

canadensis, and also new observations; for instance, Vol. I, p. 310, Oxalic acid in the treatment of atrophía ab lactatorum, which remedy has been supposed to be applicable to this form of disease, but up to the time of this publication it was not known to have been administered successfully. The authors have overlooked in this form of disease Bismuth and Kreosote, which have often cured the above-mentioned disease.

Vol. I, p. 317. In the treatment for Colic we miss Belladonna and the characteristic indication for this remedy, "better when leaning against something hard."

Vol. I, p. 318. Under Plumbum the important characteristic symptom "as if the naval was drawn to the spinal column," has been overlooked.

Vol. I, p. 340. In intestinal intussusception we miss Thuja and Lycopodium.

Vol. I, p. 606. In the treatment of measles we miss Kali bichromicum, Antimonium crudum and Scilla.

Vol. I, pp. 649 and 650. On Therapeutics of inflammation. The authors have rather largely indulged in speculations and lost sight of Homœopathy; they say, "Thus, in gastritis we might give Belladonna which is a vaso-motor excitant, or Arsenic which is a tissue-irritant to the gastric mucous membrane. And, perhaps, a better effect would be obtained by alternating the two remedies than by giving either singly." To generalize in this manner is rather a violation of all our avowed principles, but to alternate Arsenic and Belladonna will not perhaps be worse, but would certainly be a great blunder. Either Arsenic, Belladonna, Bryonia, China, Nux vomica, Pulsatilla, or any other remedy will surely cure gastritis, *solely and alone, provided* the totality of symptoms indicate *the* medicine, but to give one because it is an irritant or the other because it is an excitant, is ridiculing our law of cure.

Vol. I, p. 658. Treatment of Abscesses. Apis and Lachesis have been overlooked.

Vol. II, p. 85. Affections of the Prostate Gland—Thuja and Digitalis, the most important medicines, have been omitted.

Vol. II, p. 149. Under Fistula lacrymalis, we miss Fluoric acid.

Vol. II, p. 158. Under Rheumatism, Rhododendron, Kalmia and Ledum have been forgotten.

We do not wish to fatigue the reader of the REVIEW by extending our corrections, nor will we dwell on the negligence of the proof-reader, as the printer's mistakes (the errata) will be easily corrected. The authors have diligently attempted to aid the physician in his practical duties, to such physicians as sustain the theoretical views of the author's, the work will afford the assistance they seek; other physicians who have followed diligently and assisted in the developing progressive Homœopathy will express a desire that the time and energy in book making might be better spent in the completion of the true and only homœopathic work on Therapeutics—the *Materia Medica*.

A. LIPPE.

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ALTERNATION OF REMÉDIES

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It is the peculiar glory of the homœopathic system of medicine, that, acknowledging man's inability to trace out or to understand the ultimate processes of nature, it bases itself, not on theories, but, like other natural sciences, on facts as they are observed in the world of man. A result of this peculiarity is an entire freedom of criticism and of opinion. It frees its disciples from all dogmas and simply asks them to look and see. It takes as its principle the principle of all true science—*first establish the facts*. These once fixed, any theory which will perfectly account for them is good.

At the present time and indeed for a long time, much is being and has been said and written about the alternation of remedies. It is assumed by many, and it seems to me to be the tendency of the leading minds of our school, that one remedy only should be given at a time and that each one should be allowed to exhaust its action before another is given. This rule is based upon the theory that two or more medicinal forces cannot together act upon the economy without so modifying each other that neither shall produce the effect it would if only one were acting. Now it is granted that, *a priori*, this seems reasonable and I confess that my own pre-

dilections tend in that direction. But the question is, do facts support the theory? Is it true, for instance, that remedies do not act as readily and in the same way, upon one who uses tobacco as upon one who does not? Does not the experience of every Homœopathician show that persons accustomed to the use of coffee are just as readily affected by remedies as others and in precisely the same manner? It certainly seems so to me, and, if one case can be shown in which two medicinal forces do act upon the vitality without modifying each other, it follows that there may be others, and we can in no case decide the question in any other way than by experiment, the only scientific test in all questions of fact. It is unquestionable, that to arrive at the characteristic effect of any drug it must be proved by itself, but it does not follow that, the peculiar action of a drug being discovered, that action cannot be depended upon to take place, without disturbance or modification, another drug being at the same time in action. To establish this position it is necessary to institute another set of experiments. To assume such a position without such experiments, it seems to me, is simply to dogmatize. But let it be granted that two or more remedies cannot at the same time act upon the human system without mutual modification, how shall we know but that such modification may be an improvement in the action of each, and hasten the cure? Surely this question can no more be answered *a priori* than the other, for without experience we have no data on which to reason. What is our experience, as physicians, on this point? It might be profitable for us all to ask ourselves this question. It seems to me that I have often had cases in which two remedies nearly alike indicated, administered alone, have each failed to produce the desired result; while a cure has promptly followed the alternation of the same remedies in the same potency. I well remember a case of phthisis complicated with chronic metritis, which was under my care something more than a year ago. I treated the case at first with the so-called antipsorics—Sulphur, Calc. carb., Sil., Graph., etc., usually giving only

one at a time. The case did not improve, but steadily and in a marked degree declined under any and all these remedies. At length there appeared a tearing, stitching pain in one shoulder which, taken with the other symptoms so pointed to Bell. or Bry. that I put the patient upon them in alternation. The relief of all the symptoms was prompt, decided and continued. A short intermission of the remedies however, either allowed the patient to fall back or at best to remain stationary. A repetition of the prescription always brought improvement. Having in mind the doctrine that one remedy only should be given at a time, the question naturally arose which is the remedy of these two which is producing this result, or is it the alternate action of the two? With the distinct purpose to test this I gave, after the intermission of all remedies, first Belladonna without the previous relief. I then gave Bryonia with a like result. A repetition of the alternation promptly resulted, as before, in improvement. In the course of some five or six months in which the patient was under treatment, I repeated this experiment three times with the same result each time. Under the alternate action of these remedies, in the thirtieth potency by the way, the patient has recovered a better state of health than she has before had for years.

A few days ago I had a case of intermittent fever, in which I hesitated in my choice between Arsenic and China, but gave Arsenic. Not getting the desired relief I followed with China, and still the chills continued. With this question distinctly in view, I gave the same remedies in alternation and got an immediate cure. Now how shall we dispose of such facts? Shall we say as the Allopaths do, when a Homœopathician comes in and cures their cases, that the cure is not the result of the remedies last given but of those previously administered? Or shall we say, as they often do, that the case had just reached its crisis and would have recovered without anything? Surely such argument has no more force against the alternation of remedies than it has as used by the old school against the doctrine of attenuation. Nor does the case

seem to be met by the assertion made to me by a distinguished advocate of the use of but one remedy at a time, when I related to him the first case here spoken of, "that probably some single remedy might have been found, by further study, that would have covered and cured the symptoms as well as or better than two;" for this is only a begging of the question, being only a reassertion of the point in dispute. But suppose it true that in any given case of the use of two remedies some one remedy might have been found that would as well, or better, have answered the purpose. The question is still not met, for it is to be supposed that the prescriber has given the case his best thought and study, and no such remedy has suggested itself; and the question is not could some person have done better than I, but have I done the best I could? If he has done the best he could, it certainly is proved that for him the use of remedies in alternation is better than the use of a single remedy.

But we are told, as the final and perfectly conclusive argument on this question, that the great founder of our system always taught and practised, as a law in medicine, that one remedy, and only one, should be given at a time, and that one should be left to exhaust its action before another is given, and the inference sometimes is that no one can be a real Homœopathician who does not, in his practice, conform to it. We grant that Hahnemann always so taught and practised, but does that at all effect the facts? Besides does he any where claim that he had reached the acme of medical discoveries and that there is nothing more to be learned! Surely not, and the assumption that any one must adopt this rule to be a Homœopathician is but an illustration of the tendency there is in men to follow a leader—to seek some authority outside of and beyond their own reason, whose doctrine shall be final, failing to see that the moment a man comes into such a position he has encrusted himself with a shell beyond which, so long as it remains, there is no possibility of growth.

I conclude, therefore, that this is still an open question and must remain so for a long time, for the simple reason that we

have not yet any such collection of facts as will warrant any generalization.

The building of theories to account for facts in science is a very pretty amusement, always innocent, perhaps, and no doubt often profitable. But let us beware how we make any theory our Shibboleth, the pronouncement of which shall admit or exclude a man from our brotherhood. But let us rather give ourselves to such careful experiments and observations as we may, and give to our brethren as we can from time to time the results of our studies. Thus and thus only may we hope to build up our system to the beautiful proportions of a perfected science. Any other course dooms us to the same condition of bigotry and dogmatism which characterizes the old school of medicine.

REMARKS.—We publish with great pleasure articles which, like the above, though they seem to favor a practice which we deprecate, are, nevertheless, honest expressions of the earnest convictions of men of close observation and faithful study.

Nothing could be farther from our intention than to undertake to dogmatize on this question which we admit to be an open question and on which men of eminence entertain opinions at variance with our own. It may be allowed us nevertheless, in all courtesy to our contributor, to signify the points on which we take issue with him.

1. We, assuredly, do entertain the belief that "two or more medicinal forces (or, we prefer to say, "morbific forces") cannot together act upon the economy without so modifying each other that neither shall produce the effect it would, if only one were acting." This belief has prevailed among medical men from the earliest ages. It is the foundation of the practice of Polypharmacy in all its varieties, from the complex prescriptions of the seventeenth century to the alternations of our own colleagues. For we hold with Dr. Sorge (by no means a Hahnemannian, let us add) that "the practice of alternation, as it exists among Homœopathicians is only another form of *mixing remedies* with the intention

of getting an effect compounded of the action of the two or more drugs that are alternated."

We say, this belief is the foundation of Polypharmacy. The rationale of a compound prescription we take to be the following: the drug which is regarded as the one chiefly indicated possesses, let us suppose, certain properties which would be hurtful to the patient. Another drug is conjoined with it for the purpose of antidoting those hurtful properties. Again it lacks the power to produce certain effects which are deemed desirable. Another drug is added to supplement this deficiency and so on *ad infinitum*. This entire procedure rests on the belief that these medicinal forces will so modify each other in the economy that neither shall produce the effect it would if only one were acting.

As an example, we mention Calomel and Opium, a very familiar combination. Certainly, the object of giving the Opium, is, that it may modify the action of the Calomel and in a way which conforms to the known action of Opium when given alone.

If there were no reason to believe that "two or more morbid forces (a *drug* is a morbid agent exciting a morbid force) could not together act upon the economy, without so modifying each other that neither shall produce the effect it would if only one were acting," how could we ever *cure disease*? The morbid force which constitutes the disease is acting upon the economy and we bring to bear upon the economy another morbid force, viz., a drug. Now if the latter be properly selected with reference to the former, we know that these forces will so modify each other that, on the one hand, the manifestations of disease will cease, and on the other hand, the drug will not produce pathogenetic symptoms as it would on the healthy subject. This statement rests on a basis of such "facts" as our friend invokes. On what other theory than the mutual modification of morbid forces acting together upon the economy could it be explained?

On what other theory could we explain the action of antidotes as Homœopaths understand and employ them?

How else could we explain the action of Belladonna, in large doses, in antidoting poisonous doses of Morphine (*American Journal Medical Science*, Jan., 1862,) as well as the mutual antidoting powers which Allopaths have recently discovered many of the cerebro-stimulant and spinant alkaloids to possess? How else, finally, could we explain the efficacy claimed for the practice of alternation in general and for certain instances of it in particular, such as the cases related by Dr. Hawley, and which we fully credit, where Belladonna and Bryonia in alternation accomplished what neither could do singly, and where Arsenic and China in alternation cured a case which neither alone had cured? How else but by conceding that two or more medicinal forces cannot act together upon the economy without so modifying each other, that neither shall produce the effect it would if only one were acting?

The fact that medicinal or morbid influences must be avoided when we are engaged in proving drugs is universally admitted, even Dr. Hawley assenting to it. But on what other grounds than the belief already stated?

That persons long accustomed to use tobacco or coffee are easily affected by drugs is accounted for, probably, by the fact that long habit has, in so far as those persons are concerned, caused tobacco or coffee to cease to be "medicinal forces." Use has become "second nature." That in persons not accustomed to the use of coffee, a dose of it will modify the action of another morbid, or medicinal, force is demonstrated by the effect of coffee in antidoting Opium, Nux v., Alcohol, as well as in neutralizing the beneficial curative action of many drugs, as our clinical observations often satisfy us that it does. The same statements apply to tobacco.

2. We think we may then assume, as conceded, the fact that "two or more medicinal forces cannot at the same time act upon the human system, without mutual modification." It is now asked, whether, granting this modification, we may not avail ourselves of it to derive, from the alternate use of drugs which modify each other, good effects that we have attained in no other way? There can be no doubt of the pos-

sibility of this being done; no doubt that it has been done with advantage; as, indeed, Dr. Hawley shows by two instances.

But, in the present state of our science, it would be impossible for any one to lay down rules for the selection of remedies to be given in alternation, with the view of gaining any desired modification of the action of each. Nor can we conceive of the possibility of arriving at any such law. The rule of the ancient Polypharmacy, viz.: to select remedies according to the effects which they produce singly, and to combine them with reference to the effect desired, does not always work well in practice. For example, Calomel and Opium produce a combined effect which is clearly a modification of the known action of each drug. But on the other hand, the combined action of Opium, Ipecacuanha and the Sulphate of potash in Dover's powder is quite distinct and different from that of either constituent alone, and is such as could hardly be anticipated from what we know of these constituents.

Arsenic and China produced a combined effect which cured an ague that neither singly would cure. Now if this fact were to be taken as a guide in selecting a combination for a case of uterine disease, for example, in which we were at a loss to decide between Sepia and Pulsatilla, it might lead us to alternate those two remedies. But it is not three months since one of us took charge of a case in which this alternation, on this very ground, *had* been employed, but the patient had not recovered. We were led to the conclusion that the Sepia was really the remedy indicated—that it did good, but that its effects were constantly antidoted by the Pulsatilla. We omitted the Pulsatilla, gave Sepia judiciously, and the patient promptly recovered.

When, in addition to this difficulty arising from the fact that alternated drugs do not always produce a morbid action which can be predicated in advance from their known single action, we consider that the various degrees of susceptibility of the organism to drugs indicated with different degrees of accuracy, will cause the modification by drugs of each other's

action to be different in each individual case, we clearly see that we can never be in a position to predicate, in advance, the effect of the joint administration of several drugs. We can never, therefore, have a scientific indication for such a combination or alternation. If we make it, it must always be the result of "guess-work"—of "feeling our way along," just as Dr. Hawley did in the cases he relates.

But we claim superiority for Homœopathy in that it gives us the means of selecting our remedies with a reasonable certainty of their effects, and we rightly claim that medicine should not rest content with anything short of a method which, given the symptoms of a disease, points us to a *certain* remedy if our *Materia Medica* contain it; or, given the pathogenesis of a drug, indicates to us, *a priori*, the complex of symptoms which the drug will remove.

But the method of alternation throws us back from all such certainty—back to the field of unmethodized clinical experience—back to the trackless wilderness of Polypharmacy and Allopathy.

For these reasons, briefly and imperfectly stated, from the stand-point of *science* we hold that homœopathic medicine cannot countenance nor tolerate alternation. And we have such a faith in the unity of nature as to believe that what sound reasoning shows to be erroneous, accumulated facts will prove to be mischievous and unnecessary.

3. But there is another view of this matter, from the *practical* stand-point. Dr. Hawley says, "Grant that in a given case of the use of two remedies, some one remedy might have been found that would as well, or better, have answered the purpose \* \* \* the question is not, could some other person have done better than I, but have *I* done the best I could?" Undoubtedly, yes! And, so far as the prescriber's own conscience is concerned, this answer exonerates him, even though his prescription were a *faulty* one. And in so far from the *practical* point of view, alternation may be justifiable. But will this question, "have I done the best I could," if affirmatively answered, always justify the pre-

scriber? The practitioner of one year's experience may commit a sad blunder and yet be fully justified, because being inexperienced and young, bad though his error was, he did "the best he could." Would this plea justify the same blunder ten years later? Obviously it would not. Yet is it not the tendency of this plea to satisfy a man with the knowledge he has and with the methods he pursues, and thus to blunt his zeal for greater knowledge and better methods. Remonstrate with the Allopathist for murdering patients with lancet and purge; he replies, "I act according to my light—I do the best I can." And so through all degrees of error and imperfect knowledge. It is a dangerous plea, unless its complement be always added, "I have done the best I can; but, please God, I will do better next time!"

And here we see the advantage of a sound "theory," which our friend holds in so light esteem.

Suppose a practitioner driven, as Dr. Hawley was driven, to alternate. The patient recovers. He has, nevertheless, a conviction that drugs modify each other's action in a way that we cannot foretell, and that the action of the entire organism is so harmonious that "two morbid states cannot coexist independently," and that, therefore, the morbid state being essentially ONE, there must be a possibility of one similar drug-disease to oppose it. A sound theory compels him to the belief that alternation is not justifiable nor defensible on scientific grounds, and that it can do nothing to advance our knowledge of Therapeutics, but rather confuses it. He enters in his mind a protest against the practice to which the necessities of practical duty and his limited knowledge of the *Materia Medica* have constrained him, and turns with redoubled vigor to the study of *Materia Medica*, smarting a little with shame that the exigencies of practice have found him unprepared and have compelled him to a resort which, though successful, his reason condemns, and determined that, the next time, he will, if possible, be equally if not *more*, successful and by a method which shall commend itself to his reason, and shall add to his stock of knowledge for future use.

And if, while "doing the best they can," as each case presents itself, practitioners will earnestly and systematically study the *Materia Medica*, determined to do better and better the next time, we shall have no word of reproach to utter against their temporary expedient of *alternating drugs*.—With broader knowledge they will alternate less and less, and we are very sure that they will see, as we have done, how in case after case, in which they had alternated, the recovery was in truth retarded by the mutual reaction of the drugs; and how the careful individualization of the case and the pains-taking selection of the single drug in the manner that Hahnemann advised will be, in their hands, the means to a success far beyond any ever reached through alternation.

For, though we have admitted and do admit that success is often attained through alternation and that "success is the object of all our labors," we are nevertheless convinced, through our own experience, for we have alternated in our day, that a much larger measure of success is obtained by adherence to the rule of administering single remedies, and that closer study of the *Materia Medica* and sharper investigation of cases will preserve us from those dilemmas in which we are tempted or compelled to resort to alternation.

4. And if we appeal to the experience of Hahnemann, let it be understood that we appeal to the man who both knew *Materia Medica* better than any other man ever knew it, and who had more practical experience than any of us has had. Let us not indulge in the fallacy of supposing that, because we are now practising in the fifty-ninth year since the *Organon* was published, we have had fifty-nine years experience in homœopathic practice and have been studying *Materia Medica* fifty-nine years. On the contrary, our own experience is measured only by the actual number of years we have practised; since in this matter of assimilating to one's own mind the facts of the *Materia Medica* and of seeing the correspondence between these and the facts of disease, we can borrow but little from the experience of others.

If then we, from our little experience of fifteen, or ten, or five

years, appeal to Hahnemann with his venerable experience of more than fifty years of active practice, with his unapproachable knowledge of the *Materia Medica*, of which he might justly say like Æneas "*magna pars fui*," with his unrivalled powers of observation and discrimination—if we appeal to him as "authority" on this question, at once practical and scientific, can it justly be said, that we are seeking some "*authority* outside of and beyond our own reason?" Our colleague appeals to collections of "*facts*." Is not Hahnemann's statement of his practical conclusions, a most stupendous "collection of facts?" Who ever observed so many of them? Who ever observed so well as he? Facts must be received on testimony; who ever reported more graphically and more faithfully than Hahnemann? If we doubt his ability, his capacity, his candor, what are we doing with his *Materia Medica* on the truth of which we risk our patient's lives?

This outcry against "swearing in the words of the master" has come to have a very different meaning from that of the ancient original protest. It was never meant to intimate that the opinion and testimony of him whose abilities had crowned him "king of men" should not have a royal weight of influence. [Eds.

### SCARLET FEVER.

BY P. P. WELLS, M. D., BROOKLYN, N. Y.

(Continued from page 298.)

In the series of remedies from which we may be called to select in the treatment of a case of scarlet fever, characterized by the phenomena of acute inflammatory action within the head, if viewed as standing in the order of the degree of violence which marks this variety of their action on the brain and its membranes, the first place will undoubtedly be given to Belladonna. This appears conclusively if we compare the elements of its pathogenesis given above with those of

the other members of the series. By this standard, (and it is in the order of the violence of their action we propose to consider them) the third place is as certainly to be given to *Hyoscyamus*. The value of this drug in the treatment of inflammatory affections of the brain, resulting from the action of general causes, has been fully recognized. Where the symptoms of a case are similar to those produced by the drug on the healthy, it is no bar to its use that the disease is the result of a specific poison. It would have been but natural to anticipate its efficacy in this class of affections from the success which has followed its use in the non-specific inflammations. That which would have been anticipated of its value in these cases, has been often fully confirmed by practical results. It hence becomes of the highest importance here, as with Stram., to fix the exact place of the drug in the series of those from which we are to choose in our prescription, and to establish as clearly as possible the signs which decide for its selection. Before proceeding to this, the general remark may be permitted, that the sphere of *Hyoscyamus* in the treatment of scarlet fever is a limited, though not an unimportant one. It is rare that it is more than a temporary resort for the relief of some sudden attack of the brain, which, if not controlled, threatens destructive consequences. In such cases it may be of the highest value, though never a curative for the entire disease. Its sphere seems to be limited to cases with acute inflammatory affection of the brain, or to that state between erethism and torpor, which places it in relation to Bell. and Stram., as in typhoid fever, below Stram. This will appear plain on a careful comparison of the symptoms of the three related drugs, which are liable to be repeated in those of the fever. The symptoms of *Hyoscyamus* related to scarlet fever are fewer in number, and those which are most prominent are accompanied by fewer concomitants, showing that it strikes less deeply into the vital forces than either of its allies, and that the disturbances of the vital balance which it produces, are fewer in number as well as