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THE MONTHLY HOMŒOPATHIC REVIEW.

PROGRESS AND PITFALLS.

It is the traditional custom of medical corporations at this season to inaugurate the activities of a new *annus medicus* by such considerations and counsels as seem opportune and advisable. Because as yet we have no collegiate establishment such as usually defines the form of address, we take as our auditory the whole Homœopathic Profession in this country, and pass in review those topics which at this juncture seem timely and befitting.

There are many subjects of interest which might now engage the powers and graces of the orator, but which here fall to be canvassed by his colleague the scribe. Certain which raise their heads with perennial freshness, as our familiar acquaintance, the "Law of the Dose," and "the relation of Pathology to Homœopathy," etc., call in this place for no new treatment at length. But as the "itinerant straw" has been pretty uniformly of late shewing in the direction of "to-morrow's hidden season," we shall descant on this topic of a forward movement, and review those principles of action which will ensure our progress, with a criticism of those others, often specious and alluring, which if adopted, will as certainly extinguish our virility and ensure our extermination.

There are certain pitfalls which call for especial avoidance in the onward course of our advance in real influence and value.

The first pitfall is the irrepressible tendency, especially among the weaker brethren, restive at the absence of dramatic reconciliation, to anticipate by some "short cut" the tardy but assured verdict of time on truth. These impatient souls would seek to further the recognition of our views by some manipulation of our position, rather than to adopt the more arduous, but only effective course of developing our internal scientific resources. The semblance of union is to them an important achievement; the substantial basis of concurrence may be left to take care of itself. Juggling with names is commonly an important element in their strategy; the shifting insubstantialities of diplomacy more potent arguments than the intact preservation of truth springing from the solid ground of nature. Let it be graven on our minds that the most potent influences that make for position and standing are those that insensibly arise from the quality of our work; that valuation comes from the persuasive consciousness in men's minds that we have something of value in us; that the influences which ensure credit and distinction are not created by organisation, but arise as collateral products from professional worth and work.

No sounder service was ever rendered to those under temptation to precipitate results than that of the late Prof. CHARLES PEARSON, when he showed *usque ad nauseam* that political and social schemes and devices, planned to bring about wished-for results, often quite missed their mark, ultimately achieving consequences either unwelcome or unexpected. It is especially dangerous in politics or sociology to force the pace; the bases of recognition and reconciliation are the reciprocal movements of both sides towards union, not for nebulous sentiment, but for some definite aim, some specific purpose.

The second pitfall is an ill-balanced desire for unification with the majority. This commonly proceeds from those who have lost touch of the all-important functions of minorities in scientific progress; who are forgetful that there should be a reciprocal call for union from both

sections; forgetful that an honourable independence is infinitely preferable to a union which imposes restrictions on free action. The theory of Comprehension, well known in theological circles, is not practically found elastic enough to suit all collateral needs, or to be applied regardless of stages of development or fitness of time, or to be a workable scheme without a central control armed with arbitrary power. MATTHEW ARNOLD had a true conception of this difficult problem when he hinted that the view of the majority was usually an erroneous view. JOHN STUART MILL has emphatically laid it down that individuality is essential to development; and this applies to societies no less than to individuals. And no union with the official establishment will in the least degree tempt the sounder heads among us, unless and until there are adequate safeguards for the unrestricted development of homœopathic science on its new lines.

The third pitfall—a more constant and besetting danger than recognised—is the misleading substitution of politics for science; the regarding of “paper constitutions” as equivalent to real progress in knowledge, the redistribution of professional positions as of the same worth as the further development of our scientific resources. This is the fundamental error of the professional politician, to whom the world owes little; it is the blunder nearly always adopted by his imitators—to whom we are likely to owe less. These prefer juggling with words and phrases to the fertile work of the clinical observer, the sterile machinations of those who concede and compromise, to the solid and enduring facts of the original worker. Needless to say that any system stands or falls by its inherent scientific worth, and not by the pushfulness of its party managers. These, to paraphrase an old saying of MARSHAL BLUCHER, concede with the pen for “peace” or “union” what has been hardly won for knowledge by the toil and sweat of the discoverer.

Such forget that the permanence of any scientific cause is assured, not by its past, but by its expanding present and amply-planned future; that position and pre-eminence have to go, unless they keep pace with the eternal thirst for new knowledge; and that all the best brains of any system or section of science—especially a controverted one—are required for the ceaseless addition of new facts, the discovery of new laws, the construction of new methods,

leaving to the course of time and the rectifications that it brings, the adjustment of such secondary matters as place and position. A cause that troubles itself about how it appears to others, its position and its rank, and concerns itself less about the ceaseless working of its resources, is in a bad way.

“ To those who regard the politics of homœopathic medicine as of more importance than the science, our message is clear and pointed. They can easily purchase professional peace at any time at the modest price of homœopathic death.

Already in the notice of “ pitfalls ” there is foreshadowed the character of the proper and desirable course we should follow. A simply negative policy—one of compromise, of concession, of mere statecraft—will not satisfy us. Nor will any course be approved which entails a material breach with our scientific or historic past. We owe too much to the life-work and the noble traditions of the men who have gone before, to detach ourselves from the course and the methods which they have laid down, and which we have in daily practice so often verified.

What then is our positive policy, and what the considerations which must govern its development ?

First in importance comes the paramount, the urgent necessity of seriously setting to work to increase the internal value of homœopathy by trained observation and experiment. There are among us capable judges who hold that any “ backwardation ” in the influence of homœopathy is due to the spirit of discovery having fallen into desuetude. Provings are the life-blood of our art ; we want new provings of drugs, old and new, with every detail of information, subjective and objective, that can be obtained ; checked by renewed observations and reinforced by instrumental methods. We now almost wholly rely on the work of our predecessors produced in bygone time, and make but little continuous effort to outvie or enlarge their views and observations. Take any one of the natural sciences : how during the last twenty-five years its laws have been re-modelled, its facts

and observations perpetually re-examined, its scope increased by new and totally unexpected discoveries, and its practical application correspondingly heightened in extent and accuracy. Look on this picture and on that, and silently draw the moral.

Proving, however, do not exhaust the field of homœopathy urgently calling for tillage. There is room for the establishment of a new reputation in an exhaustive inquiry into the place, the scope, and the usage of animal extracts in homœopathic treatment. There is daily need for the definition of the frequent interference of mechanical or physical obstacles in the action of the well-chosen homœopathic remedy. There is the almost unknown region of prophylactic homœopathy—the introduction of far-reaching drug force, rivalling other natural agencies, for the check of disease tendencies in their incipency. We could indefinitely extend the list of problems calling for early and thorough investigation, and for lack of the knowledge thus obtainable our hands are too often crippled and our efforts rendered nugatory.

Next in importance comes the consideration that as trained and competent scientific observers are not as thick as leaves in Vallambrosa, a training institution in research is essential for the turning out of work by trained observers. Not every one is a born prover; not all are by capacity or training fitted for examining into the recondite problems of homœopathy; nor are the conditions of time, and place, and daily work, as well as personal fitness, to be ignored. To produce accurate and continuous work in the scientific data of homœopathy we require permanent and trained supervision; a central institution in fact, fitted, elaborated, devised for scientific and literary work, for the training of men in the art of proving, for the investigation of all topics relating to disease and its homœopathic treatment.

Third, if we require more of the spirit of discovery infused into our procedure, as much or still more do we need the effective training and the proper equipment of those by whom the practice of homœopathy is to be carried on in the future. It will seem scarcely credible to our descendants—and no less discreditable to ourselves—that those who seek and require tutorial instruction in homœopathic *Materia Medica* and therapeutics are, outside of clinical demonstrations, practically left to their

own devices *plus* bookwork. A technical corporation that does not ensure the training of its (post graduate) students in its technical knowledge may expect to become technically a nonentity. But so much ink and paper have been devoted to this necessity in other places that to further dwell on it would be mere prolixity.

These, apart from subsidiary matter, are the chief elements in any positive policy worthy of the name. Certain practical considerations govern its conduct and its successful development. It is an expensive course; though where human lives and human happiness are at stake the question of expense should not be an incubus. It is an arduous and difficult policy; the usual risk is to take too superficial a view, to think too lightly of the great task, and to fail in the proportion of means to ends. It is a slowly evolving policy; the superficial or political busybody demands some dramatic result, some immediate great issue, some catch-penny, pseudo-epoch-making discovery. From work undertaken in this shallow spirit, the powers defend us!

When any sound scheme for any new departure is set forth and worth its salt, it usually stimulates the production of half a dozen flighty alternatives which are not. This secondary crop of suggestions, good for the purposes of controversy, may here and there furnish a detail for incorporation in the fuller plan. But when their adoption would mean a hasty total alteration in the plan of action, or an ill-considered change of front, then the less time spent on their serious consideration the better. One figment of hoary antiquity, almost certain to see the light when plans of action are propounded, is that battered survival of previous differences, "union with the profession." This resuscitation has a peculiar fascination for some minds, who require something to lean upon, some constant re-assurance of the propriety of their position. Such pusillanimity would give the *quietus* to all virile initiative, all natural bursts of life, and wither and destroy the very roots of our existence, on the ground that our growth and extension may be disagreeable to "the others." How different was the counsel of the lamented Dr. HUGHES. "But as regards the ground taken up by the leading maintainers of homeopathy, . . . we have no foot to stir and no pardon to ask. We earnestly desire recognition and reconciliation, but

these can only come about by a proud recognition on the part of our colleagues of the soundness of our contentions." Again, "Let us all stand firmly in our ranks, doing our duty where providence has placed us. . . . And one further caution I must add: that it is not for us to take the first step towards the reconciliation we nevertheless invite and devoutly desire. We cannot do so without misunderstanding."*

Union with the political establishment of the profession may be an ideal to dream over; practically it is a problem of the extremest complexity.

The time is not yet ripe for union. Neither we nor the majority have as yet qualified for it; and to endeavour to carry it through without previous careful preparation to ensure its working character, would hopelessly bungle and probably indefinitely postpone any union of the least value. When we have an equipment for teaching, for examining, and for post-graduate training; when our hospitals are multiplied, and our developments all over the country enable us to more easily obtain surgical and special assistance within our own ranks, then we may be welcomed as allies in the sphere of scientific discovery, as pioneers in the effective practice of the healing art. When the larger body shed their prejudice for exclusiveness, and lay themselves open to the play of discoveries and facts from without; when they lose the habit of intolerance, and substitute the catholicity of intelligent and cultivated men; when they cease to be frightened at names, and recognize the true and substantial actuality hidden from them by traditional prejudice; then the course may be shaped on their side for something like approximation of the two schools of medicine. But—however keenly we feel our present isolation—to seek fraternisation, a non-established body, a discredited remnant, an association of heretics, would be to bring dissolution on our fellowship, to ruin our self-respect, and to make us pass through the Caudine Forks with a vengeance. We desire to be affiliated with all the honours of an independent yet allied people, not a subject race squeezed into seeking incorporation with a masterful opponent.

The course of affairs in America at the present time

* The Principles and Practice of Homœopathy, London, 1902.

lends support to this view. The untrammelled development of homœopathy in the United States, its recognition by the State, the excellent work done in all departments of medicine and surgery by our colleagues, have engendered that respectful forbearance which an important and numerous association usually ensures. So that earnest apostle of unwholesome sectarianism, Mr. ERNEST HART, discovered when he undertook a fruitless mission across the water to stir up the muddy waters of Pharisaic exclusionism. Our colleagues in America, having made their progress felt, have now to watch the beginning of a movement—*proceeding from the other side*—to discover some *modus vivendi* by which all who are earnest workers in medical science can meet as colleagues and co-operators. The olive branch has been for the first time held out by the dominant sect; but these rightly recognise that to attempt any union of parties as at present constituted would be as the attempted union of oil and water. So also the President of the American Institute of Homœopathy, in pronouncing on the question, adjudged the time as not fitting and the conditions not satisfactory for any active participation by the American homœopathic profession in this movement at this time. President WOOD's capable and discreet handling of this question entirely commends itself to us, and falls into line with the views we have already urged for adoption in this country.

Our plain path then is not one of showy aggrandisement, much less one of self-abasing concession, but that of energetic, continuous, spirited work to develop the unexplored, and to work out the unexhausted, resources of the sphere governed by *similia similibus curentur*. This is our solution of the problem of the position and recognition of homœopathy; not that we faintheartedly call on others to help and better us, it is that we energetically set to work to help ourselves.

PRESENT-DAY THERAPEUTICS IN THE OLD SCHOOL.

FOR a record or estimate of the progress or otherwise of therapeutics among our brethren of the old school we always look with interest at the "Address on Medicine"

delivered at the Annual Meeting of the British Medical Association, which this year was held in Manchester in the end of July and beginning of August. The usual tone of such addresses is one of congratulation and of mutual admiration at the vast advances in "medicine" which are made, and include nearly everything except the main issue in that department, namely, therapeutics. All the advances in physiology, pathology, electricity, X-rays, serum-treatment and hygiene are brought forward as evidence of the strides that "medicine" is making, but of therapeutics we hear next to nothing, for the best possible reason that there is next to nothing to record. The address this year was given by SIR THOMAS BARLOW, Bart. (we take our report from the *British Medical Journal*, of August 2nd), who urged that "the advances in the treatment of disease have been real and satisfactory in proportion as they have been in harmony with a complete knowledge of the natural history of disease." This proposition is, of course, so far true, as unless we understand the natural history of the disease we have to treat we are at a disadvantage. But this is only one half of the knowledge that is necessary to the physician. The other half is even more important, namely, a knowledge of the natural history of drug-action, without which our knowledge is not only one-sided, but it is impossible to make use of drugs in any rational way. We must have this double knowledge in order to see the relation between the disease and the remedy, and to discover the law which harmonizes the one with the other. Without the perception of this relation, this law of mutual relation, the rôle of therapeutics resolves itself into mere empiricism. Hence, in the very beginning of his address, SIR THOMAS BARLOW says, "But at all events—and let us frankly admit it—the basis of our practice is empiricism." He then goes on to say, "But there is good empiricism and bad. The good empiricism continually strives to follow nature's lead, and shed that which is foolish and useless while preserving that which is essential. And good empiricism, by carefully recording cases, noting the results of changes made first by chance and then by design, may at length assimilate to a truly inductive process, even though there are many unknown quantities. But as nature herself seems sometimes to fail, and leaves behind vestiges and survivals in our bodily conformation which

may give rise to trouble, so we also in our treatment sometimes preserve methods of long past utility which may now and again be harmful. It is in the period of half knowledge that the practice of medicine has often cut a sorry figure. For theoretical lines of treatment, based on incomplete physiological and chemical data, have often produced melancholy failures. But the outcome of the new experimental pathology of the last quarter of a century, controlled by clinical observation, applied and tested by clinical experience, has justified us in claiming that at length our work has ceased to be fairly described as groping in the dark, and that in some definite instances medicine has justified for itself the designation of an exact science."

If this statement of affairs was comforting and invigorating to the minds of his hearers, they are easily satisfied. "Good empiricism," we are told, "may at length assimilate to a truly inductive process." It has been long in doing it—generations, in fact—and yet at the present day the goal seems as far off as ever. The present is still the "period of half knowledge," knowledge of disease without a knowledge of the pure action of medicines, and without a perception of the true relation existing between disease and drugs. Hence "medicine has often cut a sorry figure," and still does, and it, moreover, will continue to do so. Truly does SIR THOMAS BARLOW add, "For theoretical lines of treatment, based on incomplete physiological and chemical data, have often produced melancholy failures." And they are sure to do so to the end of the chapter. He then, in the passage quoted above, is able to congratulate himself and his colleagues "that at length our work, too, has ceased to be fairly described as groping in the dark, and that in some definite instances medicine has justified for itself the designation of an exact science." He reminds us of the man who was described as a "pious lad, as he was thankful for small mercies." "No longer fairly to be described as groping in the dark"! How delightful to contemplate this stage of advance in therapeutics, even though it is thus negatively expressed! And this is the twentieth century! The law that can not only prevent us groping in the dark, but which sheds the full sunlight of day on medicine, harmonizing disease with drug-action, giving the key to the use and value of otherwise useless "pharmacological" facts, is staring

us in the face every day, but our friends of the old school persistently shut their eyes to it, and resign themselves to a hope that "at length" they may arrive at "a truly inductive process." It is positively melancholy. SIR THOMAS then proceeds to refer to "some notable advances in the healing art," and first comes, as a matter of course, the treatment of diphtheria by antitoxin. The question of the value of this treatment is not by any means finally settled, and especially as compared with good homœopathic treatment. There are such varieties in cases of diphtheria, ranging from an extremely mild form which might, and does, get well with the mildest treatment, to such severe cases as to render death inevitable, that statistics, said always to be fallible and capable of proving anything, are here specially fallible. Still, sufficient evidence has been adduced to show that under the antitoxin treatment as compared with allopathic treatment the mortality in diphtheria has been much reduced, and we freely admit that it is a "notable advance" in medicine. But it must be remembered that when compared with skilful homœopathic treatment the contrast is by no means striking; and, moreover, it is a question which has not yet elicited unanimous opinion even in our own school of medicine, whether the antitoxin treatment is not in reality homœopathic. We cannot here go into the arguments in favour of this view, but there is a great deal to be said in its favour.

SIR THOMAS'S second "notable advance" is the serum treatment of tetanus, but here he has to speak in a very qualified manner. Thus he says, "With regard to the tetanus antitoxin, although the experiments on immunization of animals have not furnished us with a successful method of dealing with the severe and acute cases of human tetanus, yet some benefit has been gained in the chronic forms." This is very faint praise or cause for jubilation. So with his next example, Pasteurism and rabies, of which he says, "There surely can be no doubt of the prophylactic value of Pasteur's method so far as those who have been bitten by rabid animals are concerned, although no beneficial effects follow their employment in the developed disease." He then speaks of "the great triumph of English protective medicine, namely, vaccination." But vaccination is a pure piece of homœopathy. Next comes tuberculosis and the tuberculin treatment.

SIR THOMAS says, "It cannot be denied that the results obtained have been on the whole unsatisfactory and tentative." Of the recent "boom" in open-air and sanatoria he says, "But surely the teaching of pathology is that the fortifying of the general resistance of the individual is the most important indication of all. Real, true advance always tends to simplification, and the great fundamental therapeutic remedies are the employment of fresh air, sanatoria, of good food, and of regulated exercises. This, surely, is the inwardness of what has been called "the sanatoria treatment." These means are of course right, and only common-sense, but we object to have them called "therapeutic remedies." Therapeutic remedies are drugs, of which we hear nothing, and not general hygienic arrangements, which are common to all schools of medicine. Further, it is evident that this new fashion of sanatoria, with their rigid rules in the treatment of phthisis, is being carried to extremes, as all new modes of treatment are in the old school. Already there are signs of a reaction, and we hear frequently of cases that were not at all benefited, and of others that were improved for a time, but soon after relapsed into their former state and died. It is beneficial in some cases, but the reverse in others. We shall probably soon be told that it is advisable in certain selected cases, but that it must be reserved for such. But, in any case, we have not a word from SIR THOMAS BARLOW on the value of therapeutic treatment proper. He is probably here in the position of "groping in the dark," and hoping for better things.

Next comes malaria, and Dr. PATRICK MANSON'S discoveries as to the relation of the mosquito to malarial fever. These investigations are, no doubt, of the highest importance and interest in studying the natural history of this disease, but they shed no light on its therapeutics, or on the action, so markedly curative, of quinine, arsenic, and other ague remedies. Acute rheumatism is next included as among the "notable advances," and consists in the suggestion by Drs. MANTLE, POYNTER, and PAINE of its microbic origin. The conclusion SIR THOMAS BARLOW comes to is as follows: "This seems to be a real contribution to the natural history of the disease, and when we have learned something about the rheumatic toxins, and about the changes which supervene upon their formation, we shall probably get a more satisfactory

explanation of the action of the salicylates than is at present forthcoming, and we may hope for further light as to the treatment of the complications and refractory forms of this common disease." This is truly comforting.

SIR THOMAS is a very hopeful man, and we trust he succeeded in inspiring hope in his audience, though we should rather doubt it. The "differentiation of meningitis" is the next illustration of "notable advances" by means of bacteriology, but when it comes to the therapeutics of the differentiated forms of this disease, here is what we are treated to: "There is scarcely any group of diseases in which the resources of therapeutics have been so discouraging. It is true that in the first and third of these forms there have been a certain number of recoveries, and, although for my part I would not relinquish the use of mercury, yet it seems doubtful whether drugs have played any important part in the recovery of the successful cases. Although lumbar puncture and drainage from the cervico-occipital region have temporarily relieved pressure symptoms, they cannot as yet be strongly urged as curative measures. May we not reasonably hope that serum-therapeutics will one day find some tangible success in this group of intractable diseases?"

Here are all the "notable advances" in "medicine" that SIR THOMAS BARLOW has to give in his address for the encouragement of his colleagues. Where the therapeutics come in is left to our imagination; we are left "groping in the dark," and advised to live in hope and work away at the natural history of disease till the dawn appears to lighten the darkness.

But supposing all these examples of the "notable advances" were therapeutical, and therefore practical in the actual cure of disease, are they any evidence of a real advance towards a law in therapeutics? We cannot see that they are. They would be simply bits of treatment here and there, and part of the general empiricism which SIR THOMAS frankly admits rules the practice in the old school. Empiricism cannot do other than discover "tips" in treatment now and then, but such isolated and chance hits, when successful bring no real advance. We are just where we were. There can be no essential, real, and permanent advance in therapeutics till a law is found which gives the key to the relation existing between

disease and drugs. Such a relation *must* exist, unless we suppose that disease and its cure are not governed by the "Reign of Law" of Divine purposes, which is manifest in every other sphere in the universe. Such a law does exist; we have been preaching it for the last hundred years, and are still doing our utmost to spread the truth, and to get our friends of the old school to study it and test its truth in practice. But they persistently shut their eyes, and will not even look at it. Till such blind "groping in the dark" ceases and our opponents open their eyes to see the light which is shining on their faces, medicine—therapeutics—cannot do otherwise than remain where it is, "in the period of half-knowledge," and continue to "cut a sorry figure," to use again SIR THOMAS'S own words.

Almost synchronously with SIR THOMAS BARLOW'S address we find a less authoritative pronouncement on therapeutics in a paper entitled "Our Attitude to Drugs," by Dr. WALTER JORDAN, Assistant Physician to the Children's Hospital, Birmingham, and published in the *Midland Medical Journal* for July. This paper is not of much importance, save as indicating a sample of the existence in the ranks of the old school, of which we were fully aware already, of another type of physician, who has got a stage in advance of actually "groping in the dark," who sees some light, but for reasons of his own, which we can easily guess without the aid of much critical insight, objects to call a spade otherwise than an agricultural implement, who will in no wise admit that he ever makes use of such a vulgar tool, and who feels it his duty to sneer at those who do, and to misrepresent them at the same time. The general substance of the paper is good enough and unobjectionable. Dr. JORDAN points out that the "earnest inquirer, facing for the first time the big question of drug-therapeutics, must, I think, have his attention immediately arrested by three points: (1) The enormous number of remedies already in use; (2) The constant additions to that number; (3) The very small store of precise knowledge we have with regard to the action of these in disease." On the first two points his remarks are very common-sense; as to the third he very properly says, "Only by a determination to know what a drug can do in the body, and what is the condition

of the body into which we introduce the drug, can we avoid falling into purely hap-hazard and fruitless methods." And again, "It will not do to adopt an ultra-scientific attitude in this matter, and carry a disdain for empirical methods too far. In the past empirical methods have been relied on almost solely to reinforce our pharmacopœias, and while the method resulted in the overburdening of those volumes with preparations and substances destined to be dropped after a longer or shorter stay, it resulted also in the inclusion in them of powerful remedies whose action we are now beginning to understand, so that we can employ them in a rational manner. Our methods to this day are largely empirical, and must be so, because we have to take practical steps to get rid of disease when it is not possible to make a correct mental picture of all the variations from the physiological normal, and because we have at hand a number of drugs whose favourable influence over disease is unquestioned, but of whose precise mode of action we are in absolute or profound ignorance." But it is when Dr. JORDAN comes to speak of doses of drugs that we begin to feel interested. After saying very properly that "it is, I think, necessary to deprecate any vagueness about the dose of drugs," and that when we are told that liquor arsenicalis has been given, it is necessary to know the amount given, and how often; he adds, "The same drug, we all know, may have exactly opposite effects in small and large doses. On this fact the homœopaths built up their heresy. It was no sin in them to use doses infinitesimal in our calculation, nor drugs which in the observation of others were barren of result. It was a sin to say that every condition of disease must be cured by treatment based on a theoretical system." This is really too much of a good thing. To begin by saying "The same drug, *as we all know* (the italics are ours), may have exactly opposite effects in small and large doses," and then to add "On this fact the homœopaths built up their heresy," is extraordinary. If "we all know" this double and opposite action of drugs in large and small doses respectively, the whole question is admitted. We have but to act on this fact which "we all know," and then we have homœopathic treatment. Then where is the "heresy"? Dr. JORDAN evidently knows perfectly well what is the basis-fact of homœopathy; he, as we shall presently see, knows it in practice, and

yet he speaks of this practice as a heresy. We presume that a free translation of "*noblesse oblige*" accounts for this remarkable phase of mind and speech. He can speak freely and frankly about empiricism and its prevalence in his own school, but when he writes of a practice based on a "fact" which "we all know," his tongue is tied. He cannot call a spade a spade, but must throw dust in his readers' eyes by calling it a heresy, and then using such patronizing sentences as the following: "It was no sin in them (homœopaths) to use doses infinitesimal in our calculation, nor drugs which in the observation of others were barren of result. It was a sin to say that every condition of disease must be cured by treatment based on a theoretical system." A sin, forsooth! Dr. JORDAN'S ideas of what a sin is are unique, if he really means what he says. But such terms are beneath us to discuss. As to the "drugs which in the observation of others were barren of result," we presume he refers to drugs which in the crude state are practically inert as medicines, but which only develop their medicinal action by trituration. But he takes good care not to state this, or to tell us who the "others" are. And again, to state that homœopaths "say that every condition of disease must be cured by treatment based on a theoretical system" is really too much after having just said that the double and opposite action of large and small doses is "the fact" on which homœopathy is based, and that it is a fact which "we all know." If Dr. JORDAN considers this an honest and straightforward mode of writing, his views are, as his ideas of what sin consists in, unique. But perhaps our early training has been misguided on such matters. He then, after stating that in certain cases it is necessary to give full doses of a drug, treats us to the following: "On the other hand, I am quite a believer in small doses. Everyone now knows the value of ℥j doses of liq. arsen. in certain conditions; not quite so generally known is the use of fractional doses of calomel, by which results are obtained quite different to those the result of doses of one grain or upwards. A similar result may be obtained by giving very small doses of podophyllin as compared with those usually given. A writer in the *Queen's Medical Magazine*, which, if sometimes caustic, is never dull, seemed to hold up to scorn a prescription in which Sodii bicarb., gr. j, was the principal ingredient.

Well, with the following prescription, Sodii bicarb., gr. j, Vini ipecacuanhæ $\mathfrak{m} \frac{1}{2}$, Aq. M. P. ad $\mathfrak{z} j$, given every one or two hours, I have obtained some of the happiest results in the treatment of catarrhs of the mucous membranes."

Why now, does he not tell us what the "certain conditions" are in which one minim doses of liquor arsenicalis are of value? Why does he not tell us in what cases he finds "fractional doses of calomel" useful, and "by which," he tells us, "results are obtained quite different to those the result of doses of one grain or upwards"? Why does he not tell us in what cases he uses "very small doses of podophyllin as compared with those usually given," and which produce such good results? There is the rub. Had he done so he would have laid himself open to the insinuation that he was dallying with "the heresy," which, of course, would never do for an old school physician, whatever he thought in his inmost heart. But "we all know" that in order to get the desired effect from allopathic medicine small doses are useless. Dr. JORDAN would not treat his patients with one minim doses of castor oil for constipation, or with the same dose of tincture of opium for sleeplessness. Whereas "we all know" that since Dr. RINGER's book on Therapeutics was published the cases where small doses of drugs have been used in the old school are just those where those drugs are homœopathically indicated, where they are contra-indicated on any other rule of practice, and, moreover, where such cases would be materially aggravated by the doses of these drugs which are "usually given." Dr. JORDAN's last example of the small dose and its virtues are precisely on the same lines. Half-drop doses of ipecacuanha wine, combined with, it is true, one grain doses of bicarbonate of soda, have yielded "some of the happiest results in the treatment of catarrhs of the mucous membrane." He here evidently means catarrhs of the gastro-intestinal mucous membrane, and probably in children, as they are his speciality. Had he meant bronchitis he would have said so. Possibly one grain doses of bicarbonate of soda may have helped, but that ipecacuanha produces in full doses, mucous catarrh of the stomach and bowels, "we all know." Dr. JORDAN will not require us to state an authority for this fact, as his knowledge of the standard works on *Materia Medica* is, no doubt, sufficient without our having to refresh his memory. And here we have him speaking

so confidently of the value of such minute doses as half a minim in disorders similar to those produced by full doses of the drug.

The inference is obvious. In a so-called liberal profession and in this twentieth century, when the public are waking up to an interest in therapeutics and in medical subjects generally, it is positively melancholy to see that a physician, who evidently knows more of homœopathy and its benefits than he cares to say, should feel bound by what are really trades-union rules to keep his knowledge under lock and key, and make use of this knowledge in a more or less stealthy way, and even then to throw dust in the eyes of those who might make it disagreeable for him if he were suspected of having "views" on homœopathy, by abusing what he knows is the truth. That there are many such in the old school "we all know," but when we see it in print, carefully veiled, but none the less evident to anyone who takes the trouble to put two and two together, it becomes necessary for us, in the interests of truth and of the great cause we have at heart, to notice such phenomena, as showing how steadily, but quietly, the doctrine of *similia similibus* is leavening the old school. If only our quasi-opponents would insist on claiming the same liberty of opinion and of practice that we do, and would have the moral courage to say openly and frankly what they really think, and what is the meaning of "facts" which "we all know," therapeutics would enter on a new and healthy life, and be no longer "groping in the dark," and employing purely empirical treatment. It would be re-animated to "notable advances" by the bright light of a definite and sure law as a trustworthy guide in the treatment of disease. But till this liberty of thought, speech, and action is permitted in the old school no real advance in therapeutics is possible.

Dr. JORDAN's peroration is excellent, and as we heartily endorse it, it gives us pleasure to conclude this article by quoting it, as follows: "What I have tried to do is to show that while we have in drug-taking an easy and popular method for the banishment of disease, in the giving of drugs for this purpose we have a most complicated and difficult problem. I have urged the formation of a habit of mind with regard to drug-giving which shall be scientific, cautious, watchful, and painstaking in every detail. Only a high and rigid principle of this sort can

save us from drifting into barren, and possibly harmful, methods of drugging. If, on the other hand, we honestly try to approach our very nearest to the standard set up, we may hear with our withers unwrung the cynic's description of the art of medicine as the putting of drugs of which we know little into bodies of which we know less."

CHAIR OF HOMŒOPATHY IN THE UNIVERSITY OF WURTZBURG.

IN our last issue we had the pleasure of announcing that the Bavarian Chambers had resolved to institute a Chair of Homœopathy in one of the Universities, but we were not informed as to which University was to have this honour conferred on it. We now learn from the Berlin correspondent of the *Morning Leader* that the Chair of Homœopathy is to be in the University of Wurtzburg. The correspondent of the *Morning Leader* says: "The choice of Wurtzburg is remarkable, as it is from this University that the strongest opponents of homœopathy have come. The Bavarian Government has promised the necessary funds." No piece of news for long has rejoiced our hearts so much as this. Nothing is more fitting than that Germany should thus recognise in this manner one of her greatest sons—Samuel Hahnemann—after over a century of neglect, opposition, and obloquy. He has been such a benefactor to the whole world that, though a son of Germany, all nations are proud to recognise in him one of the very greatest geniuses that ever lived, a genius who discovered the great law of similars, and who, not content with mere speculations on the newly-discovered law, developed the system of treatment based on it so perfectly and practically that what he wrote and said is as fresh and "up-to-date" to-day as when he lived, wrote, and practised. He was far ahead of his time, and has revolutionized the practice of medicine; and the time will come when the entire profession, and, following their lead, the entire lay-public of the world, will unite in placing Hahnemann on a higher pinnacle of fame than any other of the great men of medicine.

We sincerely and warmly congratulate the Bavarian Government and the University of Wurtzburg on this

great step in the cause of scientific medicine. No fact could more clearly prove how certainly, in spite of long and bitter opposition, truth must prevail, and how homœopathy in Germany has so steadily won its way. It can no longer be styled by our opponents a "dying faith" or actually "dead"; on the contrary, a new lease of life and vigour is now opening out for it. To have the official *imprimatur* of the Bavarian Government and of the University of Wurtzburg is an enormous gain, as everyone knows its value and meaning.

We only wish that in this country we could see signs of similar action on the part of the Government and of the Universities. Such must come sooner or later, and we hope it may be sooner than we are sanguine enough to expect. Meantime, with the encouraging action of Bavaria and Wurtzburg, let us not slacken our efforts to fight openly and fearlessly for the truth, but rather redouble our activity in propagating the grand system of homœopathy, so as to hasten by all means in our power the day of its triumph as the dominant practice in the profession.

SIX CONSECUTIVE YEARS' WORK AT A PRIVATE NURSING INSTITUTION, WITH NO MORTALITY.

By GEORGE BURFORD, M.B.,

Physician for Diseases of Women to the London Homœopathic
Hospital.

WHEN abdominal ailments assume a form requiring operative relief, another important question falls for consideration beside that of operation. Should the surgical procedure be carried out at home, or is transference to a Nursing Home desirable? I make this contribution to the subject of Nursing Homes, in that the use and value of these institutions are frequently canvassed when critical illness occurs in a household, and surgical aid is in request. Such institutions, when the establishment is well found and the nursing equipment complete, lend material aid to the satisfactory progress of a surgical case, especially if of abdominal type.

The experience I here detail has been of considerable weight with me when requested to advise for or against the detachment of a patient from home surroundings, and

removal to the ordered routine of a Nursing Home. However residentially perfect, a private dwelling-house cannot always fully conform to the aseptic requirements of modern abdominal surgery; often some unsuspected abrogation of the aseptic ideal is discovered too late for rectification; and where the issues of life and death are concerned, no trouble can be too great, and no detail too minute, if these tend to assure a good result.

Should there be the least doubt as to the sanitary appointments of the house on re-testing—this should invariably be done before abdominal operation in a private residence—or should the locality be unfitting, or the detail of the establishment not easily lend itself to the heavy strain of an abdominal case, then, if the patient's condition allow of removal, there should be no hesitation in adopting this course in the vital interests of the sufferer. Every abdominal operation, without exception, risks the life of the patient, and it is my rule of practice to be hyper-particular before operation, to avoid regret thereafter.

From the various nursing homes whose resources I have had occasion to utilize I have taken for the purposes of this article that one to which I customarily send grave cases coming up to town for operative relief. The period of time embraced is six years; the cases number nearly fifty, and recovery after operation ensued in every case without exception.

About two-thirds of these cases were major operations, and the great proportion of these abdominal sections, presenting every grade of difficulty and many varieties of type. Ectopic gestation with suppurating foetus, cholecystotomy, cancer of the cervix, cancer of the fundus, uterine fibroids, ovarian tumours, double pyosalpinx, mammary carcinoma—these with others came for adequate surgical procedure. It was customary for those cases to recover with but slight variations of temperature—in several the highest point reached was 99.8°; I do not recollect a single instance of stitch abscess, and in the great majority the recovery was uniform and uneventful.

I am not one of those who can find no work of usefulness for homœopathic therapeutics in assuring and abbreviating the convalescence. In some of the cases the surgical complications—shock, pre-existing and extensive septic conditions—were grave in the extreme, and in many the

constitutional state, over and above the local defects, required much care and supervision from the physician's side. That recovery has ensued in every case, I attribute no less to the efficacy of the adjuvant therapeutics than to the thoroughness of the aseptic and surgical methods adopted.

The clinical histories of some selected cases I subjoin. In each instance I have given point and prominence to the distinguishing feature of the cases narrated.

(*To be continued.*)

STORY OF MY CONVERSION TO HOMŒOPATHY.

By MAHENDRA LAL SIRCAR, C.I.E., M.D., D.L.

We have much pleasure in reprinting from the July number of the *Calcutta Journal of Medicine* the following extremely interesting paper with the above title by its distinguished editor, Dr. Mahendra Lal Sircar. The mode of conversion of an allopath to homœopathy is always interesting and instructive, but we have seldom read any such account that is so fascinating as this is, and which shows the strong character of the man and his noble, fearless courage in standing up for his convictions of the truth, amidst circumstances which would have frightened a weaker man into smothering his convictions. Dr. Sircar was not only the pioneer homœopathic physician in Calcutta, but has been at the head of his profession ever since, devoting all his spare time to the advancement of the cause he has so much at heart, and has, in fact, made homœopathy in India what it now is. Long may he be spared to carry on his noble work. The story of his conversion has a curious and remarkable likeness to that of the late Dr. Horner, of Hull.

Dr. Horner was President of the Provincial Medical and Surgical Association (now the British Medical Association) in 1850, when it met in Hull, where Dr. Horner practised as a physician, and was Senior Physician to the Hull General Infirmary. In the following year, 1851, the Association met at Brighton, and there and then Dr. Horner was elected Perpetual Vice-president, and he had charge of the committee that prepared the notorious

anti-homœopathic resolutions. As some of our younger colleagues may not remember the gist of those resolutions, we give them here: That "homœopathy is so utterly opposed to science and common-sense, as well as so completely at variance with the experience of the medical profession, that it ought to be in no way or degree practised or countenanced by any regularly educated medical practitioner. That it is derogatory to the honour of members of this Association to hold any kind of professional intercourse with homœopathic practitioners. That there are three classes of practitioners who ought not to be members of this Association, namely: (1) Real homœopathic practitioners; (2) Those who practice homœopathy in combination with other systems of treatment; (3) Those who, under various pretences, meet in consultation or hold professional intercourse with those who practice homœopathy." These resolutions have never been rescinded. In Hull at that time the members of the medical profession used to meet once a fortnight for coffee and discussion of papers, etc. After the Brighton meeting just referred to, Dr. Horner, as having taken a leading part in the passing of the resolutions against homœopathy, and being the Senior Physician to the Infirmary and the leading physician in Hull, was asked to read a paper against homœopathy. This he readily consented to do. But like an honest man as he was, it occurred to him that he really knew nothing about homœopathy, and that it would be only honourable and wise to make himself acquainted with the subject on which he had promised to write. He therefore enquired at the chemist's for books, and was referred by him to Dr. Atkin, the homœopathic practitioner in Hull. To make a long story short, the result of Dr. Horner's investigations, first by reading and then by carefully and systematically testing it in practice, was to make him a convert to the new system. He told his colleagues and friends that if he read his paper it must be in support of homœopathy, and not against it. The paper was in consequence never read. In 1852 or 1853 he was deprived of his Perpetual Vice-presidency of the British Medical Association, and soon after his colleagues in Hull got up an agitation to have him ousted from his post as Senior Physician to the Infirmary, in which they succeeded, after he had served the Infirmary for twenty years.

Dr. Sircar will forgive us prefacing his paper by these bits of history, as his experience and story are so akin to that of Dr. Horner, and we now have the pleasure of giving a treat to our readers.

“It would not, I think, be uninteresting as an episode in the history of homœopathy in India to relate how my conversion was brought about.

“At the preliminary meeting for the establishment of a medical society as a branch of the British Medical Association, held at the house of the late lamented Dr. Chuckerbutty, on the 27th May, 1863, in moving a resolution I made a speech in which I contemptuously alluded to homœopathy as one of the various systems of quackery which, I said, owed their rise and *temporary* triumph to the regular profession being unmindful of the following facts, namely, that all diseases are not curable, that many diseases, which our interference can do nothing for, are sometimes better left alone and to nature, and that quacks and charlatans stepping in when we desert our patients, often effect cures which perhaps we had been retarding.

“I was thus, equally with my professional brethren, a hater and denouncer of homœopathy, and perhaps the most furious of them all. Like them, I had no knowledge of it except from its caricatures by orthodox opponents. This distorted knowledge derived from misrepresentation of the system, aided by the apparent absurdity of the law of similars and the infinitesimal dose, was enough to justify my refusing to read works on homœopathy by homœopaths, that is, by those who had practically investigated its claims and found them based upon fact.

“The contemptuous allusion to homœopathy in the speech referred to met the eye of the late Babu Rajinder Dutt, the most distinguished among the few laymen who had taken up the cause of the despised and the persecuted system. He, a millionaire, out of pure disinterestedness had, in his earlier career, been one of the chief instruments in bringing into favour the European allopathic system. And with that disinterestedness associated with an acute discernment, he saw the superiority of homœopathy over the prevalent system of medicine. He took up its cause with his usual earnestness, and did his best to diffuse its blessings among his fellow-citizens of Calcutta. By effecting cures, some of which were the most brilliant in the annals of homœopathy in India, he had succeeded

in making converts of some highly intelligent and educated laymen, among whom was the late Pandit Iswar Chandra Vidyasagar. But not only did he fail to influence professional men, most of whom were his friends and some of whom had owed their advancement to his exertions, but he began to be looked upon by them as one who had lost his head and was spoiled by one of the most audacious and mischievous of quackeries.

“But such treatment by his medical friends did not deter him from pursuing the course he had taken. His conviction of the truth of homœopathy from personal experience was so strong that he was sure he would be able to impart those convictions to any medical man if he would but listen to him and watch his cases. When he read my speech he found something in it, as he told me afterwards, which inspired him with the hope that he had at last found a professional whom he would be able to bring over to his side, that is, to the side of truth, if only he could be induced to hold in abeyance his professional pride for a time.

“That hope would in all probability have remained unfulfilled had it not been for a pure accident. Babu Rajinder Dutt was untiring in his endeavours to make a convert of me, but with an obstinacy which was characteristic of deep-rooted prejudice I was repelling all his arguments and refusing even to watch his cases on the plea that I could not afford to lose time and professional dignity to watch cases under a layman.

“While such struggle was going on between us, a lay friend, ill-disposed to homœopathy, handed me a homœopathic pamphlet for review for a periodical of which he was one of the editors. The pamphlet was Morgan's *Philosophy of Homœopathy*. This I thought was a very good opportunity for me to smash homœopathy and silence Babu Rajinder. The book was the first on homœopathy I condescended to read, and I thought I would write off a review of it in no time. But what was the impression after a cursory glance at the pamphlet? I was convinced that I could not review it properly before reading it a second time. On a second careful perusal the conviction was forced upon me that no opinion could and should be passed on *à priori* grounds alone on a system which was alleged to be based upon facts, and which boldly challenged an appeal to facts. But how to get at

the facts? I had no other alternative than to turn to Babu Rajinder. He was the only practitioner whose cases I could watch, and though a layman, I now did not hesitate to sacrifice professional dignity, and made up my mind to be as it were his clinical clerk in order to arrive at the truth which appeared to me to vitally concern the profession and humanity at large.

“Before taking this step I made a stipulation with Babu Rajinder. I told him that, as I believed his cures were effected by the strict regimen that he enjoined and not by his infinitesimal nothings—globules or drops, I would agree to observe cases with him, provided he would agree to keep the patients for a time at least under strict regimen alone, and give them no medicine till it should appear that further expectancy would be injurious. He readily agreed. Strange to say, and to his dismay, a few cases did recover under regimen alone, and without any medicine. But my triumph was not to continue long, for others proved refractory, and I had to give my consent to administer his medicines to them. A great many recovered, and the incurables were benefited. This fact staggered me; the efficacy was too evident to be gainsaid; and I was compelled, much against my will of course, to make trials of the medicines myself in the cases which resisted my own treatment. The result, to my mortification, was something bordering on the marvellous if not miraculous.

“These trials were begun in 1865, and in the course of a year the conviction became strong that homœopathy was not the humbug and the quackery I had thought it was. In order to be sure of the degrees of their actual attenuation I prepared with my own hands some of the medicines, and I was surprised, as I have said, at their efficacy when administered according to the principles of the system. There was truth in the system, and to further resist and oppose it would, it appeared to me, be to resist and oppose the truth. And as the truth was concerned with my professional life, and as I was member of a profession whose sacred duty it was to avail themselves of every means for the cure of disease, the amelioration of suffering, and the prolongation of life, I thought it my duty to lay my experiences before the profession.

“As a first step, to one professor of my college who

had a great liking for me when a student and afterwards, and at whose fatherly insistence I had dared to appear at the M.D. examination, I timidly communicated my altered convictions. He was horrified to learn I had somehow come to have a leaning towards the hated system. He was sure, however, that with increased and matured experience I would see through its absurdity. From the manner in which he spoke I almost thought so myself. But increased experience only brought stronger conviction, and I dared not meet him again. Some time, perhaps not less than six months after, we accidentally met as we were passing in the same street. He stopped his conveyance and beckoned me. The first question he asked me was, 'How is it you have not seen me so long?' 'Simply because,' I said, 'my convictions about homœopathy having gained in strength from extended experience, I did not expect any sympathy from him.' 'You have my sympathy always. You have made a mistake. You are a rising man and have a bright prospect before you. A time will come when we shall have to consult you. I have every hope of your mistake being rectified in due course. I will advise you not to give out your conviction yet too soon. If you do you will have to repent for it.' This was what in substance he said. In those days the professors took a great interest in their pupils and actually loved them, and the veneration of their pupils for them was unbounded.

"I followed the advice given with such kindness and warmth of affection. But I went on with my trials, which had become a necessity. With each trial the truth of homœopathy was revealed in greater splendour. To keep the truth any longer to myself would be, I considered, cowardice, which was worse than crime. I thought, in my simplicity, that the members of our medical association who had cheered me when I had denounced homœopathy in my ignorance, would at least listen to me with attention when I would speak in its favour from personal experience. And truly I *was* listened to with attention when I delivered my address 'On the Supposed Uncertainty in Medical Science, and on the Relationship between Diseases and their Remedial Agents,' at the fourth Annual Meeting of the Association in Feb., 1867. Discussion was begun and was being carried on on the subject of the address in the most sober and temperate manner imaginable, as on

other subjects and at other meetings. In fact the members were behaving as befitting members of a scientific profession, when suddenly one of them, a marine surgeon, probably weary of the calm that was reigning, raised quite a storm by simply expressing his surprise that the meeting should be discussing homœopathy instead of dismissing it with contempt, and treating a homœopath as a professional brother instead of expelling him from the Association and from the room.

“ These words had a magical effect. Every one present shared in the surprise of the worthy champion of orthodoxy. The temper of the meeting underwent a sudden change. The coolness and sobriety of scientific discussion became at once transformed, from the blow that had come down upon it, into super-heated zeal in defence of what was called rational medicine and the legitimate profession. Even some of the laymen who had been invited to the meeting caught the contagious fire. Had it not been for the interposition of one of the secretaries, who was an Irishman, the meeting would have succeeded in achieving the triumph of expelling the offending member who a moment before was a vice-president. The Bengal branch emulated the parent Association in bigotry if in nothing else. The scene was dramatic in the extreme, and is still vivid in my mind. Some idea of it was given by an eye-witness in one of the papers, and I must content myself with referring to it instead of attempting a description myself, especially as I have in contemplation the reprinting of my address with the opinions of the press thereon and on the proceedings of the meeting. The press, as will be seen, was unanimous in condemnation of bigotry and in favour of toleration in matters scientific.

“ After the meeting there was considerable and unseemly wrangling about the possession of my paper. I was peremptorily asked to leave it with the Secretaries, as it was a property of the Association. I protested, and pointed out that, having been the first secretary of the Association for three years, since its establishment, I knew positively there was no rule to that effect ; and that never before we had demanded from their authors the papers that were read at our meetings. When it was found that I was not so docile and submissive as they had thought, or at least thought they could coerce me to be, one of the members who had played the second best at the meeting

requested me to make over the paper to him, which, after considerable hesitation, I did, but not before telling him that I had no objection to giving the paper to him as a friend, but not as an office-bearer of the Association. As events showed, I was justified in my hesitation. It was with difficulty I got back the paper, and that not before the administration of a legal threat. Had I not thus insisted upon getting the paper back, it would never have seen the light. It was written off-hand and I had not even a rough copy of it. Besides, I was anxious that it should appear exactly as it was, without the slightest alteration, in order that the public might see for themselves what my actual position was, and on what slender grounds I was condemned and made an outcast.

“An outcast I actually became from the next day of the meeting. The rumour spread like wildfire that I had lost my reason, that I had yielded to the seductions of Babu Rajinder Dutt and given my adhesion to one of the worst and the most absurd of quackeries that had ever come into existence; that I had forgotten my mathematics and now believed that the part was greater than the whole. My patients, and their number was not inconsiderable, who had perfect faith in me, regretted that I should have given up my old convictions, and one by one forsook me. The loss of my practice was sudden and complete. For six months I had scarcely a case to treat. Even those who used to receive advice gratis every morning at my house ceased to come, and if anybody, not finding benefit anywhere else, did come it was only to beg me to give him my old and not my new medicines. My old master, the late Babu Thakur Dass Dey, from whom I had received the rudiments of education and who loved me as his own son, used vehemently to remonstrate with me for having brought on my ruin. There were sincere friends who offered the kindly advice of retracting! Such remonstrance, such advice was to me worse than loss of practice. My reply to my beloved tutor and to my kind friends was that I would rather give up my profession and take to any other calling, or even starve, than disavow the truth. I was prepared to brave any contingency that might happen to me for my honest convictions, and to proclaim to the world to the utmost of my power what I believed to be the truth.

“I was sustained by my faith in the ultimate triumph of truth, but that triumph, I was also sure, could only be brought about by persistent presentation and advocacy of the truth. And I felt that that presentation and that advocacy should come from one who had a regular training in the medical sciences, and therefore could speak with authority, and who would not be suspected of mistaking a mild for a grave disease. Babu Rajinder had succeeded in making converts of a few highly intelligent laymen, and he had succeeded in converting at least one professional man, and that conversion was due to observing actual success of homœopathic treatment. Babu Rajinder was a layman, and therefore, notwithstanding the marvellousness of some of his cures, he could not command that confidence from the community which he could if he had been a regular professional. He could only be sent for in extreme cases given up by the doctors, and it is not every extreme case that could be brought back to life from the jaws of death. Failures must necessarily arise, and failures in his hands were bound to be fatal to the advancement of the cause for which he was labouring with such earnestness and zeal. He was at first associated with one who, though he called himself an M.D., was in reality a layman who had necessarily no proper acquaintance with the medical sciences; and latterly he was associated with a professional who was more a religious enthusiast than a zealous physician. He more often practised hydropathy than homœopathy, and thus courted failure in many an instance. One of his unlucky failures was related with dramatic effect at the meeting. The association of such a practitioner with Babu Rajinder, far from bringing credit, brought in many cases serious discredit upon the system. Babu Rajinder felt it, but he could not help it; and therefore he was on the look-out for one who could co-operate with him and then take his place.

“I thus found myself forced to a position for which I could scarcely think that I was competent. It became but too evident that I must not content myself with merely practising the system in which I had recognized the germ of a beneficent truth capable of indefinite development, but that I must help in that development, and do all in my power to diffuse a knowledge of it among the profession and the public. I had no hopes of doing this through the medium of orthodox journals, of which there

was only one in India at the time, the *Indian Medical Gazette*, the columns of which were shut against me even for reply to unfounded charges and slanderous accusations. I thought it not only inadequate, but undignified, to do so through lay journals. I saw that I must have a journal of my own if I was to fulfil my mission at all. The attitude of the press of India encouraged me to take the risk, and a journal with an unsectarian name was started from January, 1868.

“The appearance of the journal had at once the remarkable effect of silencing the opposition from my lay countrymen, perhaps because they saw that I was not quite the fool they had believed I had become. The opening article giving expression to ‘Our Creed’ of catholicism in medicine, succeeded in disarming criticism even of my most hostile opponents. Even the *Indian Medical Gazette*, ‘though declining to agree with him in the principles of his creed,’ could not ‘but commend the spirit and perseverance which have induced Dr. Mahendra Lal Sircar, single-handed, to start a ‘Journal of Medicine’ in Calcutta.’ How far the Journal has been instrumental in furthering the cause of homœopathy in India I must leave it to the future historian of medicine to determine. This much is certain, that since its appearance the spirit of intolerance and bitter opposition has nearly vanished, at least so far as external manifestation goes, and that orthodox physicians have begun to feel the power of homœopathy so far that they very seldom give up cases, lest they should go to the homœopath and be cured.

“The Journal has been in existence ever since, but had to remain in suspended animation from time to time owing chiefly to illness, which was often serious enough to disable me from all work, but partly also to public duties which were imposed upon me by Government or my countrymen, and left me no time for my self-imposed task.

“The very few distinguished laymen whom Babu Rajinder had converted were, needless to say, my first supporters. They trusted me with cases which in the beginning I invariably treated in conjunction with Babu Rajinder, as my own knowledge of homœopathy then was not adequate enough to inspire me with confidence to undertake unaided the treatment of any serious and complicated case. I found homœopathic treatment to

be greatly more difficult than old school treatment. Whereas I could prescribe off-hand if I had to treat a case after orthodox methods, I could not do so if I had to treat after the method of the new school, without a great expenditure of thought in interpreting the symptoms and signs presented by the patient, and of time in consulting books to find a remedy to correspond with those symptoms and signs. Though I had improved my knowledge of the *Materia Medica* during the 'starvation' period of six months when I had no practice, and therefore nothing else to do, I found the injunction of Hahnemann but too true, that no conscientious physician ought to consider his knowledge of the *Materia Medica* minute enough to enable him to dispense with the necessity of consulting it in every case. After an experience and study of nearly forty years I find the same necessity still existing, and I would advise every practitioner of the new system not to lose sight of the Founder's injunction.

"I need hardly say that in the course of a short time my practice began to look up. Disease is no respecter of person, of school, of creed. People cannot afford to endure suffering or lose life for the sake of a favourite physician or of a favoured system. They do not care after the methods of which school they are treated so long as their ailments are cured. If the old school could effect cures in all cases, or even in the majority of cases, the new school would have had no footing; indeed, there would have been scarcely any necessity for it. But it was notorious that except in surgical diseases the old school had but few genuine cures to boast of. It was a signal failure in cholera, scarcely less so in chronic diarrhœa and dysentery, in fevers which were not amenable to quinine, and in the vast majority of diseases for which no specific had yet been discovered. It was because of my having observantly watched these failures that I was induced to look beyond the bounds of my orthodoxy, and it was because I found the homœopathic method superior to the old methods that I had given in my adhesion to the system, in the face, as has been seen above, of virulent opposition and persecution. And, therefore, notwithstanding the collapse of my practice, I had not given up all hope. I had not to wait long. My persistency in the path I had chosen brought in its reward. Uncured cases began to come to my out-door, and their cures began to spread the cause.

“ Thus the 16th February, 1867, has been memorable in the history of the medical profession in this country. On that day a native member of the profession was the first in India to stand up for a reform in medicine, and for this he met with opposition and even persecution similar to that which had attended the reform in the land of its birth and in other parts of the world. Upwards of thirty-five years have elapsed since that date, and considering the circumstances of the country the progress of the reform has not been unsatisfactory. There has been growing appreciation of homœopathy throughout India. Numbers of the native members of the regular profession have openly adopted the system and are practising it with success. Though income is no criterion of true success, yet the fact that several homœopathic practitioners are enjoying respectable incomes shows that the system must be in favour with the community. This is evidenced by another fact which is no less significant. We have no regular and adequate organization for teaching the principles and the practice of the system, notwithstanding the existence of four (!) homœopathic schools in Calcutta, and the necessity has been felt of resorting to America for due qualification. Some had actually gone to that distant land of freedom and come back equipped with degrees from her recognized Homœopathic Colleges, and others are following their good example. Who would have thought of undergoing heavy expenses and of undertaking long and perilous journeys for homœopathy if there had been no demand for its practitioners ?

“ The evidence of the law of demand and supply in favour of homœopathy is supplied by another fact, namely, the growth and multiplication of lay practitioners. Had it not been for the evident superiority of the new system over the old, people would not have trusted their health and their lives to laymen. We must thankfully acknowledge the debt we owe these men for their help in the spread of the cause. For India as a whole, as will have been seen from the above, the pioneer of homœopathy was a layman, from the fact of his having succeeded in converting a professional; and laymen practitioners are the pioneers of the system in the villages and towns of the mofussil. This seems to be a necessity all over the world; in our country it has been particularly so, no doubt because of the conversion of so few regular

practitioners. This necessity has not been an unmixed good. And no wonder. We cannot expect laymen to maintain the dignity of a system which requires for its successful practice the highest knowledge of the collateral sciences, against an opposition intimately acquainted with these sciences. We are free to acknowledge, and we do so with the greatest pleasure, that some laymen practitioners are wonderfully successful by dint of their honest and laborious search for symptoms, which it is to be regretted the regular practitioner in his pride so much neglects. One of these practises homœopathy purely out of philanthropic motives. Every morning he gives advice and medicine gratuitously to over one hundred poor patients. When pressed he visits patients at their houses, but he never takes any fee. He is imbued with the true spirit of homœopathy, but unlike others he has no ambition to pass off as a doctor. He keenly feels his want of knowledge of anatomy, physiology, and other auxiliary sciences. He would give up his self-imposed task if the poor whom he treats would get the regulars to pity them. To such a man we should feel grateful for his services in our cause, and gratitude demands that I should give out his name. He is Babu Dina Bandhu Mukerjee, of Shibpur, a clerk in a Government office."

THE BRITISH HOMŒOPATHIC CONGRESS AND
THE ADDRESS OF SYMPATHY AND LOYALTY TO
THE KING.

THIS message to His Majesty the King from the recent Congress we printed in our issue of August. The Hon. Secretary has received the following reply from the Home Secretary.

HOME OFFICE, WHITEHALL,

28th August, 1902.

SIR,—I have had the honour to lay before the King the loyal and sympathetic message of the Homœopathic Doctors of Great Britain and Ireland in Annual Congress assembled, on the occasion of His Majesty's severe illness.

His Majesty was pleased to receive the same very graciously.

I am, Sir,

Your obedient servant,

(Signed) A. AKERS DOUGLAS.

D. Dyce Brown, Esq., M.D.,
29, Seymour Street, W.

REVIEWS.

The Therapeutics of Fevers: Continued, Bilious, Intermittent, Malarial, Remittent, Pernicious, Typhoid, Typhus, Septic, Yellow, Zymotic, etc. By H. C. ALLEN, M.D., Professor of Materia Medica in Hering Medical College, Chicago. Philadelphia: Boericke & Tafel, 1902.

Dr. Allen, in his preface, gives the key to his teaching. He says there: "Since the publication of the first edition in 1879 (A Monograph on Intermittent Fever) an extended experience in the treatment of chronic intermittents has conclusively verified the teachings of Hahnemann, that the most obstinate and intractable cases occur chiefly in psoric or tubercular patients, and the more deep the dyscrasia the more protracted the fever. And the clinical fact, which has been verified by many observing homœopathic physicians, that the suppressive treatment by massive doses of quinine as practised by the dominant school, is not only positively injurious, but often fatal. Massive doses of Peruvian bark or any of its alkaloids can only suppress the symptoms and thereby increase the sufferings of the patient. Hahnemann's lesson in the *Chronic Diseases* in the treatment of acute syphilis must be applied to fevers of all types, and acute diseases of every name and every kind irrespective of habitat. If, as he affirms in the *Organon*, acute diseases 'are generally only a transitory outbreak, an explosion of a latent psoric affection,' the symptomatic expression of this or any other dyscrasia must be included in the anamnesis, and the remedy selected from the totality thus obtained. This volume includes the therapeutics of typhoid, typhus, and fevers of every grade and name, from acute sporadic and epidemic intermittent to the malignant type of the malarial fevers of the tropics. But the same law of cure, the same rule of practice, applies to each patient. It is the patient, not the fever, that is chiefly and especially to be considered. It is the individual with his extra peculiar idiosyncrasies and constitutional inheritances with which we have to deal. The symptoms of the latent psoric affection must be analysed as fully and completely as the symptoms of the 'transitory outburst,' the fever paroxysm. As a rule the family history is much more suggestive of the curative remedy than the rapid pulse and high temperature, and should be carefully studied. When discovered, the constitutional miasma—psora, sycosis, syphilis, tuberculosis—should be especially noted, for here will often be found the key with which to unlock the secret of the severity of the attack or the relapsing tendency of the fever. It is the

experience of the author that if the remedy be selected from the totality of the objective, subjective, and miasmatic symptoms, the patient may be cured in any stage of the fever. It is not necessary for a typhoid or any other fever to 'run its course.'

This last sentence is rather a "tall order." We have heard of typhoid being aborted in the first stage, but never before have we learned that it can be "cured in any stage." With such views of practice it is not to be wondered at that Dr. Allen gives no general description of the various types of fever named on the title-page. Indeed, he hardly names them separately afterwards, and the whole of his remarks antecedent to his *Materia Medica* are summed up in 39 pages out of a total of 538 pages. In fact, the main fever treated of in the book is intermittent fever, the others taking a "back seat," and in speaking of the *causes* of intermittents he says: "The object of this work is to deal with therapeutic facts, not with speculative theories. The author has no theory to advance, and none to disprove except such as interfere with the successful homœopathic treatment of this *bête noir* of our profession. We are unable to offer an intelligent explanation of the *causes* of sporadic or epidemic intermittent fever that will bear the test of scientific investigation, and Hahnemann's *one therapeutic fact* is worth more at the bedside than all the theories that have ever been advanced." We note, however, as rather remarkable that Dr. Allen never alludes to the recent investigations of Dr. Patrick Manson on the influence of mosquitoes in the production of ague. Whether our author thinks them of any value or not, one would have expected some allusion to them in a work devoted so very largely to the treatment of this disease. The logical result of Dr. Allen's views of treatment is that his "therapeutics" consist in an alphabetical list of 133 medicines, of each of which he gives a summarized pathogenesis, chiefly, it is true, relating specially to fever symptoms, but necessarily also including the whole constitutional pathogenesis. In other words, it amounts to a "boiled-down" *Materia Medica*, with the fever symptoms brought into special prominence. There is an excellent repertory which prevents the necessity of wading through this alphabetical list of 133 remedies to find what one wants. But, after all, a full *Materia Medica* and a good repertory are at the command of all of us, without such a work as Dr. Allen's, and will give us the same information. Of course, from Dr. Allen's point of view, and as any one of the 133 remedies may be required in any case, one remedy is no more a remedy than any other except in an individual case, and consequently the only way to do is to give this alphabetical

list and the pathogenesis of each. But while it is perfectly correct to say that in homœopathic practice the individual has to be treated, and not a disease, and that it is necessary therefore to elicit all the special individual symptoms in any given case, and to be guided by them in the selection of the remedy, yet all physicians know that all diseases, and especially fevers, have a certain general, but well-marked, association of certain symptoms which together picture a disease. These pictures vary in different epidemics, and, when noted, constitute a special type of the fever. But, still, there is the essential co-presence of symptoms so well marked as to enable one to say without difficulty that it is a case of such and such a disease. And in the therapeutic management of such, physicians find that for their successful treatment a comparatively small number of medicines is required. This comparatively small list is found to cover the symptoms of the majority of such cases, and it is only the minority that is found to be so out of the prevalent type as to require one to search for some special medicine suited to this individual case. In this use of comparatively few remedies the individualizing is fully carried out, as these remedies are found to fully cover the symptoms, while the generalization that finds only a few medicines to be required is perfectly scientific and correct. Of course, as we have said, cases that do not correspond to the typical and marked symptoms have to be still further individualized, and the right remedy sought for. According to our ideas, Dr. Allen's work would be more practically useful, and more widely read and studied, had he thought well to take these points into consideration, and described the various principal types of the different forms, with a *full* account of the pathogenesis of each of the comparatively few remedies indicated in these types, with their special indications in their order of importance and not alphabetically; and following this with the other remedies equally valuable in individual cases, with the indications for each which he so admirably gives us in this book. This plan, we think, would enable the student especially, but also the practitioner to get a firmer grip of the whole therapeutic resources of homeopathy, by fixing in his mind the marked features of the leading and generally required remedies, and so enabling him to see with full knowledge of the details of disease, and of the drugs, when his patient did not correspond in symptoms with those "leaders", and when it was necessary to seek for other more accurately indicated remedies. The work shows a vast and enviable knowledge of the *Materia Medica*, and will involve hard study on the part of its readers. It is most handsomely got up.

 NOTABILIA.

BRITISH HOMŒOPATHIC SOCIETY.

THE council at their last meeting made the following arrangements for the coming session.

The meetings of the section of Surgery and Gynæcology to be held on Thursdays, November 6th, February 5th, and May 6th.

The section of Medicine and Pathology to meet on Thursdays, December 4th, March 5th, and June 4th.

The section of Materia Medica and Therapeutics to meet on Thursdays, January 8th, April 2nd, and Wednesday, May 1st.

Dr. Goldsbrough was appointed Editor of the Journal.

In view of Dr. Johnstone's election as a Vice-President of the Society, Mr. Knox Shaw was appointed to be Secretary for the Session.

Dr. Roberson Day will deliver his Presidential address on Thursday, October 2nd.

RICHARD HUGHES' MEMORIAL FUND.

WE have much pleasure in publishing the following very kind letter to Dr. Blackley from Dr. J. H. McClelland, of Pittsburgh, U.S.A., and in assuring him of the high appreciation of all his colleagues on this side of the water of the ready generosity of our American colleagues in aiding the Hughes' Memorial Fund, and thereby proving in such a tangible and handsome manner, their sense of the value of the life services of our departed friend in the cause of homœopathy, and of his loss to them as well as to us.

FIFTH AND WILKINS AVENUES,
PITTSBURGH, PENN.,
July 19th, '02.

DR. J. GALLEY BLACKLEY, London, England.

MY DEAR COLLEAGUE :—Announcement having been made by President Wood at our last Institute meeting that it would be agreeable to have the American homœopathic profession participate in raising a memorial fund in recognition of the life and services of our late colleague and dear friend Dr. Richard Hughes, a subscription was immediately opened, and I was asked to take charge of the matter. The enclosed list of subscriptions was made on the spot, with the exception of a few that I have secured since. Upon my return from the summer vacation, I hope to add very materially to the

list. In the meantime, we beg that you will take charge of this fund and add it to your own. I herewith send you a draft on London covering the sum subscribed, £178 2s.

With very best wishes, believe me,

Faithfully yours,

J. H. McCLELLAND.

HUGHES MEMORIAL FUND.

AMERICAN SUBSCRIBERS TO THE HUGHES MEMORIAL FUND.

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James C. Wood, M.D., Clevedon, O.	50 00	C. E. Walton, M.D., Cincinnati, Ohio.	10 00
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A. P. Stauffer, M.D., Hagerstown, Md.	2 00	C. F. Bingham, M.D., Pittsburgh	25 00
J. C. Fahnstock, M.D., Piqua, Ohio.	5 00	D. W. Horning, M.D., Minneapolis, Minn.	5 00
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R. S. Copeland, M.D., Ann Arbor, Mich.	10 00	E. E. Austin, M.D., Minneapolis, Minn.	5 00
W. A. Dewey, M.D., Ann Arbor, Mich.	10 00		\$870 00
Additional Subscriptions will be forwarded as received.			

STRYCHNICINE.

DR. M. G. BOORSMA (*Bull. de l'Inst. de Buitenzorg*) has discovered a new alkaloid in the leaves of *Strychnos nux-vomica*. The alkaloid has been found in young as well as old leaves, in ripe fruit pulp, in the hard fruit shell, and in the thin epidermis which covers it, and has been given the name of strychnicine. One kilogramme of the fresh leaves yielded about 100 grammes of the alkaloid, which forms crystalline needles containing no water of crystallisation. It is obtained by treating an alcoholic extract of the leaves with water and lead acetate, adding hydrogen sulphide to remove the lead, separating the liquid, and, after partial evaporation, washing with benzol, making alkaline, and shaking out with benzol. At 100° C. it does not lose weight, at 240° C. it begins to become brown, and at higher temperatures it forms a dark mass. In comparison with the other *nux-vomica* alkaloids, strychnicine is only slightly poisonous.—*Monthly Magazine of Pharmacy, &c.*, Sept.

ANTI-TYPHOID INOCULATION.

THIS recent mode of attempting to prevent the occurrence of typhoid fever in those exposed to it, and also of treatment of actually developed typhoid, is, we need hardly say, still *sub judice*. It has its warm supporters, and also its keen opponents. Statistics have been issued from time to time, giving diverse results, some seeming to show considerable success, and others nearly complete failures, while injurious effects were produced in a large number of cases. We, as homeopaths, desire always to keep an open mind on any new treatment, and to endeavour to draw impartial conclusions on any reliable evidence brought before us as to the value or otherwise of such treatment, independently altogether of

the question whether these examples of "serum therapy" are not after all homœopathic in action. Into this question, however, we do not propose to enter here. But we gladly call attention to an article on the subject contributed to the *Lancet* by Professor A. E. Wright, of Netley, and summarized in the *Times* of Sept. 5th. Professor Wright endeavours to elucidate the statistics he has collected, and to exclude possible fallacies in figures, and in conclusions from them. Instead, however, of analysing these figures and conclusions ourselves, and so letting it be suspected that we are biassed one way or the other, our readers will thank us to reproduce as we do, an extremely able, impartial, and judicial leading article on this subject from the *Times* of Sept. 6th. It will be seen how ably the question has been handled, and our readers will get a better idea of the gist of Professor Wright's paper than if we had adopted another course. Those who wish to get the full details for themselves can see them in the *Lancet*, or in the *Times* summary of Sept. 5th.

"The article on the results of inoculation against typhoid fever contributed to the *Lancet* by Professor A. E. Wright, of Netley, of which we published a brief summary yesterday, is worthy of notice as a striking example of an endeavour to escape from the errors so often incidental to the employment of figures for the purpose of establishing a conclusion. The very familiarity of the saying of the great Sir Robert Peel, that nothing is so misleading as facts, except figures, somewhat tends to render us forgetful of the wit and wisdom which it embodies, and hence we are often called upon to inspect a very large edifice of conclusion, insecurely balanced upon a slender foundation of scanty and misinterpreted statistics. The difficulty of the application of the numerical method to inoculation against a prevalent disease depends partly upon the impossibility of ascertaining whether the inoculated persons have actually been exposed to the contagion in the same proportion, and to the same extent, as the uninoculated; whether, that is to say, there has been a real and complete or only a partial and deceptive resemblance between the conditions of living to which they have respectively been exposed. Professor Wright lays down the general rule that, as statistics are put together by human creatures, they can never be absolutely accurate; but he adds that they are not, therefore, wholly to be condemned, inasmuch as it may be possible, in any given case, to arrive at a conclusion as to the *maximum* and *minimum* of error to which the particular figures under consideration have been exposed, and thus to discover to what extent, or rather within what limits, they may safely be relied upon for guidance. The garrison of Ladysmith, according

to the official returns, was composed of 12,234 officers and men, of whom about one-sixth, or 1,705, were inoculated, and were exposed to the infection of typhoid after an interval of time ranging from two months to eleven months. In the period between November 2, 1899, and February 28, 1900, 1,489 cases of typhoid fever were recorded among the uninoculated and only 35 among the inoculated, instead of about 250, which would have been the the due proportion. Eight deaths from the fever were recorded among the inoculated and 329 among the uninoculated, the percentage death-rates among the two groups being respectively 0·47 and 3·12. Subsequent inquiry into the accuracy of these figures showed that, of two deaths set down to typhoid among inoculated officers, one was due to a casualty, and that the other officer was not inoculated with "anti-typhoid" vaccine. Among the uninoculated officers a *minimum* of ten deaths occurred, instead of only five as stated; while it is highly probable, or almost certain, that similar inaccuracies might have been discovered in the returns relating to the rank and file. In dealing with such instances Professor Wright endeavours to arrive at the possible *maxima* and *minima* of error, and to make allowances in accordance with them.

"When this has been done the evidence still appears to point to a very decided diminution of the liability to typhoid as an effect of inoculation, although the rate of mortality among those actually attacked does not appear to be in the same degree diminished. In some of the returns it would even seem to have been increased; but these, when closely examined, are found to have been at least open to vitiation by inaccuracies which it is difficult to explain, or the explanations of which can at best be no more than conjectural. Thus, with regard to one group of typhoid patients in Tintown Hospital, Ladysmith, it is said that there were 30 inoculated and 265 uninoculated, and that the deaths were two of the former and five of the latter; figures which appear to show a mortality of one in fifteen among the inoculated, and of one in fifty-three among the uninoculated. On further inquiry, Professor Wright discovered that the figures given really rested upon no *data*. It is not stated whether the patients were originally registered as inoculated or uninoculated, or whether a special record was made (or supposed to be made) only with regard to those known to be inoculated, in which case the accidental omission of such a record would cause the name to go into the uninoculated class. There were no records of age, and the mortality set down for the uninoculated (one in fifty-three) is suspiciously low for a typhoid case mortality. The statistics would be altogether deceptive if fevers less fatal than typhoid were diagnosed as typhoid among the uninocu-

lated, and if no such error were committed in the cases of the inoculated ; while it is to be noted that the alleged mortality of the uninoculated referred to does not agree with that of the total of the 803 typhoid patients treated in this hospital. That mortality, according to information given to Professor Wright by the principal medical officer, Major Westcott, was 18·6 per cent. We constantly meet with examples in which persons entertaining certain opinions about questions in dispute, questions of sanitation or of some other kind, appeal with considerable confidence to so-called statistics of an exceptional character, and cite them as evidence in favour of, or against, the view which they are seeking to establish or to refute. The vaccination controversy has been fruitful of illustrations of this tendency, and Professor Wright will have done good service to the cause of truth if he succeeds in teaching caution with regard to the acceptance of figures of a somewhat unusual character, which should manifestly always be submitted to a rigid scrutiny before they can safely be employed in the formation of conclusions. As a general result of his investigations, Professor Wright speaks of a "twofold," and even of a "fourfold," reduction of typhoid mortality by means of inoculation. It was, we believe, an Irishman who described something as being "twice as little" as something else ; and twofold and fourfold reductions are a little puzzling to the uninitiated. We hope the meaning is that the mortality will certainly be reduced to one half, and possibly to one quarter, of that which we have to deplore at present.

"It will be recognized by those who read Professor Wright's article, or even the summary of it which we printed yesterday, that his favourable estimate of the actual and probable benefits to be derived from inoculation has not made him either unconscious of, or indifferent to, the risks which may attend upon the practice ; and hence it becomes the more important, in view of the terrible amount of disablement inflicted upon armies by the disease, that our military authorities should neglect no means by which its ravages may be diminished. We need scarcely recall the various letters and articles in which Dr. Leigh Canney has again and again urged upon the public and upon the War Office his belief that typhoid fever may be entirely prevented by taking proper measures to supply boiled water to the troops, and to induce or compel them to exercise sufficient self-restraint to drink no other ; and his views have now been so completely tested in the great encampments made in connection with the Nile engineering works that they can no longer be regarded as hypothetical. We have from the first expressed a doubt whether the arrest of water-borne typhoid would suffice

entirely to prevent the disease among armies in the field, exposed to the ordinary conditions of the march and the camp, and whether dust and flies might not remain as channels of contagion ; but, however this may be, it is at least certain that the prevention of water-borne typhoid would enormously simplify the problem, and would greatly assist in dealing with any residuum of fever, either by inoculation or by any other means which experience might suggest. Dr. Canney maintains that it would not be impossible, even if difficult, to educate the bulk of the Army to a recognition that drinking unboiled water was a military offence, and an offence which was rendered serious by its tendency to produce sickness and incapacity. We regard it as a libel upon the soldier to suppose that he would be either unable or unwilling to exercise a certain amount of self-denial for the attainment of a manifest good ; and we cannot but regret such speeches as one which we reported yesterday as having been made by Surgeon-General Sir William Deane Wilson, who said that enteric could not be prevented with an army marching all day under a burning sun, with men suffering from great heat, who could not be driven away from water which was slimy, muddy, and dirty. This assumes the impossibility of something which is declared by Dr. Canney to be possible, an opinion in which, until experience proves him wrong, we feel compelled to agree with him. Given a proper organization for supply, thirsty men need not be called upon to wait more than ten minutes for their drink ; and soldiers, who know by experience the meaning and the terrors of typhoid, are quite intelligent enough to control an appetite which in their case ought never to be allowed to reach the degree which is produced by a real deficiency of fluid in the system. The endeavour to teach them would at least succeed with a large proportion, and the experience of these would speedily have its effect upon the rest. Even apart from the risks of fever, instruction in the proper economy and proper use of drinking water would of itself be of great value to all who were called upon to endure the fatigues of a campaign."

THE RELATION OF EPILEPSY TO CRIME.

At the recent annual meeting of the American Neurological Association (June, 1902) Dr. W. P. Spratling, medical superintendent of the Craig Colony for Epileptics, New York State, in an interesting contribution on the Relation of Epilepsy to Crime, pointed out that the epileptic "fit" in

its classical form only constituted one-half the picture of the disease, for the mental states consequent upon the fit were of the most varied and abnormal forms, and constituted another aspect of the disease which possessed both clinical and medico-legal importance. Such manifestations varied from a condition of stupor or stupidity to one in which extraordinary crimes have been committed. The insane epileptic of asylums had little facility for crime, partly because he was vigilantly supervised and partly because he was less potent for evil owing to his being more or less demented. There were many epileptics who were not, or did not become insane, and although the exact proportion was not easy to determine it could not be less than 15 or 20 per cent. of the total. An epileptic who is sane at other times may, while in the peculiar mental condition following upon a fit, commit the most open and extraordinary crimes. In this state, argues Dr. Spratling, he stands forth as a curious anomaly under the law of responsibility, for with consciousness subverted or destroyed, the brain may give expression and effect to impulses of a dangerous or abnormal nature without possibility of self-control. In many cases the danger of epilepsy does not cease with the passing-off of the convulsion and post-convulsive stupor, for there frequently follows a subconscious state, lasting from a few minutes to several days, during which the condition of the patient is best described as one of mental automatism with loss of the sense of personal identity. He then acts like a mere machine, being able to go about as usual and able to take his meals and to perform sundry other commonplace actions as was his habit before the attack. And yet he is all the while absolutely ignorant of what he is about—his mind is a blank. In such a state he may do a criminal deed without premeditation or criminal intent, "as a railway engine may kill a person while running away without its driver." The same curious and interesting phenomenon of mental automatism may appear in cases of so-called psychical epilepsy, in which motor convulsions do not take place, but a state of acute frenzy and "blind" excitement occurs, lasting for a few hours or days. This form of psycho-epileptic frenzy is, says Dr. Spratling, never momentary, as a study of over 1300 cases has convinced him that it lasts some time. The most difficult medico-legal problem connected with epilepsy is encountered in the purely psychical forms of epilepsy—those uncomplicated with convulsions or crude motor manifestations. Here the patient may commit the most strange acts, such as wandering about without any consciousness of locality or sense of

personal identity, until he finds himself stranded away from home and friends, when normal consciousness eventually returns, a condition to which reference has already been made in these columns. It is not safe to oppose an epileptic while he is in such a state, as he is apt to become combative and dangerous. Thus a case is cited by Dr. Spratling of a man who had a seizure of *petit mal* while in an elevated railway in New York. By the time the guard reached him he was all right, but when the time came for him to leave the car some minutes later he made no move to go. The guard took him by the arm to move him, when the patient struck at him violently and a fight ensued, which led to his being arrested and locked up. When he came to his senses he was greatly mortified at being in jail, and on showing his card of epileptic seizures was permitted to go. He was, says Dr. Spratling, unconscious when he struck the guard, and had no intent whatever of doing wrong. Dr. Spratling concludes by citing other cases where dangerous and even murderous attacks on persons have been committed by epileptics during the stage of mental automatism which may follow a fit, and he points out the close analogy of this state to insanity.—*Lancet*, July 29th, 1902, p. 290.

A FATAL CASE OF BELLADONNA POISONING.

A PARTY of children from Portsmouth, spending a holiday in West Sussex, ate a number of berries from the plant of the belladonna, or "deadly nightshade." They were taken ill soon after, one a boy, aged 11 years, who had eaten more freely than the others, seriously. All showed the usual symptoms—flushed face, dry throat and skin, the latter covered with a scarlatiniform rash, rapid pulse, widely dilated pupils, and a restless delirium. In the case of the boy an emetic did not perform its work for some time, and he never recovered from the effects of the poison, death ensuing a few hours later. Evidence at the inquest showed that the other children would have eaten more of the fruit but for the peculiar sweet flavour.—*Lancet*, July 9.

ANTITOXIN IN DIPHTHERIA.

WE always act on the advice "*audi alteram partem.*" We have heard so much, on what seems indisputable evidence, of the value of antitoxin in the cure of diphtheria, and of the

reduced mortality under its use, that we think it only right to publish evidence on the other side, especially when this treatment is contrasted with that of homœopathy proper, waiving the question of the possible or probable homœopathic action of antitoxin. In the *Medical Century* of June a paper on the "Treatment of Diphtheria" appears from the pen of H. W. Westover, M.D., St. Joseph, Mo., in which after describing the homœopathic treatment of the disease, the following passages on the treatment by antitoxin concludes the paper:—

"The remedy which has produced the greatest sensation of late years is antitoxin. It holds a prominent place in the treatment of diphtheria, and by many is considered a specific. There has been a flood of testimony extolling its use, and favourable mention has been made of the antitoxin treatment in homœopathic journals, the stock argument seeming to be a reference statistics. However, a careful examination of these statistics, coupled with a study of the disease at the time and place where the statistics were secured, and comparison with other statistics, will show that the apparently successful use of antitoxin is merely a coincidence; that the serum treatment is not a specific, and that it is not more successful than other measures. It appears that an easily influenced profession has been misled by a popular fad, believing a 'post hoc' is a 'propter hoc.'

We should remember diphtheria is a disease characterized by widely varying mortality at different times and in different places. One prominent manufacturer of antitoxin has been running a catchy advertisement in many medical journals, stating the mortality attending the use of their serum in Chicago was $47\frac{8}{100}\%$.

"On the other hand we learn that cases treated at the *same time in the same city* without the serum made a rather better showing, and that the diagnosis in *each case* was confirmed by the health board. This same firm fails to mention the results that followed the use of their antitoxin in the Municipal Hospital, of Philadelphia, where \$1,400 worth of their serum was used in the month of December, 1897, with a mortality of $33\frac{1}{3}\%$.

"The following table shows the results obtained when every case received antitoxin:—

1896.			
Month.	Cases.	Deaths.	Mortality.
January	14	3	21.42%
Februrary	15	4	26.66%
March	44	9	20.45%
April.....	31	7	22.58%
May	59	16	27.11%

1896.			
June	32	11	34.37%
July	28	7	25.00%
August	36	8	22.22%
September	48	20	41.66%
October	62	15	24.19%
November	90	19	21.11%
December	94	20	21.77%
1897			
Month	Cases.	Deaths.	Mortality.
January	91	23	25.27%
February	68	17	25.00%
March	44	18	40.90%
April	67	13	26.86%
May	70	19	27.14%
June	84	23	27.28%
July	83	20	24.09%
August	85	18	21.17%
September	68	18	26.47%
October	126	21	16.66%
November	96	29	30.20%
December	111	37	33.33%

“ Any homœopath should be able to make a better record.

“ A study of this table shows that in September, 1896, the mortality was more than double that in March, with about the same number of cases, and in October, 1897, the death rate was 16.66%, and in March it was 40.90%. Although antitoxin was used in every case there was a great fluctuation in the fatality. When a similar fluctuation occurred in the hospitals of Paris and Berlin, coincident with the use and the omission of the serum, it did much to strengthen reliance on antitoxin, although it was a fallacious and misleading experience. When we find such varying results attending the use of antitoxin we cannot consider it a *specific*, because if it actually produces a low mortality it should give the same results wherever and whenever used.

“ We will now notice the report of the health department of New York City from December 11th, 1899, to March 17th, 1900 :—

Week ending.		Cases.	Deaths.
Dec. 11th,	Diphtheria	255	40
Dec. 11th,	Laryngeal Diphtheria	15	10
Dec. 16th,	Diphtheria	273	37
Dec. 16th,	Laryngeal Diphtheria	9	3
Dec. 30th,	Diphtheria	257	46
Dec. 30th,	Laryngeal Diphtheria	10	7
Dec. 23rd,	Diphtheria	274	42
Dec. 23rd,	Laryngeal Diphtheria	14	12
Jan. 13th,	Diphtheria	276	41
Jan. 13th,	Laryngeal Diphtheria	15	12
Jan. 20th,	Diphtheria	287	41
Jan. 20th,	Laryngeal Diphtheria	10	10

Week ending.	Cases.	Deaths.
Jan. 27th, Diphtheria	285	53
Jan. 27th, Laryngeal Diphtheria	28	8
Feb. 3rd, Diphtheria	322	55
Feb. 3rd, Laryngeal Diphtheria	13	8
Feb. 17th, Diphtheria	310	43
Feb. 17th, Laryngeal Diphtheria	29	11
Feb. 24th, Diphtheria	285	50
Feb. 24th, Laryngeal Diphtheria	17	14
March 3rd, Diphtheria	282	54
March 3rd, Laryngeal Diphtheria	264	46
March 10th, Diphtheria	264	46
March 10th, Laryngeal Diphtheria	14	7
March 17th, Diphtheria	256	45
March 17th, Laryngeal Diphtheria	17	14

"It will be noticed this report is incomplete. It is on account of the report for the omitted weeks having been mislaid, but we find a total of 3,626 cases of diphtheria with 593 deaths, giving a mortality of 16 35-100 per cent, and 212 cases of laryngeal diphtheria with 130 deaths, or a fatality of 61 32-100 per cent.

"Doctor Shurly, of Detroit, Mich., reports 230 cases treated with antitoxin. The first series of 100 cases shows a mortality of 31 per cent; the second series, 20 per cent; and the last 30 cases 6 2-3 per cent.

"This shows a remarkable fluctuation, and a general average less successful than I believe attends the homœopathic methods without the serum.

"Stanley in the *British Medical Journal*, reports a series of 500 cases of diphtheria, all of which were treated with antitoxin. The deaths in the series were 80, or 16 per cent. Without inserting further statistics, it is sufficient to say the reports of the health boards of New York, Brooklyn, Boston and Philadelphia show there are more deaths from diphtheria and croup than in any recent year except 1894.

"When we reflect that every case revealing the Klebs-Loeffler bacillus, no matter how trivial the case, is tabulated as diphtheria, and that antitoxin has been almost universally used, we are compelled to believe the mortality has not been lessened by the serum. Furthermore, after observing the practice of other physicians, and from our own personal experience, this opinion is confirmed, and we are forced to the opinion that the favourable reports made are on account of the coincidence of recoveries and not on account of cures produced by the use of antitoxin.

"As a result of over twenty-five years of experience in the practice of medicine, I affirm that under homœopathic treatment, pharyngeal diphtheria should only exceptionally prove fatal, no matter how extensive the membrane, how great the

glandular involvement, or severe the toxin infection. Heretofore I have believed nasal and laryngeal diphtheria were usually fatal, but during the past few years the homœopathic treatment has been superior to serum treatment. For these and other reasons, I am constrained to believe the serum fad is a delusion, and am content to abide by our own time-tried remedies."

GLUCOSE IN URINE OF LOW SPECIFIC GRAVITY.

THE following from Dr. S. W. Carruthers, of Cornhill, appears in the *British Medical Journal* of June 24, 1902 :—

It used to be the custom with some insurance companies not to require an examination for sugar in the urine unless the specific gravity were high. It is well known that sugar may be present in more than mere traces with a perfectly normal specific gravity, or even with one that is slightly subnormal.

In an insurance case the other day I found the specific gravity of the urine to be barely 1004 when newly passed ; it rose to 1007 after it had cooled down. I was astonished to get a very marked reaction with Fehling's solution, so marked that after standing for ten or fifteen minutes there was a copious precipitate of red cupric oxide at the bottom of the tube. I confirmed this result with the safranine test, and then proceeded to the phenylhydrazine hydrochloride method, obtaining plentiful and typical crystals of glucosazone. The urine was unfortunately thrown away without my making a quantitative examination as I had intended, but there was no doubt as to the amount being very considerable. I have once before had a distinct reaction with Fehling's solution in urine with a specific gravity of 1005, but was not able on that occasion to do more than assure myself that the Fehling's solution was in good condition, and had no opportunity of confirming the presence of glucose by other tests.—Quoted from the *Calcutta Journal of Medicine*.

A CALCAREA CARB. POINTER IN INTERMITTENT FEVER.

By DR. A. W. K. CHOUDHURY.

WHEN you can study your intermittent fever case properly and thoroughly, and, in selecting medicines, when you select the right one, there is scarcely any fear of being unsuccessful, and the success is apparent after the first dose. Repeat not

your medicine every two, three, four or six hours, like the allopath, in the apyrexia; give one dose (practically the minutest possible dose) in the apyrexia—be it perfect or imperfect—daily, once in quotidian and double quotidian, and double tertian (a pair of tertian fever-paroxysms—one day the severe paroxysm, the next day the mild); one dose every third day in tertian fever, and one dose every fourth day in quartan cases in apyrexia. It is better to administer the dose a little before the termination than in the commencement of apyrexia.

The above are the general hints in the treatment of intermittent fever homœopathically.

I give below a practical hint for the treatment of intermittent fever with calc. carb. This remedy has many characteristic symptoms in general, but I here intend to mention one of its true symptoms for the treatment of intermittent fever. In his work, *Therapeutics of Intermittent Fever*, Dr. H. C. Allen, in describing the times of calc. carb., mentions E. C. Price (I don't know whether he is Dr. Eldridge C. Price, of Baltimore) as furnishing the symptom. It is that the *time of accession of fever is one day at 11 a.m., and the next day at 4 p.m.*

In my practice I have seen many cases of fever of this type. The fever will continue in this manner, getting one paroxysm at 11 a.m. one day, and another paroxysm at 4 p.m. the next day. But these two paroxysms have some differentiating peculiarities in each of them. The paroxysm that comes at 11 a.m. is the severer one, and the 4 p.m. paroxysm is the milder one. The 11 a.m. paroxysm is complete in all its stages, while the 4 p.m. is an incomplete one. For illustration, I give two cases which occurred recently in my practice.

(1) Patient, a female Mahommedan, aged about 15 years, came under my treatment, 16-10-1901, for an attack of intermittent fever. Her case was taken as minutely as was usual with me. Causticum was thought to be the right remedy, and a dose of the 200th was given her. The dose produced no good effect, and why was this failure? It was owing to my inability to ascertain, in the first examination, the type of the fever. In treating intermittent fever one must ascertain first the type, failing to do so he is not certain of success in all cases with the first dose. So it becomes a necessary thing to allow time to establish type of the fever. I gave her placebo the following day, found the fever unabated and unchanged in its course, and could ascertain then the exact type of the fever. The type was as follows: 11 a.m., a paroxysm, the severe one, one day; 4 p.m., a paroxysm, the milder one, the noonday. The 4 p.m. paroxysm was

without chill and less severe, and was without thirst ; apyrexia was incomplete.

According to Dr. E. C. Price, as mentioned by Dr. H. C. Allen, I thought it advisable to administer her *calc. carb.* So *calc. carb.* 30 was given her, daily one dose in apyrexia, and three doses were given. She got rid of fever by these three doses, and no more medicine was required. She then continued under placebo and gradually improved.

(2) Here is another case of intermittent fever of the above sort. The patient here was a boy of about four years, who had been under my treatment and improving under puls., when he got fever of the above type ; a severe paroxysm at 11 a.m., with chill one day ; the next day the mild paroxysm without chill at 4 p.m. ; diarrhœa in chill and heat of fever. Father noticed more marked heat along the spine.

A dose of *calc. c.* 30 was given ; one lesser paroxysm a day ; that day the child had almost no fever ; another dose was given the next day, and there was no more paroxysm of the fever. Bowels improved gradually. He got the first dose on the 28th of October, 1901. He required no more medicine.

The above sort of fever which has some resemblance to the fever of *eupat. perfol.*, which has a worse morning one day and a worse afternoon the next day. Here our homœopathic physicians should be careful not to mistake one for the other.—

Satkhira P.O., Calcutta, India.

From the *Homœopathic Recorder*, April.

DIABETES INSIPIDUS AND LYCOPODIUM.

By DR. BERLIN.

(1) During May of last year M. L., from S., consulted me for hoarseness and a cough attended with whitish, rather watery, expectoration. The fauces were deep red and covered with mucus, and this was accompanied with a constant sensation of dryness and soreness. It was, therefore, a catarrh of the fauces and larynx. The patient had gargled with lemon-water, and put compresses around his throat at night ; he had given up smoking and drinking spirits and beer ; nevertheless the ailment had continued unchanged for two weeks.

I directed him to gargle with salt-water, putting a piece of salt as large as a coffee-bean into a tumblerful of water, so that the water only tasted slightly of salt. I allowed him to continue the compresses around his neck, forbade the use of spirits and beer, as also of all sharp food, as also of very cold or very hot dishes, and gave him internally ammonium brom. 2, every three hours, three drops in a teaspoonful of water.

On June 1st the patient informed me that the hoarseness had been removed in four days, and that the other ailments had disappeared very gradually, though his throat was not yet quite in order, as he was still somewhat hoarse after long continued speaking. I repeated the same remedy in the third potency, five drops every three hours.

(2) After about two weeks the same patient returned and complained that for several months he had been very thirsty, drinking six to eight quarts in twenty-four hours, discharging a corresponding quantity of urine. He felt dreadfully weary and wretched. There were no other ailments. His appetite was very good, so that he could satisfy it as little as his thirst. In the last months he had also become very much emaciated. My suspicion of mellituria was not confirmed by an examination of the urine, for it was free from sugar. It was quite pale, almost like water, and with a very slight specific gravity, namely, 1,004; nor was there any sign of albumen. The patient did not look so very bad. It was evidently a case of diabetes insipidus as distinguished from diabetes mellitus. But little is known as to the cause of diabetes insipidus. Mental emotions, concussion of the brain, and syphilis are given as the leading causes. But I could not discover any such cause in the patient. There is also very little known as to the exact nature of this disease. It is supposed that it is due to a disturbance in the secretive function of the kidneys, particularly a disturbance of the nerves in the central nervous system which regulate this secretion. Prof. Struempell says of it:—

“The issue and, in consequence, also the prognosis of the disease are mostly unfavourable. It is cured but rarely. Its course is mostly very much drawn out, and the disease may last for years, and even tens of years.”

It is especially this unfavourable prognosis made by allopathy which causes me to publish this case. At a meeting of the Medical Society in Breslau, in the year 1894, Medical Counsellor Dr. Schweickert, has highly recommended lycopodium in high potency, and I accordingly used it at that time very successfully in a case, the record of which I cannot now find. So I gave my patient, the second case of diabetes insipidus coming under my observation, lycopodium 30, three drops morning and evening, directing him to call again after he had used up the medicine. As to diet, advised him to resist his thirst as much as possible, and always to drink only a sip at a time, and to avoid all sharply spiced food entirely, as this would increase his thirst. Else he might continue to live as before.

The patient did not call again, which I, of course, took as

an unfavorable sign, as showing that the medicine had not acted, and that the patient had gone elsewhere. But this was not so, for a few months later a peasant called on me saying that he had sought for relief from a number of doctors, and now also wished to try homœopathy because he had heard from Mr. L. that I had helped him so quickly in his urinary treatment after he had tried all the doctors. That patient had not mentioned that fact to me. On questioning the peasant more closely he reported that the great thirst and excessive micturition had diminished after the third day, and that he now felt all right. This the patient had told him. These facts were also confirmed to me later on by the recovered patient himself when I accidentally met him.—Translated from *Beipziger Top. Z. f. Hom.*, Nov., 1901.—*Hom. Recorder* Jan. 15, 1902. Quoted from *The Calcutta Journal of Medicine*.

NEW PREPARATIONS.

“FORCE” FOOD.

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OBITUARY.

JOHN CLIMENSON DAY, M.D. (Lond.)

In our last issue we were unable to do more than announce the fact of our loss in the death of Dr. Day, and promised a fuller notice in our present number. This we now have the gratification of presenting to our readers.

Dr. John Climenson Day, born at Walsoken, near Wisbeach, on January 11, 1824, was educated at Chatteris in the Isle of Ely, and later at Biggleswade. He early showed a great interest in natural history and the study of natural objects. The sandy hills near Biggleswade gave him the opportunity for collecting, and he often spoke of his rambles there with his school fellows.

On leaving school he was asked to choose his career in life—that difficult question, which at first he mistook, and was apprenticed to a chemist at Stamford, Lincolnshire. Here he gained his first acquaintance with the medical profession, and found in his fellow apprentice—Wm. Kitchen Parker—a congenial spirit who was also an ardent naturalist. Whilst here these two young men would make botanical excursions very early in the morning, before business hours, and collect plants and birds and all natural objects which were found abundantly in the Fen districts around Wittleseamere, before this was drained. A fine herbarium was the result of this industry, and there were few English plants with which he was unacquainted, and unable to give the Latin name and natural order.

Entomology was studied at the same time, and he made a good collection of local insects, many of which (*e.g.*, the swallowtail butterfly, *papilio machaon*) are now rare or extinct in England. One recalls the account he gave of his delight at first finding the Grass of Parnassus (*parnassia*

palustris), and his admiration of the lovely Bog Bean (*menyanthes trifoliata*). A true botanist, he admired the wild flowers more than the cultivated specimens of the horticulturist. A bunch of wild flowers gave him infinite pleasure; he recognized them as old friends, and would recall the adventures he had in first finding them.

A favourite story would be told to children, accompanied by suitable action, of the finding an owl's nest in a hollow tree, with several young birds; how by the aid of a stocking they were safely hauled up from the nest and taken home as prizes of the expedition. Animals were brought home and dissected, and thus arose the taste for comparative anatomy, and the desire for the medical career.

Some of his friends observing his tastes and abilities encouraged him to enter the medical profession, and in the year 1842 he joined the London hospital. Here he had a very brilliant career, holding posts in the hospital, and taking the Gold Medal in the Practice of Medicine in the Session 1845-6. He took the diplomas M.R.C.S. and L.S.A. in the year 1846, and the same year he graduated at the University of London with honours in all the subjects he took up, and gained two gold medals, *i.e.*, gold medal in physiology and comparative anatomy, gold medal in surgery, third in honours in medicine, alone in honours in structural and physiological botany. Many years after (1884) he passed the examination for M.D. (Lond.), and on that occasion he and Mrs. Sophia Bryant, D.Sc. (Lond.), were singled out for special congratulations by Sir James Paget, who eulogised their example, showing how persons engaged in the most engrossing work had so utilized their leisure as to qualify themselves to pass severe examinations.

On leaving the hospital he was offered a post in the old East India Company, but he had no wish to go to India, and so settled in general practice in Camden Town, London. It was after some years that a patient put into his hands Dr. Yeldham's little work entitled "*Homœopathy in Acute Diseases*." He was greatly impressed with what he read, and determined to put it to the proof. This he was able to do *sub rosa*, but having convinced himself of the truth of homœopathy, one day made up his mind to "throw physic to the dogs," and emptied the contents of his dispensary away. He was a very firm believer in the power of medicine, and was one of the earlier school of homœopaths.

He lived in probably the most eventful period of medicine. Whilst he was a student, anæsthetics were discovered, and he was one of the first to administer chloroform at the London hospital. Previous to this time the surgeons operated whilst

porters held down the patients, and this probably gave him the distaste for surgery which he always retained. He was intimately associated with Dr. Drury and Dr. Yeldham, and had a profound respect for Dr. David Wilson, and "old John Epps," as he frequently called him. Although he was often asked to join the British Homœopathic Society and attach himself to the hospital, he never did so, his retiring nature shrinking from such publicity. He was greatly beloved by his patients and those who got to know him, but he had a natural reserve which comparatively few were able to understand. He had a strong individuality, and his bright, cheery, and hopeful manner will ever be remembered by his patients. A favourite saying of his was:—"Never give way to despondency, much less to despair." He had the courage to express his convictions regardless of consequences. His religion was so real that no notice of his life would be complete without reference to it—he absolutely scorned the conventionalities of life. Extravagance and fashion he would have none of. A young man just starting to try his fortune in the New World was told to "live soberly, righteously, and godly in the present world." Many of his acts of charity will never be known, as he set his face against the publicity of the subscription lists. Many of the poor he attended gratuitously, or for nominal fees to spare their feelings; the widows and the fatherless were always specially singled out for help.

Last November he took influenza from a poor man he was attending gratuitously, and this was followed by bronchitis; but although he had a severe attack it was thought when the spring came he would recover. However, he never got strong, and his increasing weakness gave cause for uneasiness lest some undiscovered trouble was sapping his strength. Although he was frequently seen in consultation with his son, Dr. Roberson Day, by his old friend Dr. Dyce Brown, so insidious was the disease, that its true nature (lymphadenoma) was only diagnosed late in his illness. He passed away peacefully on August 17th. Wordsworth's lines, which he often quoted, might fitly form his epitaph:—

"Type of the Wise who soar but never roam,
True to the kindred points of Heaven and Home."

PROFESSOR RUDOLF VIRCHOW.

BEING a homœopathic Journal, we make it a rule not to notice the death of any medical man outside of our school, unless in exceptional cases. The death of Virchow forms one of those exceptional cases, and we should show ourselves wanting in respect for a really great man, a very unique character,

and one whose name is a household word in our profession, were we to pass by his death in silence. Instead, however, of penning remarks of our own, we prefer to transcribe from the *Times* of Sept. 6th an extremely able, interesting, and impartial estimate of his life, of his remarkable character, and of his work in the domain of medicine, and in other fields of labour. He died on the 5th of September, in his 81st year. The *Times* article is as follows:—

“A great master of science and one of Germany’s most illustrious sons has passed away full of years and honours, and has left behind him a record of intellectual achievement unsurpassed in our time for vigour, variety, and distinction. The name of Virchow has become familiar to the man in the street, but probably few even of those who are best able to appreciate his work realize its full extent, so multifarious was his activity, so versatile his genius. The rule which warns us that versatility is a snare, and that a man can only hope to excel in one thing, is sound enough for mankind in general, but now and then the ages produce an exception. Virchow was such an exception, and one the more remarkable because the ever-increasing specialization of our time makes it more and more difficult for a single brain to do first-class work in more than one field of activity. Those who attempt more are apt to be mediocre in all, or else they have something morbid—some touch of insanity in the proper sense of the word—in their constitution, and die young, prematurely burnt up by the fire of their own energy. But this singular man attempted many things and was mediocre in none, and so far from having any morbid strain about him, he devoted a very long life to arduous and incessant labour, in which complete sanity of judgment was, perhaps, the most conspicuous feature. Darwin, Pasteur, Helmholtz were pure men of science who eschewed the turmoil of public life; he stands by their side as a great pioneer in the most obscure of all sciences, but he also led a political party for years, and crossed swords on equal terms with one of the greatest statesmen of the century, and at the same time he accomplished more practical work in social and administrative reform than any other individual of his time. Yet he remained free from the taint of ambition and self-seeking. Perfectly simple and modest, he coveted no honours, nor could any decorations have added to the lustre of his name. He respected himself and others, but most he respected truth and justice. A great controversialist and fearless in utterance, he never hesitated to speak boldly and strongly without any regard to the eminence of his opponent or the prejudices he assailed; and this outspoken manner made enemies, as it

generally does. But his mind was always open, his judgment clear and undisturbed by prejudice. If he held strong opinions and expressed them strongly, it was because they were founded on knowledge and reason. Thus he did not hesitate to run counter to the current of popular and professional opinion and of Court favour when his distinguished colleague Robert Koch made the world gape by announcing the famous cure for consumption. Virchow calmly examined the evidence, and pronounced against it on definite grounds. On the other hand, when a few years later the medical profession—having burnt its fingers once—was timidly boggling at the sister discovery of antitoxin for diphtheria, he flung the weight of his authority into the opposite scale with equal firmness, and boldly declared that it was the bounden duty of every practitioner to use the new drug. The whole world shares with Germany the loss of this calm, wise mind, with its brilliant endowment, its immense erudition, its lofty purpose, and inspiring example.

“Rudolf Virchow was born at Schifelbein, a small town in Pomerania not far from Stettin, on October 13, 1821. His parents were of the middle class, and possibly of Jewish descent, at least on one side. There was a suggestion of Semitic blood about his features. Of his early years we have few details, but he must have taken to medicine on leaving school, for he took his degree as *Unterarzt*, which is equivalent to bachelor of medicine, at Berlin in 1843, after a course of study at the Friedrich Wilhelm Institute. He at once applied himself to the scientific rather than the practical side of the profession, became pro-sector of anatomy at the Charité Hospital, and in 1847 external lecturer in pathology at the University of Berlin. Having thus got his foot on the lower rungs of the academic ladder, he soon gave evidence of uncommon vigour by founding, together with Reinhardt, the *Archiv. für Pathologische Anatomie und Physiologie*, which he continued to conduct year by year through all his multifarious interests and distractions down to the closing period of his life. This scientific periodical forms a store-house of original research and observation extending over more than half a century, and typifies the steady, indefatigable yet ardent spirit of the man whose name it bears. While he was engaged in this work, teaching and himself learning pathology, an incident occurred which had a marked effect on his subsequent career. A severe outbreak of typhus fever was in progress in Silesia, and Virchow, who had already made his mark as a young man of unusual ability, was commissioned to investigate it. This fever, it may be observed, was once the most destructive of epidemic disorders among

the poor, exclusive of such exotic visitations as cholera and plague. It is highly infectious and very fatal, bred by poverty and starvation, and fostered by overcrowding and filth. In some countries it is still rife, but it has become very rare in all advanced communities, and is rapidly dying out. Virchow found a state of things in Silesia which shocked him, and with characteristic contempt for everything but truth he wrote a report denouncing in strong terms the evils he had seen, and recommending extensive reforms as the only remedy. The report made a great stir, and had a threefold influence on his career; it brought his name prominently before the public, it turned his thoughts from science to social questions, and it rendered him suspect in the eyes of official authority. This was in 1848, and in that period of political uprising the next step was almost inevitable. Virchow became a politician, and a leading speaker in the democratic clubs of Berlin. Election to the Prussian Assembly followed, but it was rather an Irish proceeding, as he was under Parliamentary age and could not take his seat. The speedy reaction in favour of the Government left the young Radical stranded in politics, and hopelessly damaged by his outburst in the timid circles of academic authority.

Here he might have fallen to the ground and become a discredited hanger-on with a grievance. But his work had been too good. The weakness of Berlin was the opportunity of Würzburg, which offered him the chair of pathology. Once more he devoted himself entirely to science, and the outcome was the famous *Cellular Pathology*, published in 1856. It is difficult to explain in simple language the importance of this work, or even the precise meaning of its title. Pathology is the science of disease, or of life under morbid conditions; it is a step beyond physiology, which is the science of life under normal or healthy conditions. No exact line can be drawn between them, because it is impossible to say where health ends and disease begins; but, obviously, pathology embraces a great many conditions beyond the range of physiology, and is by so much obscurer and more complex. Its real object is to provide a firm basis for rational treatment. In order to deal with disease otherwise than by sheer empiricism, we must know what it is, what are the processes going on which constitute disease. Before Virchow's time these questions could only be answered in a vague and confused manner, which left room for divers theories, none of which contained any real enlightenment. Disease was thought to be a sort of entity taking possession of the body or of particular tissues and to be treated as such. Doctors treated the disease by rule of thumb; they did not treat the

condition which gave rise to it, because they did not know what it was. Virchow forged the key to unlock the closed door, and established the principle on which all subsequent study of the nature of disease is founded. He showed that the cell is the unit of life in morbid as well as in healthy conditions, and that every cell, originates directly from a pre-existing cell. Even the most abnormal structures are derived from normal cells, driven to abnormal development by injurious agencies. This gives an intelligible working hypothesis, which subsequent research has only confirmed. Bacteriology itself, with all its developments, of which aseptic surgery is one, is based upon it. Like most of the great steps in science, Virchow's cellular pathology was founded on the work of others, notably of Schwann and Müller, and Paget's profoundly philosophical work on surgical pathology had been published a few years before; but the broad grasp and the masterly command of details which could gather up the fragments into a complete and luminous whole were entirely his. In short, he gave the Art of Medicine a real scientific basis.

“The book created a profound impression in scientific circles. At one bound the writer reached a leading position, and Berlin University, recognizing the fact, offered him the important appointment of Professor of Pathology. His career was now completely assured, and the students who thronged his lecture room spread his fame far and wide. For some time he devoted himself entirely to scientific work, and published a great number of papers on many subjects, all marked by the same originality and power. But presently public life attracted him again, and he chose a less stormy field for the exercise of that reforming spirit which still animated him and, indeed, remained with him to the end of his life. He became a member of the Municipal Council of Berlin, a position which gave full scope to those remarkable powers of organization which were not the least striking of his qualities. The capital of the German Empire owes much to him. Were hospitals to be erected, the water supply improved, drainage to be carried out, the police to be reorganized—Virchow was always consulted. In these and many other matters his was the guiding hand and the inspiring brain. He carried on this practical work for many years, while immersed in teaching, in original research, and in the production of innumerable scientific memoirs. Yet even this was not enough for his extraordinary energy. In 1862 he was elected to the Prussian Chamber by no fewer than three constituencies, and he took his seat for one of the electoral colleges of Berlin. The fiery days of his youth

were passed, but, though more than forty years of age, he threw himself into Parliamentary life with the ardour and elasticity of a young man, and speedily rose by sheer ability to be the leader of the Radical party. In this position he had his famous passages of arms with Bismarck, which were long remembered against him in Germany, but could be regarded with equanimity by those outside the arena of party politics. He was a determined opponent of the Reactionary school, and in January, 1863, persuaded the House to carry a resolution condemning the Government. He was a notable speaker, and his language was said to be extremely violent—by his political opponents, who were not, perhaps, the best judges. Others, who have crossed swords with him in scientific matters, have said something of the kind; but disinterested observers have seen rather in his controversial methods the mind of a strong man convinced of his ground and not afraid to say so. This is somewhat irritating to opponents, no doubt. It so irritated Bismarck, when in 1865 Virchow defeated the Government on the motion to create a navy, that he challenged the Professor to a duel.

“When war broke out in 1866, Virchow found another sphere for his activity. In the campaigns of that year and of 1870-71, he assisted in the organization of the ambulance work: and war had the sobering effect upon him that it has had upon other sincere minds, even in politics. He considerably modified his opinions, though he continued to lead the *Freisinnige* party until 1878, when he resigned the leadership to Eugene Richter, and retired from active political life, on the ground that he could no longer be useful. In 1880 he became a member of the Reichstag, and, though he took comparatively little part in affairs, he was as plain-spoken as ever, when he did say anything, and earned no little unpopularity thereby.

“In science his chief work was in pathology, but his range extended far beyond that sufficiently absorbing study. He was deeply interested in public health, in anthropology and archæology, and was an acknowledged authority in all. He was equally at home among Dr. Schliemann's investigations in the Troad and in the controversy with Huxley over the Neanderthal skull, which was disposed of by his superior pathological knowledge. He visited London several times, but the most important occasion was in March, 1893, when he delivered the Croonian lecture before the Royal Society in the great theatre at Burlington House. He reviewed the progress of physiology in a masterly address, which all the most eminent scientific men of the day gathered to hear. In 1898 he delivered the Huxley lecture at Charing-cross

Medical School, and took for his subject "Recent Advances in Physiology," which he handled with all the ardour of a young man and with the breadth of an old one. Lord Lister paid him an eloquent tribute of admiration, and the venerable Sir James Paget, with characteristic modesty, spoke of himself as Virchow's pupil. The completion of his 80th year last autumn called forth a world-wide demonstration of respect, which probably still lingers in the memory of our readers. Until his last illness he never ceased to work" .

CORRESPONDENCE.

To the Editors of the "Monthly Homœopathic Review."

HUGHES' PRINCIPLES AND PRACTICE OF HOMŒOPATHY.

61, SHREWSBURY ROAD,

BIRKENHEAD,

September 5th, 1902.

DEAR SIRS,—Please allow me, as a personal friend of the late Dr. Hughes and one who has worked with him and is acquainted with some of his many excellent qualities, to endorse and emphasize your justly appreciative notice of the above-named book.

I agree with you that it is almost the perfection of a Homœopathic Theory and Practice, "unique in our homœopathic literature; "an epitome of all that is known about the nature, origin and practice of homœopathy"; "a masterly epitome of the best and most modern homœopathic practice." It should certainly be in the hands of every homœopathic practitioner, being well calculated to render homœopathic treatment of disease increasingly easy and successful. Dr. Hughes has performed for us the labour of search and selection of the most appropriate medicines for the majority of diseases. And the book is in one handy and well-indexed volume.

There is one remark in your otherwise correct appreciation of the towering height Dr. Hughes had attained as a disciple and exponent of the Master to which I think he would himself object, *viz.*, your calling him "the greatest of Hahnemann's disciples." No one can have a higher opinion than I have of Hughes as a disciple and exponent of Hahnemann, but there have been and are other great and faithful disciples. Hughes was certainly one of the greatest.

Yours truly,

JOHN W. HAYWARD.

NOTICES TO CORRESPONDENTS.

. *We cannot undertake to return rejected manuscripts.*

AUTHORS and CONTRIBUTORS receiving proofs are requested to correct and return the same as early as possible to Dr. DYCE BROWN.

The Editors of Journals which exchange with us are requested to send their exchanges to the office of the *Review*, 59, Moorgate Street, London, E.C.; or to Dr. DYCE BROWN, 29, Seymour Street, London, W. Dr. POPE, who receives several, has retired from practice for the last two years, and now lives at Monkton, near Ramsgate.

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Communications have been received from—Dr. ROBERSON DAY, Dr. DUDGEON, Mr. KNOX SHAW, Dr. BURFORD (London); Dr. J. W. HAYWARD (Birkenhead); Dr. A. C. CLIFTON (Northampton); Major DEANE, R.A.M.C. (Calcutta); Messrs. BOERICKE & TAFEL (New York); Messrs. CADBURY, Bros., Ltd., (Birmingham).

BOOKS RECEIVED.

Associated Kidney Lesions in Women. By James W. Ward, M.D., San Francisco. *The Present Status of Homœopathy.* By James C. Wood, M.D., Cleveland. *Report of the Plague in Calcutta*, June 1901 to June 1902. By Major Deane, R.A.M.C., Calcutta, 1902. *The Homœopathic World*, September. *The Vaccination Enquirer*, September. *Dioptric and Ophthalmometric Review*, London, September. *The Homœopathic Recorder*, August. *The Medical Brief*, September. *The Hahnemannian Monthly*, September. *The Homœopathic Envoy*, September. *The Minnesota Homœopathic Magazine*, August. *American Medical Monthly*, August. *The Clinique*, August. *The Pacific Coast Journal of Homœopathy*, August. *The Medical Times*, New York, September. *The Medical Century*, September. *Leipziger Populäre Zeitschrift für Homöopathie*, September. *Allgemeine Homöopathische Zeitung*, August 28 and September 11. *La Propaganda Homœopatica*, Mexico, August. *Boletim de Medicina Homœopatica*, Feb., March, April, San Paulo. *Annaes de Medicina Homœopatica*, June. Rio de Janeiro.

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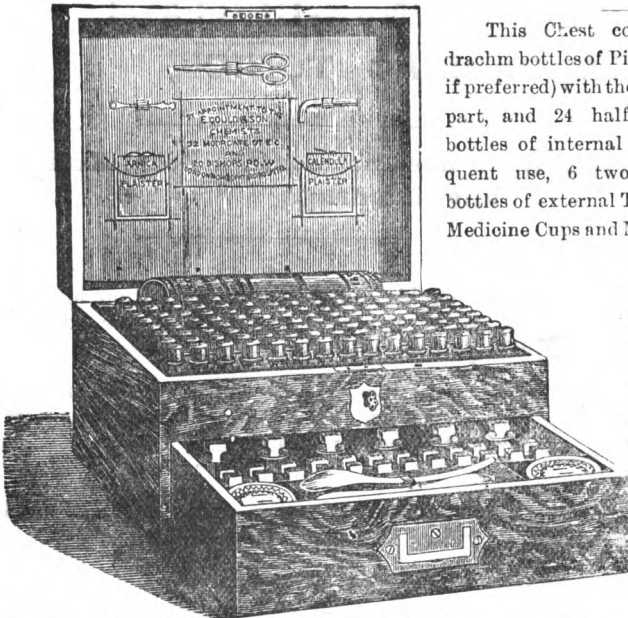
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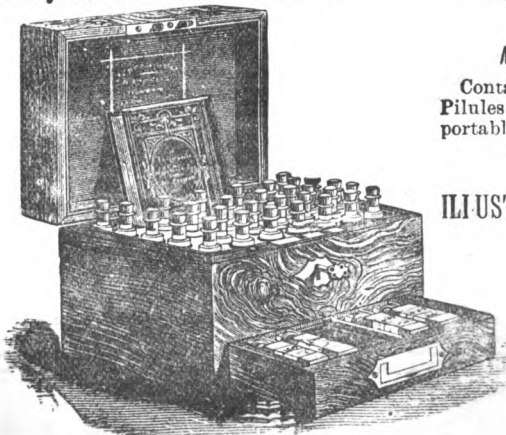
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