

to double his ordinary fee. If such consultations are very frequent, this may be occasionally remitted at his discretion. When a general Practitioner is himself called in consultation, he is entitled to the Consultant's *minimum* fee of 21s.; but this may be relaxed at the desire of the Practitioner previously in attendance. Midwifery consultations among Class 1 of patients, should be charged according to arrangement between the patient and Practitioners. Among the other classes, the second Practitioner is entitled to the same fee as the first, independently of the length of time he is detained.

9. *Attendance on Domestic Servants.*—When paid for by their employer, they should be charged as per Tariff. When paying for themselves, they may be charged as if belonging to Class 1 or 2. If the employer send for the practitioner himself, or distinctly avails himself of his advice, he is to be considered as the responsible person.

10. *Two or more Patients in one House.*—When these are members of one family, and paid for by one person, half a visit is chargeable for each beyond the first. When this is not the case, the full charge should be made for each.

11. *Midwifery.*—From use and wont, the fee for Class 1 is generally understood to cover the after visits, when those are few and all goes well; also, with those toward the bottom of this class, it may be reduced to 15s., if paid within a month. In the other classes the after visits are not included, unless previously arranged otherwise. When operations, such as the use of forceps, turning, &c., are required, the Practitioner is entitled to an additional half fee. The Tariff allows considerable latitude in the Midwifery fee, owing to the very great diversity of charges hitherto usual with different Practitioners.

12. *Abortions.*—In ordinary premature labor the same charge is allowable as in midwifery. In attendance on abortions, the visits required should be charged as such, *plus* an additional charge for detention, according to No. 5.

13. *Vaccination.*—This is not included in the midwifery fee. It may be charged according to the visit or visits required, if performed by the regular medical attendant; otherwise, as a special visit.

14. *Certificates.*—Simple certificates may be charged as ordinary visits or letters of advice. When special investigation or responsibility is involved, as in Life Assurance or Lunacy Certificates, 10s. 6d. to 42s. should be charged, according to circumstances.

We re-publish this tariff here because we think with those who have issued it that "it may serve as a reference in case of disputed charges, and thus prevent litigation, and promote a friendly settlement. It will also be a useful guide to the junior Practitioner, who is often in doubt as to the proper remuneration to which he is entitled for his services."

But while it is only just that pecuniary remuneration for medical services should be demanded of all who are able to make them, let us, as a profession, never forget that humanity compels us to go as readily and cheerfully to the aid of the sick poor who cannot, as to that of the richer man who can pay us. As homœopaths, it is our duty to spread our system among the poor to the very utmost. To this end our Hospitals and Dispensaries are established. The work they have accomplished is great. More zeal in sustaining them, more care in prescribing at them, may render this work greater still.—*Monthly (London) Homœopathic Review.*

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SIFTING THE DRUG SYMPTOMS.

BY CONSTANTINE HERING, M.D., PHILADELPHIA, PA.

In Vienna and in Prague, according to Heyne (page 36), the *cæsarling*, *Agaricus cæsareus*, is not allowed to be brought to market; it is forbidden by an order of the police. Why forbidden? Because the *cæsarling* has a red hat, and so also has the *Agaricus muscarius*, and because the former might be confounded with the latter! The ordinance is certainly a well-meaning, paternal one, and likewise judicious. In the first place, by means of it the ignorant are protected from injury; in the second place, by means of it this noble fungus is protected and preserved for him who alone deserves to enjoy it. The solitary connoisseur knows that the spathous naked *pileus*, the yellow flesh on the outer circumference of the *stipes* distinguish the prince of the fungi. But above all, he is made certain, by the pure yellow color of the *lamellæ*, that there stands before him the most complete of all the agarics—the noblest, the fairest, the one in flavor all others surpassing, the one highly-renowned since the earliest ages—of which even Pliny speaks as that to be preferred (Hist. Nat. c. 22)—that the imperial agaric stands before his eyes, and is in his hands, and is thus worthy to reward

him who knows it, and to pass over into his juices and blood.

But few may have had the good fortune to hold both in their hands at the same time—in the right the cæsar, in the left the fly-killer;* but whoever is so fortunate as to be able to lay before himself Harper's work on the fungi, let him compare the first plate with the last.

As nobility of sentiment displays itself in every word, and imprints itself on every stage of development—so here nobility is to be seen in every line, even down to the burgeoning egg—on the other hand, in every line of the fly-agaric, the malignant, the adder-like, the toad-like, even in the very egg. There is healthy, laughing, red gold, like apples; here a sinister, livid aspect, by reason of its sickly warts.

Just so it is in the Hahnemannian materia medica. What in Austria is called the *police*, is called, in relation to the materia medica, *criticism*. Our police requires that we should rather leave patients uncured than cure them by means of symptoms among which there are perhaps false ones, because these symptoms come from provers whom it pleases us to regard with suspicion, or because they were observed on patients.

The police allows no mushroom with red hat; scientific criticism allows no symptoms from patients, no symptoms from timid dosers, no symptoms which appeared after the potencies, etc. The police say: they have red caps; they may be poisoners; away with them; we are no connoisseurs! Our criticism goes still farther, for it not only says: they *may* be false, but it says: they *are* false! Surely this criticism might learn so much as this from the police, as to say: they *may*, they *might*, they *could* be! For precaution's sake let everything that resembles them be culled out. Many of our critics go still one step farther. They not only say: these symptoms are altogether good for nothing; no, they regard them as a kind of scabby sheep; they think that even the good symptoms may be poisoned by these bad symptoms!

* The *Agaricus muscarius* or "Bug Agaric" is used as a fly-poison.—D.

It reminds one of the tooth-puller of the last century, who taught the people that the black teeth must come out, because otherwise they would infect the rest. In like manner they would tear out the carious symptoms, so that the rest might then stand so much the firmer; but that, in consequence, the entire row of teeth becomes loose, can only be shown by the result on many thousands of maltreated persons.

That this horrible delusion has fixed itself in the best heads like a mold upon the brain, and disseminates itself probably after the manner of such yeast sporules floating in the air, let an example show.

In a quarto volume begun in 1852, an honored commentator of the provings of Kali bichrom. says (p. 4, note): "I have subjected the narratives of the provers to what may appear somewhat rigid criticism; and in the fear of incorporating any useless or doubtful symptoms, may have left out many that really belong to the drug, and which may turn out to be valuable. But I hold that it is better to reject many real symptoms than admit *one false one*, as one false symptom *tends to vitiate the whole by destroying our confidence in the rest.*"

Truly we must be thankful for this, that a man has the courage boldly to write and send out into the world such horrible nonsense as this. Thereby we learn things which, otherwise, we should not have understood how to regard as possible.

There are then really men, physicians, Homœopaths, who have "confidence" in the collection of drug symptoms; And what sort of confidence? A confidence in the hundreds and thousands of symptoms of the various remedies—a confidence more tender than the sugar manikin upon a macaron—one jolt and down it goes!

"One false symptom tends to vitiate the whole." What is a false symptom? Neither is there a single one, among many thousands and thousands, which *positively* and *certainly* is a false one, nor has it ever been, in a strictly scientific

manner, demonstrated of any single one. We have hitherto only suspected, we have sought to make it probable, very probable, in the highest degree probable; but all this is no proof!

There are many "false symptoms" in all probability—it could not reasonably be expected to be otherwise; and, for this reason the old school too knew nothing better to say in opposition to this greatest product of the century, than to throw suspicion on the whole of it. Now come along such imitators of our opponents, and think if they throw suspicion on single symptoms—for a proof that the suspicion is well-grounded has never yet succeeded even in one single case—then not only must these symptoms be thrown out, but even the good, the true ones along with them! If, among the servants of a house, *one* falls under suspicion, we hang them *all*; for the "sugar manikin" of our *confidence* has come to grief! In this way have they thought to rescue science and to build the highways of truth!

But by what means do we find out that any symptoms whatever, of any drug, really and truly belong to that drug? There must be ways and means, for even the most conscientious, most careful prover, the most attentive observer, may possibly err; for he remains a man, and "to err is human." That such a thing is possible, and is to be taken for granted, follows from the very words of the same critic; for not without emotion do we read his confession that he may, in his zeal for the rescue of our science, "have left out many that really belong to the drug;" and he is willing to admit that these "may" perhaps, later in the course of time, "turn out to be valuable." But how shall these unlucky symptoms *begin* to "turn out to be valuable" if, in the collocation of symptoms in the Symptomen-codex they have been *already thrown overboard*?

There is but one way in which we can, from time to time, render single symptoms more probable; in this way many symptoms may gradually turn out to be valuable; but this is only possible on condition that we do not throw them overboard into the jaws of the ravenous sharks of criticism.

But this way is that of the strictest method, the method of Hahnemann who, a full half-century before Apelt wrote his theory of induction, solved the great problem practically in the very same manner. In the same way we must continue to travel; we must develop after the manner of all sound growth, and we shall attain what Hahnemann had in view—mathematical certainty.

To this may belong:

1. Proving on the healthy, with or without poisonings.
2. Observations on the sick.
3. Cures of groups of symptoms.
4. Collocation of all these symptoms in all their relations.
5. Comparison among themselves of the symptoms of each remedy.

The last, the great essential, the estimation of the value of the single symptoms, the sifting, the separating, the valuing and everything else that is based on this estimate of the value of single symptoms—in the region of the art: determination in the choice of the remedy; in the region of the science: columns and arches to be erected—all this, however, is most plainly impossible unless we first have collections of all the symptoms—collections the completest possible! These, too, must be in the *hands of everyone*, in order that a few prominent ones may not, through their preconceived opinions, confuse and throw dust into the eyes of *the many* who ought to see for themselves, whether by means of clouds of learned dust from the quartos and folios which they flap open and shut, or through the whirlwinds of sand which they raise.

We must have our materia medica before us *accessible to all*, and just as it was proposed in this journal (*Allg. Hom. Zeit.*, 69, 12, 89, and *AMERICAN HOM. REVIEW*, Vol. V, p. 88), on the one hand historico-genetic monographs as the foundation of the science (*AMERICAN HOM. REVIEW*, p. 90); on the other hand the pure symptomatology in the encyclopædic form as the foundation of the art. All of our contentions bring us not a step forward, and they tend, in spite of our thirty years'

war, at the very best, only to a peace of Westphalia, that is, to a still greater distraction. The three editions of the *Organon* will certainly not unite us, but these two collections of the materia medica might at least *render a sound and healthy criticism possible.*

In the lottery to raise money for the completion of the tower of Cologne Cathedral, every ticket costs one dollar, and it is hoped that the two towers will be built simultaneously. In the enterprise for the building up of our two towers, each share is to cost five dollars. That is a difference to be sure; but on the other hand, there a building only is to be completed; here one is to be begun. And whereas there it may chance that one gets something; here everyone is sure of getting his portion. But to contribute to our enterprise is by no means to come over to our party; and one may contribute for no other reason than this, that he approves the simple accomplishment of an enterprise, solely that the thing may at last come into existence, and that the world may have it. But perhaps even this is asking too much; if so, then we must nurse it quietly until the world is ready to have

[D.]

ALTERNATION OF REMEDIES.

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

(Concluded from page 177.)

The two principles on which alternation has been sought to be defended, have been stated as follows:

1st. "Each drug has its own specific sphere and manner of action; hence that remedy acts in a particular manner, upon a particular organ or tissue or upon a particular set of organs or tissues."

2d. "Attenuated remedies act upon the system only by virtue of their homœopathicity to the disease by which the system is at the time affected." * *

The artificial disease intentionally produced by a drug, and the natural disease which results from the usual morbid predisposing and exciting causes may, for all purposes of argument, be regarded as identical. Principle No. 1 might therefore, with equal propriety, be expressed thus. "A disease has its own specific sphere and manner of action; hence that disease acts in a particular manner upon a particular organ or tissue or upon a particular set of organs or tissues."

A corollary of this proposition is, that, inasmuch as the drug and the disease respectively act in a particular manner and upon particular organs or tissues, they leave other organs and tissues of the body altogether unaffected, in their normal state and performing their healthy functions, and liable to be themselves attacked by some other disease, which may affect *them* in its own particular manner, and may run a *simultaneous* and *independent* course.

This is the argument for alternation which is constructed upon these principles: a certain disease affects in a peculiar way certain organs or tissues of the body, leaving the other organs and tissues in the fulfilment of their normal functions. Attenuated medicines may be administered to cure this disease. Attenuated medicines "act only by their homœopathicity to the disease." "They are inert" so far as action on the healthy organs or tissues is concerned. Homœopathic medicines act only where, says Dr. Drysdale, they find that preternatural susceptibility to their action which inheres in organs or tissues diseased in a manner similar to the morbid state which those drugs can produce on the healthy.

These attenuated medicines then may be given for the case we have supposed. They will act upon the diseased organs and tissues, but will not affect the *healthy* ones at all. Now then, during the existence of this disease which, as stated, affects in a particular manner certain organs and tissues, and leaves all the rest undisturbed, a second disease may attack the individual (may we call one who may be thus divided up an *individual?*) acting in some other particular manner, upon some other particular organ or tissue.

To meet this new disease, coexistent with the original, it is affirmed that a second remedy homœopathic to it may be administered simultaneously with the former remedy. Or rather, since to administer it simultaneously might involve the risk of the chemical reaction of the drugs, the remedies may be alternated. It will not interfere with the former remedy, because attenuated medicines act only by virtue of their homœopathicity to the disease (to which they are respectively homœopathic). Thus each drug "will go to its own place," like the respective members of a well-trained coach team when the winding horn announces that the coach is ready for the new relay!

Such is the argument for alternation succinctly, and, we think it will be conceded, fairly stated. In a former discussion of this subject we showed that, assuming natural disease and drug disease to be, for the purposes of this argument, substantially the same thing, it was *proving too much* to demonstrate that two or more diseases coexisting in the body could not and do not affect each other. This would render a cure of a disease impossible; for in the act of curing, we propose to cure a *natural disease* by creating in the body a *drug disease* which shall annihilate the natural disease and shall, in that very act, be itself annihilated. When a certain form of rheumatism exists in the body, we give Bryonia, and our intention is that Bryonia shall so act upon the body (producing therefore, virtually, a *bryonia disease*) as to cause the disappearance of the rheumatism and at the same time not to leave in its stead a bryonia disease nor any other disease. But if two diseases coexisting cannot react upon and modify each other, how could a cure take place? The proposition is evidently too broad; the argument proves too much.

Dr. Drysdale provides against this objection by limiting the alleged possible independent coexistence of diseases, to diseases which act in a dissimilar manner upon remote or unrelated organs and tissues.

According to this view, two diseases might coexist and not modify each other, if seated in organs distant from each

other or, which is equivalent, a natural disease may exist, and a drug may be given which acts only upon organs remote from those which are the seat of the disease, and (if the doses be strong enough) may produce its peculiar drug disease without at all affecting the natural disease which is already present.

In other words, it is only diseases (whether natural or drug diseases) which are homœopathically related to each other, that are incompatible and may not coexist without modifying each other.

Dr. Drysdale and several other physicians hold these views, and appeal to their own observation and experience. If we may be allowed to draw an inference from Dr. Drysdale's remarks (*Annals of British Hom. Soc.*, No. 17, p. 375), he believes that a hepatitis may supervene upon an already active pneumonia, and that each may run an independent course, and be treated independently by alternated remedies, without modifying each other or blending into one morbid state.

Our own experience has led us to very different conclusions, and we have on our side the names of colleagues, who, not having deviated from *the modes of practice* observed by Hahnemann's *early disciples*, do not feel compelled to acknowledge and lament, as Dr. Drysdale does, that their practice is "not proportionally so successful as was that of Hahnemann's early followers." (*New Repertory*, Introduction.)

How can this direct contrariety of inferences from observation be explained? We have altogether too profound a respect for the observing and reasoning powers of our dissentient friends, to set them a particle below our own! There must be an opposition in some of our methods of observation, or principles of inference. It will be found, we think, in the fact that to the word "*disease*" we respectively attach very different ideas. We mean by disease the aggregate of those symptoms presented by the sick man, which are characteristic of his particular deviation from a healthy state. The colleagues from whom we differ speak of disease as denoting

a definite pathological and pathologico-anatomical change of the functions and tissues of some definite organ or system of organs.

In this latter view the patient might present evidence of those pathological changes in the organs and functions of respiration, to which the name pneumonia has been attached. Here then is *one* disease, pneumonia.

He may also present evidences of those pathological changes of function and tissue in the liver which give rise to the name hepatitis. And here would be another, coexistent disease hepatitis; and these two pathological and pathologico-anatomical conditions might, we freely admit, from the pathological and nosological standpoint, run their course to resolution or to destruction without so modifying each other as to coalesce or in any way blend or be confounded; for the lungs and the liver can never collide.

But, let it be borne in mind, we are discussing a question of practical medicine and not one of pathology or of nosology.

How would this case look from *our* standpoint? The patient has, we suppose, dyspnoea, stitching pain in the thorax, hard, dry cough from tickling behind the sternum, scanty and occasionally bloody sputa—headache on the vertex, and sharp fever. He is worse at night; his pains are much aggravated by motion, and are relieved by repose and by warmth.

We have selected Bryonia for him—had previously diagnosed pneumonia (physical signs aiding or confirming our diagnosis). Now he gets, in the hepatic region, sticking pains, fullness and tenderness; he has bilious vomiting and bitter taste. Aided again by physical signs, we diagnose hepatitis, complicating the pneumonia; but the symptoms still indicate Bryonia, and so we continue that remedy.

But, we shall be asked, suppose the new symptoms do *not* indicate *Bryonia*, but, on the contrary, some other drug, will you not give that other drug for the liver and continue the *Bryonia* for the pneumonia!

To this we reply that in so far as our observation has taught us, in such a case the supervening of these new symptoms (of the liver namely) will have so modified the whole organism, including the diseased respiratory organs, that *Bryonia* will no longer be indicated by any symptoms; but the aggregate of characteristic symptoms of the entire sickness will now indicate some other remedy which will apply to and will cover the whole morbid state of that individual.

Here we are again at issue. Dr. Drysdale and his friends appeal to certain observations which we will now briefly discuss.

In the discussion which followed the reading of Dr. Drysdale's paper in the British Hom. Society, Dr. Russell took the same ground as Dr. Drysdale. He said, "that two morbid specific actions could occur simultaneously in the human body, and each pursue its course without arresting or modifying the other, is a proposition entirely at variance with the opinions of the old pathologists who flourished before the time of Hahnemann. * * It is of great importance to us to know whether this pathological doctrine be in accordance with the larger experience and observation to which we now have access."

We could hardly have two diseases more specifically distinct than TYPHUS and SMALLPOX, yet, in the following narrative, we have a description of the two running their course, side by side, without either interfering with the other." The case occurred in the London Fever Hospital, and is quoted by Dr. Murchison.

Dr. Russell proceeded to say: "There are many instances of a similar kind on record, and we must frame our theories so that they shall embrace this new category of cases. Suppose we encounter a case, and there are such on record, of a combination of scarlet fever and of typhus, how are we to deal with it? Are we to engage only one of the two destroying agents, and let the other alone until the first be entirely subdued? If we believe it is impossible for two medicinal actions to proceed *pari passu* in the animal economy, this is

the rational course to pursue, and the one recommended by Hahnemann!" (This is an inexcusably lax statement. Hahnemann nowhere ever recommended that we should make only a part of the patient's ailment the basis of our choice of a remedy; he always strongly insisted on our considering the "totality of the symptoms," and this would include both scarlet fever and typhus.)

"But, if two entirely different *natural* morbid processes can coexist in the human body without the one affecting the development of the other, what reason is there why there may not be two artificial simultaneous series of morbid phenomena, each equally independent of the other? If scarlet fever, or if smallpox, can each run its course, while at the same time typhus is doing so likewise, why may not Belladonna and Arsenicum each run their course when given in alternation?" (*Annals British Homœopathic Society*, xvii., p. 399 *et seq.*)

In reference to this last question, we may ask would Dr. Russell expect to get pure symptoms of any drug if it were proved simultaneously with another drug; would he believe that the two drug-diseases would run an independent, simultaneous course in a prover? Would he practically believe it by consenting to rely upon a *materia medica pura* made up in this way? Or, which is the same thing, would he trust to a *materia medica* composed of the results of an artificial (drug) disease running "an independent course simultaneously" with "a natural disease?" In other words, would he trust provings made on sick persons?

We know he would not; for he is one of those who reject or suspect the provings of many of our most valuable drugs, because symptoms observed on the sick are included in the list, or because the provers were not careful enough to exclude other morbid or toxic agents while proving.

But, this inconsistency aside, let us take notice that in these remarks, and in this citation of a case of the coexistence of two diseases, Dr. Russell speaks altogether from the standpoint of the pathologist and nosologist, and not at all from

that of the practical physician—the standpoint of therapeutics. He thinks he has proved his case if he has established, what nobody is disposed to deny, viz., that two nosological abstractions, called by distinct names (to wit, scarlet fever and typhus), may coexist.

Does he forget that nothing is more firmly established in homœopathic practice than that the "name" which we may feel authorized to give to a sickness in no wise determines our *treatment* of the sickness. Because we call the sickness typhus, we do not *therefore*, of necessity, give Arsenicum, or scarlet fever Belladonna! We may as often have to give Arsenic in a case that we should call scarlet fever, and Belladonna in a typhus.

What then guides us in selecting a remedy? Nothing save the aggregate of the characteristic symptoms. We might divide and subdivide the symptoms and signs that the sick man presents (including under the designation "symptoms and signs" everything that distinguishes that sick man from himself when not sick) into a dozen nosological groups each having a distinct name, and yet the aggregate of these symptoms might (and we think it always would) point to one single remedy as corresponding to, and indicated by the entire morbid state of that sick man.

To prove his case by the argument and instances quoted, Dr. Russell must show that the symptoms of the scarlet fever and of the typhus, respectively, were incompatible in the case cited by him, and that they did not combine to present the characteristics of one common remedy.

For this is the point of the whole discussion. No matter how many of the pathological groups which are dignified by the names of distinct diseases, may coexist in a patient, if they be so blended and mutually modified as to indicate, in the aggregate, one single remedy, there can be no call for *alternation*.

Inasmuch as Dr. Russell rests his argument for alternation on the possible and frequent coexistence of distinct specific forms of disease, it is incumbent on him to show that these