

A letter to Professor J.Y. Simpson, President of the Royal College of Physicians ... concerning the resolutions recently passed by that body in reference to the therapeutic practice commonly called homoeopathy / by William MacLeod.

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A.
LETTER

TO

PROFESSOR J. Y. SIMPSON,

PRESIDENT OF THE ROYAL COLLEGE OF PHYSICIANS,

&c. &c. &c.

CONCERNING THE

RESOLUTIONS

RECENTLY PASSED BY THAT BODY IN REFERENCE TO THE
THERAPEUTIC PRACTICE, COMMONLY CALLED

HOMŒOPATHY.

BY

WILLIAM MACLEOD, M.D., F.R.C.P.

&c. &c.

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NOTE.

DR MACLEOD regrets that distance from the press, and other circumstances, have delayed so long the publication of the following remarks. But the interest of the subject is not a transient one, nor even chiefly personal. For the convenience of the reader, and to avert the chance of misapprehension, Dr Macleod has subjoined two Appendices, containing—1. The Resolutions of the College of Physicians, as printed in a recent number of the *Monthly Journal of Medical Science*; and, 2. Professor Alison's statement of the process by which, in the Allopathic system, the action of Remedies is conceived to be ascertained.

28th June.



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LETTER.

BEN-RHYDDING, OTLEY, YORKSHIRE,

June 20, 1851.

SIR,—When I received the recent resolutions of the Royal College of Physicians of Edinburgh, condemning practitioners of Homœopathy, I did not at first consider it incumbent on me to take public notice of them. Doubtful whether they could fairly be applied to one holding the principles in accordance with which I have regulated my professional course, and, at the same time, prepared to disregard their denunciation, and deny the authority of their requisitions, in case of their being so applied, I discerned no pressing practical necessity to take upon myself the office of remonstrant, or to act otherwise than as if the resolutions had never existed. But circumstances have in so far changed. The period which has elapsed since the promulgation of the decree of the College is a brief one ; yet enough has transpired to convince me, that, on the ground of the contents of that decree, an attempt is being made to misrepresent

the claims and injure the practice of Homœopathy, not by controverting or even examining its doctrines, but through means of disreputable imputations regarding every one who has embraced it ; and it is not consistent with my feelings, or sense of duty to my friends and myself, to evade my share of the responsibility by preserving an ambiguous silence, or to pass unnoticed questionings from whatever source—anonymous or official—as to my honesty and honour. I regret only that I must mix up so much that is personal with the discussion of a subject rightly belonging to abstract dispassionate science ; but in the present peculiar position of affairs, I deem it necessary to state without scruple to the profession, and to explain to the public, the rights which I claim as a Physician, and the grounds of the independence I shall continue to exercise.

I.

There are still many in Edinburgh not unacquainted with the history of my early medical career. I began it an ardent student, and during nine months each year, and for years together, I was an occupant of the dissecting-room, on an average, six hours a-day. I laboured with zeal in the pathological department of the Royal Infirmary for upwards of three years. I performed nearly all the weights and measurements made there during that period. I devoted much of my time to

microscopic studies. I went round the wards of the hospital, when the physicians were not there, regularly for several years, watching and examining for myself every case of interest and practical importance ; so that I might be able to comprehend, with as much completeness as was possible, the progress of diseases, their varied phases and tendencies, and also the effects of the treatment pursued. I assisted in the examination of almost all the bodies of those who died in the hospital. I then compared the diagnoses of the disease made by the physicians in attendance, and analyzed carefully the effects of the remedies used in the treatment. I cannot, of course, avoid assuming, that to such studies I brought an average amount of previous culture, of intelligence, and discrimination ; and my early companions can tell that I was neither slothful nor indifferent. And what, even thus early, were my conclusions ? Allowing for all important and inevitable error in diagnosis, I could draw no inference save this, viz.,—that the practice pursued was erroneous, based on no settled principle, without science, and consequently a blind and therefore necessarily a bungling Art. Nay, not only erroneous ; I was forced to conclude also that such treatment is dangerous, frequently undermining the constitution for life, and sometimes hastening Death. Then I formed the resolution—one which I have religiously kept—never to have to do with what is commonly called the Drug or Allopathic Practice, but to devote my energies to the study and teaching of Anatomy and Physiology.

Soon after the period to which I am referring, a circumstance occurred which had great influence on my future views. While I was connected with the Argyle Square School of Medicine, one of the physicianships to the Old Town Royal Public Dispensary became vacant. The late Dr Campbell, lecturer on midwifery, requested me to apply for it. This I at first declined, but after some pressure, and the promise that I should not be asked to attend, I consented. One afternoon I was unexpectedly called from the dissecting-room by a Pupil in the Dispensary, to visit a person resident in the Cowgate, who was seriously ill. None of the other physicians being in the way, I was obliged to go. When there, the Student asked me whether I should like to see a case treated homœopathically. This was at the time when Dr Wood was publishing his very inconclusive papers upon that subject. I had not read these papers, nor in fact did I know anything of the system. Out of mere curiosity, however, I went and saw the case. There, and for the first time, I met Dr Russell, who entered into a short explanation of what is termed the Homœopathic Law. I became interested, and, after reading upon the subject, I made up my mind to put it, as far as I had the opportunity, to the test of experience. I did so, for I think, about a fortnight, in the Royal Public Dispensary, when an unforeseen circumstance interfered with my plans. This was near the close of our winter session; and I resolved to take advantage of the vacation, and examine the subject closely at the Homœopathic Hospital

in VIENNA. For four months I watched with much anxiety the results of the practice there; studying at the same time the negative practice pursued by Dr SKODA, and the allopathic under one of his colleagues. After carefully comparing the results during that time of these several treatments, I was forced to the belief that the homœopathic is the most efficient; the negative, the next; and the allopathic, not only the least so, but really dangerous, and *that* even in the most skilful hands. I did not become a Homœopathist, for my experience had been as yet too limited to allow me to accept the System in its entirety. Thus far, however, I felt on secure ground;—I regarded it established, by the unquestionable facts I had witnessed and scrutinised, that *medicines, given in quantity not amounting to more than the ten thousandth part of a drop of the pure tincture, have, when properly administered, a powerful effect in controlling and destroying many diseased actions.*

While in Vienna, accident brought me into contact with various persons who had derived benefit from what is now rather unhappily termed the *hydropathic* treatment. Being a searcher after truth, and never caring much from whom it came, provided I could discern and appropriate it, I proceeded to one of the most celebrated establishments of this description then in Germany. I remained there for some time, and my acquaintance with the phenomena of life soon enabled me to comprehend the principles upon which the treatment is based. Before seeing much of its actual results, I recog-

nised the scientific importance of Water as a curative agent ; and because of the close though indirect relations of the subject with my favourite pursuit, Physiology, I devoted myself for a time to an earnest investigation of it ; chiefly, however, with theoretical aims, as I then had no intention to practise this peculiar treatment. But I saw likewise, that unless in the hands not merely of a medical practitioner, but of one thoroughly and pre-eminently acquainted with the phenomena of life, Hydropathy might hazard the worst results. Simple though it appears, it is an edged tool, unfit to be used by the ignorant. The Physician who wishes to follow out this branch of the practice of medicine ought to be a sound Anatomist ; he must have studied with care, and practically, the development of tissue and of animals ; he must have made himself acquainted with the varied states of the vital force in the different epochs of life ; he should be no meagre Physiologist, and must have a keen diagnostic sense of the reactive powers of the frame.

I returned to Scotland, a believer in the efficacy of minute doses of medicine, when properly administered, in the cure of many diseases ; and with a glimmering of the pathological laws upon which this efficacy is based : but with no desire to join any sect. My intention was to work out earnestly and quietly my own views, to assume no name, and to avoid all dogmatism. With this object I recommenced lecturing ; and I confess I then longed for the appointment of Physician to the Royal Infirmary ; hospital practice being essential

for the realisation of my aims. But—as recent occurrences render it unnecessary to state—the opinions towards which I felt disposed, were no favourites with our authoritative medical men : and I quickly discerned that in Edinburgh my medical experience was likely to be limited within the sphere of private practice, which, although best for the pocket, is not sufficient for testing opinion. Aided by a Friend of some rank and great interest, I then thought of office in a distant clime, where to a certain extent I might have realised my hopes. At this critical juncture, however, and when I least expected it, a fit opening occurred in my own country. The office of physician to this establishment at Ben Rhydding had become vacant. When I saw the spacious building, and dwelt on the rare opportunities it might afford for the development and promulgation of Truth, I was moved so powerfully, that on the spot I offered to the chairman to accept the appointment with little regard to pecuniary remuneration, provided I should be allowed to perform its duties absolutely free from control, and irrespective of sect or party. The inestimable boon was granted me ; and, at the same time, my offer of sacrifice generously refused. I have since spent five happy years in discharge of the duties of my office, and have enjoyed the privilege of treating no fewer than six thousand cases.

II.

I have been forced into these statements, Sir, simply because I will not have it said or suspected, that any opinion of mine has been formed without due consideration, or before I had exhausted every opportunity within my reach, to obtain that information which alone could enable me to judge. Such proof, indeed, would not, in ordinary circumstances, have been either offered or required. But it is not to be overlooked, that by those writers in the professional press, who have chosen to take part in this controversy, insinuations of the most unworthy description have been, and still are being, lavishly scattered, with a view to damage at all hazards the reputation and authority of the objects of their displeasure ; and I have thought it a duty to truth to place on record, such undeniable facts, as seem sufficient to meet a course of remark, to which no member of a liberal profession ought ever to have stooped, and which, but for the public considerations involved in the discussion, I would certainly have treated with scorn.

Let me pass to a more welcome and important task. I mean to explain now, without reserve, and as distinctly and briefly as I can, the nature of my position and opinions regarding the points in dispute. The College has thought fit to send forth one challenge, which, for reasons I shall soon give, I do not mean to accept : at least I shall accept it in my own way ; I shall lay bare

all my heresies—if heretical I be—so that no doubt or hesitation, in so far as I am concerned, may interfere with the future proceedings of that corporate body. The mere statement, however, that I am or might be fancied a homœopathist would not suffice for my present aim. Before penalties are inflicted they should be righteously apportioned: for which reason it seems essential that those relations be understood which the obnoxious system can be said to hold to medical science and its resources. Apart from clear and well-defined notions in this respect, it seems to me impossible to determine accurately, to what extent certain deviations from current opinion, involve, or can be supposed to involve, disregard of obligations to the College.

I. There is one grand range of diseased actions, in the treatment of which neither homœopathy nor any system of pure therapeutics ought to play any part: I mean the ailments springing from disarrangement of those fundamental CHEMICAL and PHYSIOLOGICAL operations which build up and sustain the vital frame. From ORGANIC CHEMISTRY the physician acquires an exact or scientific knowledge of the substances of which the healthy structure is composed; and he may thence deduce a sound hygienic theory of regimen. If disease has arisen from deficient supply or excess of any essential element, and if by skilful diagnosis he has detected the fact of this excess or deficiency, it is of course his duty to regulate regimen by the discovery, and to exhibit if necessary, although

not in the form of what is usually termed *food*, whatever can re-adjust the composition of the organic structure. I allude here to the introduction of substances furnishing in abundance oxide of iron, phosphate of lime, &c. Organic chemistry demands, indeed, more. It requires, for instance, that we see to the presence of substances whose function it is, as *media*, to facilitate the progress of decomposition and renewal ; but as nothing in homœopathy is connected with this practice, I shall not prolong consideration of it. I would remark, however, that even in this, apparently the simplest branch of the practice of medicine, the most skilful can scarcely boast that his knowledge is final or complete. In recent years serious innovations have disturbed former opinions ; unsettling, often inconveniently, what was assumed to have been settled long ago. Witness the astounding, or, to say the least, the very sweeping dogmata of MULDER and LIEBIG.*

Much more important and comprehensive, however, is that branch of the healing art which draws its practice from a right interpretation and use of the PHYSIOLOGICAL laws. It needs not at this time to be recalled to any one, that a power or energy unlike every other that is known, and which we term the VITAL FORCE, presides over the entire functions of the organic structure, and gives harmony to the action of each individual part. Now,

* LIEBIG has thundered against Homœopathy. It is the fault of Genius to seek to extend unduly the authority of its favourite conceptions and spheres of Inquiry. The fault is a mere speck in magnitude ; but LIEBIG has not escaped it. There is only a faint relationship, and certainly no collision, between the objects of his Investigations and the aims of Homœopathy rightly understood.

I am certain that I agree with our best physicians in confidently asserting, that the greater proportion of the ailments which come under treatment arise in the languor or undertone, general or local, of this preserving and controlling energy. In their origin, at least, they are functional derangements merely, to be cured of course only by the restoration of the power of the function ; an office which no *medicine* can perform ; for although a drug or artificial stimulus may relieve obstruction and produce temporary excitement, it never can strengthen the tissue or organ which primarily gave way, and therefore permanently remove the patient's name from the visiting-book. It is thus a foremost question, *By what means can the VITAL FORCE be re-invigorated ; by what appliances can we restore the energy it needs, so that it evolve a healthy organism ?* Nor is the answer to seek : it was given long ago by the father of English medicine on his deathbed—the immortal SYDENHAM :—“Do not despond,” said he, “for medicine : I leave with you the three great physicians—AIR, WATER, EXERCISE.” The maxim is now old ; nay, though seldom expressed so tersely or by such authority, it is as old as all truth, and the existence of man's ordinary instincts ; but the cordial acceptance of it in professional practice is not yet so old. I am not certain, indeed, that individuals are not still to be found, who might pamper a patient, amid heated rooms, by administering substances, foreign to the living structure, under the name of *tonics* ; just as, in previous times, wine and brandy were considered ele-

ments of strength. Nevertheless, and notwithstanding the persistency of a few stray aberrants, it can be said with rejoicing, that, within the last fifteen or twenty years, the views of SYDENHAM have advanced with most rapid strides towards their rightful influence. In the course of a brief portion of that interval, the necessity of obedience to physiological laws has been recognised, and sought to be enforced in practice by the great Councils of our Nation ; for *this*, and nothing less, is the significance of the growing triumph of sanatory Legislation. Nor can I avoid remarking, that the same period records the origin of what is unquestionably (I avow it nothing the less willingly as I may now do it in safety,—coveting nothing of martyrdom*) the greatest practical benefit conferred in modern times on mankind by the healing art :—I mean,

* I point, for the assurance of our timid hydropaths, to an article in the last number of the *British and Foreign Medico-Chirurgical Review*, on the use of baths—an article slight in itself, but sufficient to mark the direction of the wind. It is mainly an analysis of a volume, by an excellent and instructed American physician and hydropath, Dr John Bell of Philadelphia, bearing the prudent title of “A Treatise on Baths.” Taking his cue from the title, the reviewer ventures to allege that the doctrines in question had long influenced medical practice—a delusion readily forgiven him, since he would help the said doctrines to be recognised as part of authorised practice now. He assents, in the course of his remarks, to almost every important advantage claimed by hydropathic practitioners for the use of water—among others, to its efficacy in scarlatina, typhus, &c. He assigns the credit, not, of course, to unlucky hydropaths, but to certain spongers; forgetting, however, to ask, or at least to answer, two questions—1st, Whether the said spongers would not have sponged more largely, and with surer effect, if they had *systematically* studied the subject, as the hydropaths, amid all their theoretical, and, still worse, practical, blunders, certainly attempted to do? and, 2dly, Whether these same errors might not have been lessened, if not entirely avoided, had the spongers co-operated in so far with the hydropaths, inquiring along with them and correcting them, instead of treating them with such denunciations as were fashionable in times of which the reviewer and I may alike recollect, since they are not so very long bygone?

the study and scientific development of the use of Water as a natural stimulus. With the efficacy of this agent in raising the tone of the languid frame, men have probably been acquainted from the date of the origin of our race ; but it seems to have been strangely overlooked, that it can be employed in a greater variety of ways than any other similar hygienic or curative agent. Besides having the power to augment the general vigour, it can be made to increase the circulation of the blood, and regulate its movements ; it enables us to strengthen the organs of digestion, to stimulate the bowels, to raise the tone of the brain—thereby giving to the nerves of volition a greater control over the excito-motory system—a system upon which the existence of many diseases greatly depends. I have already expressed myself as to the skill and caution requisite in using an agency so powerful ; but in proportion to the hazard springing from an injudicious intermeddling with such a power, is the extent of the sphere of its applications, and the amount of its beneficence. Nor do I believe that we have yet duly appreciated either its marvellous flexibility, or the variety and magnitude of the resources which it lays open to the considerate Physician.*

* The beneficial actions of water are, the Hygienic, Therapeutic, and what may be termed the Chemical. By its Hygienic agency we increase the general vigour of the frame. By aid of it, as a Therapeutic agent, we obtain command over classes of *specific* diseases, where the cause resides in the derangement of special organs. Its applications in this latter sphere have not yet been formulised ; and certainly they require the exercise alike of the caution and scientific resources of the Physician. Lastly, it promotes chemical decomposition within the frame chiefly as a *medium*, but at the same time with great effect. It is no

II. In regard of the details of a subject, so large and arduous as the pure physiological treatment of disease, it can scarcely be expected that a numerous body of men should arrive early at unanimity; but I hazard nothing in asserting, what is enough for my present purpose, viz., that the practitioner who does not lean principally on SYDENHAM'S memorable maxim—who ventures to lay violent hands on any structure, or to introduce into the system substances naturally hostile to it, unless the application of that maxim, and all effective direct action on the vital force, have been forbidden by paramount and uncontrollable circumstances,—that such a practitioner has not sought the best aid of modern Science towards alleviation of the ills of humanity, and has not the support of the soundest Authorities of our time. This being true, the field of the present discussion is considerably narrowed; for it follows, that only in cases of *abeyance on the part of Physiological practice*, ought THERAPEUTICS, under our present lights, to be resorted to,—THERAPEUTICS signifying, technically, *the introduction into the organised System, of substances alien to its composition, and for the most part inimical to its structures, in order that, by specific action on individual parts, certain curative changes may be produced.* Several causes may affect

secret how much the vigour of a part in all animals depends, *within certain limits*, on the rapid waste of tissue and the equally rapid supply of new materials. Water assists in this way in removing from the organism, through the excretory organs, the different effete matters, either naturally formed in them or taken in from without. Moreover, its effects are remarkable in bringing out of the system, drugs which may have remained in it for years, to all appearance *dormant*, but probably causing great and long-continued ill health.

the applicability to individual instances of physiological practices ; in some cases, perhaps, the circumstances and position of the patient ; but the leading hindrance is this,—derangements long neglected may issue in diseases so rapid in action, that the physician dare not wait for a comparatively slow, even though sure restoration, by the agency of natural stimuli. The question as to treatment thus becomes, in so far, one as to *time* ; a principle which excludes also from the field of existing debate, that class of serious cases in which the rule is prompt and decisive action, in which the practitioner has not the opportunity of introducing much systematic treatment, but is obliged, at whatever risk, to enable the sufferer to survive for three minutes if only he can prevent his dying in two.* Now, whatever be in this latter remark, to which, indeed, I care not to attach importance, one thing I think must be plain, viz., therapeutics, as above defined, is, under any of its forms, only that *remnant* or *residual*—although very important—portion of the art of medicine which the higher, more assured because more natural Practice, has not yet absorbed. It is, so to speak, *outfield* ; not to be neglected, indeed, even as an outfield, but on which it is the hope and, as I solemnly believe,

* I might add to the sphere of *common ground* at present, that practice which still continues more especially and strictly *Empirical*. For instance, we have learned, from the researches of Matteucci, Dubois, Raymond, and others, the existence of relations between galvanism and the activity of the vital force ; although the *mode* of these relations continues unknown. A ground has thus been established for an Experimental or Empirical application of this energy in certain cases ; but it is only from a further advanced Physiology in this direction, that we can expect *laws* or *fixed principles* by which to regulate definitely such applications.—The same remarks apply to the external use of caustics, &c.

the destiny of Physiology to make, every year that passes, some novel, salutary, and memorable inroad. Who knows better than you, Sir, that the drug practice of our day is that which least of all can be expected to preserve its existing influence? Reflect for a moment on the principles, if I may call them so, not of any remote century, but which commanded allegiance some fifty years ago! How many of them have given way before ever-increasing knowledge! How many impediments to the application of true curative principles have been removed by the progress of society and the amelioration of social conditions! How many derangements we can now arrest in their origin, preventing them from developing into morbid disease; and even where disease has supervened, how different the treatment! Look at that appalling mortar in our museum, which belonged to your illustrious relative CULLEN; compare it with your own; and sure I am you cannot dream that *yours*, like the famed jug of Linlithgow, shall go down as a fixed measure of capacity to all future time! Well, then—and concerning the fact let there be no mistake anywhere—it is *because of their relations to this same Outfield*, because they hold what are denominated heretical views regarding the method of using it, in the mean time,—because of *this and this alone*, that from your seat as president of the College, you have consented to pronounce against certain of your fellows the severest condemnation, to inflict the heaviest penalties within the power of any corporation. You did not, indeed, expel them, but you proclaimed, that if they

were men of honour and honesty, they would confess their unworthiness and withdraw; nay, further, you warned, perhaps I should say debarred, all who owe you fealty, from holding intercourse with them, or recognising them in any case as instructed, skilful, or trustworthy physicians. Surely this was a harsh proceeding, and not easily justified. Would you refuse, for instance—say to the first Physiologist of Europe—the privilege of your membership simply because he may not condemn the principles of Hahnemann? Could such a man throw no acceptable light on the nature of disease? Might he not aid in diagnosis? Nay, Sir, I am not sure but there are points connected with your own special and very remarkable practice, in reference to which, notwithstanding his heresies, he could offer certain quiet counsels not wholly irrational. You have considered apparently, and decreed the taint of homœopathy to be so malignant, that the mind it has infected can be sound nowhere—the knowledge with which it mingles, no matter how extensive or profound, productive only of bitterness to mankind. If this be true, the taint must indeed be a black one. I must beg you to examine it with me.—In the scheme which follows, I have used my own language, and expressed simply my own views: I believe that, in the main, they are concurrent with Hahnemann's (although perhaps he did not see with sufficient clearness the general principles upon which his maxims rest); but in all such cases I dislike employing the phraseology of a sect. The first and second columns are placed in op-

position to each other ; nor does there appear at present any bridge leading across.

SCHEME OF PREVALENT THERAPEUTICS.

I. PRINCIPLES OF RATIONAL OR SYSTEMATIC PRACTICE.

1. If the causes of disease were always known, or within reach, the process of cure would consist in destroying or abating them. But though causes cannot always be reached, the organic seat of the disease is generally known. Certain classes of diseases affect certain tissues or organs ; and the same tissue or organ, when diseased, or unnaturally excited by any cause, gives rise uniformly to the same class of disagreeable sensations, or *symptoms*.

2. It is found that two diseases cannot co-exist in the same tissue ; the more powerful one uniformly pressing out or extinguishing the other. If, when a tissue has become diseased from unknown causes, it were possible to introduce into the same tissue, by some known or controllable agency, another disease of due amount or intensity, it follows that the original one might be extinguished ; and as the indirect disease or excitement depends on the presence of an irritant we command, *it* too could be withdrawn at will, and its effects destroyed.

3. Medicines are artificial stimuli, in the main hostile to the organic structure, which act specifically ; that is, a certain medicine irritates primarily its special

II. PRINCIPLES OF EMPIRICAL OR TENTATIVE PRACTICE.

1. The causes of disease are in no case entirely known ; but, unless we can approach these, we must find the greatest difficulty in ascertaining to what extent the exhibition of a supposed remedy really exerts beneficial effect on morbid changes.

2. To discover the remedy for a disease, observations must be multiplied on individual cases to which any particular remedy is applied ; and the varying circumstances of these cases carefully noted ; so that extensive statistical evidence be obtained as to the efficacy of the remedy, and the influence of the other causes acting simultaneously.

3. The discovery in question is retarded by the following causes : —1st, The natural tendency of diseased action to spontaneous abatement, which is very fre-

tissues or organs. If such specifics could be found for every tissue, or, what is the same thing, substances capable of evolving diseased actions *analogous* to those of every known disease, we should obtain a sure and complete *Materia Medica*; for, on the ground of our second principle, a method would then exist for overcoming any unhealthy action. But, to discover these specifics, we have merely to test medicines on the healthy organism, and to arrange them into classes and species, according to the diseased symptoms they produce. For constructing such a *Materia Medica*, there is thus open to the inquirer an unlimited sphere of experiment.

4. The precise mode in which medicines change the condition of a tissue is unknown. It may be by chemical action, or by the excitement of physical energies, such as electricity, &c. But it is established by experience, that on certain diseased states medicines act powerfully if administered in very small quantities minutely subdivided or in solution, and when the doses are repeated at brief intervals. This truth was suggested by various and obvious processes of nature; but the physiological theory of small doses is still a desideratum.

quently mistaken for the effects of the remedies. *2d*, The unknown natural influence of other circumstances in the situation of patients; as of the antiphlogistic regimen when depletion is employed, or of change of season or scene during the operation of tonics. *3d*, The extreme diversity of diseased actions in themselves, although belonging to the same class; and therefore corresponding doubt as to the probable result, independently of all remedies. *4th*, Diversity of constitutions, causing important modifications of the result under any practice.—There is thus no sure test or *experimentum crucis* with regard to any remedy.

4. Assuming that medicines act in proportion to their quantity or weight, it necessarily follows that there can be no virtue in infinitesimal doses.

N.B.—The first three of the foregoing clauses are transcribed, although not verbally, from Professor Alison's "Outlines of Pathology," Chap. IV. See *Appendix*, II.

III. RESULTS AND PHENOMENA ACCEPTED IN BOTH PRACTICES, AND REFERABLE TO THE PRINCIPLES OF PRACTICE I.

And now as to the *taint*. With the plan of battle distinctly before us, I think I may fairly ask you to place your finger on those precise mistakes or treasons, for which,

according to the newspapers, I, and others, have been condemned? It is clear, that a doctrine involving *several distinct questions* of corresponding importance, cannot with propriety be *summarily* judged or shoved aside merely by a convenient dyslogistic use of the technical term "Homœopathy."

I. Is it, in the first place, because I cannot assent to the certainty of the conclusions of the tentative system, or accept as infallible, its *Materia Medica*? Tell me, then, as a preliminary, what thoughtful Physician of the present day, does, in very truth, that which I decline to do? I am not speaking, be it observed, of any man's speculative belief, or supposed belief; but, of the manner in which that *Materia Medica* is regarded by skilful men as a *guide in practice and actual dealings* with disease. I aver it, Sir, as notorious, that every successful Physician has *his own Materia Medica*, notwithstanding his subscription to what the College appears to consider an uniform and obligatory Test. In the whole annals of Legislation, I venture to assert that no greater failure is recorded, than yours must be, if you intend to lay down *absolute rules* regarding the action and employment of allopathic remedies. Only examine the case as Dr Alison states it, by light of the first principles of logic; and I am sure you will agree with me, that *if* accurate results have been reached, success has been attained under circumstances so complex and so hopeless, that it should be accounted for ever, a very miracle of inductive effort. It

appears, on the first blush of the matter, that you undertake to group together a number of most varied and composite phenomena, and to detect their Laws and elementary principles,—without aid from any one *guiding* or *superior* scheme, without the possibility of forming even a legitimate hypothesis ! But passing this by, look at the *number* of elementary principles in operation in *each* of the observed phenomena ; and say, what hope there is of disentangling them *empirically* ! Take, in illustration, *one single drug*, and suppose we would ascertain the efficacy of its exhibition, in reference to *one disease*. As Dr Alison most correctly warns us, we can draw no safe conclusion from *one* experience ; because many modifying circumstances, whose specific influences are also unknown, combine with the operation of the drug, and assist in producing the result. We shall take the number of these circumstances at *ten* ; in which case the inquiry would be as follows :—upon a patient, under one disease, ten unknown influences and a drug are acting ; and we wish to find the exact therapeutic power of the drug over that disease ! Is it not, indeed, a promising problem ? And does it much encourage you to proceed further,—to draw up what Dr Alison might consider statistics sufficient for completing the theory of the one drug, or to formulise its efficacy, not in one disease only, but on unhealthy action in general ? No wonder, indeed, that vagueness, uncertainty, incoherence, prevail in such a field ! Well might SKODA, the first consulting physician in Vienna, reply, with his emphatic German shrug,

on my speaking of a laborious work on *Materia Medica*, then new and somewhat talked of in this country, "*Es ist gar nichts.*" Can we marvel at the doubts which harassed the last hours of excellent BAILEY, lest "he had killed as many as he had cured." Doubts, I believe, without ground; but shewing not the less forcibly the state of his convictions; doubts which no one should entertain, not a shadow of them, regarding many admirable friends of the sick man, notwithstanding their speculative profession of allopathy. These men practise, however, not in concurrence with their system, far less in blind obedience to it,—drawing, on the contrary, wisdom and authority from a personal experience conducted with rare sagacity, integrity, and kindness; but *then* the PHYSICIAN is made, not by the uncertain and useless DOCTRINE, but by the precious qualities of the MAN.

II. I incline to the opinion, that the error with which we are charged is not resident in our mere abnegation of tentative practice. Generally speaking, scepticism has a respectable, if not a fashionable, air about it, and is seldom disliked in our tone-giving *salons*. At any rate, I find myself in this respect in very good company. When I term ROKITANSTKY the foremost pathologist in Europe, I am not wide of the mark; and certainly he has as little respect for allopathy, and its sandy foundations, as is professed by BARON LOUIS.—Let me turn, however, to the *positive* belief I have been induced to entertain; for, as usual, the cause of offence is probably

there. And I find in it *three distinct points*, respecting each of which I venture to put specifically the question I have already asked :—

First, In clauses first and second, under the column “Rational or Systematic Practice,” I have given what seems to me the physiological ground of Hahnemann’s well-known maxim, “*Similia similibus curantur.*” I stand, in the meantime, by that explanation ; and observation and experience alike embolden me to stand by the authority of the maxim. If my error is here, I would beg to recal to the College, that the truth of that supposed error shines forth through many of the most remarkable facts recorded in the history of medicine ; and I shall venture to say farther, that the same error is every year obtaining additional influence over ordinary practice. Can we have forgotten the name of the immortal JENNER, and the achievement connected with it ? What has WILLIS testified of the sweating sickness of 1485, terrible as the plague ;—*that its ravages were stayed by agency of a SPECIFIC*—the specific being SUDORIFICS. Again, thus writes Dr Alison—“In a very few instances only we can ascribe to certain remedies, a *specific* power, known only by experience, and apparently unconnected with any sensible effect of counteracting certain morbid actions, and so preventing their injurious effects. The best example is cinchona, or its alkaloid, quinine, over intermittent fever. The effect of certain remedies, usually called ‘alteratives,’ on the results of certain specific inflammations—of mercury, in various inflammations ;

of sulphur, in scabies ; of colchicum, in rheumatism or gouty inflammation ; of iodine, or sarsaparilla and other vegetables, on certain forms of inflammation of the periosteum and skin—although less powerful, may likewise be called specific, in the present state of our knowledge.” *Specifics* they indeed are ; and that “sensible effect in counteracting certain morbid actions,” which Dr Alison cannot connect with them, is derived simply from this—they produce in the healthy frame that very *character of diseased action* which, as he has stated, they cure. Strange that so experienced a pathologist, and so learned a physiologist, could let a fact so remarkable slip by him without notice ! Stranger still, perhaps, that, with Jenner’s great discovery so long before us, it should have fallen to Hahnemann alone to feel assured of its relationship with some great yet unseen general principle ! The English, indeed, are proverbially averse to generalising ; but I believe we could never have put aside warnings so emphatic, had not the edge been worn off our judgments by our unhappy familiarity with mere empiricism in medicine ; a familiarity of endurance so prolonged, that we have almost fallen into the error of considering empiricism, essential as a foundation of safe Practice.

Secondly, Next in order comes the principle, that *the power of specifics may be discovered and tested by experiment on the healthy organism*. Is this untrue ? Or, rather, if the foregoing homœopathic law be correct, is it not an immediate and incontrovertible inference—that

the method referred to is as *sure* as it is *fertile*? It has always appeared to me most important, as a verification, or rather confirmation, of Hahnemann's views, that everything needful to the formation of a complete system and practical code, flows out of his first principle ; while, at the same time, such consequences are not involved in it by way of latent assumptions. And certainly Dr Alison cannot object to *this* special conclusion, inasmuch as he recommends a similar process of experimenting, as an aid in his own system. But, alas for Empiricism : it is here attempting to thread the labyrinth without a clue !

Lastly, We reach that formidable *caviare* to the multitude--the subject-matter of most of the wretched wit, and more absurd abuse, expended in this country on the discoveries of Hahnemann ; viz., the "infinitesimal doses." It would, doubtless, astonish not a few, to be assured that this portion of the subject has really no connexion with the scientific foundations of homœopathy. It is essentially a *separate substantive proposition*, bearing on the question as to how medicines act on organised tissues ; and, as such, it professes to stand on the precise ground claimed in support of the opposing proposition, that efficiency is proportional to *weight*. The decision as to the comparative accuracy of these two doctrines rests, in the meantime, wholly with *experience*. But, besides full and satisfactory results already derived from such experience, I could claim on behalf of Hahnemann's practice, singular confirmation from ordinary Medicine,

and equally singular analogies with great processes in Nature. Who will venture to assert, for instance, that the *really infecting* matter which produces disease in vaccination, is not an infinitesimal dose,—not the matter introduced, but the really influential or infecting part of it? Is it not the belief, too, that infection comes from the diseased body, or from malaria, through what we inhale? And yet who ever discovered, so as to *weigh* it, the virus surrounding a patient stricken with the plague, or that issuing from the Pontine marshes and tainting all the atmosphere? I grant, indeed, that it were well had we here any real physiological or positive light to guide us to conclusions, or to sustain our inferences from experience. But the deficiency characterises both systems alike. It were scarcely fair to brand the *one* because of an imperfection which avowedly inheres, and at least to an equal extent, in its opposite. I leave it to the thorough-going partisan to maintain that the *theory* of homœopathy is in all respects what a final theory ought to be; but it is surely enough, if, amid this, generally allowed, universal darkness, regarding the *mode* of the action of medicines of any kind, I can point to *success* in justification of my own principles of exhibition; not certainly implying that success may not often also seem to attend an apparently opposite procedure. Facts which now appear conflicting will, no doubt, be eventually harmonised: until the period of full knowledge supervenes, however, no man should be blamed for continuing that system of administration which experience shews

him to be connected the most closely, with the expectation of beneficial results.

You, Sir, I am sure, will not so far mistake me as to suppose that I have put forward these observations by way of formal discussion concerning the great scientific question. My purpose, on the contrary, is quite a limited one; and, to accomplish that, perhaps I have said enough. I have wished to state plainly the nature of my own views and practice, so that, regarding this part of the subject, there be no ambiguity or mistake. But I have wished also to state the form in which alone, I think, the question can be looked at *judicially*. As a defender in the case, I deny that the wholesale condemnation which you have issued is competent. In so far as your sentence can effect it, you have denied us the rights of physicians *in toto* (the right, I mean, to meet our fellows in consultation), while the supposed heresy you intend to condemn extends over only a part of a Physician's practice, and that the part which is confessedly unsatisfactory. And in one simple finding you include judgment upon at least *three different counts*, essentially independent of each other, and which ought not to have been confounded. I object to that judgment, therefore, as bad in every view that can be taken of it. It is bad in equity and in law; in form, as, I believe, in substance. On these grounds, I request from the College reconsideration of its recent course. Whether that should lead to *reversal* of their entire finding, or of any of its parts, it is not my function

to decide. But one thing it is my function to decide, and mine alone. I have carefully considered the effect of my opinions, and the obligations under which I am laid by my Fellowship. I feel that I have done nothing in thought or act, in practice or speculation, to forfeit the honour I once asked, and continue to value. I therefore, Sir, reply to the requisition of the meeting over which you presided, respectfully, but firmly, and without a wish to interfere with your future course and decision, *that I do not withdraw from the Corporation, and that I cannot obey the other portions of its late Resolutions.*

III.

It cannot be incompetent, and I trust, Sir, you will not deem it unbecoming, that I now use my privilege as a FELLOW, to enter a brief but unqualified protest against these recent proceedings. Looking at them without reference to any personal interests or applications, I consider them eminently hazardous to the future reputation of the COLLEGE, unjustifiable in our present position, and not calculated to exert any beneficial influence in furtherance of the public safety.

I. With details of similar occurrences, found, I believe, in the History of every branch of Science, it is by no means my intention to detain you. I do not recollect, indeed, any memorable accession to our knowledge, which,

on its introduction, did not encounter the firm, persistent, though powerless, opposition of existing Corporations ; but none need be ignorant that to warnings of this sort, quoted for the sake of regulating present conduct, the answer is always at hand—"HISTORY is only an OLD ALMANAC ; and though people committed blunders *then*, CIRCUMSTANCES are different *now*." It will, however, I think, be conceded, that the frequent occurrence of such "blunders" must be referable to some general *cause* ; and that on a clear apprehension and appreciation of that cause, depends our ability to derive wisdom from Experience, and to illustrate the Present by aid of lights from the Past. Now, the errors into which corporations have fallen may be traced to a serious mistake as to the true object and function of such associations. Regarding the high value of scientific associations, no man, indeed, can doubt ; but I suspect that their usefulness is confined to the exercise of the power which combination gives, towards promoting the discovery of POSITIVE TRUTH ; and that when they endeavour to consecrate NEGATIONS, or, what is the same thing, to discourage or bear down—by force also of combination—separate and individual exertion, they fail, because they *must* fail ; not *usually* merely, but *necessarily*, and therefore *uniformly*. The influence of any compact and earnest Society in assisting the private inquirer—whether by clearing his road of obstacles, warding off discouragements springing from popular ignorance or hostility, or by sustaining him with direct encouragement, manifest-

ing sympathy with the high feeling which carries him on, and it may be gaining for him substantial though indirect assistance,—influence of this sort must certainly be always salutary, and there are cases in which its effects might be immense. Acting in co-operation for ends so beneficent, men are bound together by all that is generous and energetic in our nature, and there is scarcely a possible object which they cannot then accomplish ; but if—misled by the consciousness of what they can do and have done, when fulfilling their generous mission—Societies turn round, in expectation of exerting an equal influence in the *opposite* direction, they commit a very fatal mistake. Who knows not, I beg to ask, the serious difficulty experienced by the most earnest searcher after Truth, in his endeavour to *realise* the claims of any new system, foreign to his previous modes of conception ? I believe that minds even of the freest and most unbiassed texture, cannot find themselves in contact with views upsetting their own, without experiencing at first a strong and unreflecting *antagonism*. Nor is this mere prejudice, or symptomatic of wilful blindness ; it results necessarily from the influence of habit—from the fact that our convictions are dear to us, because, when sincere, they affect our whole mind and life. The Germans have described the difficulty well : they say that at first we necessarily look from our own *stand-punkt* : whereas, in justice to Truth, we should try to pass across to the *stand-punkt* of the other inquirer. It is precisely the old case of the silver and golden shield. Now, if this be notoriously

true, even with the sincere *individual* who has felt especially called on to look at the new question, how slight the probability of an equitable negative judgment on the part of a *number of men*, very few of whom can have been personally moved towards a strict examination of the question; whose intellectual faculties are as various as their faces; who have enjoyed no corresponding opportunities; who, as a mass, never sought out the *stand-punkt* of their opponent, and are bound together solely by a common sympathy with that first, irrational, but strong feeling of antagonism? I question not that within every such Body a few may be found who have done their utmost to comprehend the subject—reaching, by a path which they deemed fair, a hostile opinion; but if men are not raised above the frailties of Men, simply by becoming Corporators, even an *unanimuos* opposition can rarely argue any strict scrutiny or sound deliverance regarding a Scientific Truth. An harmonious vote, indeed, will, in such circumstances, proclaim that the feeling of *Antagonism* is universal; but one has only to inquire into the *grounds* of that antagonism—the impulses which have produced each vote, and the delusion concerning the “unanimity,” at once disappears. On the front bench, for instance, you may have some one, with his head high up amid Aristocratic Ideas, who hates innovation, and never descended to the vulgarity of acknowledging a *Novus Homo*—of admitting a new conception in the course of his life. Beside him you will have Old Men, who, having attained respect and authority, are possessed by the conservatism

natural to Age. Turning to other benches, one needs not miss the timid, the easy, the fortunate—men who consider what *is*, safer than what *might be*, who dislike trouble, or find the world prosperous enough as it goes. Scattered about—each a separate atom—are your “original Thinkers,” your men of crotchets; crotchets all containing probably, some truth, but which strangely enough are valued the most by their Parents, on account, not of their beauties, but of their deformities—because of the amount they contain of what is exaggerated or false. Exclude such classes—which exist in every society—and how many remain? With regard to that remnant too, can you predict real uniformity of opinion, when, as in the case before us, the disputed matter is not a single point, but essentially *composite*? In regard to *which part* is it, that each separate judge would sustain the righteousness of his black ball? Truly it were easier to extract, on Dr Alison’s principle, the specific action of a drug from one solitary experiment; easier to interpret the darkest hieroglyphic, than to give consistent or rational significance to the contents of such a ballot-box! Unless, indeed, through some singular concurrence of causes, no determination, thus come to, can possibly stand; and small marvel is it that decrees so originating have no force except in recoil; that they never have shaken, and never can shake, the purpose or perseverance of the Man who, through patient and dispassionate meditation, has gained the track to discovery.

II. Further, however: no man, no Society at least, seems to me entitled to interfere authoritatively with the honest efforts of others to attain some object of public importance, unless it can be shewn either that the same end has already been attained, or that, through some better process, success is at hand. It ought to be held as a canon, that until Science has reached its *ultimate* Laws, it is essentially *free*; so long as the knowledge of these Laws is in the distance, no hand of authority ought to be laid on the Inquirer who is struggling to attain it. Now, Sir, of that Empirical System, which in this country we find pitted against our Rational Therapeutics, I mean to say little more. I have already shewn its extraordinary looseness *as* an empirical scheme; and I have asserted that it is losing ground yearly; I have called attention to the fact, that the use made of it in practice is so various, that, apart from the books in which its formulæ are written, no one can say with certainty what it is. Let me narrate merely one or two circumstances that came under my cognisance years ago, as illustrations; and then I shall ask again, what is really the value of the system, in virtual defence of which our College has felt it necessary to put on at present so hot an antagonism, and to send forth these denunciations. Formerly—I do not know if it is so now—there were several fever wards in the Edinburgh Royal Infirmary, of which three Fellows of the Royal College of Physicians had the charge. One physician had the top ward, another the middle ward, and a third the low ward.

It happened, that on the same day, three young persons of nearly the same age, ill of typhus fever, were admitted into the hospital. The disease was of equal severity in each, and the stage of complaint the same in all. What was the treatment, think you, pursued in those three cases, by the three Fellows of the College? Of course it should have been the same, at least if the *system* be correct; for the Physicians in question would choose the best. But, Sir, it was not the same. He in the top ward bled his patient with lancet and with leeches. He in the middle ward physicked his patient with drastic purgatives; and if he saw a large, comfortable effect, he gave praise, and was content. He in the low ward, again, gave whisky, wine, and opiates. What was the result of such deplorable freaks? I refer you to the statistic-book; I have no doubt you will find it there!—In the University formerly, two Professors used to lecture, on alternate days, on clinical medicine. It happened once that each had, at the same time under his care an acute case of *pericarditis*. The Professor, who lectured upon his case on Monday night, said, in substance, as follows:—“Gentlemen,—As to the treatment of this disease, it has been the practice to give large doses of mercury so as to bring the constitution under its action; and to effect this as rapidly as possible, small quantities of opium are usually combined with it. This practice I, however, believe to be erroneous; for I have observed the progress of the disease unchecked, even during profuse salivation. The most efficient remedy—in

fact our *sheet-anchor*—in this disease is *Tartar Emetic*. You will have noticed the large doses I have given of this remedy, and yet the patient seems not to suffer from it. In fact, the constitution in this disease, as in some others, has a remarkable tolerance for Tartar Emetic.”

When the lecture was finished, I left the hall, fancying I had learned some great truth, and knew better than an hour before, how to save life. On Wednesday evening, during the same week, in the same hall, and to the same students, the other Professor lectured. The lecture was devoted to the acute case of *pericarditis* under his care in the hospital. After describing the case, and giving a sketch of the character and progress of disease, he spoke in substance as follows :—“Gentlemen,—It is a remarkable thing that there should be any difference in regard to the mode of treatment to be pursued in a disease such as this. I believe it is the Italian and French schools which advocate so very strongly the employment of Tartar Emetic ; but I would strongly urge you to put no confidence in this remedy ; for if you do so, you will lean on a broken reed. Our *sheet-anchor* in this disease is *Mercury* ; under the action of which you must bring the patient as soon and as freely as you possibly can—even bleeding is of little importance in comparison with the use of mercury. The two combined, *i.e.*, mercury and blood-letting, is of course best, but at all events use mercury, and never trust to tartar emetic.” What effect was produced on the minds of the students by such opposite teachings I cannot say. I can, however, speak for *one*.—

He walked down into the noble quadrangle in bitterness, and gave involuntary utterance to the words, "No wonder that Sir James Macintosh forsook the study of medicine."

III. Assuming that it cannot be the wish, as it has not been the practice, of bodies like our College, to descend gratuitously into the arena of controversy, I would put the question, How far it may be expected that, by their present action, they can forward any public object, or avert danger from the public health? In the first place, what effect can follow from the Resolutions, on minds already favourably disposed towards the denounced doctrines? Let us notice how Homœopathy now stands, and the nature of its hold on opinion. Has it been accepted only by the ignorant? Is it professed only by quacks and impostors? Let not the College be deceived. Our leaders should not shut their eyes to the fact, that the system in question is assented to by thinkers as enlightened and acute as any in this age, and that its disciples are not of the multitude. Is a man like BUNSEN, for instance, or Archbishop WHATELY, or indeed any one who can probe more than skin-deep, likely to be deceived by the superficial but meaningless unanimity of that ballot-box? The list of its professional cultivators, too, notoriously contains the names of as reputable and instructed physicians as any in Europe. Not one solitary practitioner, I believe, will that unhappy decree turn from his course; nor will it shake the faith of a single patient who has tested the system by experience.—Then again, as to the general

public? Is it not the most probable result of a simple authoritative decree like the one we are considering, that it will rather rouse prejudice against it? Those tales of futile and frequent exactions of this sort, in former times, are rife enough to reach and fill the general ear; and, on the face of present circumstances, there is nothing peculiar, nothing to satisfy any one that the old blunder has not been repeated. The decree, I have said, is simple and unattended. It is an utterance from Authority regarding certain matters still under inquiry; and it is nothing more. It is unaccompanied by any effort at persuasion, or by the faintest promise of service in the way of clearing up existing difficulties. One great, one inestimable, service the College might have rendered; nor is it yet too late. It is a service, likewise, in perfect harmony with our English idiosyncrasy, which, as I have said, is averse to generalisation. I refer to the establishment of a great Hospital, wherein the operations of the new system of cure might be seen and examined by all; furnishing alike to the Student and advanced Practitioner, the only true means of reaching correct judgment on a matter of paramount, professional as well as public concern.

And now, SIR, permit me to say, in conclusion, that the necessity of writing as I have done, and the change of relationships which that necessity in so far involves, are the reverse of agreeable to me. Division from former friends, and a loosening of early associations, cannot but be painful;—such things are no slight augment

of the anxieties besetting every conscientious mind, when its ancient convictions are being shaken. I esteem it fortunate, however, that an effort—it may be, slight—to vindicate the rights of inquiry, and ask justice for men whose offence can seem to *you* so little serious as mine, should have been demanded of me personally and as a Fellow of the College, under *your* Presidentship. Concerning your own action in these recent deliberations, I know positively nothing; but I shall not question the sympathy of one who has never been blamed for resisting novelties himself; who has perhaps introduced more serious and startling courses of practice than any other cotemporary British Physician; who is certainly not bound by formulæ of colleges, but, on the contrary, who has ventured, on the strength of his own will and resources, to walk all but singly along a most perilous path. Recollections of former times also re-assure me. Were I to arrange and compute the value of their influences, I know not—if I have erred—how far I might not plead in palliation *Te Duce*. You have not forgotten that small room, approached by the long passage, where our lamented friend Dr JOHN REID once lived and studied. Free speech was there, free thoughts, and criticism unchecked. Youth passes, and with it many dreams and impulses. We think, when we begin, that we are to storm the world, but, alas! the world too often storms us. Many find it safest to capitulate, and permit themselves to be sold. Still all these early impulses are not foolish. For myself, I would cherish, as

before, fair hopes of success and auspicious fortune ; but, foremost, that old and firm resolve, to remain, whatever betide, by Honesty and Intellectual Independence.

I have the honour to be,

SIR,

Your obedient Servant,

WILLIAM MACLEOD.

TO PROFESSOR J. Y. SIMPSON,
&c. &c.

APPENDIX.

I.

RESOLUTIONS OF THE ROYAL COLLEGE OF PHYSICIANS OF EDINBURGH, AND OF THE ROYAL COLLEGE OF SURGEONS OF EDINBURGH, REGARD- ING PRACTITIONERS OF HOMŒOPATHY.

AT Edinburgh, within the College walls there, the 9th day of May 1851, an extraordinary meeting of the Royal College was held, pursuant to a resolution agreed to at the last quarterly meeting, and of which extraordinary meeting due notice was given—the President in the chair. The following resolutions were moved, seconded, and unanimously agreed to :—

1. That the Royal College of Physicians of Edinburgh did, several years ago, publicly express its opinion of homœopathy and homœopathic practitioners, by peremptorily declining to admit into its body a candidate for its fellowship who belonged to that denomination ; and, consequently, that no Fellow of the College can possibly be ignorant of the light in which all those who practise homœopathy are regarded by the College.

2. The College regrets that, notwithstanding this decided expression of its opinion, more than one of its Fellows, after being admitted in a different character, have endangered the reputation of the College by becoming homœopathic practitioners ; and the College expresses an earnest hope that these Fellows, seeing they have thus virtually separated themselves from the College, will spontaneously sever their further connexion with an institution which repudiates them, and from which they can derive, as merely nominal Fellows, nothing else than a false position and a spurious credit.

3. The College feels the more bound thus to express its opinion, seeing that those Fellows who have become homœopathists, and any other medical practitioners who follow homœopathy, must necessarily be aliens to the other Fellows, and to the profession at large; inasmuch as no Fellow of this College, or any other physician, can, by any possibility, without derogating from his own honour, and the honour of the profession, meet practitioners of homœopathy in consultation, or co-operate with them in the other common duties of professional life.

4. That, although the College has not thought it expedient hitherto to take any active steps for disclaiming those Fellows who have become homœopathic practitioners subsequently to their admission to the College, nevertheless, since it has the power of dealing summarily with those who act in a manner so unbecoming the character of a physician, it reserves its right to exercise that power when it shall be so advised.

Signed in name and by authority of the College,

J. Y. SIMPSON, *President*.

II.

OF THE ACTION OF REMEDIES IN GENERAL, AND THE EVIDENCE OF THEIR EFFICACY.

(*Extracted from DR ALISON'S Pathology.*)

It seems important to premise some general observations on the difficulty of judging of the real influence of remedies over diseases, and on proper means of surmounting those difficulties, before giving a general sketch of the resources of medical practice.

Several causes may be pointed out as very frequently misleading us in our estimate of the power of remedies over disease :—

1. The natural tendency of most kinds of diseased action to spontaneous abatement, to which the action of almost all remedies is subservient, but which is very frequently mistaken for the effects of remedies used.

2. The unobserved action of other circumstances in the situation of patients, besides the use of any remedy of which the value is attempted to be established,—*e. g.* of the antiphlogistic regimen, when depleting

remedies are used, or of change of season or of situation, when tonic remedies are used.

3. The extreme diversity of cases to which the same name, and even the same pathological description is correctly applied, as to extent, or intensity, or malignity, and therefore as to the result to be expected even when no remedies are used.

4. The diversity of constitutions in which the same kind of diseased action may be excited, and the difference of result thence to be anticipated under any practice that may be employed.

These peculiarities attending the observation of the effects of remedies, necessarily vary the conditions of each individual case in which such observations are made, and make it impossible for us to vary them artificially, so as to have a complete *experimentum crucis* as to the effect of any remedy. They not only vitiate many inferences as to the good effects of alleged remedies, but often blind us to the injurious effects which they may produce in the course of diseases. The same difficulties do not attend the observation of the effects of remedies intended only for the attainment of one great object of medical practice—palliating symptoms in cases which are regarded as hopeless. But even in these cases, it is often very difficult to judge how far temporary alleviation is purchased at the cost of subsequent aggravation.

While these circumstances greatly embarrass all conclusions that can be drawn as to the efficacy of remedies, it is also to be remembered, that certain obvious moral causes naturally lead most men, in cases of doubt, to exaggerate, rather than undervalue, the importance of their own interference with the natural course of diseases.

The means which naturally suggest themselves as fitted for guiding our judgment under these difficulties, as to the safety and utility of remedies, and which have been adopted, with more or less success, in all ages, are the following:—

1. We multiply as much as possible the observations of individual cases to which any particular remedy is applied, noting as carefully as possible all the varying circumstances of these cases, so as to obtain *statistical* evidence as to the efficacy of the remedy, and the influence of other causes, simultaneously applied, too extensive to be vitiated by accidental contingencies. If all alleged remedies had been *specifics*—*i. e.*, supposed to exert a direct power over diseased actions, not referable to any more general principle, or if the same disease had followed the same course in all persons, we could have obtained statistical information as to their efficacy in this way, equally certain as the sta-

tistical evidence we have of the power of the exciting causes of any disease. But when it is considered that the great majority of remedies have certainly no specific power, and that their action is believed to be only subservient and auxiliary to the provisions of nature for the spontaneous decline of diseases, and to be dependent on the influence which they exert over the functions of the body in health; that they are useful, therefore, only in certain periods and circumstances of the diseases to which they are applicable, and that the natural cause of these diseases is exceedingly various, and liable to alteration, as already observed, by different causes, both internal and external, to the body, many of which are extremely obscure, and beyond our control—we can easily perceive that the application of statistics to the influence of these remedies on disease, must be liable to great ambiguity, and that the application of other species of evidence, for practical purposes, is really necessary.

2. We therefore watch the progress of the symptoms, after the application of the remedy, in individual cases, and observe whether there is such alteration in these, when compared with the usual progress of the disease, as gives us reason to believe that the natural process for the restoration of health is promoted.

3. But, again, adverting to the statements already made, as to the great variety in the extent and intensity, and natural progress, of different cases which are referred to the same place in the nosology, we can easily perceive, that the mere observation of external symptoms, without reference to the real nature of the internal changes producing them, will not sufficiently inform us how far the remedies used really exert a beneficial effect on morbid changes. We find it necessary, therefore, not with the view of applying reasoning *à priori*, but in the view of availing ourselves of all the facts known by experience which illustrate the subject, to inquire into the intimate nature, or pathology, of the diseases thus treated, and endeavour to ascertain whether the changes actually going on within the living body are such as it is reasonable to think that the remedy in question can, directly or indirectly, modify or control.

4. This makes it necessary to inquire farther into the real nature of the change which that remedy can effect on the body—*i. e.*, as to its mode of operation, which necessarily requires our inquiry into what has been called its “physiological action,” or the mode in which it affects the healthy body, from the knowledge of which, compared with our knowledge of morbid actions themselves, we can frequently draw an

inference, with more or less confidence, as to its real efficacy in averting morbid changes, or promoting their favourable termination.

Being thus led to see the importance of scientific or theoretical knowledge in regard to the real action of remedies on the living body, we observe, that almost all those on which we can place any reliance in practice are referable, according to what we believe to be the mode of their action, to one or other of the following classes.

In a very few instances only we can ascribe to certain remedies a *specific* power, known only by experience, and apparently unconnected with any sensible effect of counteracting certain morbid actions, and so preventing their injurious effects. The best example is the power of cinchona, or its alkaloid, quinine, over intermittent fevers. The effect of certain remedies, usually called alteratives, on the results of certain specific inflammations; of sulphur on scabies; of colchicum on rheumatism or gouty inflammation; of iodine, or sarsaparilla, and other vegetables, on certain forms of inflammation in the periosteum and skin, —although less powerful, may likewise be called specific, in the present state of our knowledge, although it may be inferred, with probability, that the action of some of these remedies may be referred to other heads presently to be mentioned.

The beneficial action of all other remedies, besides these specifics, in diseases which admit of cure, is only auxiliary to the provisions of nature for the spontaneous cure of diseases. Although incapable of arresting the course of morbid actions, they can frequently modify them, and counteract those changes which, in the circumstances of individual cases of disease, are most immediately dangerous. We can have no doubt that in this way they frequently save life, and generally, when prudently used, place the body in circumstances more favourable to the spontaneous decline of diseases. This is done by an influence on the body during the diseased state, which we think capable of explanation by what we know of the operation of those agents on the healthy body.

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