matory action which speedily ensued, and which, though actively treated, terminated in vegetations.

On the other hand, the cauliflower excrescence of Dr. Clarke is seldom accompanied by pain; indeed, I may say, never by pain so severe as that which precedes the formation of vivaces. The former is a still more insidious disease than the latter: and hence Dr. Clarke, in his original paper, lately republished by the Sydenham Society,* informs us that he had never found it less in size than a blackbird's egg.

Another remarkable point of distinction seems to flow from the preceding. It regards the nature of the discharge. That from cauliflower excrescence is generally limpid and watery, hardly soiling the linen, and becomes only occasionally bloody, as at the menstrual periods, which are not often disturbed at the first, or after some violent bodily exertion, capable of breaking the delicate structure of the growth. Whereas, in cases of vivaces, the discharge is either bloody or purulent, and ere long it becomes very fetid; evidently proving the much greater amount of vascular action going on in the latter than in the former instance. The watery discharge from the cauliflower excrescence seems to be a mere mechanical transudation of serous fluid mixed with the increased mucous secretion of the irritated vagina, and sometimes, also, with the cast off cells of the growth itself. In all such cases, too, when hæmorrhage occurs, small brainy masses may be found in the discharge, identical in structure with the cauliflower excrescence. Such hæmorrhage, therefore, is likewise mechanical, flowing from ruptured substance of the growth. But it is far otherwise with vivaces. They often die and come away as putrid matter, but never seem to break from delicacy of structure; and the hæmorrhage in these cases must proceed either from the progress of the original ulcer, or from the smaller vegetations from its surface, which still retain their vascular nature. The longer and more bulky growths are to all appearance *evascular*, being white and spongy, and filled with cheesy substance like concrete pus. For, as I remarked above, the ordinary discharge, in cases of vivaces, is always purulent when it is not bloody; this, of course, arises from the progress of the ulceration; which is continually sustained and prevented from healing by the mass of its own unhealthy vegetations.

This leads me to mention, as a fourth distinction between these two kinds of uterine growth, that vivaces do not shrink in bulk after death, as happens in such a remarkable manner with the cauliflower excrescence from the os uteri. But I regret that, for reasons formerly mentioned, I cannot speak with precision on the minute internal structure of vivaces. If, however, I am warranted in concluding from the history of such cases, especially those points in it to which I have just adverted, that vivaces are nothing more than exaggerated vegetations from an ulcer of the womb, matted together by inflammatory effusions, and perpetuating the morbid lesion from which they originated, then nothing can be more different than their internal structure compared with that of true cauliflower excrescence. For, as I have elsewhere more fully explained (*Edinburgh Monthly Journal of Medicine*, for Nov. 1849), the latter growth consists of a congeries of simple and compound cells, some of which are expanded into delicate bags containing blood-corpuscles. These bags exist chiefly on the margin of the excrescence; and when they burst, they leave the fibrils or hair-like processes, which mark the latest period of its history. It seems to be the peculiar property of these cells to withdraw serum from the blood in the uterine vessels, and to permit its exudation through their walls, thus constituting the greater part of the discharge. But of course this ceases at death; and then the cells collapse, or in other words the excrescence shrinks away. But no such function is performed by vivaces, and no such shrinking occurs in them after death; and therefore I do not think the inference unfair, that no such mechanism exists in them as in the cauliflower excrescence.

* Essay on Diseases peculiar to Women.

I have not hitherto introduced the question, whether or not vivaces are malignant in their nature. The opinion of Levret as to their being incurable, and some of the cases recorded by Herbiniaux and others, would seem to indicate that such really was the case. But I am inclined to think that vivaces may or may not be malignant, according to the character of the ulcer from which they spring. This opinion is founded on that which has already been stated and proved, with such data as I possess, regarding the pathological significance of the vivaces themselves: and I think it unnecessary to enlarge upon this point at any greater length. Suffice it to remark, that on this important feature of the case will depend the prognosis and the treatment.

I do not assert that all cases of malignant disease are incurable, when I say that malignant ulcer of the body of the womb belongs to that category. Its advance is generally considerable before it is clearly diagnosed, and even then it is out of the reach of efficient means of eradication. And when the vivaces have sprung from a non-malignant ulceration, it may still be impossible to root out the morbid parts before the patient's condition has become a hopeless one. In some cases, however, it may be attempted by ligaturing the long vegetations—making the cord cut as well as strangulate the growth,—and then cautiously but effectually applying a caustic substance. I should myself prefer the common caustic, because its action, as well as the bleeding, might be conveniently arrested by injections of vinegar and water. As soon as possible thereafter, alteratives and tonics should be given to change and improve the action of the internal surface of the womb. But I feel that I cannot, with propriety, even attempt to frame directions for the treatment of vivaces; and I therefore leave it to others to draw the practical inferences deducible from the preceding pathological data regarding that very rare but most interesting affection of the uterus.

Glasgow, June 1853.

METHODS OF EXPRESSING THE PHENOMENA OF AUSCULTATION.

By W. O. MARKHAM, M.D., Assistant Physician to St. Mary's Hospital.

Nothing more fully demonstrates, if any demonstration of the fact were needed, the incompetency and faultiness of our present methods of expressing the phenomena of auscultation, than the circumstance, that each successive writer upon the subject finds himself compelled, as he proceeds in his task, either to criticize, or alter, or entirely renovate the existing nomenclature as it falls into his hands. The particular terms of it fail to express, or express but feebly, his idea, or perhaps pervert it altogether; or they are made to indicate something very different from their essential meaning. The structure is thus, in his sense, weak, and he endeavours, by patching, to brace it up for his occasions. How comes it that he fails in being more successful than his predecessors?

If our view of the case be correct, there is a radical defect at the very bottom of our present system, inherent in it, and of necessity tainting every part; so that each addition and alteration, which is made in the direction of the original idea which presided at its establishment, is but a conversion or an engrafting of one defect into another. When a superstructure bears marks of weakness, is it unreasonable to suppose that its basis must be at fault? In the matter of auscultation, I believe that the truth of this may be readily shown. On this point, however, I do not wish to linger here, as I intend soon to bring the subject somewhat more fully before the profession. I would, however, venture briefly to suggest as a remedy for the manifest evil referred to, the adoption of a nomenclature founded on the rational system of observing auscultatory signs, which has been promulgated by the modern German school.

I need hardly observe, that our present method of no-

nomenclature is essentially French; and like the French, it is founded on the supposition, that to each auscultatory sign a distinct pathognomic value should be affixed. The modern German method abhors this idea; it asserts, (and I believe it would be hard to prove the negative of the assertion,) that there are very few, if any, auscultatory signs which are pathognomic of any one particular pathological condition of the internal parts, and of that alone. It is contented to record the sign as it presents itself to the ear, in the plainest and simplest terms. It calls fine bubblings such, not unwarrantably premising them to be the crepitating *râles* of pneumonia, and thereby prejudging and forcing the observer's mind towards, it may be, a faulty diagnosis; the real value of those fine bubblings it seeks elsewhere, extrinsically to the sign itself. Auscultatory signs, after this manner of viewing them, become in the physician's mind the resultants of certain physical causes, of reciprocal actions between fluids and solids, and as such to be calculated and measured according to the ordinary laws of sound, not converted into sure and positive exponents of particular morbid states. The German method takes us as far as physical facts warrant us in advancing; the French leads, and has led us, to a pretence of knowledge far beyond what facts justify us in assuming. The one keeps auscultation in its place, using it as a handmaid to diagnosis; the other makes it a master-key, of itself sufficient to unravel the knowledge of disease.

I am far from assuming that the nomenclature which I here present, in accordance with these principles, is not open to many objections; but of this I feel assured, that the adoption of some similar method will alone enable us to find a way to escape from those errors, which in my belief must of necessity attach themselves to our present system.

PROPOSED METHOD OF AUSCULTATORY NOMENCLATURE.

1. *Respiratory Murmur.*

- a. Vesicular. { Feeble, loud, smooth, coarse, distinct,
interrupted, etc.
- b. Bronchial. { Feeble, loud, smooth, coarse, distinct,
interrupted, etc.
- c. Respiratory murmur, accompanied by metallic echo or amphoric resonance.
- d. Indeterminate respiratory murmur.

2. *Vocal Resonance.*

- a. Indistinct humming.
- b. Thoracic voice. { Weak.
Loud.
- c. Bleating voice.
- d. Voice accompanied by metallic echo and amphoric resonance.

3. *Dry and Moist Sounds heard during the movements of the Respiratory Organs.*

- a. Dry Sounds. { Hissing.
Whistling.
Sonorous.
Crackling.
Clicking.
Friction.
- b. Moist Sounds. { Fine. } Scant; abundant; ac-
Small. } companied by me-
Bubbles. } tallic echo and am-
Large. } phoric resonance.
Gurgling.
- c. Indeterminate dry and moist sounds.

I shall not entrench on the space of the ASSOCIATION JOURNAL by entering into a full explanation and defence of the above terms. To a certain extent they speak for themselves. I know very well that such a nomenclature will find no favour with the admirers of "*ampullæ et æscquipedalia verba*"; and that it may be advantageously modified there can be little doubt; but I maintain that the principle on which it is founded is good, and must sooner or later be generally admitted.

The original communication of Dr. Theophilus Thompson,

which lately appeared in the ASSOCIATION JOURNAL, is the cause of my making these observations. It is satisfactory to find that the modern German plan of viewing the phenomena of auscultation is already working its way amongst us, though I cannot congratulate Dr. T. Thompson on his having arrived at any successful application of it; for, by his proposition, much leaven of that faultiness, for which it is substituted as a remedy, is still retained. I am, however, pleased to find an authority like Dr. T. Thompson enforcing the necessity of our coming to some clearer comprehension of this matter; and I am gratified that he should have used as his reasonings for the necessity thereof, the ideas collected together by myself in the preface to a translation of Skoda's work on *Auscultation*, etc. I feel rather anxious to place this last remark upon record, as otherwise it might hereafter seem, and especially to the readers of the proposed paper to which I have alluded, that I had copiously borrowed my reasonings from Dr. T. Thompson.

(Larges Street, Piccadilly, June 3rd, 1853.

OBJECTIONS TO THE USE OF CHLOROFORM.

By SAMUEL BEECROFT, Esq.

SINCE the use of chloroform has become so general, and so much is written in our periodicals in its praise, and so little against its use, I hope that I shall escape the charge of presumption in venturing on the following remarks.

I would first state my doubts of its use being beneficial in any case, when we know that its effect in lulling pain is produced at the expense of the blood being poisoned by the introduction of an excess of carbon; under which state there is great disposition to the production of local congestion. I appeal to common sense, if a patient in this condition and undergoing an operation is so likely to do well, as one in whom it has been the care of the surgeon to keep the blood as healthy as possible.

For my part, I feel sure that convalescence cannot be so certain when chloroform is used, as when the blood is free from its poisonous agency. But some will say, that we have evidence to the contrary in the fact of patients constantly making rapid recoveries when this anæsthetic agent has been used, and I cannot deny that such recoveries do take place. But, we know that a great many would recover if they were rendered insensible with alcohol; but we should not like to risk operating under these circumstances; and we find a great analogy in the action of spirituous drinks and chloroform. Death is produced by large and rapidly administered doses of each; and we find in both instances, after death, the same congestion of vital organs.

I have frequently used chloroform, and I confess that I have never seen it the means of accelerating the convalescence, as is stated by some. How can rendering the blood impure cause a rapid recovery? The supposition is absurd. I have, however, often seen headache, sickness, etc., produced by it for some time after its use; and I have the impression that in some cases I have read, where death has occurred a few days after operating, it might in some measure have been owing to the chloroform.

The last two cases in which I used it were, hydrophobia, and dislocation of the humerus into the axilla of seven weeks standing. In the case of hydrophobia, the patient was certainly quieted by it, but he died in less than twenty-four hours after the first active symptoms of the disease set in; and in all the cases I have read, where it has been used, this disease appears to have run its course very rapidly. In the case of dislocation, although we put the patient in great jeopardy, by the chloroform causing convulsions followed by stupor, under which the muscular system was completely powerless, we yet failed in the reduction; and if I were again to fail after bleeding, etc., in reducing a dislocation, I should have no hope of succeeding with chloroform. I give these two examples, as this agent has been so strongly recommended in the treatment of similar cases.

Having attended a good share of midwifery, and having