

METHODOLOGICAL OBSTACLES IN HOMŒOPATHIC RESEARCH

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MR. PRESIDENT, LADIES AND GENTLEMEN,

It is with considerable misgivings that I stand before you now, for my qualifications for reading this paper are minimal. My experience of research is limited to work with tuberculous meningitis in the early days of streptomycin, para-amino-salicylic acid, and isoniazid therapy, at a time when the natural history of the disease provided the necessary control. That means, a method was employed which is recognized as valid in clinical research, but in which neither placebo administration nor blind controls play a part.¹

For the last three years, Homœopathy has held me enthralled. As one began to penetrate into its mysteries, the question of the HOW and WHY and WHERE of the action of these many remedies became ever more insistent, and when last year I was asked whether I would be willing to consider some research in the field of respiratory diseases I accepted the challenge with naïve enthusiasm. This is the place, therefore, to confess to you that, though my enthusiasm continues, I can lay before you no data of staggering import. My studies have led me ever more convincingly towards one conclusion. Hence, when asked to read this paper to you, I accepted, being quite aware of the many reasons for not doing so, one of the foremost being my own lack of practical experience.

Before one can discuss the methodological obstacles in homœopathic research, it is obligatory to ask oneself what is the aim of such research? Are the problems inherent in this field of medicine the same as in other fields? And to what extent are the methods used in these fields applicable to homœopathic research? What then is, or are, the aims of homœopathic research? Do we wish to convince the unbeliever of the action of our potentized remedies, and so become more acceptable to our colleagues? Do we wish to discover or uncover the truth of and idea behind the Law of Similars? Or do we wish to find out the How of the action of potencies?

It would take too long to give an account of all the work done with potentized remedies *in vitro*. Those interested are referred to a paper read by Dr. H. Boyd at the International Homœopathic Congress in 1956,² and to that of Dr. James Stephenson entitled "Review of Investigation into the Actions of Substances in Dilutions Greater than 1×10^{-24} ".³ More recently, Mme. Kolisko's work has been made available in one volume.⁴ All these attempts and labors leave one in no doubt that these succussed microdilutions have a specific action. W. E. Boyd has also demonstrated the difference in activity between simple microdilutions and succussed microdilutions (i.e. potencies).⁵ The basis of all this experimental work is along orthodox lines

of experimentation, though specific technical difficulties presented themselves and had to be overcome.

In practice, the use of homœopathic remedies is based on the results of human experimentation. Hahnemann introduced this method of collecting evidence of drug activity by giving to volunteers, including himself, the remedy in the crude state, or later in low potency. It is upon these provings and their elaboration, with additions from toxicology and therapeutics, that our materia medica is based. Bodman⁶ reviews this situation in his paper published *THE BRITISH HOMŒOPATHIC JOURNAL* in April, 1935. Dr. Stephenson,⁷ in a paper published in 1960, advocates, however, the conducting of provings along the lines of a double blind trial.

A recent number of *Hippocrates*⁸ contains a critical review of the literature on drug provings in connection with a proving of *Bryonia alba* carried out in keeping with the demands of modern science—i.e. a response to Dr. Stephenson's request. It shows that those "provers" who were given placebo developed most of the *Bryonia* symptoms, often in more exaggerated form. That surely proves nothing more than that provers are suggestible, and does not detract from the validity of the symptoms developed by those taking *Bryonia*. Quite how to explain the symptomatology of placebo provers is another matter. In connection with this, it is worth while to take note of recent articles by Arthur Koestler in the "Week-End Review" of the *Observer*. He quotes from and discusses a recent symposium on "Control of the Mind", organized by the University of California Medical Centre in San Francisco, and sponsored by the Schering Foundation. Certain experimental work was mentioned, in which it was quite clear that the subjective attitude of the observer on the one hand, and the observed on the other, influenced results. In the first instance, rats were given to two groups of research workers. The object was to teach them to run the maze. The workers were informed that one group of rats came from an intelligent strain, and the other from a stupid one, though in fact all rats were of the same strain. The results showed that the so-called "intelligent" rats learned to run the maze more quickly than the so-called "stupid" rats! The only explanation Rosenthal (the instigator of this trial) could offer was that the bias in the worker's mind influenced the rat! But how this was done he confessed not to know. The other series of experiments involved students who were given a pill. One group was informed that this pill was dexedrine, a well-known stimulant and energizer; another group was told that they were sleeping pills. In fact, however, all the students were given either dexedrine or placebo. It was shown that the group which thought they had swallowed "pep pills" were full of pep, while those that thought they had swallowed sleeping pills became sluggish and sleepy, regardless of whether they had been given dexedrine or placebo. These admittedly few instances make one doubt the value and validity of even double blind trials; for who knows what is in the observer's mind?

We seem to feel compelled to look for methods in research amongst those used in orthodox circles, perhaps with the hope that then one's results will be accepted, and through the acceptance of our work we will be acceptable to our colleagues—respectable and not "outré". The methodology of orthodox clinical trials has been clearly set out in a symposium published in 1960.⁹ Dr. A. Bradford Hill says in his contribution to the symposium: "The aim of the controlled clinical trial is very simple: it is to ensure that the comparisons we make are as precise, as informative and as convincing as possible." As I understand it, a corollary of all these attempts so ably described in this book and other papers is the *removal* of the *individual factor*, the eradication from the consciousness of all involved of the fact that one is dealing with a patient—an individual manifesting in a certain manner at a certain moment in time and space. They are means of studying the effect of drugs on disease by random selection of cases and statistical analysis, and so diametrically opposed to our aims.

It would not even be fair to say the method is at fault, though even in orthodox circles its validity has been questioned.¹⁰ But the method is not applicable. In homœopathic practice we cannot collect a series of patients suffering, say, from asthma or migraine, or functional dyspepsia, etc., and treat alternate cases with placebo and the others with a drug. For the essence of homœopathy is the treatment of the individual with a remedy the symptomatology of which corresponds to that of the patient. So there is no *one* drug. And placebo control does not make sense. All I think one can hope to do in this field is to collect well documented cases with full indications for the use of a remedy or remedies, and assess the progress in relation to reported progress prior to receiving homœopathic therapy. It proves little in the light of placebo work, for so many more factors above the introduction of a new remedy enter into this new phase of the patient's life—hope being perhaps the most pertinent. I am not denying the positive result following the taking of a remedy, but I am pointing out the difficulties inherent in assessing its specific effect.

And so I should like to state here quite categorically that the methods used by our orthodox brethren with regard to clinical research are not applicable in this, our particular field of medicine. As stated above, orthodox scientific methods, bearing in mind the sensitivity of the material used, are applicable to test tube research, even to Mme. Kolisko's work. The difficulties are technical, and not methodological, however difficult the interpretation of the results might be. And so it would appear that I have reduced multiplicity to singularity—the plural to the singular; for the title of this paper reads: Methodological Obstacles in Homœopathic Research. I have tried to point out that the obstacle lies in applying an inapplicable method. This method of conducting clinical trials is born out of the materialistic scientific thinking of this age, well usable in this materialistic, mechanistic age of medicine, where we get ever further away from the individual. We must

therefore go to another source and find other ways of thinking about the phenomena we meet, study, or attempt to treat.

I coined the title some little time ago, not fully realizing that I would be driven to the logical conclusion that there is in fact NO way in which we can conduct clinical trials, no way in which we can collect and present a well analysed series of cases which would be acceptable to the statisticians, and so to posterity. But I do not feel defeated, nor should I like defeat to be the effect of my deliberations. A "NO" on one plane arouses in one the desire to find a way out—to find a positive approach even if one cannot append to it the well-sounding name of clinical research.

Samuel Hahnemann was not only a physician at war with the medical practices of his time, he was also a great experimental scientist. He observed and collected his observations until gradually a pattern showed itself—a pattern which we call a drug picture. Observation, that is, direct observation of phenomena, is something relatively neglected in our days, though we all have to go through the stages of experimental work in physics, chemistry, physiology, etc., etc. And observation alone is not sufficient, it must be coupled with right relating, relating in right order until we arrive in the Goethean sense at the idea, the underlying principle or pattern of a thing—the urphenomenon. Hahnemann did not invent the Law of Similars; he discovered it, and illustrated it, and used it. What faculty allowed this one man to do so? Perhaps when we find the answer to this we have found the answer to our problem of methodology! The presence in man of a faculty or sense different to those usually employed must be acknowledged, and rediscovered, retrained, and the observations obtained through it rightly connected. We must learn to read again the Signature of all things in the macrocosm, as also in the microcosm of man, for surely that is one of the meanings of the Law of Similars. We must add together the observations of a Hippocrates, a Paracelsus, a Goethe, a Hahnemann, a Kent, a Schlegel, a Steiner, only to mention a few, and we must leave out the work on the level of the signature of formulae of a Leeser. From Synthesis we must proceed to Analysis, for we can only analyse something that constitutes a whole, not something which is a collection of unrelated facts or observations. We are not dealing with the haphazardly arranged pieces in a kaleidoscope making first one pattern, then another. That we are dealing with patterns is not to be denied, but with archetypal patterns, manifestations of universal laws, of which the Law of Similars is but one.

And now I should like to conclude by quoting a short passage from Goethe's scientific writings, in which he discusses the method of investigation introduced into science by Bacon. Goethe writes: "In the range of phenomena all had equal value in Bacon's eyes. For although he himself always points out that one should collect the particulars only to select from them and to arrange them, in order finally to attain to Universals, yet too much privilege is granted to the single facts; and before it becomes possible

to attain to simplification and conclusion by means of induction (the very way he recommends), life vanishes and forces get exhausted. *He who cannot realize that one instance is often worth a thousand, bearing all within itself; he who proves unable to comprehend and esteem what we called Urphenomena, will never be in a position to advance anything either to his or to others' joy and profit.*" (Italics are the author's.)

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