slightest consideration of the nature of irritability informs us how frequently its amount must change. Where muscles are connected with the nervous centres, and used from time to time,—used differently, not only as kinds of exercise and occupation, but also as the passions and feelings vary, it may be assumed most fairly, that their irritability both very often and largely alters. Even in muscles perfectly and constantly quiet by reason of paralysis, I have observed a change in their amount of irritability as tested by galvanism, which I was unable to explain, and have succeeded in exciting reflex actions at one time, when I entirely failed to provoke them at another. Did observations on muscular irritability concern the condition of the fibre only which makes it manifest, they would possess but a partial attraction; but there seems reason to conclude that the fibre is strictly dependent on the nerve, and that the property in question is not simply muscular, but both nervous and muscular. Whether the fibre act on direct irritation, or mediately through the irritation of a nervous trunk, it seems reasonable to infer that the principle in action is the same. As the motor nerve is supplied with its attribute by the cord, so the muscular fibre derives from the nerve its contractile power. Has it ever been conclusively demonstrated that the muscle can, under any circumstances, contract altogether independently of the nerve?

There is but one *vis nervosa*. Not only the direct actions of Haller and the reflex actions of Dr. Hall, as the latter physiologist has taken pains to show, but the contractions of volition, emotion, and the phenomena of irritability immediately flow from and exemplify it; and all exhaust the fibre they stimulate in proportion to the intensity of their action.

And whatever be the mode by which it becomes fixed, its vigour is renewed by rest. Why a paralyzed and wasted muscle should at times be found to acknowledge a degree of galvanism, whereto that which is healthy and well-nourished makes no visible response, may be reasonably explained by that state of perpetual or comparative repose which the former, as it may be completely or partially affected, of necessity observes.

The measure of irritability might probably be determined in different pathological states, and also in the same pathological state at different times, with a precision that might reward the inquirer. Is it not to be supposed that the muscles of the epileptic would prove less susceptible of the galvanic force immediately after the paroxysm than just before it? Is it to be thought that the long restlessness which attends many diseases, the extreme exhaustion which waits on others, the depressing influences of violent emotions, the different conditions both of the quantity and quality of the circulating fluid, produce no changes in the irritability of the muscles capable of being measured and compared? It is not to be imagined that galvanism can instruct us as a test of irritability in paralysis alone; but rather to be thought that it has the power of apprising us of the condition of that property, under various circumstances, in which the investigator should be informed of its degree.

Not to pursue the subject of irritability because all the uses of the wide inquiry are not clear at present, is to exhibit a cold indifference to investigation, from which nothing can be hoped. "Applications come late," as that illustrious philosopher, Sir John Herschel, significantly remarks. But they do come in the end; all truth is fruitful. There is yet need of a collection of carefully ascertained facts in reference to the many branches of a question, whereof we have come only to the threshold. The accumulated experience of many observers would be of the greatest moment. Nor let any one desist either from this or any other inquiry, because he has not a name, because he fancies that his observations will have no weight. "We are, I say, to strive after personal experience, not to rely on the experience of others; without which, indeed, no one can properly become a student of any branch of natural philosophy."*

P.S.—In reference to the opinion of Professor Müller, which I have quoted in the preceding observations, concerning the action of small and distinct portions of muscles, I would draw attention to the following remarks of John Hunter, who appears to have come to a similar conclusion. "A muscle that has but one determined use, or rather, a muscle that has but one point of origin, and one point of insertion, always acts wholly when put into action, so as to bring about its effect on the part of insertion—I believe never one part of the muscle

only, or only a few of its fibres, but the whole. . . . . But in spasm in a muscle a very few fibres may be seen to act;—in short, any number may act, as it is not the motion of the part that is the intent of the action."—Hunter's Works, Palmer's edition, vol. iv. p. 263.

INSTANCES OF PHARYNGEAL POUCHES.

Read before the Medical Society of London, May, 1848.

By WALTER C. DENDY, Esq., M.R.C.S., &c., London,
LATE PRESIDENT OF THE SOCIETY, ETC.

I AM induced to relate briefly the two following cases,—the first now under my attention; the second, that of a right reverend prelate now deceased,—as I believe discussion on their nature may tend to elucidate the condition of pharyngeal obstruction.

Mr. G. S.—, (afflicted from his youth with frequent attacks of spasmodic asthma,) about eight years ago, for some affection about the tonsils or uvula, placed a peppercorn in his mouth, which suddenly slipped backwards, and lodged in the throat. It remained in its bed for more than a week, causing irritation and constant cough, by which effort it was eventually dislodged. Ever since this he was aware of the temporary lodgment of foreign bodies in the same position; still he regarded his case with no anxiety. Gradually, however, these lodgments seemed to increase, and larger bodies became arrested in their course; the secretion of saliva also was very profuse and distressing. After the lapse of a year, wine and other stimulants so irritated the fauces, and especially the uvula, as to produce severe hysteric cough, attended by a rush of blood to the head, and vertigo, which occasionally caused the patient to fall. The respiration also had become latterly much more difficult.

About two years ago the uvula was excised, by which the complaint was much aggravated. It is clear that there is a pouch formed in the pharynx.

At the present time this gentleman is compelled to fill this pouch with soft solids to the extent of two ounces, ere he can cause any food to pass into the stomach. He can at any time eject the contents of the bag, whether accumulated saliva or food, without any effort, by merely bending the body forward. The malady interferes with the function of the larynx, as if he attempts to read, a profusion of saliva flows over the fauces, and a severe cough is directly induced.

The complaint under which the bishop laboured came on imperceptibly, and could not be satisfactorily traced to any especial cause. Although it continued gradually to increase, and required constant filling ere food could be swallowed, it created little annoyance for several years. Eventually it became more troublesome, the contents being sometimes spasmodically ejected from the pouch, so as to prevent the passage of food into the stomach. Bread, and wine and water, seemed to be retained the most easily.

In this case the breathing was never materially affected; yet in both cases the respiratory system was occasionally deranged. The former patient laboured under severe chronic asthma; the prelate had often suffered under relaxed sore-throat, as it was termed, and eventually he died of pulmonary disease.

It may be added, that at one period of his life an abscess burst in the abdomen, connected with the gall-bladder, from which two large biliary calculi were expelled one morning, as the bishop was sitting quietly at breakfast.

These pulmonary conditions may perhaps have constituted the predisposition, rendering the pharyngeal bag, by the repeated effort, relaxed, and prone to distention, or puckering; and we can easily understand how this condition, constantly recurring and progressing, may produce a pendulous pouch.

In the first case to which I have alluded, there was an origin, palpable and recognised even by the patient—the lodgment of the peppercorn in the throat.

In the "Medical Observations and Inquiries" of 1769, a case is recorded, in a letter from Mr. Ludlow, of Bristol, to Dr. William Hunter, precisely analogous in its character to that of the bishop; its exciting cause, however, resembling that in the case of my present patient. The stone of a large whiteheart cherry having lodged in the throat, deglutition was almost entirely suspended for three days, and during that period the patient was subject to incessant cough, which, in the end, dislodged the stone. After the lapse of a year, small portions of food returned constantly by a sort of ruminating effort. The bag progressively increased in capacity, so as to contain, at length, several ounces of fluid; in the end, no food whatever passed into the stomach, but was returned within a short period of its lodgment within the pouch.

* See Harvey's most admirable and eloquent "Introduction" to his observations on "Animal Generation." Works of Harvey, edition of the Sydenham Society, p. 157.

As to the nosanatomy of these curious cases:—The pouch, I believe, usually dips down between the œsophagus and the vertebræ, occupying, in fact, the place of the œsophagus, which is pressed against the trachea, and suffers attenuation of its coats from this compression. It is also strained or puckered by various degrees of traction, constantly increased, of course, by gravitation and the weight of the ingesta. This at length forms a valvular communication between the pouch and the œsophageal canal, immediately below the cricoid cartilage, in front and opposite the fifth or sixth cervical vertebræ behind,—thus not only impeding or entirely obstructing deglutition, but even preventing the introduction of a bougie or hollow tube, through which aliment might be admitted into the stomach, unless the relative position of the cavities be distinctly understood.

Now, what are the especial tissues which constitute the pouch?

May we not place the cases to which I have referred in the same category with those diverticula or pouchings, not only of the intestines, but of those other tubes which are frequently submitted to the influence of contraction and dilatation.

The lodgment of foreign or indigestible bodies will sometimes pouch the stomach itself. Amongst other persons, Dr. Baillie saw a capacious pouch thus and therein formed, which contained five halfpennies.

The cells of the colon are often immensely distended by scybala, especially in young women, although apparent regularity of defecation may have averted the slightest suspicion of such a state.

Although the etiology may differ, we may adduce, as analogies, those dilatations found above the contractions of tubes, whether by spasm or thickening of coats. We may refer to the bulging abscess and infiltration behind urethral strictures; to the bagging of the bowel, when its calibre is diminished or contracted; and I may express my belief, that many cases which have been considered to be merely stricture, are indeed closely allied to those with the recital of which I commenced my observations.

And does the bag consist, or not, of the entire pharyngeal tissues?

I believe that it may consist of thickened mucous membrane only, and that continued and progressive pressure (as in the lodgment of a body in the sacculus formed by the rather sudden contraction of the pharynx, into the œsophageal tube, increased by the effort of severe coughing, especially when combined with pulmonary disorder, as in the case of my present patient and the bishop) may have actually split or separated the muscular fibres of the canal, thus constituting a hernia of the mucous membrane analogous to the formation of true aneurism from muscular effort, or, perhaps, more nearly allied to those cases of double bladder, one of which is recorded in the last volume of the *Transactions* of the Medical Society. The coats of gastric pouches are usually much attenuated, and that not from inflammation or ulceration, but from mere mechanical pressure.

Now, on the assumption that many of these interesting cases may have been mistaken for œsophageal stricture,—a simple contraction of canal,—we may confidently point to one probable cause of difficulty or failure of the introduction of the bougie, in consequence of the *valvular orifice* (for such, indeed, it may be termed) of the œsophagus, and the constant slipping of the bougie into an artificial cul de sac. A point in practice may hence be deduced. In cases of difficulty, it would be well to place the patient nearly on his back, so that the pouch may rest on the spine, and then to press the bougie forwards on the trachea, at the same time uplifting the trachea itself. If the existence of a pouch be determined, the same plan should be adopted, and this immediately after the emptying of the pouch.

Regarding the permanent relief or obliteration of these bags, I cannot yet speak practically. I contemplate, however, the employment of a canula and sponge, slightly wet with a very strong solution of nitrate of silver, if the symptoms of my present case should, by their aggravation, warrant this proceeding. At the same time, I should impart nutrition as much as possible, or altogether, through the elastic tube introduced into the stomach, so as to close the mouth of the pouch, and put it, as it were, out of office.

I believe that relief might be ensured on one or other of these principles,—either the shrinking or contraction of the bag, by its being kept empty and compressed, or by the adhesion of its sides, so as to produce complete obliteration; for although the mucous membrane is not so prone to adhesion as the serous tissue, we have very sufficient analogies to warrant that it might ensue in such a case.

Storey's-gate, St. James's-park, June, 1848.

Analyses OF

COMMUNICATIONS IN MS. RECEIVED FOR PUBLICATION IN THE LANCET.

EMPLOYMENT OF CHLOROFORM IN MIDWIFERY.

Strictures on the Remarks of Dr. Barnes relative to the Employment of Chloroform in Midwifery Practice—Employment of Ether and Chloroform in conjunction.

By W. D. EMMETT, Esq., M.R.C.S.E., Darlington.

DR. BARNES'S communication to THE LANCET of April 22nd, bears the impress of preconceived judgment against the use of chloroform, and I think Dr. Barnes will, on reperusing his second paragraph, wish it had been somewhat qualified. Dr. Barnes ought not to require being told that there are many remedies which are Protean in their effects. I might refer to opium, but will name a more common one, brandy; a small dose will stimulate, a larger cause recklessness; increase the dose, and coma will supervene. It will cause sickness, and it will allay sickness; it will increase the pulsations of the heart, and it will diminish their frequency; it will heighten the mental faculties, and it will destroy them; it will cause laughter, and move to tears; and finally, it will give strength to the body, and it will make it a powerless and insensate log. Lo! here is a mighty caduceus!

I do not believe the many newspaper reports of untoward results after using chloroform would prove correct, if fairly examined; neither do I think the assertion of Dr. Barnes to be correct, when he charges with cowardice the majority of our profession, and not cowardice only, but wilful and culpable neglect. That different results follow after varied doses, or even after the same doses on different people, is by no means surprising; but it would be a rare puzzle for physiology if it were not so. I will again quote Dr. Barnes: "We have evidence that chloroform arrests uterine action, that it increases uterine action; we have evidence that it causes relaxation of the os uteri and perinæum, and that the parts continue unaffected by its use; we have evidence that it produces alarming syncope, congestion, and convulsions, and yet that it cures convulsions and cerebral congestion. Such conflicting testimony is extremely embarrassing." I assert that such testimony is not conflicting, and that it is extremely valuable. It does not compel us to suspend our judgment as to what are the real effects of chloroform in labour, for they are summed up, and well too, by Dr. Barnes himself. Chloroform increases expulsive pains, and it diminishes them, and this the practitioner must bear carefully in mind, for according to the effect required, so must the dose be augmented or reduced. I imagine that most observers will have arrived at the conclusion, that it is by no means always requisite or prudent to employ chloroform; but I doubt not, it, or some other anæsthetic agent, will be generally admitted, after the tide of prejudices which have set against it has turned, to be a most valuable adjunct to conducting with safety our patients through the stages of labour.

I would earnestly wish all parties, both in favour of and against chloroform, (and the remark will bear on many other points,) not to allow themselves to confound the *post hoc* with the *propter hoc*. I would here, for example, refer to ergot of rye, which many obstetricians condemn on account of its fancied injurious effect upon the foetus. This imputation I do not believe to be based on solid grounds. I would simply ask in what cases do practitioners generally expect to find the child still-born? I suppose, after severe and prolonged labour; and is it not in such cases that secale is generally administered? Instead of having an injurious effect upon the child, I firmly believe there are many children saved by it who would have been irretrievably lost if the patient were allowed to linger on hour after hour, until, perhaps, the forceps terminate the case—a proceeding I do not particularly admire. I am confident that chloroform properly administered will not increase the frequency of forceps operations. If Mr. Wilton used the forceps three times, it proves nothing against chloroform. There have been at all times parties who have a *penchant* for anything of that sort, and who have acquired great manual dexterity by so frequently assisting nature; not that I would on any account impute an over-fondness to operative procedure on the part of Mr. Wilton.

Since the above was in type, Mr. Emmett has forwarded us the following note:—

"In THE LANCET of June 3rd, I have read the account of Mr. F. H. Harris, concerning the administration of chloroform in