (and it is a very trifling one), is that it is rather more painful to withdraw them. This is best obviated by cutting them very close to the bend before removing them. Other kinds of wire seem almost as useful as silver. A thin iron wire has recently come into use here, and appears to answer equally well. It is said not to rust or corrode.

Original Communications.

EXPERIENCE IN MEDICINE:

BEING THE INTRODUCTORY ADDRESS DELIVERED BEFORE
THE LIVERPOOL MEDICAL SOCIETY,
OCTOBER 7TH, 1858.

By THOMAS INMAN, M.D.

[Concluded from page 927.]

In all these instances, we recognise the fact that experience alone did not teach wisdom. Day after day infants were born; and day after day the doctor-midwife tried with his finger whether the presentation was natural, and the birth progressing favourably; yet none ever asked himself in what way did children really lie? The book of Nature was wide open, but none cared to read its pages. We can scarcely help comparing observers such as these to the Arabs of Egypt or the Neapolitans of the middle ages. For years the latter knew of certain walls which evidenced the existence of buildings beneath the soil—an engineer drove a tunnel near the spot, and bored through the proscenium of an ancient theatre—yet the site of Pompeii was unknown until other heads and other eyes came upon the scene. They found nothing but what was equally patent to others, but what those others had never sought. As Belzoni was to the Egyptians many a discoverer is to us at the present day. The denizens of the desert were familiar with vast numbers of ancient relics—they had used many a pillar for a landmark, and had gathered together under the architrave of many a massive temple to shelter themselves from the scorching heat of the noontide sun. Their lazy eyes would rest on the strange sculptures of the frieze, as the fragrant smoke from their pipes faded into the clear air, and those who were story-tellers doubtless wove strange tales about the animals portrayed on the walls—and there their experience ended. But by and by came a man from the western countries, whose sole aim seemed to be to disinter these stony masses from the encircling sand. To the Arabs he appeared a wild visionary, a treasure-seeker, or a lunatic—they were contented with things as they were, and had no desire for farther knowledge. With untiring energy, however, he set them to work, and procured for them and all the world besides an increase of knowledge of which the most apathetic were proud. So it has been in medicine—people have been contented with things as they have been accustomed to regard them; if any medical sphinx has had a head, few have cared to ascertain whether beneath that head there was a proportionate body; we have known *something* of this fane, of that pyramid, and been content with our knowledge, and every new explorer has been

regarded as a mere theorist or vain visionary until he has succeeded in proving that that fane enshrined a noble statue, or the temple contained some wonderful sarcophagus or other sculptures. Each one, then, who previously bore arms in defence of ignorance is anxious to abandon them—and to show that his sympathies are in favour of progress.

There are few real inventions or discoveries in medicine—our advance consists simply in a clearer reading of the facts open to all. As the Arabs might have discovered the same things as Belzoni did, had they had the mental power, the inclination, and the means, so might any one of us have made the same discoveries as Nagel, Jenner, and Simpson, and a host of others whose names are embalmed in our memories.

But if so called medical discoveries are simply clearer readings of facts, how comes it that all new discoveries are so much opposed when first they are announced? If the proverb "*Experientia docet*" is believed, why should a large and extended experience receive less favour than a small and contracted amount? The reason is partly due, as we have stated, to that indolence of mind which refuses to inquire into novelties, and partly to that self love which leads us to depreciate results which we have not helped to bring about.

As long as Belzoni was unknown, the Arabs formed the sole guides to the buried temples—they knew every stone, and could tell accurately the various levels of the drifted sand; but when the new man came and found chambers, vaults, and halls, they were no longer the exclusive, or even the best guides—their occupation was mostly gone, their power transferred to another. So it has been in medicine; for his contemporaries, to acknowledge that Harvey was right, was to allow that they were wrong. If Sydenham's method of treating small pox was correct, that of his fellows must be bad—to allow that he had discovered in his experience what they had failed to recognise in theirs, was to acknowledge at once that his powers of observation and thought were superior to their own. Human nature could not do this—medical magnanimity could not stretch so far; consequently the man must be assailed, and his experience derided as mere theory and closet teaching.

The following remarks from the paper in *Blackwood*, August 1858, from which we have already quoted, are very apt. Speaking of Harvey's discoveries, etc., it says—

"Seventeen centuries is a vast space of time for the elaboration of the discovery of a fact which, now we know it, seems so obvious that our marvel is, why it was ever unknown, and the moral of the story lies precisely there, teaching as it does the remarkable servility of the mind in the presence of established opinions and the difficulty which is felt even by eminent men in seeing plain facts: so hoodwinked are we by our preconceived notions. To those who are unfamiliar with the practical parts of science, it seems singular that men should continue acquiescent in errors so baseless; that they vanish immediately they are challenged; and to men who have never trained themselves in the difficult and delicate art of observation, it seems singular that facts extremely simple when observed should continue to be overlooked. But the truth is, observers are at all times rare, because new observation requires singular independence of mind; and unhappily those who never made an observation themselves are always ready to dispute the accuracy of new observations made by others."

Again: "Thousands daily see what they believe they ought to see. Vesalius became a great man, and the father of modern anatomy, because he had sufficient strength of mind to use his own eyes and say what he saw."

Again, speaking of the discovery of the lymphatics, the author says: "These discoveries following in such quick succession greatly disturbed the equanimity of the faculty, who swore by Galen, and could not tolerate the idea of the ancients being supposed to have overlooked anything: '*Un chacun invente à présent*,' was the indignant sarcasm of Riolan, the most renowned teacher of his day."

The same feeling exists amongst us all at the present day; and we cannot endure the thought that our favourite authors can have been wrong in their principles and practice, or could have left anything for their successors to discover.

Another point of considerable importance in calculating the value of the experiences accumulated by this or that man or class of men, is the estimation in which they hold certain symptoms during life, and certain pathological signs after death. Thus, the experience of Broussais has been materially injured by the modes in which he regarded certain symptoms, and his associating them with disease of the stomach as illustrated by certain pathological appearances. In his opinion certain signs indicated the presence of inflammation. On that

idea he built his observations; but when it was shown that this foundation was untenable, his experience lost nearly all its value: it was a grand mansion built on a quicksand.

Take another instance from the writings of Dr. Gooch. In his time certain symptoms were considered necessarily to mark definite lesions of an inflammatory type. And who can even now be surprised if headache, vertigo, delirium, convulsions, apoplexy, and mania, with increased vascularity of the brain, and sometimes fluid between its membranes, and in the ventricles, should be considered as positive proof of the existence of inflammation of the brain; and requiring for their cure the most energetic measures. As long as reporters confined themselves to a description of symptoms and the results of treatment, we might consider their experience trustworthy; but the whole would become almost valueless when we found, from farther experience, that the cause of the complaints was inanition, produced by an inadequate quantity of food. Take again, for an example, the false experience that has arisen from the estimation in which mania has been regarded as a symptom. In days gone by, mania was considered as a proof of cerebral inflammation, of excessive action, and exalted power; it was met by bleeding general and local, by tartar emetic, opium, and a variety of other remedies, intended to control action, diminish power, or soothe irritation. But what was the result? Let Dr. Conolly be heard when he says that such treatment was repeatedly the means of aggravating the disease, and converting a hopeful case into one of hopeless insanity. But it may be answered here, that experience after all induced practitioners to modify their treatment, and to change their tactics from an exhausting to a restorative plan. I grant the point at once, though, in doing so, I must mark the different ways in which, on this very subject, different practitioners will read the proverb "*Experientia docet*." One starts in life with the old ideas of mania; in every case he carries out the old doctrines, and occasionally meets with success. By and by a friend points out to him the antecedents of his patients; tells him that experience shows that mania is the result of privation, misery, starvation; that it is attended with many of the signs which attend starvation, fever, thick saliva, stinking emanations, rapid pulse, brilliancy of eye, and increased irritability of hearing, and other senses; that experience farther shows that his plan of treatment is most commonly prejudicial; and that the patient sinks and dies just as he appears to be getting well, and ends by urging a different course of practice. No, is the reply; I have *experience* in my present method of treatment; I have none in the one you propose; mine has been sanctioned by the experience of the past, and been endorsed by the highest authorities; your proposal is theoretical, and I will wait till it has been put to the test of trial. On the other hand, strong in his reading of the proverb "*Experientia docet*," the so-called theorist puts his views to the test, and meets with a result worthy of the soundness of his observations. Both profess to be led by experience, yet one remains content with repeated failures, the other goes on to almost habitual success. Even the great mind of the first Duke of Wellington was not proof against this failing, as it is well known that it was his fondness for Brown Bess, the well-tried musket, whose worth he had recognised in many a hard won strife, which prevented, during his life, the introduction into the army of the Enfield, or the breech-loading, or other rifle, or, indeed, any other arm which was under the ban of novelty.

Again it is to be remarked how much the value of past experience has been vitiated by the vague manner in which certain words have been used to indicate a condition supposed to be always of a definite kind; thus inflammation has been made to do duty for many different forms of disease, and each observer has had his own peculiar interpretation of the word. Thus, some call all those cases, *inflammation*, where there is pain and tenderness on pressure; others consider that the same disease is present when there is increased redness and swelling; others again consider increased secretion as in coryza, bronchitis, gonorrhœa, etc., as being evidence sufficient of the presence of inflammation; while others are contented with the presence of certain general symptoms, such as rigors, feverishness, etc. With ideas so vague respecting the disease, it was almost impossible to get any definite experience respecting its true nature and treatment. The experience of one man differed from that of another, and the student was bewildered in the search after the true inventor, whose teachings were to make him wise.

A still more dangerous impediment to the gaining of wisdom by experience consists in the plan adopted by some pathologists of not only including tendencies, or eventualities into definitions,

but even giving them the highest place. Thus, one set of pathologists give the name of cancer to certain new growths having certain present physical characters and past history; they found their experience on what they observe respecting such, and probably enunciate that the disease is curable in certain proportions. Another set, however, of observers declare that nothing can be considered as cancer unless it return again after operation, like many a wart does; spread to distant parts, like leprosy; or carry off the patient. However much experience may teach the former, it is tolerably clear that it will not teach the latter any lesson worth learning.

The same remark was once applicable to phthisis, which was considered to be absolutely incurable; (as it is yet even now by some); if any one ventured to announce the cure of a case, the experience of centuries was cast in his teeth, and he was told that his diagnosis had been incorrect, and his conclusion consequently faulty. Farther experience has satisfied us as much of the fallacy of the old dictum about phthisis as it ought to satisfy us about the idea of the curability of cancer.

Perhaps the most fertile source of false experience in medicine is an error into which we are all more or less apt to fall, and in which a vast section of medical professors are at the present moment securely reposing; viz., that of attributing success to the medicaments employed, or of supposing that the plan adopted is certainly the true one, because the patient has been clearly benefited by it immediately, and for a short time afterwards. Of the former we need say little, as the danger of the "*post hoc ergo propter hoc*" reasoning has been theoretically familiar to us from our first entrance into the profession. The latter is an error more dangerous than this, and not the less so because it is comparatively little known. It is an error which requires to be most carefully guarded against, and one which, for its detection, requires the closest attention in practice. I am not aware that it has ever been specifically brought under the notice of this society, but it was certainly introduced incidentally in Dr. Cameron's paper on inflammation. In that he alluded to the effects of bleeding in pleurisy, and recorded that, in the twenty-nine cases in which he had employed it, it had been followed by immediate relief in all, though in the majority that relief was not sustained. There can be no reasonable doubt that the immediate relief, commonly following venesection in pneumonia, was and still is the cause of the strong opinions entertained by so many of the propriety, and even necessity for this plan of treatment in this disease. The natural conclusion of practitioners is that experience proves the value of the remedy while it is theory alone which opposes it. But that experience is not to be trusted which is founded upon a superficial basis, and we therefore inquire whether the so called practical men *appeal sufficiently to experience*; we pursue the subject and find, as Dr. Cameron did, that, although bleeding gives immediate relief, it is subsequently followed by a progress far less satisfactory than if venesection had not been adopted! What now becomes of the boasted experience of the former, and the sneer at the theory of the closet physician? Let us turn our attention for one moment to the results flowing from such superficial experience. A few weeks ago some cases of pneumonia were related in one of our journals as occurring recently in Milan. (*Medico-Chirurgical Review*, July 1858, p. 11). One robust countryman had been cured of pneumonia, so runs the account, in January, by eighteen bleedings, but he was left *sickly for four months subsequently*; in the May following he was readmitted with a second attack; he was bled fifteen times in nine days, and took tartar emetic at the same time. He died on the 28th day, having been bled thirty-three times in all! This to be sure was a failure; but the practitioner had his success. One strong countryman went out perfectly well on the 26th day after being bled twelve times. A pale sempstress was bled nine times, lingered on in the hospital for fifty-six days, falling into a slow consumptive fever; she was then fortunately transferred to the hospital for chronic diseases, where she was plentifully fed and comforted with wine; she left at the end of a month with a slight cough, but with a fine colour, joyous health, and in good flesh. Another patient, a robust countryman, was bled nineteen times in twelve days, and took tartar emetic for ten days, when it was exchanged for kermes mineral. He survived; but when he left the hospital after sixty-nine days, he had oedema of the legs, diarrhoea, excessive pallor, and weakness! Now we cannot suppose that this Dr. Acerbi did not consider that his treatment was warranted by experience; he doubtless argued that as relief always followed bleeding, therefore, whenever relief was required, bleeding should be resorted to; and so faithfully adhered to this conclusion, that he slighted the

farther experience procured by the more important observation, that patients thus treated were killed frequently in the process of cure—they were bled to death that they might live and breathe comfortably!

It appears to me that there is no one single point in which the superiority of the present race of medical practitioners is shown more conspicuously than in their taking time into consideration as a test of successful practice. We are no longer content in curing this or that symptom, or knocking down this or that disease; we no longer place faith in medicaments because patients get well after using them: we require to know whether the cure is complete as regards the patient's general health, and whether it has been effected in the shortest possible period of time. Dare any of us boast of having cured a pneumonia by bleeding, if we laid the seeds of phthisis or of general dropsy? or could we with greater faust boast of the cure of a bronchitis after six months treatment?

It is not only in pneumonia that we find such instances of false experience as I have adverted to: we see it equally in apoplexy and allied disorders. Thus I could point to cases where an apoplectic patient has been cupped and bled with such immediate relief that "stertor" has left the breathing, and consciousness has been suddenly restored. Nor would I wish to say that these cases are not common. To them the practical man points with triumph, and lays down the authoritative law, that experience vindicates the free use of the lancet in apoplexy; and adds, that only theoretical visionaries would oppose it. He takes his stand on our text, "*Experientia docet*," and feels impregnable. By and by, however, another remarks that relapses occur very frequently after bleeding; that softening round the clot is not uncommon; that it is rare to find the patients making a rapid or complete recovery; and that death occasionally happens even while the blood is flowing. Experience also shows that many come round from apoplectic seizures without any treatment at all. And, with all this, the once-called visionary asks, *What does experience teach?* Who reads the volume of Nature the most correctly—the man who looks at the first page only, or the one who tries to look at all?

It is not to venesection alone that the foregoing remarks apply: they are equally applicable to the use of purgatives and other medicines. Experience, says the practical man, has long convinced me that colocynth is an aperient, and that steel is an astringent; and, as a natural result, he gives pills to overcome habitual constipation, and steel to control purgation. But is his experience reliable? Experience, while it tells him that colocynth purges, tells him also that, after the purging is over, the bowels are more constipated than they were before; it tells him that this condition of things is associated with a certain state of the system to which the name atonic has been given; it tells him that temporary stimulation is commonly followed by more prolonged depression; and that common-sense people prefer to leave their bowels to Nature, rather than take pills and make them worse: it tells him, too, that spurring a tired horse does not bring its rider faster to a distant goal than would a feed of corn or a draught of water. Experience tells all this clearly. But is it always listened to? By some it is; and, as a result, steel or other tonics are given for the cure of constipation. *Mere theory*, says the practical man! *Experientia docet*, is the unlooked-for answer. *Mutatis mutandis*, a similar line of reasoning has induced the enlightened practitioner, under certain circumstances, to give an aperient for the cure of diarrhoea.

The effect of false or insufficient experience is conspicuously seen in the use of stimulating aperients for the cure of flatulence and visceral obstruction. An individual comes to the practitioner, complaining of flatulent distension of the bowels. The remedy proposed is some brisk carminative purging medicine: it is taken; and the patient, to his great satisfaction, reports the discharge of a vast quantity of flatus by stool or otherwise. Seduced onwards by the apparent success, the remedy is continued; the daily reports are satisfactory; nevertheless, after the lapse of weeks, the distension is as great as ever. Lured on by false experience, the practitioner increases the strength of his medicaments, and for a day or two their effects are perceptibly good, but, after that, things are as bad as ever. Now, we may fairly ask, has experience taught that man truly? or, to put it more correctly, has he read the book of Nature right? Experience ought to have told him that the bowels are always flatulent in the dead; that they are flatulent in most cases of adynamic fevers; that the stomach is flatulent after prolonged fasting; that flatulence is common after severe confinements attended with loss of blood;

that it is common in mesenteric disease, in dyspepsia, and in hysteria; that it is increased by a slop diet; that it is worse at night than in the morning, and the like. In other words, experience teaches that flatulence indicates great want of vitality in the mucous membrane of the intestines; and it teaches that this cannot be cured by *exhausting* that organ still more by stimulating cathartics; that what is wanted is increased *power*, not increased action; and the former can only be obtained through the constitution generally. Experience leads one man to use aperients; another to eschew them, and give cod oil and other tonics.

Instances of a similar kind might readily be multiplied, to show how two individuals, each guided, as they suppose, by experience, may come to directly opposite conclusions, from the simple fact that one observer is contented with a *less amount of experience* than another. We prefer, however, to pass on to another important phase of our subject.

We have hitherto been contemplating the various scholars of experience, and how, in the same school, various readings are adopted of the same facts. Let us now inquire whether the teachings of Experience are always the same; whether she really does or does not give the *same lessons* to all her pupils. A question of more importance to the thoroughly enlightened physician it is almost impossible to put. It is a question which at the present moment is agitating the British medical world. The experience of our younger days, say Dr. Alison and Dr. Watson, is not the same as that of our mature years. True, replies Dr. Bennett; but it is only because you see things in a different light. Not so, say the former. Our memory carries us back to the time when diseases were characterised by an amount of power and vigour which we rarely meet with now; they resembled tropical diseases in their shortness and intensity, and our treatment had to be sharp and decisive; and certainly we may add, that it was successful in the widest sense of the term. We know, they add, that this type of disease has gradually changed; and with that change our practice has varied too. Our present plan of treatment, they urge, has been the result of actual change in the nature or type of disease, and not of change in our own theoretical views; and they farther add, that every physician, whose experience extends over many years, is well aware of the fact that stimulants are far more necessary in some epidemics than others of the same name, etc. The answer to this line of argument is ingenious, but apparently inconclusive. Without entering more closely upon the question, let us advert to a few considerations in favour of Drs. Alison and Watson's view. We know, as a general rule, that pneumonia is *now* of an asthenic type; but, every now and then, a single exceptional case is seen, where a large bleeding alone can give the patient a chance. We do not lay any stress upon this case, because it is an exceptional one; and we are right. But when we are told by the elders of the profession that what is now exceptional was in their day regular, and that what is now the rule was then the exception, surely we must conclude that Experience taught at one time a very different lesson to what she does now, and may do hereafter. Again, we may ask, does not experience teach very different lessons to the army surgeon, whose work is on the field of battle and amidst the strife of men, to what she teaches to the hospital surgeon, whose labours are exerted amidst more peaceful scenes; to the one, who only operates upon men whose constitutions are in first-rate condition; and to another, whose patients are in a far different state? Has the country doctor the same lesson presented to him as is put before his town brother? and does the experience of England suffice for the practitioner of India? and does the experience of one epidemic, say of scarlatina, make us perfectly prepared for the next?

The teaching of experience is equally uncertain respecting the action of medicines. At one time, the physician can cure the majority of his fever patients with quinine; at another, it seems to act as a poison. Dr. Addison, I think, cured, as he thought, twenty cases in succession of Asiatic cholera by sulphuric acid; but his next twenty all died. Dr. G. Johnson cured a large proportion by castor oil; a drug which in the hands of others proved positively harmful. Calomel affords us a still better illustration. Dr. Eyre, of Hull, introduced this as a cure and preventive for cholera. It was adopted in a great number of places; and, amongst others, in Liverpool, where I was enabled, by the careful reports of friends, to ascertain that its administration was generally attended with the happiest results. Yet at that same time, in a large London society, it was stated that not one single instance was known in the metropolis where even the smallest benefit had followed the

use of the drug. As the authorities on both sides were competent observers, what other conclusion can we come to than that experience taught those in London a different lesson to what she taught here? Liquor arsenicalis was originally introduced for the cure of ague; yet I have never found it do good; and ipecacuanha had once an equally good character in dysentery, a disease in which it is never used now. Take still farther the example of lime-juice, originally introduced by Dr. G. O. Rees some years ago for the cure of acute rheumatism, a disease for which he still prescribes it. In my own practice, I have only once seen prejudicial effects follow its administration, and only once have been disappointed in my anticipations of its influence upon the disease; nevertheless, other observers equally competent with myself have lost all faith in its curative powers. Are the opposing pupils of experience under these circumstances to contend against each other, under the idea that one alone can be right? or must we allow that the school-master which we all reverence is not uniformly consistent.

For my own part, I feel compelled to come to the conclusion that experience is not unvarying in its teachings; and, to speak tautologically, that the greater *experience* we have of *experience*, the less we can place implicit reliance upon it. We have thus attempted to show—

1. That individuals do not habitually set that value on extended experience that proverbial philosophy should induce them to do.
2. That to understand the full value of the experience of others, it is necessary to be habitually gaining it for oneself.
3. That independence of mind is as essential to the growth of personal experience, as undue deference to authorities is prejudicial to it.
4. That the difference between the practical man and the theoretical consists in the endeavour made by the latter, to have a more extended basis of experience than the former is contented with.
5. That false experience has hitherto been the bane of medical science.
6. That experience misleads us by being insufficiently extensive; by being based upon false premises; by signs being misinterpreted, and the like.
7. That we are liable to error if we consider the teaching of experience as invariable, and certain.
8. That there is reason to believe that there are circumstances modifying the nature of diseases and the action of remedies, with which we are at present unacquainted.

The natural result of all this must be, that no one can feel that he has ever attained the *ne plus ultra* in medicine. A learner at his entrance into life, the physician must continue so till death. Every step he takes must be weighed as cautiously as the elephant tries the strength of the plank which is placed as a bridge for him to pass over. Professors may teach, and observers may experiment with the most brilliant success, yet in the next generation their conclusions may be overthrown. But of one thing, we may feel certain, that where there are so many earnest minded individuals at work testing the true value of past theories, and cautiously framing new ones, medicine will ultimately attain as great a certainty as it is possible in the nature of things for her to do. Let us ever remember that the proverb is—*experience teaches*, not *experience has taught*; and we may then each of us add our *quota* towards that desirable consummation.

RUPTURE OF AN OVARIAN CYST: RECOVERY.

By G. GODDARD ROGERS, M.D., Medical Registrar to
St. George's Hospital.

IN the JOURNAL of October 23rd, Dr. Charles Farrar calls attention to a case of accidental rupture of an ovarian cyst. I am able to comply with his request, and to furnish two accounts of similar cases. One, which is detailed below, happened in private practice some years since; the other will be found in the section of the JOURNAL devoted to hospital records, and occurred recently in St. George's Hospital.

The patient, Miss K., was for some time under the care of my late grandfather, but had been absent from his neighbourhood for a long period, when the accidental cure of her complaint was brought about. Amongst various letters and papers, I find a communication from the medical gentleman then in attendance, relating the circumstances to my grandfather.