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RETROSPECT.

In the retrospect to which the commencement of a new volume of such a periodical as the REVIEW, naturally invites us, several important considerations present themselves. Going beyond the more immediate past and looking back to the earlier years of Hahnemann's life, we notice that for a long period after the idea of the homœopathic law and method became clear in his mind, he made frequent and very earnest efforts to commend this idea to the acceptance of his professional colleagues. Every form of argument and of demonstration was employed by him to persuade Hufeland and the profession generally, to investigate and to accept the homœopathic law of cure.

It was not until many years had been occupied with fruitless labors of this sort, not until, in response to these efforts, he had been ridiculed and branded as an infamous impostor, that Hahnemann separated himself from the body of the medical profession, and defiantly flung to the breeze the banners of his new school of medicine.

The earnestness and long continuance of his efforts to reconcile fidelity to his convictions of truth and harmony with his professional brethren, may serve to show us how

highly he prized this harmony. On the other hand, the unflinching determination with which to the end of his long life he accepted ostracism and contumely and personal suffering and poverty, when recreancy to his convictions had become the only alternative, should make us bow with reverence before a devotion to Truth and Duty which has had no parallel among physicians.

We may, at this day, with profit, ask ourselves whether we estimate as highly and emulate as earnestly as we should do, this latter trait in Hahnemann's character.

The majority of Hahnemann's immediate pupils stood by him faithfully in his entire separation from the dominant school of medicine. Some, however, who adhered to the new system, deprecated Hahnemann's apparent hostility to the old school, and sought by every means to bridge over the chasm which separated the Homœopathist from medical men at large. That this was a very praiseworthy endeavor is not to be denied; for not less in professional than in social life is it a delightful thing for "brethren to dwell together in unity," but it should not be forgotten that the dwelling together is possible only where "unity" is possible—where this is unattainable there should be a wide and acknowledged separation.

The attempt to smooth over the radical and inevitable differences between Homœopathists and Allopathists led, at a very early period, to the ignoring of certain fundamental principles involved in the science of Homœopathy. This evil steadily increased, until, fifteen years ago, the practice of the great majority of Homœopathists bore very little resemblance indeed to that of Hahnemann and his stricter pupils, and their success was proportionably less; but little, if at all, greater in many respects than that of the best allopathic practitioners.

Hufeland declared that Homœopathy, if it should prevail, would "be the grave of medical science." This was the keynote of the objections made by Hahnemann's opponents.—And these objections had so great influence over many of his followers that they endeavored, in every way, to accept the

theories and philosophy of the old school while adhering to Hahnemann's practical method. The true and manly and safer course would have been to claim as Hahnemann did that, experience having shown the homœopathic method to be true, a philosophy of medical science with which Homœopathy is incompatible, must be, *ipso facto*, a false philosophy. We have gained nothing by our endeavors to conciliate the old school and to accommodate our doctrine to theirs. On the contrary, we have lost both the respect of the more enlightened Allopathists and what is infinitely more important, a clear understanding of our own position.

Hahnemann declared the pathology of his day to be an unsafe basis of medical treatment, and proved that Therapeutics could *never* be based on Pathology; for the reason that Pathology is a science of Hypothesis respecting the nature and processes of morbid action and must always be speculative and uncertain. Homœopathists were at once charged with ignoring or neglecting Pathology, and many of their numbers have been so intimidated by this hue and cry as to resort to very painful, almost ludicrous exhibitions of a devotion to pathological science as a source of indications for treatment, which would be absolutely incompatible with true homœopathic practice. For, a Therapeutics based on pathological indications must, of necessity, be a system of broad generalizations, while to the true homœopathic practice the strictest individualization is an indispensable condition.

Hahnemann gave us a *Materia Medica Pura*, in which the slightest effects of drugs, not merely those which could be observed by a looker-on, but also modifications of sensation, thought and emotion, perceptible only to the prover, were carefully recorded in such a way as to make the effects of each drug most clearly distinguishable from those of every other. A cry went up at once from the ranks of the old school, against the puerility of these alleged provings, and the absurdity of prescribing for serious diseases on the strength of such "trivial" symptoms. It was affirmed that subjective symptoms of which the majority of each proving

consists, are almost valueless to the prescriber as an indication for treatment. A great many Homœopathists were deeply moved by these allegations and proceeded in various ways to expurgate the *Materia Medica*, striking out the subjective symptoms and seeking the characteristics only in the few objective symptoms which the provings contain. The injurious influence thus exerted on the practice of Homœopathy throughout the world has been almost inexpressibly great!

Hahnemann taught the efficacy of small doses. He showed that when drugs are prescribed according to the homœopathic law, it is indispensably necessary that the doses be small, and that *infinitesimal* doses are more efficacious than large ones.

Nothing has brought more opprobrium upon Hahnemann from the Allopathists, nor more ridicule upon his followers than this question of the dose. And many Homœopathists yielding to this clamor and shrinking from this ridicule, make a merit of disclaiming any fellowship with Hahnemann on this point and loudly proclaim their willingness, in the matter of large doses to "go as far as he that goes the farthest."

We have thus the spectacle of a large body of professed Homœopathists denying their master in the three fundamental points of his system—the indication, the remedy and the dose! And all this, as much through lack of moral courage to brave the obloquy which attaches to the strict Hahnemannian, as from honest difference of opinion.

All profess allegiance to the homœopathic law, "*Similia similibus curantur*," but the party of which we speak denies every one of the conclusions above alluded to, and to which this law conducted Hahnemann. An inevitable consequence has been a mournful deterioration of homœopathic practice, until now, save in the practice of the strict Hahnemannians, there is rarely seen an example of those rapid, almost magical cures which gained for Hahnemann and his pupils their world wide fame.

Thus it is ever that hesitation to proclaim and stand up for

the truth at whatever cost, brings along with it its own punishment!

It is a gratifying fact, that a marked reaction began about twelve years ago, and is now going on. Greater desire to thoroughly understand the science and art as Hahnemann taught them; greater faithfulness in the study of the *Materia Medica Pura*; greater boldness in professing the peculiar doctrines of Homœopathy, are manifest on every side. For much of this auspicious change we are indebted, before all, to the teaching by pen and by practical example of the lamented von Bœnninghausen; for much also to the faithful labors of Wurmb in the Vienna hospital, the results of which have been given to the world by Kaspar and Eidherr; and for much to the arduous labors of Dr. Drysdale in the compilation of the *British Repertory* and in the various essays in which he explains the nature and merits of that work, and urges the necessity of a faithful study of the *Materia Medica*. The change which a faithful study of Hahnemann's writings and especially of the *Materia Medica* will produce in the practical views of the student, is strikingly and happily shown by a comparison of Dr. Drysdale's Essays in the first volume of the *British Journal* (1843), with his remarks on the *Repertory*, in volume eighteen of the same journal (1860).

The direction which this reaction takes, in so far as the indication is concerned, is well expressed in the following words by Dr. Drysdale:

"No one has rightly understood the *Examination of the Sources, etc.*, nor the *Spirit of the Homœopathic Doctrine*,* who can imagine that the time has come, or can ever come, when clinical experience can supercede the pure symptoms as the final indication for specific Therapeutics. Nevertheless, the tendency of many is to go to this extreme; for, if we look through the homœopathic practical literature, both standard and periodical, we find that nine-tenths of the indications are

* Hahnemann's Lesser Writings, English edition, pp. 696 and 748.

merely clinical, and no pains are taken to bring out the correspondence of the pure symptoms. Whither is this tending? Let us see. Allopathy now a days is a very different thing from what it was; mainly, I think, from the indirect action of Homœopathy upon it and also from the borrowing, directly from us, many specifics which are used often in a simple form; also the use of specifics is partly acknowledged as a desideratum, and partly adopted practically under the names of tonics and alteratives; but the indications are always purely clinical and empirical. Now, in as far as we rely on clinical indication alone, wherein do we differ from the ordinary school? In no way, except that, being superior in numbers and having the command of more men of talent in hospitals, they will beat us with what were originally our own weapons. Our only resource, then, is to go back to the more diligent cultivation of our special field, viz.:—the *Materia Medica*. There we have scope enough to recover lost ground and get again far ahead; for, granting all that Pathology and clinical experience can teach us—and I would of course avail myself of it to the very fullest extent—how far does that bring us in determining the one right medicine required in a system of specifics? A very little way indeed. Very often it offers us a free choice of twenty to fifty medicines, all equally eligible—a kind of liberty and equality for which we may spare our thanks, as most likely only one or two of them can be specific. Let any practitioner seriously think over the cases that present themselves in one day's average practice, and tell us how many are well pronounced examples of pure inflammation of the large organs or other well-defined diseases whose course is definite and symptoms sufficiently fixed to enable us to fix the specific *ab usu in morbis*. A very small number it will be; and applying this to the practice of medicine at large, we come back to Hahnemann's proposition, that *no two cases are exactly alike*, a fact that strikes at the root of all attempts to perfect a system of specifics by experience in disease."

The same point has been discussed with great ability and

in a still more practical manner by writers in the *Allgemeine Homœopathische Zeitung*, and especially by its able Editor, Dr. Veit Meyer, whose published cases of diseases treated purely according to the totality of the symptoms, have given a peculiar interest to recent volumes of that periodical. D.

CHARACTERISTIC SYMPTOMS.*

BY PROF. DR. J. HOPPE, OF BASLE.

Homœopathy has not as yet given prominence and emphatic utterance to the fact that the decisive symptoms, in diseases and drug-provings, are, in many respects, insignificant—indeed *very insignificant* and *trifling*. Nevertheless Homœopathy has not been unaware of this fact; she has only not expressed it in sharp, clear words and thereby given a full currency to the importance and to the apparent unusual nature of this circumstance. For, when we are taught that, in examining a patient, we must go to work in a sharp and thorough fashion, and must investigate with indefatigable zeal until we find out the peculiar phenomena which give us our rule of action, it is clearly implied in this very teaching, that often these peculiar phenomena do not present themselves obviously in the foreground, but are, on the contrary, insignificant and *apparently* trifling.

And when, further, we are taught how to conduct ourselves when proving a drug, in order to allow the symptoms to develop themselves and not to pervert and cloak them by accidental phenomena, as well as to take cognizance of them undisturbed, the conclusion, in like manner, follows from this teaching that the *decisive, instructive* and *important* symp-

* "On the insignificance of those symptoms in a proving and in a disease which determine the choice of the remedy." By Professor Dr. J. Hoppe, of Basle. From the *Allg. Hom. Zeitung*, 68, 105, April 24th, 1864.

toms do not always stand, fair and square, full before the eyes.

In truth, it *has* been seen and felt that the determining symptoms present themselves in many respects insensibly. But every body has, timidly as it were, kept this fact to himself and not given prominence to it, as the *experience which furnishes the rule*; we have silently accorded to it an importance which we have not been willing to express publicly, aloud, clearly and boldly.

And we have been *timid* in this matter, *because* we have regarded it as a token of incompleteness and of unripeness, that we are obliged in many ways to avail ourselves of so insignificant and trifling symptoms. But he who has been initiated into this *open secret*, that the very determining symptoms are often insignificant, and has been in the habit of profiting by it—he has ever distinguished himself by an especial practical ability. Yet in doing so he might fall into the opposite danger, viz: of taking these insignificant phenomena in a subtler sense than the progress of science up to the present day enables us to do and justifies our doing—a misfortune which, where the investigations were truthfully made, may indeed damage the individual, but can only benefit the science.

Let us then say it—emphatically, loud and frankly—that the determining symptoms appear in many respects to be insignificant and unimportant, and let us proclaim it to be a requisite condition, that in proving drugs and in examining patients, the insignificant symptoms are not to be neglected, but even to be noted and regarded with *especial* care.

It is true, that which we here say. This truth has its analogy in every department of science. And this truth has its necessary fundamental basis.

A slight sticking or digging pain in the teeth, when proving a drug, a slighter increase of heart-beat, an inconsiderable pain in the throat, a somewhat unpleasant taste, which recurs only at long intervals, a slight change in the stool, a somewhat restless night, a slightly more abundant sweating,

a somewhat depressed or a somewhat exalted state of mind, etc., etc.—these phenomena are to be taken notice of; and it is, in good part, only when these are reckoned in, that the entire picture of the disease or of the drug-action is made out.

And has not also the diagnosis of diseases its difficulties and its subtleties? Is the diagnosis of iritis always so dazzlingly obvious; the diagnosis of pneumonia always so rudely palpable; the distinction between diphtheritis and catarrh always so striking, as it seems to be in very well developed cases?

There are, also in these cases, phenomena which appear insignificant and unimportant, but yet are so important that they *decide* the whole matter.

And as it is in diagnosis, so it is also in microscopy, so in all departments of medicine, so in all sciences, so also in all affairs of daily life.

Where no dazzling facts present themselves, the examination, the investigation must keep to the insignificant circumstances, and it lies in the nature of the thing, that these occur *more frequently* than the *striking* indications and are often even *more important* than the latter.

And if the grosser changes in and upon the tissues are often but little developed, *how much more may* this be the case in regard to such phenomena as present themselves only as the subjective expression of excited tissue-functions? For the insignificant and apparently unimportant phenomena, on which often so very much depends, are manifestations of the tissue activity—also manifestations which are as yet pretty much undisturbed and not yet covered over and concealed by the *results* of the excited or perverted tissue functions. They are *initial-phenomena* and they may easily be lacking *there*, where a process has become *developed* in its full extent; they are lacking, for example, in a fully developed pneumonia, whereas at the beginning of the same they may be still present and may indicate the peculiar character of the existing irritation.

The "peculiar," the "characteristic" symptoms—these are to be regarded as the determining ones; but we must, at the same time, never forget, and we must always say emphatically, that these symptoms may be *very insignificant ones* and that we have to seek them, for the most part, in the series of *little and unimpressive phenomena*. Accordingly, free from all feelings of timidity, we receive these *seemingly insignificant* phenomena within our field of investigation. What microscopic research is, in the case of small objects, the same, in semeiotics, is the scientific investigation of the trifling, unimportant, subjective and objective phenomena of disease, and he, who cannot labor in this field of the small and the few, can never be a master in either department of science. The riper spirit adventures into the depths, whose limits are immeasurable and whose products may indeed, to the uninitiated, seem insignificant, unimportant, trifling and profitless.

D.

THE QUESTION OF THE DOSE.

BY AD. LIPPE, M.D. PHILADELPHIA, PA.

This question which still remains open and awaits accumulated statements by the experiment is nevertheless approaching its final solution.

I agree fully with Dr. Wm. Arnold when he says in his article on the "Solution of the Question of Doses" in the *Homœopathische Vierteljahrschrift*, January, 1864: "When we investigate the question of the doses we must hold fast to acknowledged chemical, physical and physiological facts, etc."

Certain points have been clearly established by incontrovertible statements, and from them we can draw correct conclusions. These points are:

I. Cures related by the administration of low potencies and crude drugs.

II. Homœopathic cures related by the administration of the higher and highest potencies.

III. Comparative experiments related by Dr. Edherr—showing that the higher the potency given in pneumonia the shorter the disease, and the quicker the cure.

IV. That where low potencies did not cure but only aggravated the case, the higher potency cured.

V. When the high potency did not cure, the lower potencies gave relief.

I. Nobody can pretend to deny this fact. The first attempt to apply the homœopathic law to the curing of the sick was made by administering crude medicines, and had this attempt failed, had it not proved the correctness of this law ("Similia Similibus Curantur"), all further progress in the development of the new art would have been checked at the very outset. These first experiments not only proved the correctness of the fundamental law, but gave rise to the development of the most important homœopathic law—"the dynamization theory."

The *Materia Medica* which is the fundamental structure upon which our system of cure rests, was improved in the same proportion as we learned to observe the fact, that potentization develops new, before unknown, curative powers of the crude drug; and this we learn from the provings of potentized medicines on the well and the administration of the same preparations to the sick.

From the relation of cures by crude drugs and lower potencies, we are only aware that all cures that have ever been made, are now making, or eventually will be made, are according to our fundamental law, but further we learn nothing; these cases proving nothing against the theory of dynamization.

To show the correctness of my assumption I now return to Dr. Wm. Arnold's article above referred to, and his case of a cure of polypus of the nose in ten weeks by *Calcarea carb.*—*vulgo*, Lime water.

This case contains nothing instructive; we do not learn

why Calc. carb. was given instead of the other medicines known to have cured the same *disease* under *certain conditions*. Such a report we might reasonably expect from a physician who calls himself a Homœopathist. The logical conclusion the physiological school would arrive at is this: Lime water has cured a polypus of the nose; *ergo*, polypus of the nose must *always* be cured by Lime water—if strong enough.

The cure we do not doubt; but while it was not based on the acknowledged homœopathic principles, but was made according to a pathological notion, it is valueless; such is our fate if we treat the disease and not the sick.

The case *may* prove that the curative virtue of Calcarea carb. (and many other medicines) is not developed in the 3d potency; that if a higher, 15th, or 30th, or as high as the 200th, had been given it would not have been necessary to use Lime water for ten weeks.

II. We will continue to consider the Polypus, and the alleged proof by Dr. Arnold, that low potencies and even crude substances, are preferable according to his comparisons and experiences, and we will see what we can find in the homœopathic literature on the cures of polypus of the nose by Calc. carb.

Allgemeine Homœopathische Zeitung, Vol. 10, page 55.—Jahr relates: "A man suffered for five years with polypus of the nose; he had the polypus repeatedly extirpated, but it always returned; he sneezed frequently, and it was always accompanied by a profuse flow of mucus. Calcarea carbonica cured both polypi completely in *ten* days, and thirteen months later they had not reappeared."

Allgemeine Homœopathische Zeitung, Vol. 8, page 371.—Dr. Syrbius relates: "A child one year old had a polypus in the left nostril as large as a strawberry. Calc. carb. 18 three doses, one every day, caused the polypus to disappear. After a year a similar polypus returned in the right nostril, and was soon cured by three more doses of Calcarea carb. 18, daily one dose."

Dr. Syrbius relates a case of a boy fifteen years old cured of polypus of the nose by four doses of Calcarea carb. 18 daily one dose.

Dr. Speer relates the case of a woman, fifty years old, who for six years had a polypus in the left side of the nose larger during the full moon; three doses of Calcarea carb. 30, repeated after 21 days, cured her entirely.

These four cases were all treated with higher potencies, and the result does *not* prove Dr. Arnold's assertion. The cures were performed in a shorter time by higher potencies than was Dr. Arnold's case with Lime water.

Cases of polypus of the nose have also been reported to have been cured by Kali. bichr., Phosphor, Sulphur, Marum verum, Sanguinaria and Staphysagria. It would be more to the advantage of progressive Homœopathy if the learned Dr. Arnold had stated clearly and distinctly what characteristic symptoms induced him to select Calcarea carb. as the *only* truly curative homœopathic remedy; and he might have stated at the same time the characteristic symptoms that have and will again, very likely, indicate other remedies in the cure of the polypus of the nose. A cure can only be called a homœopathic one, if the characteristic symptoms of the remedy are similar to the characteristic symptoms of the disease (the sick).

Many such instructive cases have been published in the homœopathic journals, and in the same ratio, as the cure performed by the least medicines (the smallest dose of one remedy) have the cases been clearly reported and has the science gained by a confirmation of the provings and the practical rules laid down by Hahnemann. Every well informed member of the profession sees at one glance why that and no other remedy would cure the disease, and he thus adds to his stock of knowledge. On the other hand the cures reported to have been made by massive doses are wanting in accuracy, and are generally based on pathological notions; they carry no information with them, they are not even instructive, and lead to the belief that names of diseases can be cured instead of "the sick."

Of the cures reported by alternate remedies I wish not to say anything here, as they belong to *Eclecticism*, not to Homœopathy.

III. The same experiment has been made by many competent practitioners in private practice, and with precisely the same results; but the testimony of such men will not have the same weight as Dr. Eidherr's report, coming from a large body of physicians, and after the lapse of so many years.

The figures very clearly show that the duration of the disease was, under the 6th dilution, 19 days; 15th dilution, 14 days; 30th dilution, 11 days. Is it not reasonable to expect then that the same disease under a still higher potency would have lasted a much shorter time?

Should not Dr. Arnold make the experiment? He should if he would hold fast to acknowledged chemical, physical and pathological facts. But does he do so? In the article above referred to he says: "The medicinal power of the drug may be developed in the same degree if the first trituration is continued for six hours, instead of making six triturations, one in an hour. The first trituration, continued for six hours, must act *stronger*, because a larger number of fine particles of medicine act on the organism than in the sixth trituration, as there only the sixth part of *equally fine particles* comes to act on the organism."

Certainly Dr. Arnold cannot be in earnest! If so, where are his arithmetical calculations? Did he ever try to find how small the particles of the sixth centesimal trituration (made according to Hahnemann's advice) of Mercury, or any other metal, are; and how small, or rather how comparatively large, the small particles of the same substance in his first trituration, continued for six hours, are? The microscopic examination will give him light on that subject. Should he make the proposed examination, he will come to the conclusion that his proposition is false, and he will (may he!) doubt his own ability to make any further "observations and notes." It remains questionable what

general conclusions can be drawn from statements made by *such* observers.

IV. Under Article 3 we have already seen, by Dr. Eidherr's reports, that the higher potencies cure in a shorter time than the lower potencies. We find in the homœopathic literature cases published which go to establish this principle; and no observer was more apt to report correctly on this subject than the late Dr. Bœnninghausen.—(Vide his Three Precautions.) Hahnemann cured with the smallest doses decidedly quicker, better and surer than he did with the lower potencies. The evidence in this direction laid before the medical world is fast accumulating and remaining uncontroverted, and finally must become an established truth. The only admissible evidence contra would be to relate fully a case in which the truly curative homœopathic remedy has been selected, and when administered in a higher or high potency had not produced in a reasonable time any beneficial effect, and that the case, remaining unaltered was then promptly *cured* by a low potency, or the crude drug.

V. Doctor Dunham refers, in page 535, Volume IV. of this REVIEW, to a previously reported case—to *one* case in which a lower potency gave prompt and complete relief when the higher failed. Neither can this case, or an accumulation of similar evidence to the purpose, prove anything when we wish to settle the question, What doses are preferable in the (homœopathic) cure of the sick. Here is the question of cure (*sanatio*), not relief (*allevatio*); and when we discuss the one (*sanatio*), we only allude to, but do not discuss fully the other (*allevatio*). While the same laws hold good in the one as in the other, it is very likely that if the truly curative remedy can be found, a high potency will give prompt relief, and will be less apt to inflict lasting harm to the incurable case for which relief is asked; but this is at present an outside question and may be discussed later.

On page 202 of Vol. IV. of this REVIEW, Dr. Dunham in his articles on Doses, censures the "Radicals" severely, but undeservedly, and finally he says: "They seem to glory

not so much in the *truth* for the sake of which they separated, as in the mere fact of separation."

To my best knowledge, the "Radicals" have not separated from *Homœopathy*, but they are extremely anxious to hold fast to it, as Hahnemann left it to us, a legacy, and if possible unfold it through the same means by which the master developed the healing art. Those who deny all of Hahnemann's teachings and his practical rules, who slander and misrepresent his true disciples, and who prefer to coquet with the physiological school, have separated themselves from Homœopathy, and should not have the audacity to call themselves Homœopaths. What we could gain for our cause by clinging to them, or by compromising with them, history has taught us; and what we should do, *what we are now doing* with them, has been clearly shown by the sages of our school—(vide Bœnninghausen's Three Precautions).

Those who have separated themselves may think better of it in course of time, and they may return or rather embrace Homœopathy which they have never before accepted, save in name and without a good cause; they will then learn that a relief is not a cure, and that the statements that a *disease* has been *relieved*, while a high potency *failed to cure*, is no link to the evidence against the doctrine that high potencies are preferable in the treatment of the sick.

How near are we to the solution of *this* question? This solely depends on the testimony to be offered by the man of low doses. As far as the present indisputable testimony goes, the higher potencies have the evidence in their favor, and should we draw no other conclusion from the testimony now accumulated in support of the correctness of the much feared potentization theory, our gains would indeed be very great. If that question is once settled beyond dispute—and I consider it so settled by the evidences before us—the denial of its truth will become equivalent to a denial of Homœopathy. And if further experiments for the solution of the question of doses are only made by Homœopaths, the testimony which will follow hereafter will all be of one kind.

THE BASIS OF TREATMENT.*

BY CARROLL DUNHAM, M. D., NEW YORK.

Hahnemann throughout his works takes every opportunity to urge the insufficiency of a pathological theory of the nature of a disease as the basis of the treatment. He everywhere urges that the only sure indication for every case is to be found in the totality of the symptoms which the case presents. One would think that nothing could be more clear and convincing than his arguments on this subject.

His opponents declared that his method ignored medical science, left no scope for pathology and diagnosis, and reduced therapeutics to a degrading mechanical comparison of symptoms. Very many Homœopaths have so far deviated from Hahnemann's method as to endeavor to blend, with the use of his doses and remedies, an application of pathology as a basis of treatment. This endeavor can never be successful, inasmuch as the function of pathology is to furnish, not an indication for medical treatment, but simply a means of elucidating and collating the symptoms. The result has been a sad falling off from the standard of success in practice which was established by Hahnemann and his pupils. The points at issue are illustrated by the following case:

Willie M., four years old, was brought to me December 3, 1863. He had been healthy since February, 1863, when he is reported to have had a long attack of gastric fever, from which he finally recovered with the affection about to be described. This was a dyspnoea and wheezing, distinctly perceptible at all times when the child was awake, and which, on making any exertion, were very much aggravated, and resulted in an attack of convulsive cough with difficult inhalation, the whole paroxysm resembling precisely what is described as Millar's asthma or *Laryngismus stridulus*. It was

* Read before the Hom. Med. Society of Oneida county, N. Y., June 21st, 1864.