CRITIQUE OF HOMŒOPATHY

By

O. LEESER

M.D., Ph.D. (Berlin)

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Made and printed in Gr. Britain by MAXWELL, LOVE & CO. LTD
Bradley's Buildings, White Lion Street, London, N.1

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FOREWORD

In November, 1945, a book "Critical Views on Homœopathy" by Dr. D. K. de Jongh* was brought to my notice.

On page 144, in small print, the author is good enough to concede that my writings on homœopathy differ favourably from much that he has reviewed, and that it would be worth while to start a discussion with me.

As it happens, I am still alive. So I have had to decide whether it would be worthwhile reading this book of 458 pages, and entering into a discussion with de Jongh. I will say that the author's industrious endeavour to inform himself on the subject before criticising it, has strongly prejudiced me in his favour. It is rare for would-be arbiters of homœopathy to take this precaution.

For many years, no comprehensive attempt at criticising homœopathy has been made in any country. Now de Jongh has undertaken this task under academic auspices, the original having been presented as a dissertation to the Faculty of Medicine at the University of Leyden. One may be permitted to consider this up-to-date review as representative of what academic Medicine has to say on this "thorny problem" (cf. Prof. de Jongh's foreword). Whether de Jongh's treatise has, in fact, supplied tenable

* "Critische Beschouwingen over de Homœopathie," 2nd ed., 1943. N. V. Noord Hollandsche Uitgevers Maatshappij, Amsterdam. Foreword by Prof. Dr. S. E. de Jongh.

answers to the academicians, is the subject of our present inquiry.

The book has the merit of tackling the task systematically. The origin of the cardinal principal of similarity, H a h n e m a n n ' s teachings and their later development are followed up to contemporary expositions of the homœopathic doctrine in various countries. Then this symposium is subjected to a critique point by point. As the history and the present state of homœopathic thought and practice are intimately connected, it is inevitable that the main issues should have to recur more than once. In following the course of the book, my anti-critique, too, had to deal repeatedly with the same kind of argument. As far as the principal differences of scientific attitude are concerned, their illumination from divers aspects may even be found of some advantage. Methodological discussions, though of the greatest practical consequence, tend to make dry reading. No apology, therefore, is made if such passages have been enlivened here and there by a more personal approach, and written in a lighter vein.

My manuscript reached Dr. d e J o n g h early in January, 1946. So far he has had no time to comment on my views, but intends to do so after publication.

HIGH WYCOMBE.

O. L.

12*th March*, 1946.

HISTORY OF HOMŒOPATHY.

The origin of homœopathy as a scientific method of treating diseases dates from Samuel Hahnemann's publication of his findings in 1796. The main principle of using the similarity between the actions of medicinal substances and the manifestations of disease as a guide to the remedy has existed, though in embryonic state, from time immemorial. The first clear expression of the general idea is found in the works of Hippocrates. What de Jongh asserts about the relation of Hippocrates to homœopathy, and of homœopaths to Hippocrates, and what he infers (pp. 10/11) is not altogether correct. He omits to say that Hahnemann himself quoted the relevant passage in which Hippocrates anticipated the homœopathic method of applying medicines. de Jongh quotes it from H. Schulz. Had de Jongh consulted the text of " On localisation in man " he would have been struck by the painstaking wording of the whole paragraph by the Greek author. He could not then have suspected the passage of being merely incidental. Moreover, he would have noticed, contrary to his allegation, that the *Corpus Hippocraticum* does in fact follow up the juxtaposition of the " allopathic " and " homœopathic " methods by a number of examples of homœopathic treatment, viz., for stranguria, coughing, fever and vomiting. But it is not so, as he avers, that the homœopaths have claimed Hippocrates to be exclusively or overwhelmingly on their side. It is not only

Tischner, from whose "History of Homœopathy" de Jongh quotes, who does justice to both aspects of Hippocrates's conception of healing; as far as I am aware, every homœopathic author who has dealt with this historical question does the same. What I, among others, have pointed out is that Hippocrates by the careful wording of his sentences, appears to indicate that the homœopathic method leads to healing, while through allopathic treatment only the symptoms of the diseases are removed.

As regards Rademacher's "Erfahrungsheillehre" (p. 19) de Jongh does not realise that this came into being 50 years after Hahnemann's inauguration of homœopathy, and that Rademacher took his start from Paracelsus. He might have added that Rademacher ridiculed the "Narrendosis" (crazy dosage) of homœopathy and that nevertheless a number of homœopaths, including myself, have tried to preserve a few good conceptions of his, especially the differentiation of pathological conditions by means of the remedial substances to which they react in a specific way*

A fundamental divergence of approach and scientific attitude is involved when we come to de Jongh's line of reasoning in respect of Hahnemann's experiment on himself with cinchona bark. Firstly, where does Hahnemann assume that the syndrome resembling intermittent fever (for it was not simply "fever") was a common or universal phenomenon of cinchona action? Where does he say that such an action would explain the antipyretic effect of

* cf. my exposition in "Lehrbuch der Homœopathie, Spez. Teil, Arzneimittellehre, A: Die mineralischen Arzneimittel" Hippocrates—Verlag, Stuttgart-Leipzig, 1933.

cinchona? No, Hahnemann did not and did not need to assert more than what he had observed, namely that cinchona bark can elicit such a syndrome (not that it usually does or must do so). As regards explanation, he was concerned only with refuting Cullen's unsatisfactory views. As to himself, the observation opened up a problem of practical implication; he was little concerned with explaining the mode of action of cinchona; in fact, he did not give any such explanation. Hahnemann was concerned with potential actions and the proper method of using them for curative purposes. He knew beforehand that cinchona bark was helpful in intermittent fever. His problem was, whether his particular cinchona observation had a more general bearing on the curative faculties of medicinal agents. This is quite clear from the first two sentences of his annotation as quoted by de Jongh. Hahnemann suspected the same connection for such intermittent fever-remedies as Coffea, pepper, Arnica, Ignatia and Arsenic which are able to provoke a " kind of fever." In a truly scientific manner he put his query about a practical principle for applying observations to further experimantal investigation and only after six years did he feel justified in making a general statement; and that not for speculative purposes, but for the very practical one of improving the method of selecting remedies.

The issue, however, goes deeper. It is not one of Hahnemann versus de Jongh, but of Hahnemann versus orthodox Medicine. Hahnemann and homœopathy are primarily concerned with the knowledge of potential actions, emphasis being laid upon accurate observation. The statistical frequency of such observations is secondary. The knowledge sought and applied in homœopathy concerns foremost the qualitative relations of actions and processes. In this attitude of enquiry homœopathy is extremely well supported by the revolution in scientific thinking during the last 45 years (reckoned from Planck and Einstein), but which has hardly penetrated the precincts of official Medicine. The absolutistic-quantitative-analytical attitude still prevails there. Typically de Jongh aproaches homœopathy with an *aut-aut*, all-or-nothing attitude; is it true or untrue? instead of: does it improve our knowledge and practice. The difference between these two attitudes is fundamental, on it depends what the term "scientific" implies. There are strong reasons to suspect de Jongh of still adhering to the nineteenth century attitude while Hahnemann anticipated the modern one.

It is significant that de Jongh again and again (p. 27, and on numerous other occasions) insists on "bewys," i.e., proof in a mathematical-speculative (a priori) sense where only correct and conforming observation is possible. How, for instance, can two phases of the reaction of an organism to a medicinal agent be proved otherwise than by observation? Hahnemann observed a contrast between the first and the second phase of action; whether his observations were unduly generalised is quite another question.

On p. 29, de Jongh imparts to Hahnemann's assertion a crude, positivistic-empirical interpretation, though Hahnemann expressly states that the detailed observations of symptoms supply the conception needed for curing the disorder. Why should one not derive a principle to act upon, a method, from ordered observations, and then test this principle subsequently by experience? But de Jongh argues against his own misinterpretations and then accuses Hahnemann of lacking in critical faculty! It may be that Hahnemann in the later stages of his teaching and writing was inclined to generalise too much, but in this (as in many other instances) de Jongh has been misled by his zeal in proving too much against Hahnemann and homœopathy.

From the fact that Hahnemann makes no statement on posology in a certain context (cf. p. 31), one can infer at best that he considered dosage as a secondary issue.

Very likely the reading of Hahnemann's "Organon" gave de Jongh no unmarred pleasure. But then it was not written for de Jongh's pleasure but for the training of doctors, and that in a systematic manner after 20 years of pertinent experiments.

The fact that de Jongh freely and without "reservatio mentalis" speaks of "laws" (similia-*wet*, *wet* of Arndt-Schulz, etc.) like any homœopath who has not become emancipated from absolutistic thinking, is but another indication of where he still stands in the theory of science. Hahnemann seldom succumbed to this temptation, and certainly never put his methodical principle in the assertive form "curantur," but in the optative "curentur." This is not a purely linguistic matter as Bier among others supposes. Hahnemann was a first-rate scholar.

de Jongh completely misinterprets a quotation from Hahnemann (p. 32): "If one should find in experience (as one does) that a given symptom of a disease were removed only by such a medicinal substance which has amongst its symptoms (as produced by it in a healthy organism) a similar one, then it is probable that this medicine by its tendency to provoke like symptoms would be capable of eliminating similar symptoms in this disease." de Jongh alters this extremely cautious sentence into one expressing a necessity that symptoms (sic) of a disease can only be cured by substances which of themselves can cause similar symptoms in a healthy person. Then de Jongh, of course, finds it very easy to condemn the statement as a purely deductive one, to which the empirical facts are forcibly adjusted afterwards. Hahnemann begins with experience and very cautiously deduces his generalisation. This does not mean that Hahnemann was always so cautious in his statements; indeed he was not. But that does not justify a critic's reversing and distorting the factual and logical sequences, in order to make the s.s.c.-principle appear to be a dogma!

Hahnemann's attempt to give an explanation of the healing process through a "disease" provoked by the simile (cf. p. 33) is, in itself, of quite secondary importance, and does not affect the value of the method at all. Though I do not agree with Hahnemann's exposition, I must say it is by no means so stupid as de Jongh would have it seem. The main objection to Hahnemann's assumption is that 'disease" figures therein as a static entity. This, however, escaped de Jongh's "inescapable logic," because he himself has not yet got beyond that antiquated way of thinking, as his immediately following exposition on arteriosclerosis beautifully illustrates. Had de Jongh used his brains constructively rather than with a destructive intention, he would have seen that "the same kind of disease" acquires a sound meaning when it is understood as the reactions of the same kind of organism, manifest as symptoms either in spontaneous disorders or in response to a medicinal stimulus.

As regards provings, or drug-experiments on healthy persons, (p. 38) the report of one of Hahnemann's disciples could have shown de Jongh: firstly, that unsusceptible provers were rightly discarded by Hahnemann, for the provings are carried out to investigate and ascertain susceptibilities to the particular test substance: secondly, that the names of the test medicines were known to the medical observers, but probably not to the other provers. There must, of course, have been a number of provers, as so many symptoms are recorded from women, but none named. The names given are those of the supervising doctors. Hahnemann himself has given a full account of the procedure*. Even if the names of the substances were known to all, it would have told them precious little. It is to be feared that even at this stage, when the provings have been done, it would make little difference to de Jongh as a prover if he were told that the substance was derived from, say, Pulsatilla or Chelidonium. Anyhow, precautions like that, keeping the names secret until the end of the experiment, are, no doubt, better and were applied later. The possible and probable defects of the early provings have been sufficiently dwelt upon in homœopathic literature; it would be better if the critics did the work instead of suggesting it to others.

A black page, not for homœopathy as he alleges, but for de Jongh as historical critic is where he states (pp. 40-41) that Hahnemann kept to himself money donated to him for the Leipzig Homœopathic Hospital. With the greatest ease he could have verified (from Haehl I. p. 227 and documents in Vol. II.) that this was nothing but a mean slander spread by an anonymous enemy of Hahnemann's. de Jongh must either bring forward new

* cf. HAEHL, HAHNEMANN, Leipzig, 1922, II. p. 107.

documents corroborating his statement or else he should publicly revoke his mistake.

de Jongh's valuation of Hahnemann's mental state as abnormal (p. 41) can conveniently be taken for what it is worth, as it clearly depends on what is normal to de Jongh.

Criticism of Hahnemann's theory of chronic diseases and of the psora-theory in particular comes easily when it is superficially made. And de Jongh takes it all too easily (though not so narrowly as the "scabies-critics" used to do). The main problem of the "Chronic Diseases" has not even dawned on de Jongh when he concludes "that Hahnemann's psora-theory is a perfectly untenable and fantastic speculation introduced by Hahnemann in order to explain the failing of his homœopathy, and to justify new therapeutic measures which did not conform to his former opinions."

The problem before Hahnemann was that he observed, over prolonged periods of chronic diseases, quite diverse syndromes in alternation and vicariation (eczema—asthma bronchiale as a very common example. I have seen the two in regular rotation with a third syndrome, profuse hæmorrhages from the rectum). Hahnemann's error, in my opinion, was that he tackled the problem in the nineteenth century manner (so commonly persisted in by the orthodox school of to-day!) namely, by thinking in terms of a cause instead of conditions. Thus he came to miasms instead of constitutions (i.e., conditions of persons). Nevertheless, the practical inferences of Hahnemann were not so bad as de Jongh is pleased to surmise. From the very fact that Hahnemann had already 41 tested remedies for the "psoric" diseases, de Jongh could have seen that experience had guided Hahnemann to a method supplementing his former one. By tackling the problem of chronic diseases, namely by finding from experience a limited range of medicines particularly suited to alternating diseases, Hahnemann made possible another advance, additional to that of the general basic method. This advance has hardly begun to be exploited in homœopathy. A critic who does not even see the problem is hardly entitled to have his judgment taken seriously.

Thus, in his theoretical causal approach, I cannot agree with Hahnemann and admit the breakaway from his hitherto broader attitude of conditional thinking; his theory therefore appears to me wrong, but his pragmatic tackling of a profound and pressing problem was a great step forward in Medicine, an advance which is much in need of further development. I wish my own contribution to this end to be considered as constructive, and not destructive, as a continuation of Hahnemann's attempts to solve this problem. As I see it, the main practical advantage of this new thought lies in the fact that it enables us to take into account decisive events in the patient's past history, not only in his *status praesens*, when it comes to selecting the curative agent. The incongruence in the development of Hahnemann's thought and practice is only on the surface, underlying it is very sound and continuous progress. This may suffice to relieve me from refuting in detail the ill-conceived criticisms of de Jongh on this subject until he has thought it out for himself more thoroughly.

A critic who himself so dangerously plays with words instead of facts as de Jongh does, would be well advised not to accuse Hahnemann of this fault (pp. 44/51). Hahnemann had at least 20 years' experience of homœopathic treatment to his credit when he wrote on this theme. It is quite misleading to describe Hahnemann's empiricism as the crude procedure of accumulating sensory data only; Hahnemann's conception of the totality of symptoms, with differentiation between essential, important and less important symptoms, doubtless implies rational order, just as any modern diagnosis which de Jongh glorifies in contrast. The difference is that Hahnemann's conception leads to a diagnosis of a diseased person, whereas the old school does not go beyond disease-diagnosis. One can, and should, of course, make both. The question is only which in practice, with its enormously variable situations, brings us further; and that again is a matter of trial and experience. de Jongh apparently still treats "diseases" as real entities, instead of diagnostic conceptions, though (p. 53) he himself imputes this error to Hahnemann.

As regards Hahnemann's dynamism (pp. 47-51) de Jongh could have used a far better and simpler argument against Hahnemann, namely, that "dynameis" or forces have disappeared from modern science. Had Hahnemann lived in our time he could have saved himself all the trouble of looking for immaterial forces, life-force as well as the force of gravitation. But in 1810 his attempt was not so badly conceived and was substantiated by learned reasoning. If de Jongh still "believes" in the force of gravitation, so much the worse for him.

In agreement with de Jongh, I see no useful point in discerning primary and secondary effects (pp. 51 and 52), but then Hahnemann does not give this distinction as a "glorious discovery without furnishing proofs," nor as a fact, but as his opinion on an observed sequence of events. The arguments against this generalisation of biphasic events are the same as against the Arndt-Schulz "law," the "Wirkungstypenhypothese" of Koetschau and the many other more or less graphically described action-curves of modern authors; they are abstractions which do not conform sufficiently to the actual pattern of observed events.

Hahnemann demanded a painstaking and accurate case history; this improvement of method is acknowledged by de Jongh, but (pp. 53 and 55) he totally misunderstands its meaning. If it had not been for the practical purposes of a better therapy, if homœopathy could not make use of these details by its method of comparing the total pictures of symptoms, then this conscientious accuracy would be a mere nicety. As the method stands, accuracy of details becomes a need. Is it not better that Hahnemann should have recorded all the observed details which are so amusing to de Jongh (and to so many critics within and without homœopathy) instead of suppressing them merely to please de Jongh and his like? One can readily neglect those symptoms if they prove negligible, but cannot replace them by the imagination if they are not kept on record. I would recall to de Jongh the reply which M. Schlegel gave when Donner wrote: "*Wir müssen den homöopathischen Garten ausmisten*": "*Tun Sie das nicht, Herr Kollege, jeder Gärtner freut sich, wenn er Mist im Garten hat.*" (Sorry, this repartee cannot be translated without losing its flavour.) No, the investigation, whether a symptom was due to the medicine or not can wait, but if the symptom is not accurately recorded its value for consideration and use is annulled. If de Jongh cannot take simply stated psychic symptoms *au* *sérieux*, he shows in what past era of Medicine he still dwells. Before any critique comes the plain statement of what actually happened, even in pharmacology.

When H a h n e m a n n recorded symptoms which he considered not "pure," i.e. not from healthy persons, he put them in parentheses or said so. This apparently escaped d e J o n g h. His criticism on this point is irrelevant (p. 56).

As to the doses with which H a h n e m a n n made his provings, d e J o n g h quotes a passage (p. 128) of the 6th posthumous edition of the Organon where the 30th potency is recommended. Had he consulted the 4th edition of 1 8 2 9 he would have found (p. 129) that H a h n e m a n n recommended as dosage for provings "such quantities as one usually employs in prescriptions against diseases," i.e., substantial doses. In 1829 when he was already at the age of 74, H a h n e m a n n had done all the provings he is known to have supervised and published. Thus his later utterances, at the most, can have influenced any later provings. The passage has nothing to do with the reliability of H a h n e m a n n 's provings nor with the (wrong!) formulation of the s.s.c. by the "modern" homœopaths (I would certainly disclaim them to be modern) concerning the reverse action of great and small doses. Once more d e J o n g h makes much ado about nothing.

The high potencies are obviously the critic's delight. H a h n e m a n n in later years gave and recommended the 30th centesimal and I will not deprive d e J o n g h of the pleasure of hearing that I did so as far back as 1919. I was conscious then that I had abandoned "the exact quantitative basis" just as H a h n e m a n n, but I knew also that those who gave "units" of imponderable substances (tuberculine, etc.) had done exactly the same. This is still so with the biologically tested action-units of some "antibiotics." Hahnemann measured in action-units, "potencies;" and whether this or that potency under certain conditions acts or does not, only experiment and experience can tell. The scientific problem confronts only those who use these potencies and how they try to solve it temporarily must be left to them as long as experimental work is necessarily inadequate for the task. The choice of potency being a secondary issue in homœopathy, the "low" and "high" potencists may quarrel amongst themselves as much as they like; they remain homœopaths all the same.

de Jongh's argumentation (on p. 62/63) greatly taxes the calm and patience of his reader. He accuses Hahnemann of the following inconsistency: First, Hahnemann says that he will not speculate on the nature of diseases but restrict himself to an accurate description of the observed symptoms. Then it turns out that he does not use all the facts on an equal level, but regards some of more value than others. This is called "speculation" by de Jongh, hence Hahnemann has violated his own principle. It remains de Jongh's secret what the valuation of data has to do with speculation on the nature of disease. Every schoolboy knows, that we cannot make an observation without using our reason, but apparently de Jongh goes on to imply that "reasoning is speculative" (while it is so, of course, only if it has no actual contents). According to de Jongh's interpretation any "official" diagnosis would then be speculation. because he can hardly deny that making a diagnosis implies rational arrangement of data according to importance (pathognomonic symptoms, etc.), thus valuation. What Hahnemann intends by conception (sic) of the symptoms arising from either a medicinal substance or in " spontaneous " disease is nothing else but a diagnosis, only not of any static entity, but of actions as related to a living person, i.e., a whole in process. This diagnosis of the diseased person is fundamental to homœopathy and de Jongh's misunderstanding makes further debate a Sisyphean task. First he must learn the rules of reasoned debate. That is not all. de Jongh then goes on: this valuation of symptoms is a purely " subjective " criterion, and as this criterion is essential for applied homœopathy it appears that the selection of the remedy, the central task of the homœopath, is a rather arbitrary matter. Reasoning, valuation, is, of course, always done by a person, whether it is done in order to make an ordinary disease-diagnosis or a diagnosis of a diseased person or of the effects of a substance on persons. But is it therefore arbitrary? If the homœopath asserts that " headache worse from warmth, aggravated by wind " is more accurate and therefore more significant within the totality of symptoms than " headache " (unqualified), his reasoning is certainly not more arbitrary than, say, the assertion that " sugar in urine " is more significant for diagnosing diabetes mellitus than " thirst."

One may as well pause to give a thought to the old juggling with " subjective-objective." " Subjective," limited to a person, readily assumes the stigma of uncertainty. " Subjective " is then what the other fellow sees and thinks, but " objective " is what *I* see. The same with autistic-undisciplined thinking, it is always that of the other fellow. For example, de Jongh says he is an " objective " critic and will certainly assert that my views are " subjective " (as indeed they are, just as his are) and that my thinking as that of a homœopath is autistic-undisciplined. I shall have to take it—coming from him. I do not intend to imitate him in his game, but shall take as criterion, whether observations are accurate and reasoning correct, and disregard "subjective-objective" polemics. By hook or by crook de Jongh must arrive at his obviously preconceived idea that homœopathy is a matter of speculation and faith. Thus: (p. 63) Hahnemann says in essence: the dose (or rather stimulus) for homœopathic use is too small to bring the normally balanced processes in the organism out of order, while the simile acts on these already excitated parts (or rather processes) in the organism which are unstable, specially in the direction of the actions of the simile (the wording of § 155 of the "Organon" is very complicated, but the meaning is quite clear). Now de Jongh comes along and argues: "The homœopathic medicines, which are strong enough to sweep away all disease phenomena are too weak to bring about, of themselves phenomena in the organism. It is indeed remarkable how the whole of nature is arranged just so as to satisfy the demands of Hahnemann. If, however, that should not be so, is it worthwhile to examine how much science there is in the principle of Hahnemann and how much phantasy, wild speculation and, perhaps, credulity?" All these effusions about a certainly not far-fetched exposition, viz., that a small stimulus is more likely to evoke a response from processes already in disturbed equilibrium than from those in normal equilibrium! To a "primitive, naïve, unscientific thinker" as any homœopath is in de Jongh's eyes, such a state of affairs seems fairly obvious, or at least in conformity with our daily experience on unstable and stable equilibria. However, one never knows what a "scientific" thinker can make out of a simple observation.

Against Hahnemann's unitary conception of mental diseases, de Jongh pits his dualistic opinion (pp. 67/68). Hahnemann clearly regards body and mind as two aspects of the same "whole in process" and as a primitive thinker I fully agree with him; just as structure and function are to me aspects of the same organised whole. But I have no intention of reducing advanced scientific thinkers to my primitive state of mind. If the separation of psychic diseases, symptoms, etc., from physical diseases, symptoms, etc., is the achievement of the modern era it may be hypermodern to go back to the naïve conception of unity. Anyhow, I am sure that the unsophisticated average doctor in his every-day work takes the unitary view. And even the specialised psychiatrist would look foolish with his massive shock-therapy if he professed a separation of physical and psychic processes and their disorders.

Ordinary people are quite aware that their thoughts, emotions and impulses are the most distinctive manifestations of their person. Hahnemann and primitive homœopaths, like myself, accept this and therefore make special use of psychological symptoms when it comes to distinguishing one syndrome from another, as in the selection of the most suitable stimulus. de Jongh, however, concludes that these "futilities in the mental sphere" are undesirable and that their use makes the results unreliable, because it cannot be demonstrated that in such a case the homœopathic medicines have a "really pharmacological" action, I doubt whether further debate on these lines serves any purpose.

Then (pp. 68/69) de Jongh reprimands Hahnemann for his inconsistency, because, in epidemic diseases, he advises that the prominent symptoms not only of one patient but of all the equally affected persons under observation be taken into account. Clearly that involves diagnosis of the disease as well. It shows that Hahnemann was not the doctrinaire de Jongh would have him be. When a disease-diagnosis gave him further indications for selecting the fitting remedy he used it. So do we. But there is a slight difference in basing therapy primarily on disease diagnosis. The dogmatic critic sees only the theoretical antitheses, not the reasonable attitude in practice. If there is a "specific" for any disease it would be foolish not to use it. But what if there happens to be none?

As regards dietetics, de Jongh confuses Hahnemann's dietetic advice, viz., to avoid substances capable of interfering with medicinal actions, with his views on general dietetics. These are dealt with elsewhere by Hahnemann. If de Jongh had read them he might be able to judge, whether Hahnemann's dietetic precepts were advanced or not.

The procedure of potentizing ("dynamisation" as Hahnemann called it, meaning the liberation of latent forces) inevitably arouses the supercilious scorn of de Jongh. He probably has never looked under the microscope to see what happens to insoluble substances when they are triturated according to Hahnemann's directions. He might then have thought twice before judging the whole as "quaint nonsense." Before entering on that technical subject again, he may do well to polish up his knowledge of colloid chemistry. The question where the limitations of such subdivision and regular distribution lie could then be discussed on a more dignified level.

Even the *unitas remedii* (p. 72) viz., that one should give one remedial unity only, because it has been tested as such on healthy persons, i.e., not mix remedies, is not exempt from de Jongh's aggressive impulses. Many remedies, he says, are complex *eo ipso.* Does de Jongh know of any substance which cannot be analysed further? If one has the analytical mind one should be consistent and forbid the use of opium, even of atropine, for it contains d-hyoscyamine together with l-hysocyamine, each acting differently; and so forth *ad infinitum.*

Hahnemann assumes that all sensitive surfaces are able to receive and propagate the remedial stimulus. This makes it obvious to de Jongh that the action of the remedy was not a pharmacological problem in the modern sense. Actually Hahnemann's assumption says neither more nor less than the "reflex actions" in any textbook of pharmacology. Further, I cannot see anything funny in the fact that Hahnemann in his eighties, tried to achieve the mildest possible action via the olfactory nerve, instead of the oral mucous membrane. The fact that later on, at the age of 85 years, he abandoned this mode of application shows that he was still able to learn from experience. One may wish the same to de Jongh for his 85th birthday.

It is significant that the "black-or-white" critic defines his task (p. 78) as an attempt to decide whether homœopathy is a formidable lie or a divine truth! A nice little job to set himself!

The passage on the comparison of Kant and Hahnemann (p. 78) ending with "Hahnemann the antipode of Kant in the field of Medicine" must be read in the text to be fully appreciated. I take it as a hint that de Jongh has read the "Critique of pure reason," but it could do him no harm to do so again.

LATER DEVELOPMENT OF HOMŒOPATHY.

After his complete liquidation of Hahnemann's teachings de Jongh still finds it necesary to deal with the later developments in homœopathy up to the present. As doubtless every homœopath has accepted something of Hahnemann's teachings, viz., the use of the similarity of symptoms for choosing remedies, this part would seem superfluous if the first part be correct. However let us follow his main points.

Regarding the 18 theses on the true meaning of homœopathy as proclaimed by Wolf (pp. 90-92) I find myself to a certain extent in agreement with de Jongh. The main blunder of these "theses" is to state the s.s.c. as a "law of nature" instead of a practical method of therapy. The second blunder, and this is of greater import to the subsequent development of homœopathy, was to make the therapeutic rule which concerns the qualitative relation of remedy and patient dependent upon the contrast in quantity only. If only the quantities are taken into account, viz., that diseases can be cured by small doses of those medicinal substances of which large doses can provoke similar diseases in healthy persons, it becomes a very incomplete and vague empirical statement, very far from a "law," even if one concede this term as admissible in science at all. There are more relations than "quantity" to be considered if a therapeutic method is to be applied properly; reduction of quantity of dosage from that used for provings is an evident consequence in most cases, but as the susceptibility of persons varies considerably, it is by no means an absolute postulate. Not only are "small" and "large" very relative terms, but they say nothing about the physical state of the remedy which in very many cases is more decisive than the measured quantity of the substance. The W o l f-theses are, in my opinion, an unfortunate attempt to adapt the homœopathic theory to the one-sided quantitative thinking of the nineeenth century—still persistent in the present, hence rightly called the "old," school of Medicine. From this fundamental blunder, so wisely avoided by H a h n e m a n n, flow all the inadequate quantitative "laws" or "hypotheses," like A r n d t-S c h u l z, K o e t s c h a u, etc. It is significant that this blunder is made chiefly by that group of homœopaths who style themselves as "*naturwissenschaftlich-kritisch*" (W a p l e r, etc.), meaning that those who do not accept their one-sided quantitative attitude are *eo ipso* unscientific and uncritical. I have always preferred to be unscientific and uncritical according to their interpretation of the terms.

d e J o n g h uses the phrase *similia similibus curantur* though the context refers to a postulate which can be expressed only in the imperative-optative form *curentur* (p. 92).

d e J o n g h is right when he says that amongst the homœopaths there have been (and possibly still are) some to whom the homœopathic teaching has a religious character; but to conclude from this that homœopathy h a s a religious character or is a religion arouses certain doubts about the scientific character—though not of homœopathy.

H u f e l a n d ' s wise counsel "let us have not homœopathy, but a homœopathic m e t h o d within rational Medicine! Not homœopaths, but rational physicians using the homœopathic method at the appropriate opportunity and in the proper manner!" (pp. 95/96) has no value to de Jongh because it is outdated by his modern and therefore superior views (on rational Medicine or on homœopathy?).

de Jongh apparently has more sympathy with men like Ed. Martin and Fr. Kuechenmeister (p. 96) who abandoned their previous homœopathic convictions rather than forego their professorship (a professor is one who professes his convictions!) than for men who did the opposite like Rapp and Jmbert-Gourbeyre. He could have added here E. Schlegel who, after he had qualified, refused to exchange his homœopathic convictions against an M.D.

A justified query regarding homœopathic writings is put by de Jongh in the following (p. 103): "How can these constitutional features, concerning *habitus*, complexion, etc., come into the homœopathic drug picture, while they obviously cannot be produced by that remedy in a healthy person?" In answer to this very reasonable enquiry may I quote an extract from my "Memorial lectures at the centenary of Hahnemann's death" 1943 (at present in the press):

"Modalities are pointers qualifying the observations which result from such biological experiments as our provings; pointers not only to knowing but also to using the tested substances. . . As to the constitutional modalities, in provings, say, of Phosphorus, one certainly cannot produce lean, delicate, sensitive persons of fair complexion with reddish-blond hair; nor plump, sluggish people by testing Graphites; nor wiry and tough ones with dark complexion by giving Nitric acid.

Yet, if such types show themselves particularly susceptible to the action of the respective substances, one rightly notes it as a valuable pointer to the constitutional conditions."

Only when discussing Bakody's appreciation of Hahnemann's achievements (p. 105) does de Jongh find himself compelled to admit that "Hahnemann was one of the first who attempted to obtain knowledge of the actions of medicines by the experimental method." That is something and might appropriately have been mentioned in the main chapter on Hahnemann's work, before condemning it wholesale. Perhaps a little more attention to this point might bring de Jongh to understand why some people still consider Hahnemann as one of the great and rare reformers (or if he prefers revolutionaries) in Medicine; though the "experimental" issue is only one of several of the same rank.

Rightly, de Jongh criticises (p. 106) Bakody's narrowness in establishing a limit to potentisation at the 6th decimal potency. In fairness, however, de Jongh should add that at the beginning of this century 1:1,000,000 was considered as dangerously homœopathic! *Tempora mutantur et nos in illis.*

As to the "Arndt-Schulz law" (p. 107) I must refer de Jongh to my criticism in "Grundlagen der Heilkunde," 1923 and "Jahreskurse für aerztliche Fortbildung, 1925." The grounds for my repudiation of this law, rule or whatever it may be called, seem to me more thoroughly given there than de Jongh states his. I take it from this and other instances, that the "Grundlagen" have escaped his notice though a second edition was issued in 1927 (Hippokrates Verlag Stuttgart). In 1921 I sent the manuscript of this part to H. Schulz. In a letter H. Schulz replied:

"I have always thought that this 'biological law' provided a sound scientific support for homœopathy." He did, however, not go into my arguments. At a later place de Jongh infers that my criticism of the Arndt-Schulz rule was due to the consideration that experimental refutation of that rule might undermine homœopathy. No, at that time, 1920, when I wrote this passage, nobody had bothered about experimental refutation of Schulz's tests in support of the "law." My objections were simply of a scientific kind (apologies to the really scientific thinkers!). The "scientific-critical" group in homœopathy (Wapler, Bier, Donner, etc.) have therefore duly recognised me as their opponent. I still consider the one-sided quantitative interpretations of homœopathy, be they Arndt-Schulz's or Koetschau's as a lame compromise. Equally Schulz and his "scientific-critical" followers seem to me to see homœopathy much too narrowly when they base it on the affinity of medicines to particular organs or tissues. I shall therefore consider the whole extensive debate on this issue as beside the point

In the criticism on Emil Schlegel (p. 116 ff), E. Schlegel's consciously "unscientific" attitude must indeed be unpalatable to de Jongh who calls him a mystic. I can assure de Jongh from the all too few discussions I had with E. Schlegel that I always found him better acquainted with up-to-date knowledge in physics and biology than all his "scientific" critics, not to speak of his great and well-founded interest in the theory of science (epistemology) and particularly in Kant.

Surprisingly de Jongh omits Dahlke from the representative figures at the beginning of this century. Certainly Dahlke was one of the few, and many minor authors in de Jongh's review might well have been dispensed with in his favour. Dahlke, a buddhist monk living in Berlin, would have further supplied a good example of the "eccentricity" of the homœopaths alleged by de Jongh. Frequently de Jongh complains of the enormous variations and divergencies of opinion in homœopathy, a fact which according to him makes it so cumbersome to deal with. Indeed, homœopathy rightly allows fullest freedom of thought and action to those who make use of it and those who do not. I may add that I myself had differences of opinion with Dahlke who objected to my attempts at teaching homœopathy. To Dahlke homœopathy was unteachable, not suitable for the "*profanum vulgus medicorum*" (den gemeinen Aerztepöbel, as Hahnemann says). Dahlke wanted homœopathy given "like a torch" from one to another fully prepared for it. I could not take his view, otherwise I would certainly not have taken the trouble of writing a textbook on the subject.

As regards Bier's intervention in 1925, I agree that it has added nothing new to the store of thought and practice in homœopathy. At the "Bier-Abend" on 29th June, 1925, in Berlin the scientific discussion was on such a low level that I could not make up my mind to speak there and then, though I had made a long journey for this purpose. But I must correct a faulty quotation by de Jongh (p. 135) on this matter. de Jongh says that Leeser "expects that Heubner will perhaps admit his mistake (?) openly one day, for to the beginner (!) it is incredibly difficult to understand the homœopathic theory." What I wrote is this: "Also as regards Heubner a growing understanding for homœpathic thoughts can be noted. Of him one can expect that after further penetration into the homœopathic method he will publicly acknowledge correction of his views. We (meaning the homœopaths—O.L.) have also to consider that, from our side it has not been made so easy for newcomers to find out, from theoretical approach, what is valuable in homœopathy." That is something quite different from what de Jongh imputes to me! (I hope that he has been more careful with quotations, which I am not able to check, from other authors.) My impression of Heubner, at the time, in spite of complete disagreement with his opinion was that he had the character to profess without fear or favour his convictions whatever they might be or become, and that he was not a petrified specialist. A later correspondence with Heubner and his revised statements (cf. p. 259 of de J's and p. 81 of this book) have confirmed me in this view of his personality, I am glad to say.

de Jongh again distorts my words in saying (p. 136) that in my view the sulphur-treatment of furunculosis might become a "Schlager" (a "hit") in homœopathy. What I said was that the ordinary practitioner, who can hardly be expected to devote further years to a full study of homœopathic *materia medica*, might find an approach through a kind of "Schlager" (routine indication based on disease-diagnosis), and that sulphur in furunculosis is just that. Now, once again that is something very different. de Jongh may not know that the old sulphur-furunculosis indication had been recommended to Bier by Stiegele for that very purpose. I regret to this day that the mentality of the practitioner in accordance with his training makes such short-cuts necessary to start him thinking. Also, I prefer a man like Heubner to the many opportunists who so suddenly discovered their homœopathic inclinations after Bier's intervention.

Coming to de Jongh's interpretation of my writings, or what he has read of them (on pp. 137-148), I must first thoroughly disappoint de Jongh when he connects me with materialism. How he arrived at this view remains an enigma to me. Already in my "Grundlagen" of 1923 (written in 1920, but not published earlier because no medical publishers would, at that time, accept a homœopathic work!) I spoke out very clearly against "materialistic, mechanistic, vitalistic, realistic and idealistic" views in science. This book has been missed by de Jongh in spite of his admittedly great zeal in reading homœopathic literature. However, if de Jongh wants any "isms" for designating *in abstracto* my views on nature, I venture to offer him "actionalism" and "integralism." Being a primitive thinker I realize that I have to explain myself a little more.

I am concerned only with actions, happenings, events, and for these I am prepared to accept the action-quanta as ultimate units of analysis. I am not deep enough a thinker to fathom reality, in fact Kant has cured me of this trouble. de Jongh will therefore pardon my naive feeling of slight discomfort when he so frequently speaks of the "real," meaning to him obviously what all "scientific thinkers" are able to observe. Indeed I hold even with primitive old Heraklitos that each and every action is unique, cannot recur as the same, but only as if it were the same, i.e., as similar. Thus we would be able to observe only recurrences of events; but surely with continuity in them and constancy in their sequence so that one certain kind of event will not be seen to occur without certain preceding events conditioning it. There we have—in order fully to understand this regular sequence of events—to make use of our reason with the category of causality and to conceive the necessity in the recurrence of events or processes. Hence a primitive thinker may even hesitate to speak of a "law of causality in Nature" because he does not presuppose any absolute necessity of the sequence of events, but conceives necessity as inherent in any order to be established within a concrete sequence; he is first an observing, then an acting part in Nature. Further, he becomes aware that he cannot use the other categories such as quantity and quality independently of each other in observing the course of events. We assume that he has reconciled himself to the interdependence of space and time since Einstein. In short, by integrating his perceptual and conceptual faculties this primitive comes to think in interrelated "wholes in process." He avoids by this integration the trap into which a "scientific thinker" might easily fall, namely that he uses one form of perceiving or one category of conceiving separately, or perhaps one after the other without due regard to their intimate interdependence. In short, the primitive thinker arrives at a kind of "holism," but rather shuns to call it so, because it might convey the assumption of static wholes instead of wholes in continuous process. Therefore he calls it, for the time being, an integralistic attitude to nature. In this he finds himself confirmed by modern physics. He even holds a unitary conception of living and non-living process-structures (he would say beings or objects, were he not afraid of introducing the static view through a back-door). He is no longer troubled by such dual aspects of "action as a whole": particles and waves, structure and function, matter and force, body and mind, objective and subjective, in short with all those scholastic niceties of materialism and spiritualism (or psychism), with structural and functional diseases and the rest of it. This "actionalist" may then even see, and naively acknowledge, degrees of specific quality, ranks of organised wholes, of lower or higher integration for specific action; he may find values in accordance with the integrated complication of "wholes in process," grades of value to be gauged by specific performances in relation to a more comprehensive whole-in-process; he differentiates even between thought-performances, and values one thought as better than another. He is no longer restricted to measuring quantities, to take all "objects" to pieces before he can do so, he does not expect to be able to build up the whole by simply putting his pieces together, nor take the parts of his analysis as equal and the same, thus finds mechanistic possibilities severely restricted. In brief, he is no longer a "scientific" thinker of the true brand. So much for my materialism which, I trust, de Jongh will now angrily (because it was the last hold) cross out after my name.

Perhaps de Jongh will believe me now that nothing was further from my mind than "law giving," and will cross out those "laws" once and for all in connection with my views. The principle of optima of action to which I have repeatedly referred is nothing else but a formula comprising the conditions of space, time, quality, quantity and form which have to be considered as interdependent with regard to any particular action. Whether it is vague or perhaps even a banality I must leave to the higher judgment of de Jongh. Yet, my formulation might serve as a reminder to those who possibly have stressed sometimes the one or the other condition of a particular effect too exclusively, e.g., quantity, while neglecting the quality and form of an agent.

As regards my expositions on physical aspects of the potency-problem at the Arnhem Congress, 1934 (pp. 137/138), I have no record and, therefore, cannot check whether de Jongh's misunderstandings may be due to erroneous publication of my communication or not; though the quotations de Jongh gives still have my approval. This short communication was read to an audience which could be assumed to be acquainted with the general physico-chemical problem of potentisation and with the views I had expounded in earlier writings ("Grundlagen der Heilkunde," 1923, and "Homœopathie und Biochemie," 1932, Reclams Universal Bibliothek, Nr. 7175"). The only new feature in the Arnhem paper was to draw the attention, with respect to the potency problem, to some consequences of the second thermodynamic principle. If I have spoken on "entropy" I have, no doubt, used this term in the same sense as any physicist or textbook of physics does and not made such nonsense out of it as de Jongh imputes to me. Entropy is a measurable index of the irreversible loss of energy which ensues when a "system" or "object" changes from higher temperature (i.e., greater frequency of motility) to a state of lower temperature (lesser frequency of motility). I am sorry to have to use such cumbersome phrasing, but am afraid to use mathematical symbols and formulæ instead. A consequence of this principle is that a system of higher order, higher organisation (or however one may call this higher grade of integrated actions) contains something which is irretrievably lost when it is reduced in the direction of chaos. My reasoning was that, by more and more regular subdivision of particles in an indifferent medium, increasingly higher grades of order are obtained. That this is so can easily be seen under the microscope by examining the first grades of potencies of a trituration of suitable substances (e.g., metals). This "higher grade of order" implies a measurable increase of potential activity; measured namely by the entropies, the energy which would irreversibly be lost by reducing a definite higher "system" back to a definite lower one, say the 3rd decimal to a 1st decimal trituration. I should think that this point did come out clearly in my communication and I rather suspect that de Jongh missed it. This part has no direct reference to high potencies. But it might have induced de Jongh to subject his high-handed rejection of any significance of the procedure of potentizing (or dynamisation as Hahnemann called it) to another and more cautious revision. Curiously enough de Jongh takes exception to us primitive scientific thinkers for using the modern phyiscal knowledge (is it not common good for solving our problems?).

For the benefit of other primitive thinkers may I add a few general remarks here. As I see it, the world of events, of happenings, or actions is a fluctuation between chaos and cosmos, the absolute extremes of both being outside any possible experience, because in either case of absolute completeness, there could be no more happenings. Within this fluctuation to and fro I recognize distinct "order values"; to some extent they are even measurable in terms of entropy, the amount of potential energy they would lose by degrading the order-level, e.g., an atom is more than the sum of its electrons (electron, of course, used here in the comprehensive meaning), an organism more than the total of its cells. This "more" lies in the orderly arrangement of parts related to a whole. So far as we are able to measure this order-value by comparing effects of various systems of different order-level, we arrive at quantitative indices of potentials of action. So much by the way, for those who will consider "unscientific" views.

As regards the wider problem of "high potencies" I have stated my views clearly in the two books missed by de Jongh. I notice that de Jongh dealt with my views on this problem in a later chapter (pp. 328 *seq.*) thus there will be an opportunity to deal with his criticism later.

On p. 139 de Jongh promises that he will argue with me on the purely epistemological field at the proper time. Having been always specially interested in the theory of science I eagerly looked up de Jongh's book for this feat of arms, but could not discover it. Perhaps de Jongh proposes to bring it in a second volume from his easy pen. and I would like to suggest that, in the meantime, he read the "Grundlagen der Heilkunde"; for it is there, not in the special part of my text-book, that I have dealt with these questions.

I acknowledge that de Jongh has reviewed (pp. 139-148) the introductory part of my textbook ("Mineral Remedies") generally in a fair manner; that some of my views on constitution (p. 142) appear strange to him I can well understand. Nevertheless I must point to some misunderstandings or mistakes in his review. On p. 146 he could easily have come to an understanding of my reference (p. 54 l.c.) to Loewi's Kalium-vagus-heart experiments, if he had looked up p. 105/6 under Kalium of the same book. But I have found no hint that he troubled to study the main part of the book, i.e., the remaining 643 pages! A complete misunderstanding of the tenor of this book by de Jongh becomes apparent (p. 146) when he says that, by attempting to explain the differences of substances in the pharmacological sphere (read: in respect to their medicinal actions) by their physical and chemical qualities, I tried, without particularly saying so, to explain biology in terms of the sciences of inanimate nature; that it therefore appears that I (in contrast to my confrères who are supposed to hold vitalistic views) am leaning towards "materialism." Not at all; the theme of the book has nothing to do with materialism nor with vitalism. It should be quite clear that I consider the one aspect as superfluous as the other. But I do maintain that the potential actions of a certain substance must manifest themselves similarly under comparable conditions, so that physico-chemistry of the laboratory as well as geochemistry can serve to give us a better insight into the specific actions in and on the organism. And as the natural system of chemical elements supplies an unquestionable and firm starting point for all the relations between the elements and their actions, the only queer thing is that an unscientific homœopath should undertake this kind of investigation and that the more competent physiologists and pharmacologists have not tackled this task so far.

What I have said about the order-values of related wholes should, in the light of modern conceptions of physics, have made it a little clearer why I do not see the formidable gap between living and non-living wholes in process. The revolution in modern thinking implies, in my view, that the exact sciences (physico-chemistry) have become more and more "biological," even in terms. We no longer need the 19th century language, e.g., forces, be it of gravitation or of life. I see, however that d e J o n g h still insists on it as regards physical "forces" such as gravitation. He might therefore have shown more indulgence for

Hahnemann who, at the beginning of the 19th century, assumed a life force. Apart from that de Jongh appears to stand with both legs firmly on the grounds of the 19th century, while Hahnemann at least had his eyes on the 20th century.

By the way, "biosphere" (p. 146) is not a term invented by myself but commonly used in geochemistry.

On p. 147 de Jongh promises to reply to my exposition that for the remedial action of a substance which has a physiological role in the organism, we must suppose the existence of a disorder in the physiological functions and migrations of just this substance. I have looked through the book in vain for this reply.

On p. 148 my cautious assertion "that it should be possible, by comparative investigation, to trace and estimate the actions of constituting elements (atoms) within a complex chemical substance (e.g., a salt)" is brushed aside by de Jongh with the supercilious remark: "a very interesting but alas *again* unproved opinion." If he were interested in a "proof," as far as that term is applicable at all in such a matter, then he would have examined the special part of the book which abounds in examples and attempts at this difficult analysis. I have no doubt that it can and will be done better, in fact I could improve on it considerably now after 13 years. (The book had to be published, in the prevailing circumstances, earlier than I intended.)

How does de Jongh reconcile his complaint "that it is so troublesome to criticize homœopathy properly, because the dispute (who disputes?—O. L.) is conducted on many fronts with the most modern means from divers fields of science," with his repeated statement that homœopaths are such primitive, naive, unscientific, autistic-undisciplined thinkers?

Regarding Koetschau's views (pp. 148-157) about the one-sided use of the causal-mechanistic approach in Medicine there is, in my opinion, as may have become clear so far, something in it worth the further consideration of 19th century scientists. Koetschau seems, however, to overlook that the usual quantitative approach to the problems of life is not less one-sided and misleading. Nor do I agree with Koetschau when he says: "Whoever wants to deal with exact science must confess to a strictly causal conception if he does not wish to expose himself to sharp criticism." Quite capable physicists have relieved him of such fears. That is however not to say that causal conception is not a paramount instrument for obtaining knowledge and I cannot follow Koetschau in his "aut-aut" attitude, by which he arrives at two divergent sciences, "exact" and "biological." I can see only one scientific attitude for obtaining knowledge of living and non-living processes. I cannot follow Koetschau either when he establishes "laws" for exact science and "empirical rules" for biology, and it seems indeed inconsistent when he brings his own abstractions to the fore as "Wirkungstypen-hypothese." This faces the same general objections as the Arndt-Schulz rule, however it may try to extend the dimensions for including various action-curves; they still remain pure and simple abstractions. His model may be quite suitable for teaching purposes to demonstrate which various action-curves have to be considered in a concrete sequence of observed events, but it is of no assistance in the proper task of science, viz., of ascertaining by observation the conditions under which these events come to pass. Thus Koetschau's 3-dimensional model seems to me better suited to support a *pes planus* than a scientific hypothesis (apologies to de Jongh for stealing his thunder). Koetschau, a post-Bier newcomer to homœopathy, cannot yet reconcile his "exact" with his "biological" soul, but he may meanwhile have found a way out of his impasse. In any case, great as my divergencies of opinion with Koetschau's may be, I wish to emphasise that de Jongh's last sentence (in parentheses, p. 159), where he imputes mercenary motives to Koetschau's views on prophylactic treatment, is an unworthy dig at the sincerity of a colleague. Similar untimely "drops of bile" (vide Preface of Professor Dr. S. E. de Jongh) have escaped de Jongh on several occasions. He might have used his bile more profitably for digesting some fat thoughts. As it appears, he has overfed himself with, to him, indigestable homœopathic literature, by taking it in too rapidly, without any appetite and with little discrimination. No wonder, that sometimes his bile comes up and that, in the end, he vomits the whole lot.

It is not surprising that the spirited Hans Much should encounter the full displeasure of de Jongh (pp. 161-164). Much was certainly a man who had something to say, and when he said it spoke out without fear and favour. His full agreement with all essential principles of the homœopathic method, even with the high potencies which are not essential, need not carry weight with anybody with a different trend of mind. But at least, his supporting experiments cannot be reasoned away, they have to be refuted by other experiments. As to the limitations of the homœopathic method (p. 164), I can assure de Jongh, that Much was in full agreement with my own opinions as given in "Grundlagen der Heilkunde."

(It may be of some interest to recall M u c h ' s enthusiasm when he had read my manuscript in 1921, I had sent it to him with a critique of his "Pathologische Biologie" 1st edition, for which he was grateful. He invited me to see him at Hamburg to discuss homoeopathy. He met me with the words, "That's what *I* wanted to write ! Why must the Jews always be first with good ideas ?" To which I replied, "That is because their brains are the more ancient, Herr Professor." M u c h could, however, not induce his publishers to accept a book with the word "Homœopathy" in the title.)

There is not much point in the studious search for uncritical assertions and case reports in homœopathic literature. Such search might easily disclose similar shortcomings in journals of the "old" school. Any critique of thought and practice should be conducted at the highest available level. It should, however, be put to the credit of these, mostly very busy, homœopathic practitioners with a minimum of institutional facilities, that they have, in those 150 years, produced quite a sizeable literature, and regarding their very small number within the medical profession, the average niveau can well stand up to that of the "old" school. There is plenty of criticism and difference of opinions inside homœopathy; even *enfants terribles* and jesters have their full scope (e.g., D o n n e r). This is rather a welcome sign of active interest. If d e J o n g h complains that the divergencies make his critical task so cumbersome, he might ponder how this fits with his supposition of homœopathy being a kind of religion. Most certainly I have not yet been ex-communicated for my "unorthodox" views, nor would I wish to see my opinions canonised. Fortunately there are no rulers, no credo and no "Gleichschaltung" as far as I have witnessed, and still the essence of H a h n e m a n n ' s teachings is preserved.

All the attempts at killing homœopathy have failed and are bound to fail; there is only one way of dealing definitely with homœopathy: complete assimilation of its doctrine and application by an advancing Medicine. Then the name "Homœopathy" may gladly be dispensed with. Meanwhile any doctor responsible for his own decisions and actions has equal rights to express his opinions, but that is not to say that these opinions are equally right. *Est modus in rebus.*

de Jongh did not need to elaborate that the "dynamisation-theory" still has its place in present day homœopathy (pp. 210-213). In 33 years I have not met one single homœopath who does not use potencies, e.g., of the various carbons, like graphites and carbo vegetabilis. What de Jongh calls "dynamisation-theory or hypothesis" is nothing but the use of a technique for sub-dividing a medicinal substance within an indifferent vehicle. The trouble is that all too few homœopaths have devoted enough "thought" to this important technique without which homœopathy would be a much poorer method. The exposition given in my Congress communication, Arnhem, 1934, added only one point to those I had made in the "Grundlagen" and in several other publications.

I wish expressly to be included in the list (p. 218) of those, who though they do not "believe in," have yet convinced themselves of a certain preference of some remedies for either the right or the left side. Whether Donner calls these observations nonsense and the "believers" irrational people makes no difference to me. Are the symptoms of heart- and aorta-disorders (say *angina pectoris*) felt more on the right or the left? Can it not be reasonably said that medicines with a heart affinity have in their actions a preference for the left side? Is the vegetative system symmetrical? Why have some migraine patients their attacks always on the right, others on the left side? Are not several brain centres unilateral? Must not remedies with a special affinity to an asymmetrical structure manifest a unilateral action? It is a pity that de Jongh is always sure to take up position (cf. his amazing footnote) with the all too superficial thinkers. It is, however, not superfluous to add a warning against insufficiently substantiated and too lightly made statements on definite instances of unilateral action of remedies.

Equally, blind zeal has misled Donner in his statement (ref. to p. 218/19) that caulophyllum has never been tested on women and yet is known in homœopathy as a remedy preferably affecting the female sexual organs. If he had looked up Hale's "New Remedies" he could easily have found out that his assertion is not true and so would have saved de Jongh another exclamation mark.

Regarding the local treatment of skin eruptions (pp. 219/220), many dermatologists are well aware of the often serious consequences of treating extensive eczema, especially in children, with ointment dressings. (I know of a case where the dermatologist himself admitted: "I think this child died from the bandaging up.") The chronic sequels of such attempts at suppression are experience from which further thought on Hahnemann's "psora theory" may well start, whatever interpretation one may adopt. Generalisations on these matters of empiry, for or against any local treatment, are of dubious value; nature does not take kindly to absolutistic rulings.

About constitution and contitutional remedies (pp. 220-231) enough has been said already to set de

Jongh thinking again; but I would recommend him to take into account my paper on this subject in "British Homœopathic Journal," July, 1934. I have not stated (p. 221) that the first-rank constitutional remedies are the same as Hahnemann's "anti-psoric remedies," but that it turns out that these "antipsorica" of Hahnemann's are overwhelmingly inorganic physiological substances to which I accord this special rank of being preferably suitable for influencing the inner conditions of a diseased person. This implies indeed a striking agreement, but not one of theory, in which in this respect I disagree with Hahnemann. It is matter of empiricism which can be approached by divergent avenues of theory.

Why the "modalities" are so important for finding the fitting constitutional remedy (p. 224) has, I hope, become clearer by now to de Jongh, who finds it difficult to think of reasons.

de Jongh's remark (p. 224) that constitution must not be considered static can only be underlined; it is the condition of the person in process, not only the present condition with which, however, as physicians we are primarily concerned. How decisive events in the history of a person can be utilised for selecting the remedy has already been hinted at.

Whether one attaches great factual or instructive value to such schematic tables as that of Fortier-Bernoville (p. 227-8) or not, it does not need a "cryptograph-expert" to read it but somebody acquainted with the actions of the tabled remedies; and as de Jongh is not, he might have done better by leaving the matter alone.

On p. 242 de Jongh misquotes me, so as to be able to get rid of another "drop of bile." In respect of Hahnemann's psora-theory I did not say that "it would have been better understandable if Hahnemann instead of 'psora' had said 'tuberculosis bacillus'" but "it would be better understandable to us these days, if Hahnemann had cited the tuberculosis bacillus as a cause of the 'arch-evil' psora supposing he could have done so." De Jongh infers that "I have little sense of history."

My views on the requirements for elaborating a picture of the actions of a drug from the provings (referred to as physiological pharmocology) on p. 250 are scrutinized by de Jongh and there, too, my words have been twisted. What I said was that mass experiments on healthy persons are unsuitable for discovering the essential features of drug actions; that by treating the results of such mass experiments statistically, only the most common symptoms of little importance and usefulness are brought out; that the drug provings, being a search for qualities, need a non-statistical method; viz., to find out first those persons responsive to the particular substance, then to continue with those provers only, so as to get the reactions more and more accurately expressed. Repetition of observations on the same or other provers then assists in assessing whether a certain symptom is actually connected with the given drug or not. de Jongh asserts that, by repeating previous observations in order to confirm them, I am coming back to the statistical method. It all depends upon what he calls "statistical." If he makes elaborate experiments on a few rabbits, does he call that a statistical method, too?

Further, I explain that for forming a drug picture, one has to correlate the result of the provings with all the other data available, especially those known from toxicology and experimental animal pharmacology. This latter branch appears to be claimed by de Jongh as private property, he speaks of "our" pharmacology, meaning that a homœopath must not avail himself of it. He thinks that I need an excuse for making use of these findings of others. No, I don't—and if de Jongh had taken the trouble to study only a few of the many monographs in the same book, he would not have been left in doubt where, in my opinion, the toxicological, pharmacological and physiological facts find their proper place. This use of as many data as are available from any pertinent field of knowledge does, however, not exclude my considering the observations derived from provings on healthy persons of paramount importance for the purpose of using the acquired knowledge in the treatment of diseased persons. Is it now clear that I have no reasons to excuse myself? Or are we to consider de Jongh's book as one long-winded apology for not having studied homœopathic *materia medica*? de Jongh would have understood the whole matter better if he had conducted provings himself or at least taken part in them.

However unsatisfactory it is that substances not yet or not yet sufficiently tested on persons (p. 251) have to be used, this requires no apology either.

Where the work has not yet been done, one has sometimes no choice but to rely on crude empirical data instead of accurate detail substantiated by planned tests. Of all the "talkers" about the need for a "new" homœopathic *materia medica* very few have lifted a finger to do competent work.

If Bergmann (p. 257) in an article says that there are no provings of Achillea millefolium he is mistaken, six provings are easily accessible in literature.

What, for heaven's sake, has the fraudulent dispensing of prescriptions to do with the confidence in the homœopathic *materia medica* ? (p. 258). The description of an acute phosphorus poisoning (p. 258-9) would perhaps not be so amusing to de Jongh if he had been the sufferer, but I am sure the description would then be far more "objective."

It must be a disappointment to de Jongh that his witness for the prosecution, Guttentag (one-time assistant of Volhard), is now on the side of the defence as a very active member of the homœopathic fraternity in the U.S.A. He was indeed a fierce opponent when I first met and discussed with him in 1926.

The reprovings of Martini (p. 260-265) of Sepia, Bryonia and Sulphur, his inferences and opinions and the subsequent disputes are of a period when I no longer followed German literature (1939). I have, therefore, to take de Jongh's report as a basis. These provings are said to have been made with all the precautions of experimental technique and they were negative. "With Sepia only two (out of?) provers showed nervous symptoms" which Martini, however, did not take as significant (how did he know? That could have been decided only by further tests, with varying Sepia preparations on these persons). Then Martini seems to have used the mother-tincture, a very unreliable preparation of Sepia, as every expert could have told Martini beforehand. Sepia has to be prepared by trituration, and "dynamisations" (yes!) under D3 (or 3x) are, as far as my experience goes, unsuitable. But here the purely quantitative attitude of the experimenter apparently was the first handicap, for he seems to think that even greater quantities of the mother-tincture *might* have given specific symptoms. From a homœopathic point of view—and he intended to investigate homœopathic assertions—his plan was inadequate from the start and his general conclusions precocious.

The gastro-enteritis of massive Bryonia doses could have been found in any good textbook of toxicology.

Without seeing his minutes of the tests with smaller doses (what potency?) no judgment on the observed symptoms is possible; that similar symptoms appeared during those periods also when plain milk-sugar was given is not proof enough, because in his experiments some of these bryonia-free periods were interposed in between the bryonia-periods. In a good proving the symptoms of at least a fortnight after the end of drug-giving have to be scrutinized for after-effects; alternating fortnightly periods of milk-sugar and Bryonia does not indicate that the experiments had been well thought out (Bryonia should have been tincture or dilution, there is no proper milk-sugar preparation of Bryonia! How does that accord with his scientific precautions?). Without knowing the precise details I am not prepared to say more on these experiments.

A study of the very elaborate Bryonia re-provings of the Austrian school seems to me meanwhile recommendable to anybody who busies himself with this subject.

Regarding Sulphur I have no precise data of Martini's experiments at all; thus cannot judge.

However conclusive his experiments may appear to Martini or anybody else, I maintain my opinion, based on my own experience in conducting provings, that the negative results of a series of provings allow no conclusions with regard to others in which reactions have been seen. A pharmacologist does not choose rabbits in order to examine the act of vomiting. A proving employing massive Bryonia tincture with the object of investigating details such as the aggravation of stitching pains by any movement, is doomed to failure from the outset.

To make this important point clearer I shall give a few examples of own provings. My first experiments: Kalium carb. D.30 (the provers could not possibly know whether they got medicated or unmedicated sugar) on 15 provers without any symptoms. Conclusions: none (not that these potencies could not act, that would seem to me too "scientific"). D.3 on 11 provers (4 had had enough of the tests which require attention and good will). Of these 11 only 3 provers described a small number of symptoms (some of which accorded well with significant symptoms of Hahnemann's provings). Conclusions: Only these 3 provers appear susceptible and suitable for further provings with Kalium carbon. These too, however, had had enough by then. The proving therefore appeared fragmentary and too inconclusive for publication.

The proving of other substances could be continued long enough to produce "objective" symptoms (rhagades under Acid. nitric, traces of albumen in urine under Eucalyptus). In these cases, too, I have desisted so far from publication in the hope of making the results more conclusive later by further experiments which, however, did not materialize. But in each case a number of the original provers had to be left out of further consideration because they did not react.

If Martini says that I propose to refute many negative results of control-provings he tries to gain his point by reversing the point in question. The question is whether the negative results of some experiments can refute the positive results of others. Hahnemann already stated that in provings the non-reactors must be discarded (cf. Organon). Only the positive results can be scrutinized. If the previous investigators did not mention their "nils," there seems to me little lost. But there are plenty of proving records in homœopathic literature where provers with "no symptoms" are enumerated. Regarding the sulphur-provings of H. Schulz I cannot ascertain, whether there were no non-reactors (a very exceptional occurence it would be) or whether they were not mentioned. To jump to the conclusion that because the homœopathic publications of provings contain so many positive results, my argument against significance (not the correctness) of the negative results of others is untenable is a remarkable performance, to say the least of it.

If Martini indeed assumes that the preparations (globuli or trituration, cf., p. 264) are equally suitable provided they have the same controlled quantity of the original substance, he shows such a profound ignorance of homœopathic pharmaceutics that one must wonder at his boldness in entering into the problem at all.

Further, if Martini has indeed said "that the homœopaths find so many imagined symptoms in their experiments because their social position is at stake" then he has not only let the cat out of the bag, but such a *faux-pas* forbids any further dealings with this professor.

As to the preparation of Causticum (p. 266) de Jongh could have easily satisfied his curiosity by turning over a few pages in my textbook where he would find some additional exact data besides Hahnemann's explicit directions for the preparation. They must be very incompetent homœopaths who could not advise him on this subject.

The homœopathic pharmacopœias seem to be non-existent for de Jongh. Any of them, of whatever country, gives an account of Hepar sulphuris calcareum. Hahnemann himself is, as always, painstaking in the description of its preparation and again de Jongh needed only to look up my book. Moreover, it is surprising that he did not know it from his ordinary currriculum. The homœopaths anyhow did know what they were using. Before de Jongh enters into discussion about technical subjects of pharmaceutics he is strongly recommended to consult at least one of the official pharmacopoeias and perhaps also my paper read before the "Pharmaceutical Society of Great Britain" on "Homœopathy and its pharmaceutical aspects (British Pharmaceutical Journal, 1938) and my draft of a New British Homœopathic Pharmacopœia (British Homœopathic Journal, 1943/44). What de Jongh has to say about the technical side of homœopathy is incompetent talk.

The old saying that it is easier to criticize than to do better might well be taken to heart not only by de Jongh when he deals with homœopathic casuistic, but also by Donner (pp. 277-78) whose perfect case reports I still have to see.

What de Jongh calls the thesis of hypersensitiveness (pp. 297-303) is adopted in one or the other form by every homœopath as inference from experience; only, by those who have given the problem a little more thought, it is not conceived in the static way as is the case with de Jongh (though the same applies to those homœopathic theorists who use the terms allergy, idiosyncrasy in the common static interpretation). A special sensitiveness must be supposed to exist, in respect of a particular agent, in those provers who react to it instantaneously.

This is clearly a matter of degree, there is no such thing as a cut-and-dried normal line for anybody in relation to anything, except in the minds of the nomothetic (law-giving) dictators of nature. Even more conspicuous is the special sensitiveness to a particular agent of a disordered person who manifests symptoms a s i f they were provoked by this agent. That should be evident to everyone who thinks it over. Apart from that, the initial aggravation so frequently seen, after giving the simile, is direct empirical evidence. To this phenomenon de Jongh pays little attention, probably from lack of experience. From Hahnemann's time it was used as an indication that the given remedy was in all probability (more cannot be claimed in matters biological!) the simile. I have frequently pointed out the importance of this phenomenon for a correct assessment of results regarding the question *post hoc—propter hoc* (cf. de Jongh's precious characteristics of homœopaths, p. 313) but this point has conveniently been left out of discussion by de Jongh (and, I may add, by the scientific-critical homœopaths who, like de Jongh, in my primitive opinion, uncritically apply the scientific standards of 50 years ago). The proper place for debating the "initial aggravation" would have been where de Jongh oversimplifies my views, "another theory" (p. 307), as "defence-summation" (whereas it means intensification of the reactions by an appropriate stimulus such as the properly prepared simile). The special sensitiveness, which is neither a purely qualitative nor a purely quantitative issue, is an intrinsic part of my "other theory." Until de Jongh has given the problem a little more thought it appears superfluous to deal with the many inadequacies of his review in this chapter. Only as a matter of fact it should be noted that Rall did use the method of cutaneous tests (with Rhus toxicodendron) before

Saller (p. 302) talked about it. If de Jongh had noticed it, he would have missed another opportunity for a sharp rebuke, which with him, so often takes the place of "objective" consideration (cf. also p. 359, where de Jongh reverts to this point).

As to the relation of the "Reizkorpertherapie" (meaning unspecific stimulative therapy) to homœopathy (meaning a more specific stimulative therapy) (pp. 304-305) de Jongh has again missed my "Grundlagen" which, years before Bier, dealt with this subject comprehensively.

The same applies to the relation of homœopathy to active immunotherapy and isotherapy where de Jongh, with his water-tight compartments of therapeutic methods, is strangely at sea. Also the role of the catalytic mode of action for the explanation of remedy action (p. 306) has been discussed in the same "Grundlagen" so that its perusal would have given de Jongh a better basis for his discussion than he appears to possess.

The question of the application of the experimental method to animals or human beings (p. 309) is considered by de Jongh in his usual "aut-aut" attitude. It has apparently not yet dawned upon him that the purpose for which knowledge is sought has something to do with the kind of experimental method to be followed. In a certain sphere the animal experiment is doubtless suitable, in another it is inadequate. For the knowledge of actions on human beings the method of provings is superior, but that does not mean that for properly assessing its results the findings of animal experiments should be neglected. Wherever there is an opportunity to come to a mutual understanding, de Jongh is sure to miss it.

It is amusing to see de Jongh reprimand the homœopaths for indulging in scholastic argumentation (p. 313). I should have thought that in this competition his treatise would not fare too badly.

Apparently de Jongh has not benefited from "collegium logicum"—if he ever went there. At least he must have missed the lesson on "petitio principii" (p. 313). He cites me as an example of such fallacious thinking, but does not quote me and gives, instead, his own distorted version of my statement. Here it is: "Für die Homœopathie im allgemeinen bedeutet jede therapeutische Bestätigung von Angaben die durch Arzneiprüfung am Gesunden gewonnen wird, noch darüber hinaus, eine Bewährung ihrer methodischen Vorraussetzung, der Aehnlichkeitsregel." "In homœopathy in general, each therapeutic confirmation of data obtained by the method of provings on healthy persons, means in addition—i.e., to previously discussed ways of confirming the results of provings—a corroboration of the methodical plan, viz., the simile-principle." What does de Jongh make out of this statement? "Leeser wants to verify the dubious results of physiological pharmacology (i.e., provings) by therapeutic successes, and then asserts that the therapeutic verification implies as an additional advantage, a proof of the correctness of the simile-principle." Even if one concedes that de Jongh has not understood the sentence in its context, namely that "in addition" refers to other methods of corroborating the results of provings, where is the *petitio principii* in my statement? It says (1) in a particular instance the data obtained from a proving are established as more reliable by their successful application according to the s.s.c., and (2) each single successful application in accordance with a general plan, the s.s.c., which has as one of its premises reliable data from provings supports the value of the general plan. Thus the s.s.c. is not a necessary premise for establishing the reliability of proving results, but one possible supporting factor; each particular example of therapeutic success is one support more for the general method by which it has been achieved. If that is "petitio principii," the entire inductive method of corroborating a general deduction by repeated investigation is nothing but a fruitless "petitio principii," and science might as well stop altogether. The trouble is that de Jongh cannot discern between mathematical proof and corroboration by observation.

There is another pretty piece of de Jongh's reasoning on p. 315. He asks why so many homœopaths appeal to the lay-public, in order to find there the recognition which is denied to them from a competent (sic) quarter; that is according to de Jongh a sign of primitive thinking, because a really scientific research worker will pay little attention to the opinion the lay-public has regarding his hypothesis. *Voilà*! But assuming that these doctors just do not think their old-school colleagues competent enough to judge matters homœopathic, and that they assert that it is not a matter of hypotheses at all, but of a method of treatment which might interest the public as potential sufferers from disease? What impudence of these primitive doctors to do the same that is done in the press, on the radio, etc., etc., on a vastly greater scale in the name of the authentic, scientific and most up-to-date methods of therapy!

On p. 316, by the way, de Jongh distorts the title of Compton Burnett's booklet, "50 reasons for being a homœopath," into one meaning "50 reasons why I became a homœopath," only for getting another opportunity of sneering at the primitive homœopaths.

As to my primitive opinions on chemotherapy (p. 317) I have pleasure in inviting de Jongh to peruse my booklet, "Homœopathy and Chemotherapy" (at present in the press).

It contains some material to sharpen his wits and possibly his pen.

On the same page de Jongh takes E. Haehl to task for some fault he so readily condones in his beloved Martini.

I am glad that de Jongh (pp. 318-319) stresses that he did not use the term "primitive" in the cultural-historical (meaning ethnological) sense. Otherwise the combination with his assertion that homœopathy is a kind of religion and his compelling logic might have brought him to the conclusion: all homœopaths are sun-worshippers. I recommend de Jongh to read Kepler's expressions of religious ecstasy when he had found his cosmic formulæ. Is astronomy a kind of religion because of that ?

Yes, the homœopaths are " eccentric " (p. 321) in the sense that they are outside the centre of de Jongh's thinking.

The antithesis of nomothetic and idiographic methods (Windelband) in science adduced by Tischner for the difference in scientific outlook between academic and homœopathic therapy (pp. 322-323) is no longer a matter for *aut-aut* discussion in applied sciences since exact physics have become idiographic and history nomothetic to some extent. Science uses every available method of observing and reasoning in order to obtain knowledge from it. In an applied science, like Medicine is, such knowledge, by whatever procedure it has been gained, has to be put to the test and to be judged from the result. The antitheses of reasoning are overcome in synthetic action.

THE HOMŒOPATHIC DOCTRINE.

After having smashed to bits everything that has been developed in 150 years, of homœopathy, de Jongh sets (p. 337ff) out on his task as a critic of the doctrine itself. Had he still the feeling that he had left anything intact? But we have to read another hundred pages before the "prosecutor" deems fit to change over to the rôle of supreme judge and to proclaim the death sentence on homœopathy.

If anybody expects our critic to come down to brass tacks at last, he will soon become disillusioned. de Jongh sets out (p. 341) to inquire into reasons which would justify acceptance of the s.s.c. principle as a directive of therapy. Being obviously unable to think of such reasons for himself, his criticism has to turn against the reasons given in the course of time by others. Now this is exactly what he has so gloriously accomplished in the preceding 340 pages. Thus his new efforts are but a warming-up of his previous concoction. Thrift, thrift, Horatio! the funeral bak'd meats did coldly furnish forth the marriage tables. . . .

De Jongh perseveres in ignoring the overriding reason for accepting a method, viz., that it works; and that it works better within a certain range than other available devices. The only valid answer of a critic would be that the method does not work, and that this has been substantiated by trials. Failing this, de Jongh must scrutinise the theories given by those who have convinced themselves of the usefulness of the method. They may vary in their explanations of the healing process under the stimulus of the simile. That will depend on their individual gifts of reasoning, so unevenly distributed by Nature. Even had de Jongh refuted all the arguments adduced for understanding curative effects of the simile, the maxim s.s.c. could be excellent all the same. This, however, does not mean that all theories about the connection of disordered processes and their restoration with the stimulus chosen according to symptom-similarity are equally good. Far from it. The nearer a theory conforms to the actual events, the better guide it will be to a discriminate use of the method. For any theory has to account not only for the cases in which the method is applicable, but also for those where it is not. In other words, the function of theories is not to replace, but to conform to factual findings. Those who do not want to use a practical method, and thus do not give priority to fact-finding, will always find plenty of arguments against any theory; they will find no reasons for accepting the method, since to them any pertinent theory conforms to nothing.

This is a quandary in which de Jongh struggles like another Don Quixote; or rather he reminds one of a blind man who, in a dark room, seeks a black hat which is not there. The hat is a "law" of similarity which is either to be acknowledged as true, or to be rejected as untrue. In Nature we find only recurrent events in process between which qualitative relations of similarity can be established. "True" or "untrue" makes no sense in this connection, but "more or less similar" does. Nor has any method an absolute validity, but it can be better or worse, within a definite sphere, in comparison with another method.

de Jongh has a blind spot for anything substantial in homœopathy. Yet, by his assiduidity, he creates the impression of being very keen on finding something, and that it is certainly not his fault if, after all his efforts, he finds nothing. Of this he seems to have persuaded even Prof. de Jongh, if I interpret his foreword correctly.

Before de Jongh re-iterates his old objections against the theory of homœopaths, he produces another handsome piece of scholastic tactics (p. 341/2). Hear him: "The contents of the s.s.c. principle have not remained the same, not even for Hahnemann himself. In the beginning, he used doses of the ordinary range in his experiments as well as in therapy. Later he diminished the doses in both these fields, and finally used, for his experiments and at the bed-side, his 30th potency. From this it should be clear that to Hahnemann the s.s.c. principle completely changed its contents, because to him the potencies had a totally different action from that of the 'crude' substances. Curiously enough, he has never stated that he had to revise his simile principle at a certain stage because the first version was not good." Indeed, old Hahnemann was not clever enough to foresee such a cunning stratagem! All the same he was obstinate enough to state the corner-stone of homœopathy in exactly the same words throughout the six editions of the Organon from 1810 till 1842, viz.: "In order to cure gently, rapidly, surely and lastingly, choose, in every case of disease, a medicine which can of itself provoke a syndrome similar to that sought to be cured. *Similia similibus curentur.*" His previous formulation of the s.s.c. principle, viz., that of 1796, was: "Every active drug provokes in the human organism a kind of disorder of its own, a disorder the more characteristic, distinctive and outspoken, the more active the drug is. One should imitate Nature, which sometimes cures a chronic disorder by another supervening one, and should apply, in the disorder to be cured, that medicine which is capable of provoking another disorder, as similar as possible to the former one, which will then be seen to heal; *similia similibus*."

Again, from beginning to end there is no change in the directive which concerns only the quality of the remedial substance. Hahnemann had no need to change the principle, and every homœopath up to this date upholds it as a sound and useful directive. The fundamental principle of homœopathy is in no way affected by any changes which dosology has undergone, either as to the mode of preparation or the quantity of the drug. Hahnemann was not so muddled in his mind as some super-shrewd criticasters, he knew how to distinguish between primary and secondary issues. His formulation has been tampered with by Wolf and other "improvers," who introduced their half-baked theories on dosage into the pragmatic principle; they mixed it up with their unduly generalised notion of contrasting actions of large and small doses. This alleged embellishment is a relapse into one-sided quantitative thinking, an evil so common in our era that it could hardly fail to leave its mark on the history of homœopathic thought. Hahnemann's exemplary conception is perverted even more by stating it as a law of Nature that diseases can be cured by a small dose of the remedy which, in a large dose, provokes a similar disease. The humorous side of it is that this backsliding towards confusion was, and still is, considered by some as scientific, and as such appeals to de Jongh. But it is by no means true that no homœopaths have protested against this muddle. I, for one, have. Indeed it was the crux of my opposition to the "Arndt-Schulz law" and other quantitative formulæ. Many others have simply ignored the pseudo-scientific version and adhered to Hahnemann's clear directive. But even the self-styled scientific-critical homœopaths could do nothing else but follow Hahnemann's directive; before determining the dosage they had to decide upon the kind of remedy, and to that end Hahnemann's original version was all they needed.

This being so, it is a wild exaggeration, not to say, fabrication, on de Jongh's part if he alleges that there are two or even more kinds of homœopathy. And how does he come to this profound discovery? He ponders: "a simile-principle in which the curative and the experimental dosage are equal must be based on a totally different mechanism than the other one, i.e., that in which the experimental dosage is large and the curative small; and why? because the one group of homœopaths must interpret the action of the simile in a way different from the other group." This mechanism of de Jongh's can hardly be beaten; under the impact of this blow nothing remains to us but to bow before de Jongh's conclusion of this "simple analysis of historical data," viz., that such a simile-principle can inspire little confidence. Still, he bravely tries to regain some of his shattered confidence by examining the theories of post-Hahnemannian homœopaths. It has escaped him that he has left the simile-principle exactly where it was before, by his "tale full of sound and fury, signifying nothing."

In dealing with the explanation of homœopathic drug action as stimulation of reactivity (p. 343) de Jongh reveals such a confused conception of stimuli, symptoms, disordered processes and disease that one had better give him a chance to think it all over again in private.

While I agree with the conclusions (p. 356) on the Arndt-Schulz rule, I have some objections to raise on details of de Jongh's arguments which sometimes are no better "contra" than those given "pro." I can assure him that my exposition of the conditions determining the optimal dosage of an active substance was never intended to give a solution of the Arndt-Schulz rule (p. 353) and that I have never tried to convince anybody of the correctness of this rule, simply because I am convinced that it is not tenable.

The "Wirkungstypenhypothese," quite irrelevant to homœopathy, likewise needs no further mention. Enough has been said of it.

In debating what he calls "hypersensitiveness-theory" (pp. 358-9) de Jongh makes things not better but worse by repeating his previous mental acrobatics. He confuses the simple facts that there are degrees of sensitiveness to particular stimuli in healthy as in diseased persons, with theories on allergy, idiosyncracy and what not. Others have done the same, but that is no excuse.

It is hardly necessary to repeat that I have asserted nothing so foolish as de Jongh imputes to me on p. 363. Where have I said that the simile-therapy works by bringing defence-symptoms (sic) together in the organism? I have dealt, in that connection, with the intensification of defensive processes in the organism, and that very likely such intensification is desirable in many instances; hence—sometimes after an initial aggravation of the manifestations of those processes (symptoms!)—the efficacy of a stimulus properly tuned in to the disordered person. That is something very different, it contains a minimum of theory or hypothesis and the facts cannot well be reasoned away, even by de Jongh.

The practical simile-principle can well do without the support of de Jongh's "objective proof" (p. 364); it is no "speculative construction," but de Jongh's conception of it is just that.

Lest he might still have left something standing amongst the ruins of homœopathy, de Jongh proceeds to his critique of homœopathic pharmacology. According to de Jongh (p. 365) homœopathic remedies are, in general, substances to which official Medicine attaches no therapeutic significance, which are at present not used in therapy, and of which no medicinal effects are known to the official pharmacologist. It is a sign of de Jongh's confusion to speak of "homœopathic remedies" *per se.* He must mean either remedies which are chosen according to the principle of symptom-similarity in definite cases or remedies which have been investigated as to their potential actions on healthy persons, or else preparations made according to the technique peculiar to homœopathy. In any other respect his attribute "homœopathic" makes no sense. de Jongh then warns his readers that they should not infer that the "homœopathic medicines" are something peculiar; this is frequently not so, according to de Jongh. For example, they use Avena sativa, oats (the entire flowering plant! O. L.), Calcarea carbonica, ground oyster-shell (no, the Calcium carbonate of the inner layer of oyster shells, O. L.) and Apis mellifica, finely crushed bees (no, living irritated bees put into alcohol which takes up their "poison" O. L.). Thus he says the specially homœopathic medicines are almost all substances for which no grounds exist, derived from official Medicine, for their medicinal virtues. That is true in respect of many substances, but is certainly not the fault of homœopathy. These substances have been tested by experiments neglected, alas, by official pharmacology and, furthermore, they have been prepared by a peculiar technique also neglected by old-school-pharmaceutics. Thus homœopathy has indeed acquired a great additional *thesaurum medicamentorum* for which narrow pharmacology has no use. Besides these substances, homœopathy has at its disposal all the official medicinal substances (Arsenic, Jodum, Ferrum, Digitalis, Strychnine and hundreds more), and of these it has not only the limited knowledge from laboratory animal-experiments, from poisonings and *ab usu in morbis,* but in addition the more detailed results of the experiments on healthy persons. Which of the two schools is better off for meeting the immense variety of situations with which the physician is confronted?

The full significance of the difference between academic pharmacology and homœopathic *materia medica* manifests itself only by contrast; the one stresses isolated and scattered facts as they emerge from laboratory research, the other always seeks a full and coherent picture of drugs in action, and collects and integrates all available knowledge with this end in view. The misery and boredom of official pharmacology derives from its precarious suspension between physiology and therapeutics. On the one hand, it emphasises the changes of part-structures of the organism without due consideration of the whole organism. On the other hand, it simply states the principal uses to which these drugs happen to be put in contemporary therapy, e.g. that they are demulcents, irritants, astringents, cathartics, expectorants, etc. Such singled-out properties, which, moreover constantly overlap, hinder the appraisal, on its own merits, of the drug as an entity seen in interaction with the totality of living processes. This is just what is required, and to a great extent achieved, in homœopathic *materia medica.* Here it is a secondary consideration whether the pertinent knowledge is derived from toxicology, physiology, or any other branch of science, whether it is obtained from experiments on animals or healthy persons. But the results of the latter experiments, viz., provings, serve by their nature as fitting links so as to bring order and conformity into the assemblage of data. The whole combination of potential actions, seen in their proper place, makes for a living picture of the drug. In this light, the medicinal substances no longer appear under a restricted, utilitarian aspect, but as exponents of specific activities. The new attitude which implies continuously drawing parallels between medicinal agents and reacting persons, both seen as much as possible as wholes, put the teaching of *materia medica* on a firm footing, in the line of steady progress. Anybody in the position to compare the two ways of studying this branch of science, will soon discover where the greater satisfaction and interest lie.

Once more (p. 366) de Jongh repeats his old objections regarding the reliability of the data of the homœopathic *Materia Medica*. It is not true that many provings have been made with high potencies; those which have can easily be neglected. The overwhelming number has been made with substantial doses of appropriate preparations. The possibility of "suggestion" may have not been sufficiently reckoned with in all these provings, but in many it has, and in others a wise discrimination and an elimination of the dubious data is just the task for evaluating the reliable picture of actions of any particular drug. It would be sheer folly to do away with the useful along with the doubtful data. A decision on the question which data may be incidental and which due to the tested agent, may be safely left to those, who can avail themselves of the full facts and have the necessary training for discriminating; precocious generalisations of sweeping negativity have no value whatever. Where the number of provers was too small, further work, not easy talk, is required. The same applies to those medicines of which provings do not exist and which are used on merely clinical indications. There the homœopath finds himself in the unenviable position in which de Jongh will find himself all too often in his every-day practice. Certainly a homœopath does not feel happy if he has to resort to makeshifts of clinical indications *faute de mieux*. But when it comes to homœopathic *materia medica* as it is and on which he cannot and will not rely, de Jongh reveals himself as an all-out purist while arguing thus deftly: "Some symptoms have come into the 'pure' (i.e., derived from provings) picture of drug actions merely because they were seen to disappear after application of that particular drug to patients, thus as clinical symptoms. If the introduction of these clinical symptoms were justified, the validity of the s.s.c. would have to be assumed (N.B.—de Jongh can always think of it only as a kind of law, O. L.). Therefore the mixing of clinical and pure symptoms is not justified" or as de Jongh so nicely says: "We can therefore not acknowledge the reality (sic) of these symptoms." This has according to de Jongh serious consequences for our confidence in the *materia medico* generally because such clinical symptoms cannot be discerned from others. "Therefore one cannot know of any particular symptom given in the *materia medica*, whether it is reliable." Certainly a clever device to avoid the study of such a poor *materia medica*. If de Jongh had taken the trouble of looking up the recorded provings of Hahnemann, of the Austrian group, of Hale and of many others he would have noticed that clinical symptoms are well distinguished (mostly by bracketing) in order to denote them as tentative in contrast to pure ones. Discrimination is, however, not to de Jongh's liking, so he prefers to dismiss the findings in a lump as unreliable. He cannot believe (who asks him to do so?) in such results of the homœopathic pharmacology where they are in flagrant contradiction to the general pharmacological experience. Are they? How does he know? His pharmacology has not even considered the results! We, on the other hand, find no difficulty in reconciling the data of homœopathic *materia medica* with the established facts of toxicology and of laboratory pharmacology however crude they may be. de Jongh has not troubled to study even a few of the monographs in my "Mineral remedies" which would have enabled him to speak *in concreto* instead of talking *in abstracto*.

Of the unilateral and sex-related preferences of some substances de Jongh repeats his previous criticisms (p. 367) which I have already put into proper perspective. Even if some assertions have been insufficiently substantiated so far, is that a reason to dismiss the whole issue so lightly and to conclude that the *materia medica* is unreliable because of that? If Secale cornutum has its main sphere of action on the female organs according to official pharmacology, why should Pulsatilla and Sepia not show in their actions a preference for women? But, *quod licet Jovi non licet bovi*! Specifically male or female remedies there are indeed none in homœopathy, but that Nux vomica more frequently fits men, and Ignatia women, is an everyday experience of any homœopath. And, of course, there are remedies with an affinity to the prostata (e.g., Sabal, Populus tremuloides, etc.), only de

Jongh does not know them. At last it dawns upon de Jongh that there might be some constitutional differences between male and female because their endocrine glands are not precisely equal. Meanwhile the subject has served its purpose of running down the homœopathic *materia medica*.

Why should homœopathic experimenters narrowly adhere to the sterile method of Martini who did a few tests, as he thought fit, with a minimum of understanding the issue involved? If a few inadequate negative tests could shake the homœopathic *materia medica*, built up in 150 years, nobody would be the poorer for giving it up.

de Jongh following Martini closely (pp. 369-347) seats himself in the armchair of the critic who must teach those gullible homœopaths how to avoid the traps of suggestibility in making and assessing their provings. It has already been pointed out how Martini, in order to avoid the Scylla of any uncertainty becomes stranded on the Charybdis of nihilism. Conductors of provings, long before Martini, were aware of the risk of suggestive symptoms. Hahnemann not only insisted on choosing reliable persons who had no other aim but to assist in fact-finding, but he also demanded that their reports should be scrutinised by interrogating the provers in respect of any vague symptom so as to make the records as accurate and reliable as possible. Even if some provers knew the name of the tested substance, they could not, at that stage, know the probable symptoms. Later re-provings have rightly made sure that the provers could not know what they were given and whether they were taking the test-substance or not, as far as the properties of the preparation permitted. If Martini indeed gave blank milk-sugar alternatingly with Bryonia-tincture he has himself violated the ordinary measures of precaution so grossly that he had better remain silent as a critic. However, a certain personal factor can never be excluded altogether from observations of any events, the factor of suggestibility can only be reduced to a minimum. There are limits to exactness obtainable by observation even in physics as we now know. If analysis reaches ultimate limits, a situation arises in which absolute exactness and certainty are seen to be impossible, because the very act of observing involves interference with the object of observation. In all matters biological the margin of uncertainty is obviously greater than in physics, it grows with the complexity of conditions of observation. In so far as "objective" pharmacology attains a higher degree of probability, greater predictibility and reproductibility, this "exactness" is bought at the cost of artificial restriction of the conditions of observation. Just because such restriction implies contraction of the field of observation and leads to an all too limited knowledge of the actual processes, homœopathy uses more comprehensive methods of observation of drug effects. Hence the search for supplementary knowledge by experiments on human beings, a knowledge particularly appropriate and fruitful in the treatment of diseased persons. The new experiments require other criteria than the animal experiments of the laboratory; what they miss in quantitative exactness they gain in qualitative accuracy. The compromise such as Schoeler attempts by stressing in his provings the "objective" symptoms and neglecting the "subjective" ones, slurs over the divergencies of method but is liable to miss essentials. Still, Schoeler's "objective" provings nevertheless do not satisfy the more "objective" critics; *on est toujours le réactionaire de quelqu'un.* Also in respect of provings, the "scientific-critical" homœopaths attempt a compromise with their orthodox brethren on too low a plain. Yet, there they find no mercy either. It is once more humorous to see de Jongh ask Schoeler, how he can know that this or that symptom is unessential. Martini is a good example of how the fear of suggestibility (quite apart from the desire to disprove!) can become a serious handicap in obtaining any useful knowledge of drug actions; suggestive expectation of positive results, on the other hand, may mar one proving, but that can be rectified by others. Negation of facts is the surest way of relieving oneself of further intellectual trouble in respect of them.

Next to the bogey of suggestibilty for discrediting the results of provings, de Jongh (p. 370) raises the objection that these tests can, by their nature, cover only a certain range of dosage. Quite true, but it is just this range which alone can reveal the reactions peculiar to man, a knowledge so particularly useful in treating human patients. For here lies the most direct approach to the manifestations of their disorder. Besides, nobody can prevent us from using, in their proper place, all the toxicological, animal-pharmacological and other relevant data available. A homœopath's knowledge of drug actions is never comprehensive enough, so he tries to complement it from all available sources. He finds, however, the old-school pharmacologist's knowledge most certainly insufficient in a very wide field of therapy, because of its entire neglect of a very important source of useful knowledge. Thus in its morbid aversion to quantitative inexactitude the old school does not even consider the practical advantages of wholesome qualitative accuracy in fact-finding.

Lastly, de Jongh protests that the results of provings do not allow of an exact analysis. As, of course, any actual manifestation of processes is indivisible, this argument can mean only quantitative analysis of the processes behind those manifestations. This is indeed restricted in experiments on men in so far as it is not feasible to take them to bits. Yet, even the electrocardiograph-bloodpressure- and pulse-curves, the sedimentation-rate, blood-count and all the other paraphernalia for establishing fixed values in advanced disorders are of precious little help for ordering the multifarious, variegated manifestations of a person's reactions to a particular stimulus. That does not mean disapproval of them, where they are applicable, but of the exaggerated stress laid upon them. If, in their favour, the simple data expressed in symptoms are neglected, as in Schoeler's provings, the value of this experimental method is severely restricted. Extremist nihilism, such as de Jongh's and Martini's, however, shows complete lack of grasping the import of the method altogether. Bickering about the advantages and disadvantages of animal-experiments versus man-experiments remains nothing but a piece of cheap entertainment, as long as the precise spheres and ends of each method are not properly discerned. Finally, de Jongh's critique boils down to his statement that he "believes" more in the results of laboratory-pharmacology than in the results of provings on man. Nobody will be so rude as to dispute and try to shake his belief. We are concerned here only with correct and useful knowledge.

Once more de Jongh then indulges in his acrobatics with "subjective-objective" abstracts (p.

371/372). I have not yet found out whether "cough," for instance, is to him a subjective or an objective symptom. If he had discriminated between manifestations of structural and functional disorders or between physical and psychic symptoms, one could at least debate with him, though still on a superficial plain. As it is, primitive thinkers cannot aspire to fathom the depths of his disciplined objective thinking. The unpalatable asset of homœopathy, viz., that it has the means of adapting medicinal treatment to the individual person, and not to an abstract disease only, is cleverly brushed aside by d e J o n g h by his evasion of the issue. (pp. 372/3.) The fact that homœopathy in its *materia medica* has accumulated additional data (however unsatisfactory to an " objectivist ") cannot well be denied; nor can the fact that homœopathy is able, by its comparative method, to make use of a greater range of data supplied by the individual patient. With the best will to individualisation, a therapy on disease-diagnosis must lag behind a therapy based on diagnosis of a diseased individual person. Against this simple state of affairs, d e J o n g h has nothing to bring to the fore but his old objections to the reliability of the data obtained by provings. How can one know for certain that these symptoms which sometimes appear only in one instance, are caused by the given substance and how can one know for which individual patient this substance can have significance? asks d e J o n g h. As to the therapeutic significance, d e J o n g h feigns complete innocence as regards knowledge of the homœopathic method. The significance of a certain symptom of a drug becomes, of course, obvious when the individual patient manifests just that symptom. As regards the certainty (we modestly speak of high probability only) of the causal relation of the symptom of a proving to the drug given, de Jongh then, for his convenience, separates two of my criteria: that a symptom strikes the prover as unusual is the first indication that the symptom is probably connected with the test substance taken. de Jongh thinks that I apparently attach little value to this "queer" reasoning, because I ask for further corroboration by repetition. My second criterion, the repetition of the experiment for the purpose of confirming symptoms is, however, according to de Jongh, inconsistent on my part because it is an application of the statistical method which before I had rejected. My assertion that a proving result becomes all the more reliable the better it fits in with all the other data on the actions of that particular substance is according to Martini no "positive proof" of the causal relation. Thus, says de Jongh, "homœopathic provings can at best confirm the experiences of official Medicine; if they wish to go beyond that, they have to give proofs which they cannot supply!" *Quousque tandem,* de Jongh, *abutere patientia nostra?*

The clever device "divide et impera" may, however, need some illustration. The first steps to establish a measure of probability for the causal connection (viz., unusual, striking manifestations during the proving) are according to de Jongh, invalidated because further confirmation is sought. This is, however, done by statistical procedure which has already been denounced. And the criterion of congruity with data from other sources is no positive proof, it can only establish that there is nothing against the assumption of causal connection. So far de Jongh. If he calls confirmation by a second or third or even tenth experiment "statistics," then I have nothing against its application; on the contrary. The sort of statistics which I deem not applicable to provings, searching for differentiated qualities, is the procedure of finding averages by the "great number" gauge, because it leads to assertions on frequencies of events only and not to precision and accuracy of description. It is the sort of statistics which is so often used as a drunken man uses a lamp post, more for support than for illumination. If corroboration by repeated tests be also called statistics, then I beg permission to discriminate between the two sorts and to claim the repetition of experiments as a very valuable "statistics" for our particular purpose. As I could not foresee that somebody would be unable to discern between the two ways of fact-finding, I innocently used "statistical method" in the ordinary sense.

The unsophisticated reader may deem it not quite so stupid that, by evidence combined from various sources, it is possible to obtain a reasonably high probability (not a mathematical certainty or proof!) that particular symptoms in the provings are due to the tested substance. With that degree of probability the poor homœopaths must content themselves, until de Jongh and Martini have found positive and negative certainties in biological processes (death excluded!).

de Jongh's sole witness for the prosecution, Martini, must once more appear on the scene (pp. 373/4) with his three negative provings of Bryonia, Sepia, and Sulphur, already scrutinised as far as de Jongh's report permitted. Here is, according to de Jongh, proof beyond doubt that the drug pictures of the homœopaths are wrong, and as they concern medicines frequently used in homœopathy, the whole homœopathic *materia medica* has to be viewed with the utmost distrust, nay it is completely unreliable. The conclusiveness of de Jongh's argumentation, surely is devastating. But to make quite sure, de Jongh still must, *en passant*, reveal his complete ignorance of the homœopathic pharmacopoeias by asserting, of course without giving a single example, that from the name under which a remedy goes in homœpoathy one cannot infer its composition and source and that, indeed, in homœopathy different substances appear under the same name. Who told him that? Though nomenclature in the homœopathic pharmacopoeias and some text-books of *materia medica* is by no means perfect, there is nothing like the confusion which sometimes exists in the nomenclature of official pharmacology, e.g., in respect of active principles (digitalin, phytolaccin, etc.).

Not only is the homœopathic *materia medica* unreliable, it is also unusable for its therapeutic purpose, according to de Jongh (pp. 374-376). Here he must be credited with one correct point, when he remarks that the homœopathic *materia medica* is unusable for treatment according to "modern clinical conceptions" as he calls it (I should say: "on insufficient disease-diagnosis"). Surely, that is not the fault of the homœopathic *materia medica* which has been elaborated for the very reason of improving on the old disease-diagnosis treatment and of finding the remedy which fits the individual patient. As to the disease-diagnostic part, the homœopath is in the same position as his orthodox colleague if not in a better one. He does well to come to an exact disease-diagnosis by all up-to-date clinical and laboratory auxiliaries, if not for treatment's sake then for comparing notes with his orthodox brethren. In fact this has been done scrupulously in homœopathic hospitals such as the Stuttgart Homœopathic Hospital. The homœopathic physician who has not all the technical facilities at his disposal is, even as far as disease-diagnosis is concerned, in a better position than the ordinary practitioner, because as a homœopath he has learned to pay greater attention to the detailed direct observations of the patient himself, both in respect of status præsens and of anamnesis. But it is quite a different proposition to relinquish many or all details of the drug pictures in favour of the bare bones of "exact" diagnostic signs. It may render the picture of drug actions more comparable with the data used for clinical diagnosis; but this is just what appears undesirable, because it diminishes the chances of homœopathic drug application. As an addendum the use of the diagnostic apparatus in provings is reasonable, though overvalued by those clamouring for a "new" homœopathic *materia medica.* Any attempt at replacing the accurate details of direct observation by exact diagnostic, typified signs of indirect observation is, however, a fallacy; it shows lack of understanding of the essentials of the homœopathic method; de Jongh calls it a serious inconsistency; it might be called a retrogression even, but we will not worry about trifles in wording; in principle I agree with him here.

When, however, de Jongh asserts that the homœopathic *materia medica* is unsuitable for selecting the fitting remedy in a particular case, he moves on quite different, in fact, on very weak grounds. His only support for such a sweeping statement is his own experience. Throughout his book there is no evidence that he has made even a modest attempt at studying the drug pictures (the opportunity offered by my "Mineral remedies" was apparently missed completely by him). Certainly he could not have made a thorough study of it, for that would have taken years. Thus he cannot be expected to have overcome the bewilderment of the beginner when he sees himself up against endless rows of recorded symptoms. He has not yet learned to discriminate between those that are essential and those that are not. In this confused state the selection of the simillimum by a homœopathic doctor must, of course, impress him as arbitrary. Has he devoted enough time—at least a few months are required—to learning how to overcome arbitrariness in choosing the remedy? If so, his teacher may have to share the blame for the fact that the pupil can make such a statement. For a few inadequate attempts at choosing the simile the pupil himself is responsible. Not too modest, seeing how he boasts of being a perfectly competent judge in this matter after all the trouble he has taken on behalf (sic) of homœopathy!

His performance in this book as critic of homœopathy speaks for itself. Will he match his "competence" with that of those who for 30 years or more have devoted themselves to the study of homœopathic *materia medica* and who are fully satisfied with its usefulness for finding the individually fitting remedy in the majority of their cases? If de Jongh cannot afford to spend more thought and work on homœopathic problems, he would be well advised to show a little more modesty. Is it not strange that none of my many assistants who mostly came from university clinics and great modern hospitals, has relinquished homœopathy and returned to the old school practice? Everyone has convinced himself that he could do better therapeutic work by using the homœopathic *materia medica*. Were they all fools or only primitive thinkers like myself? One can well acquiesce in the fact that, since de Jongh believes the homœopathic *materia medica* to be unreliable and unusable, he will not rely upon and use it. That, of course, is his own affair.

In his critique of the homœopathic posology (p. 367 ff.) de Jongh proceeds by erroneously (but conveniently for his purpose) separating the purely quantitative aspect from all the other factors upon which the dosage depends: such as the quality and the physico-chemical state of activity of a particular substance (he cannot apparently discriminate between the two); further the immense actual variations of conditions regarding the recipient persons for whom the *dosis optima* has to be determined. Lustily he goes to great length in repeating the old story of the limitation of the divisibility of matter as expressed by Loschmidt's constant. A single statement to this end copied from my "Grundlagen" would have been sufficient. We all agree that in the region of D.21 – D.23 (21x – 23x) (dependent on the molecular-weight of the particular substance) we come to the calculable limit where molecules of the original substance can still be expected; we may here leave any technical finesse out of consideration. It follows then that the homœopath who insists on the presence of molecules in the preparation of his remedy has at his disposal a range from the mother substance up to, say, the D.20 (20x). (Why be niggling?). If de Jongh works within this range he is bolder than many homœopaths, as he is well aware. All his further criticisms are then superfluous, as they do not concern the issue of homœopathy but only those homœopaths who are not content with that range of possible dosage.

This would, however, be too simple an attitude for a scientific thinker. Thus de Jongh must construct *in abstracto* a quantitative criterion independent of all possible cases. Of course, he finds none in homœopathy, because nobody can dictate a *dosis optima* for all the substances nor for all the patients. He can at best find certain general indications for the choice of dosage which must necessarily give a wide margin (like general statements on any action-unit-doses in orthodox therapy, e.g., of some vitamins, of insulin and other hormone preparations. But even of these general lines for adjusting the dosage de Jongh has not taken sufficient notice (the "Grundlagen" might have been helpful to him here once more). Failing to detect any cut and dried statement of dosage for all substances and for all cases, de Jongh concludes: The homœopaths obviously cannot give a proper basis to their dosology! Their experience from provings and therapy counts, of course, for nothing. How else does the orthodox man arrive at a basis for his dosology but by experiment and therapeutic experience?

(De Jongh does not know the designations of the potencies adopted in the pharmacopœias of the various countries, hence his doubt as to what scale is indicated by the established signs (p. 379). But it is useless to point out all his minor shortcomings as there are major ones in abundance.)

I agree with de Jongh that those homœopaths who set a definite limit to the range of protencies, be it D.6 or D.10, do so arbitrarily. They forget that the choice of dosage is ultimately a matter of experience; an overriding principle which de Jongh, too, conveniently ignores. Certainly, there are great divergencies amongst homœopaths as regards their preferences in dosage range. All the talk about it leads to nothing, only from comparison of accurate experience by honest workers can a greater uniformity emerge in the course of time.

Meanwhile, homœopathy may well be content with accepting de Jongh's statement "that it does not stand on a firm quantitative basis." Fortunately "quantity" is in homœopathy not such an overruling and narrow criterion as in official Medicine. The homœopath has learned that the quantity of a substance required for a particular action is often a very poor measure, that its significance depends on many other conditions and criteria. Homœopathy need not envy any results reached by one-sided quantitative considerations.

Analogies between extremely small doses used in official Medicine and those frequently used in homœopathy (p. 380) can show that "the gap between the two schools as regards dosage is not unbridgeable" (Heubner). Through merely using minute doses nobody becomes a homœopath. Nor does a homœopath stop to act as such, if in suitable cases he applies quite substantial doses. I doubt, whether any homœopath has been stupid enough to assert that all substances are pharmacologically active in minute doses, because some are, and further that he did so without taking into account the action intended. But that is just the attitude de Jongh has taken up against the homœopathic dosology, when he takes the primitive homœopaths to task for not having a firm quantitative basis.

In his argumentation on what he calls "dynamisation-hypothesis' (meaning the physical changes brought about by the procedure of potentisation) and on high potencies (pp. 380-387) de Jongh reveals such a confusion of thought and lack of elementary knowledge of physics that an apology to the reader is due if one enters into discussion with him. First he denies the fact of "dynamisation" altogether so that any attempts to explain it by known facts of physics become merely "speculative." The word "speculative" means to him, as we have already seen, any thinking about the observations of others which he does not acknowledge. "Dynamisation" (let us understand it as preparation of a substance so as to bring it into another state which facilitates other activities), is apparently to de Jongh a secret procedure of homœopathy. Has he ever pondered on the fact that the same quantity of mercury can have very different actions according to whether it is applied in bulk or as *unguentum cinereum*? And what about all the facts of colloid physico-chemistry? And of electrolysis? In brief, he should first clear his mind from the phobia that this "dynamisation" is a peculiar homœopathic affair. Then he would do well to revise his conceptions of "energy," "matter," "mass" and "quanta" in the light of modern physics. As it stands (pp. 381/2) his talk about "this form of mass" which is not indefinitely divisible and of free energy-quanta which, according to him, arise only from splitting of atom nuclei, makes no sense.

I can to some extent agree with de Jongh that vague concepts like "dynamic" have little or no explanatory value. Nor do I attach any value to hypothetical action-curves not derived from observation, unless it be that they serve didactic purposes to represent tentative conclusions of one's own experience. It is quite another thing, however, to investigate how far the new potential actions of a substance, seen after preparation by potentising, accord with known physical facts.

de Jongh cannot deny that our observations on potencies (within the molecular limits, of course) are in excellent agreement with the facts of physics. If he does, he must first refute all those supporting facts known about colloids, electrolysis and thermodynamics which have been produced (cf. "Grundlagen" and my supplementary Congress 1934 remarks). For the theory of potentisation, of bringing substances into a state of higher remedial activity, we do not need the introduction of any hypothetic "forces" (dynamis). We have sufficient support from the knowledge of physics on action and energy.

The position is different when it comes to the problem of high potencies, i.e., potentisation beyond the D.21 – D.23 (21x – 23x). This problem concerns only those who acknowledge the relevant fact, namely, that effects of such preparations have been observed. Those who deny such observations, without trying to enquire into the facts for themselves, are welcome to declare as speculation any reasoned investigation of what physics has to say about it. That will not hinder us from thinking about it. Whatever these thoughts may be, they concern only the question of high potency-action and not homœopathy as such. Further, any hypothesis on this point affects the factual issue only in the same manner as any other hypothesis on the mode of drug action affects such action, e.g., any hypothesis on the action of quinine in malaria. The difference that the high potency-action has not been generally acknowledged while, e.g., the quinine-malaria action has, does not make the hypothesis on high-potency-action merely speculative and those on quinine action scientific. No hypothesis can create or replace facts. I have never tried to establish high-potency-action as a fact by hypotheses, let alone by speculation.

The only hypothesis relating to high potencies which I have advanced (long before my Congress communication, cf. "Homœopathie und Biochemie," Reclam Univ. Bibl. pp. 48/49) is that of induction of medicinal energy into the vehicle by radiation in the course of drug potentisation. I have always pointed out that, while we have evidence from physics (1) that radiation can be produced by mechanical procedures such as trituration (e.g. triboluminescence) and (2) that alterations of organic substances by radiation, so that the ray-effects are preserved and can be transmitted by the recipient, are well known (e.g., radiated ergosterol), we have no evidence so far for the transmission of a complex rhythm of radiation specific for a substance. At this point we have no analogy with known facts of physics to go by. This was and is my opinion on the problem, but I did not claim it to be an explanation of high-potency-action. I could not do so, because I realised that an important link of a possible explanation was still missing. Whether it will be found I cannot foretell and I have not speculated upon it. My fantasies are not so volatile as de Jongh's thoughts are confused. He has not yet realised the difference between the theory of the potentisation-technique in general (dynamisation as he calls it) and a particular hypothesis concerning the high-potency-question (what he calls "contamination-hypothesis," instead of saying more precisely "radiation-induction hypothesis"). Then he argues that, if the particular hypothesis regarding the high potencies appears unsound (that it is auxiliary and insufficient for a full physical interpretation I have pointed out myself!), the theory of potentisation is unsound as well.

From the questions he fires at me it is obvious that he has grasped the contents neither of the one nor of the other thought. de Jongh questions me in the manner of a nasty school-master who tries to bully a poor pupil by shouting: "answer, if you can, but you can't anyhow." Well, these are the questions which, he says, I have not answered, but which he is fully entitled to ask, nay, must request me to answer; which questions, however, according to de Jongh, must remain unanswered; and from this fact (!) he concludes that I have allowed myself to be driven away on the wings of fantasy. By so much rhetoric and logic I am naturally quite overwhelmed, still I venture very meekly to add a tentative answer to each question:—

(1) How do I know that, by potentising, a certain "fall of potential" is increased? By comparing, e.g., under the microscope various potencies of a substance and finding that from the first to the second, from the second to the third degree (no matter whether decimal or centesimal) of potentisation, the substance becomes increasingly subdivided, its particles more and more regularly distributed within the medium and the relative distances of the particles increased. Considering the particles as bearers of potential action (say "charges" in respect of colloids and electrolytes) I know from elementary physics that increased surface, greater distance enforced by an indifferent medium (say, protective medium for colloids, dielectric for electrolytes) and greater regularity in their arrangement—all tend to elevate the level of potential energy of this substance: which potential becomes manifest in action and can be measured as energy when conditions arise for equalising such a difference of potential.

(2) What does this "fall of potential" mean? As any textbook of physics will state: the difference between a higher and a lower level of energy. The

retention of an elevated energy level in the case of a " potency " means stored energy.

(3) Does this refer to a certain mutual attitude between the medicine molecules? " Mutual attitude " is too obscure. Possible reactions between molecules of the medicinal substance themselves do not concern the matter under discussion.

(4) Or, to parts of molecules in respect of each other? The same applies. The *level* of energy of any particles, whether molecules or ions depends upon the degree of their subdivision. I trust that, in the field of electrolytic dissociation, even my inquisitive schoolmaster has plenty of examples at hand and has heard even of such experimentally proven " laws " like O s t w a l d ' s: the degree of dissociation of a weak electrolyte is approximately proportional to the square root of the dilution. That implies that here with increased dilution the dissociation and hence the potential ionic activity, e.g., conductivity, increases (cf. " Grundlagen ").

(5) Or, perhaps to the molecules of the medicine in comparison with the molecules of the medium? So far as conditions of interaction between them through equalisation of such a difference should arise; a possibility which has tentatively been considered, in respect of potentisation, by the radiation-induction hypothesis. The induction of a higher potential (of motion, heat, electric charge, etc.), to an " indifferent " (meaning low-level) medium is a very common occurrence, but whether radiation is actually transmitted to the medium in a trituration I do not know.

(6) What is a " purer " emission of energy-quanta? This is apparently a mistaken report on what I have said about *effects* of radiation (emission of energy quanta). I submitted that these effects become " purer," i.e., more distinctive of the peculiar substance, the freer, more discreet, the emitting particles are; the less interference by other particles, the greater the chance that a peculiar rhythm of radiation is discriminated by a sensitive system. This concerns the specifity, not the intensity of action on such a system.

(7) How do I know that this is brought about by the increase in " fall of potential " as recorded? The separation of emitting particles (I use this expression for the dual models, particles and waves), has been shown to constitute an elevation of the level of potential energy. The actual effect depends on further conditions. To avoid misunderstanding of technical terms, d e J o n g h may better realise what is meant from a simple experiment: anhydrous, cupric chloride is a yellow-brown substance; add a few drops of water to some of it and the colour of the solution becomes dirty green; when more water is added the colour becomes a bright green and finally, in a very dilute solution, it changes to blue. Now the copper-ion has—given the conditions of dispersed light and of a sensitive optical apparatus as man's—the specific potential action " blue " (the scientific thinker may say: it " is " blue, but a primitive thinker dare not). Only when the copper ions are very widely separated, i.e., in a very dilute solution, does their specific colour come out clearly and purely. In more concentrated solution the " blue " was interfered with by the yellowish-brown of less dissociated copper.

Similar examples could be multiplied a hundredfold and the colorimetric method of estimating the dispersion of colloids is a very common application of the same sort of experience. By the way, such colloidal dispersion has been achieved by simple mechanical trituration even without a protective vehicle.

If all this is still too complicated for de Jongh, I recommend that, at the proper season, he puts his nose straight into a big bunch of, say, carnations, and later holds one carnation only at due distance from his nose. Probably he will then understand better what I meant by " purer effects " and by actions more specific of the particular substance.

(8) Has the action of medicines any connection with emission of energy by these substances? I cannot see whether that question is of the sort which cannot be answered by ten savants or whether it entails only a platitude; so I say, yes.

(9) If so, what is then the effect in these (?) from the " becoming purer " of these emissions? So far as the question makes any sense to me, I repeat: the effect of the medicine will be more specific for that particular substance because there is less interference.

(10) How do I arrive at the revolutionary opinion that it is possible and that with such simple means as the homœopath employs, to make atoms emit quanta which are not specific for their structure? Probably the confused question refers to radiation elicited by triturating a substance. If so, this is not a revolutionary opinion of mine, but a fact observed by others and described as triboluminescence.

What I have said is, that so far we have no knowledge of such radiation being specific for a complex molecular structure and that such a complex specific rhythm of radiation is difficult even to imagine. The difficulty seems to me, however, not unsurmountable, Differences in frequencies of radiation (wave-lengths) are an index of specifity though on a much restricted level. On the side of the recipient we know certain specific qualities of action to be conditioned by distinct ranges of wave-lengths (vigantol, colours, etc.).

Though my answers are surely unsatisfactory to de Jongh, they may at least cast some doubt on the "fact" that his questions must remain unanswered.

de Jongh errs (p. 384) in assuming that all remedies in homœopathy need preparation by potentising. Many mother-tinctures are daily used on homœopathic lines. He should, however, be credited for beginning to doubt, whether in certain instances trituration might not be a sensible procedure.

Regarding the traces of elements (impurities) possibly of the same kind as the potentised substance which may occur in sugar of milk or alcohol (p. 384/5), de Jongh's arguments would be compelling from a solely quantitative point of view. Every fresh addition of vehicle might in such a case bring quantitatively more of the substance into the preparation than has been left over from the original one. But when he differentiates between "mass" and "active" mass" and realises that potentisation means increased activation, though diminished mass, things begin to look different. The *lapsus linguæ* of an unnamed author who spoke of an "intentionally added" substance instead of one "potentised from the start" may then appear in a milder light to de Jongh.

The duration of the effect of a single dosage of the simile (p. 385) is still viewed by many homœopaths very differently from what is common in school therapeutics. The homœopath sees the effects of such a stimulus as a sequence of the reactions of the organism and so long as he observes the processes going in the desired direction, towards improvement, he may rightly hesitate to interfere with repeated stimuli. He has to rely on observation and can generalise only tentatively regarding the repetition of a dose. Equally, if he does not see, in due course, any improvement in the condition of his patient, he is not only entitled, but obliged to reconsider the case and try to determine another better-fitting remedy. All these considerations are clearly a matter of observation and have nothing to do with belief, nor with the *unitas remedii.* The methodical principle of *unitas remedii* (pp. 386/7) is violated only if a mixture of substances, which as such has not been subjected to provings, is given, because then no basic knowledge is available for selecting this mixture as simile. The choice of a second proved remedy at a later stage under different conditions (we have to deal with persons in process not with static diseases!), is quite another story, outside the very reasonable postulate to give only one proven remedy at a time and to observe the reactions before interfering with another one. And I can assure de Jongh that this course can be followed quite easily. The homœopath is troubled less than his orthodox brethren by reflections about possible interference between various part substances which might differ in their mode of action; for he relies on manifestations observed from the entire drug and not on the theories about the modes of action inferred from laboratory experiments.

The conception of remedy action as "drainage" or "canalisation" (pp. 337-8) appears to me of little practical value, and as a superfluous generalisation of a very limited range of the organism's defence processes. If it is made the rationale for giving several remedies at a time, I fully agree with de Jongh that it is contrary to homœopathy. So is, *a fortiori*, the so-called complex-homœopathy.

In his following criticism of the homœopathic conception of diseases, de Jongh (p. 388) persists in his fundamental error that homœopathy originally was a hypothesis on the nature of disease. It is and has since 1796 been a practical method of treating diseased persons with medicinal substances.

In contrasting allopathic diagnosis of disease and homœopathic diagnosis of the diseased person, de Jongh (pp. 389-399) remains all too abstract by not taking into full account what practical purpose the one or the other serves. Observed events form the basis for integration by reasoning in both kinds of diagnosis. The issue is: on what observations and what abstractions, worked into each of the two mental images, medicinal treatment is preferably to be based? Even if it be agreed that the person-diagnosis is preferable to the disease-diagnosis as a basis for medicinal treatment, it by no means follows that it can be achieved and put into practice in each and every case. Even less does it follow that disease-diagnosis could be neglected altogether; for everybody knows that it is indispensable, e.g., for prognosis—a by no means negligible part of a doctor's task. A vague use of terms, like "facts," "rational" "nomothetic and idiographic, "symptoms," "modalities," "causal," "analysis and synthesis," and further, the habit of presenting differences of method as contrasts of theory excluding each other, obscures the main issue. Though these weaknesses of de Jongh's reasoning have been dealt with before, it appears unavoidable to do so again when he persists in showing them. de Jongh's definition of disease-disgnosis (p. 390), if he means what he says, would lay its value open to profound misgivings. He says: "We understand by diagnosis a scientifically, as far as possible, supported conception (or abstraction) of the pathological processes, while presuming that to us the disease-conception has a real value." If that means: we value the disease-conception as being real, de Jongh shows that diseases are to him still real entities. Debate on these mediæval ontological lines would obviously be futile. But we may take the lenient view that he means such a diagnosis serves a useful purpose and can then agree with him. When he says: "Our aim is therefore to let our therapy take, as far as possible, a rational course, based on facts derived from the medical auxiliary sciences" we are left in doubt (quite apart from the obscurity of the sentence) what "facts" and "rational" mean to him, and it is just on the meaning of these terms that the issue happens to hinge. If a patient avows that he sees white mice which others in the same room do not see, is that a fact? I should say, yes and a very important one, because it allows us to draw inferences as to the patient's intoxicated brain. From de Jongh's subsequent reasoning it is, however, to be feared that he does not acknowledge this hallucination as a fact, because the white mice are not there.

To base his therapy on the widest possible range of facts, is every doctor's endeavour. The question is only which facts he can use with greater or less advantage for a definite purpose, e.g., for recognising and for intervening in the disordered processes. And on this use of his reason depends whether his therapy is more or less rational.

The issue is not brought a step further when both opponents aver that " their " therapy is based on facts and is rational. With the good proposition that Medicine is only the servant, not the master of Nature, we may all heartily agree. It is just that we fear that orthodox therapy based on disease-diagnosis cannot sufficiently live up to it, because there is too much classification, too much abstraction and too much nomothesis (rule-giving!) in it; which is all very well so long as abstraction is done for the sake of knowledge, of recognising and understanding a particular case as another example of a known type; but when it comes to therapeutic intervention in the particular situation of a patient, then we may well dispense with a good deal of indirect knowledge about intermediary processes in favour of direct observation of the particular manifestations in just that case. Our knowledge of the intermediary processes is pieced together from such variegated sources (post mortem examination, animal experiments, etc.) that the inferences with regard to the particular case are of all too general a nature, frequently even so vague and dubious that it would be unreasonable to plan treatment upon such abstractions. The question is whether this procedure is more or less rational than: to note all the manifestations available from the particular patient, to range them in view of the medicinal intervention, with a minimum of inferences, to adjust the medicinal agent as closely as possible, to apply the plan and to watch the consequences. In diagnosis-treatment reason is foremost applied to recognition and classification of general features; accordingly the therapeutic plan is of a more general kind, in the second instance the therapeutic plan is more adapted to the individual conditions. What the one or the other calls more "rational" matters little. The type-diagnosis should in any case be made for the purposes already mentioned. The person-diagnosis comes on top of that, its value can be seen only in practice, thus not by those who reject it for higher "scientific" reasons.

To the homœopath the sum of the "symptoms" is not identical with the person-diagnosis (p. 391) but it is the material used for it. About the processes within the organism which are hidden to direct observation, the homœopath may form the same thoughts as the allopath, only in most cases he thinks it wiser to keep them in the background and to act rather upon what he can directly observe of the patient before him. He is, however, not denied the use of any other non-homœopathic method (e.g., chemotherapy, substitutive therapy) if he sees a better chance for his patient in dealing with a germ-type, or in supplying a deficient hormone or vitamin. Foremost he is a physician, but he has the homœopathic method as a valuable superadditum. It must be acknowledged that de Jongh has correctly left out of the debate such methods which are outside the issue "allopathy-homœopathy."

de Jongh argues (pp. 391/2) that there are certain symptoms (e.g., reticulocytosis in anæmia, increased rate of sedimentation) which are important for disease-diagnosis and have therefore therapeutic consequences for the allopath who acts upon such diagnosis, but not for the homœopath who does not find those symptoms recorded in "his" *materia medica.* The allopath does not find these symptoms in "his" *materia medica* either, but if he does so in his toxicology the homœopath is perfectly free to include them in the symptom picture of the particular substance (as I have made a point of doing throughout the "Mineral Remedies," cf. e.g., Arsenic). Actually there is only one *materia medica,* but the allopath takes note of only a very limited part of it. Whether the toxicological symptoms are usable or more or less valuable in a particular case for choosing the fitting remedy, very much depends upon the usefulness of the other symptoms present. Mostly there are more distinctive ones and then the general structural signs become less important. On the other hand, there is the vast number of symptoms of the additional *materia medica* built up especially for the use of the homœopathic method and this is useless to those who are not conversant with it, and who therefore are still to be distinguished as allopaths.

(I hope I am right in supposing that de Jongh does not use the acceleration of sedimentation rate as a direct indication for slowing it down by medicine.)

The "modalities" in homœopathic *materia medica* are completely misunderstood by de Jongh (p. 392) when he takes them as independent symptoms. Whether they qualify one or a dozen symptoms in the same picture, they can have no sense whatever when detached from the symptoms which they qualify. The more symptoms they qualify in the same case, be it that of a prover or patient, the more valuable they are for the knowledge of the potential reactions of a person to that agent. It should be clear enough in itself in what respect the "homœopathic" symptomatology is different from the "allopathic" and which is the richer and which the poorer; likewise, whose registering of the symptoms is the more unbiassed, his who records them all carefully for any future valuation and use, or his who neglects the greater part as "psychical futilities." The use of the symptoms differs considerably in the two methods, there de Jongh is right.

What is meant by "causal remedy" and "symptom-treatment" is a matter of definition. The introduction of the terms "nomothetic" versus "idiographic" instead of "generalising or abstractive or formulative" versus "descriptive" procedure in science appears of no advantage. The distinction is as old as the Greek schools; and "academic" (Plato) versus "peripatetic" (Aristotle) expresses the same, But it should be borne in mind that in obtaining knowledge both procedures have to be constantly applied and the measure of each determines the kind of knowledge; and, as we want knowledge for distinct purposes, here more of abstraction, there more of description, here more exactness, there more precision is required. In a general disease-diagnosis abstraction, formulation, "nomothetising"; in a diagnosis of a particular person, description in detail or "idiographing" dominates. Neither the one nor the other diagnosis, however, is made exclusively according to one theoretical pattern. de Jongh's argument appears to be: "in all science there is endeavour to come to formulæ, to abstraction, nomothesis," hence, the more of it the greater the claim to be scientific. Such a state of mind would be in urgent need of revision. The opposite statement would be equally true and would be supported by the trend in modern physics which aims at describing the pattern of events as accurately as possible, because in the last resort all formulation fails. The antithesis is solved very simply, when the purpose which a certain field of knowledge is to serve, is kept in mind. In that case there is little doubt that disease-diagnosis requires more abstraction, person-diagnosis more description; while the data of the first are more exact quantitatively-speaking, those of the second are more precise qualitatively. When we are concerned with potential actions of medicinal substances, we are faced foremost with a qualitative problem and therefore the descriptive, qualitatively accurate contents of knowledge are superior; when we want statements about the probability of processes the more quantitatively exact our knowledge is, the better.

An equally rigid attitude regarding analysis and synthesis (p.314) hinders de Jongh from seeing things in their proper perspective. Firstly, analysis is to him exclusively causal and quantitative (for there is no other than "ours," he exclaims!). If we "analyse" the actions of a substance on a person into symptoms, that apparently is not the true brand of analysis. As any use of knowledge obviously depends on synthesis for achieving its end, it is commonsense not to go further with analysing than the required synthesis can match. Even if we want to repair a watch it is wiser to leave the wheels, etc., intact for putting them together later on, instead of making a thorough analytical job of it by cutting up everything to bits of metal. In brief, the correlation of analysis and synthesis is determined by the actual purpose, the particular knowledge required. In the end, synthesis happens in action. Hence the result has to show whether all the analytical and synthetical work put into a plan was adequate. It is easy to see that quantitative-causal analysis would lose itself in infinity, if physics had not put a definite stop to it in the quanta of action, the elements as it were of a synthetic action. In our example of applied knowledge, we are satisfied that the analysis into symptoms goes far enough (sometimes dangerously near to the limits, cf.

Hahnemann's registers of symptoms!) so as to make a reasonable synthesis for planning medicinal action in a particular case still possible. We aver that an analysis of a diseased person into endless part-processes may be all very well for a future synthesis of the knowledge of the disorder, and is of value for generalising disease types, but for acting we prefer the manifest particular concreta to the inferred general abstracts. We abide by the ultimate decision of experience.

Fortunately, the homœopath is not and need not be such a one-sided idiographer (p. 396) as de Jongh wants him to be, but he regards "scientific" diagnosis-therapy as one-sided "nomothetic."

A remarkable piece of juggling is then performed by de Jongh (pp. 397-399) with the purpose of finally discrediting the subjective symptoms (meaning the complaints as the patient feels and describes them), the modalities and the notable, leading or characteristic symptoms so helpful for knowing the individual patient and the actions of a particular remedy. de Jongh states: "If the homœopath avers that the subjective symptoms are the most useful criterion for choosing the correct remedy, it follows that he believes (though not consciously) that a case of disease is fully determined in its particular phenomenology by those symptoms." The conclusion does not follow from the premise, nor is it upheld by the homœopath (if someone or other has used language in a slip-shod manner it is no excuse for de Jongh to do the same). What we know and can easily understand is that certain psychical symptoms frequently lend themselves particularly to differentiation, say restless fear in one case as against indolence in another, and when we encounter several such differentiating symptoms against the background of common ones—though these may be sufficient for a disease-diagnosis—the psychic symptoms come in very handy to distinguish one case from another or one drug picture from another. The case could be fully determined only by the totality of signs and symptoms; as far as these are not forthcoming it remains undetermined to that degree. The point is that t h e homœopath acquires and uses more information than his orthodox colleague.

Then d e J o n g h asks whether the patient's distinctive complaints are caused (or conditioned) by pathological characteristics. To this he answers: " This, as far as we know, is certainly not so. There is no connection of an intimate kind between the complaints of a patient and the pathological characteristics of his disease. Otherwise one would have to assume that the totality of all complaints from which the patient suffers, implies clear evidence of the pathological processes in his organism, in a manner which is completely alien to our thinking in pathology. This theory which is completely outside the usual thinking in Medicine, would, if upheld, have to be supported by proof, etc." Well, well, one can only pity the " usual thinking " in Medicine! Why, does it suddenly stop being causal and rational when it comes to subjective complaints of a patient? Are the complaints in an entirely imaginary sphere or are they in fact manifestations of the diseased person? Is " proof ' required that they are connected with his disordered processes? Only if the primitive homœopath makes such a bold assumption! But d e J o n g h may mean that distinctive complaints need not necessarily be the manifestations of those processes which he takes as characteristic for diagnosing the d i s e a s e. There he is undoubtedly right, but who denies that? Nobody says even that they are pathognomonic symptoms, i.e., characteristic symptoms of the disease, it is only asserted that they are often distinctive as regards the patient. And why is it so impossible to "prove" that the feeling of numbness and coldness of a person who has taken ergot is connected with his characteristic angiospasms? What then, when the homœopathic observer has noted that the coldness and numbness in such a case is felt to be worse from warm covering of the parts affected? He thinks this qualification of the subjective complaint, this modality even more characteristic and nobody will hinder him from making proper use of his knowledge. It is not a necessary attribute of the homœopath that he cannot discriminate from the often vague and varying, but sometimes also very precise description by the sufferer, between what is significant and what not. On the contrary, by his study of *materia medica* he is especially trained for this job. de Jongh reasons as if, as homœopaths, they were all gullible fools.

The notable striking symptoms, those which are uncommon, extraordinary, and therefore used for distinguishing one case from another, are discredited by de Jongh in a similar manner. Why is a pain felt in an empty stomach more notable, for instance, than pain when the stomach is full? de Jongh wonders. Because the first occurrence is known to be restricted to and characteristic of a few conditions, while the second is a common occurrence and then very much dependent on the kind of food eaten. But for diagnosis of disease, of course, de Jongh must make a small-print *reservatio mentalis*, because it may have occurred to him that, e.g., pain two hours after a meal is helpful in the *ulcus ventriculi* diagnosis; but let us avoid the term "modality," in which the poor homœopaths believe! If coughing and certain pains should both be found worse in the early morning the primitive homœopath suspects that it has something to do with the state of the person as a whole at that hour. And if for several remedies (e.g., Kalium carb., Ammonium carb.) the time of aggravation of these and possible other symptoms proves to be the same, he again suspects that the reactions to these substances are in a notable respect similar to those of his patient. But de Jongh will not have it, pain and coughing are different symptoms, therefore the early morning aggravation of the one is independent from that of the other. It must then be a mere coincidence when they happen to occur in the same patient, or—the homœopath's idea of considering the patient as a whole, cannot be so bad, after all.

The decision on the selection of "leading symptoms" is not quite so arbitrary as de Jongh assumes. On the contrary, it is the result of intense sifting of symptoms as to their distinctive significance. To those who have not even contemplated this elaborate comparative work, the outcome may well appear arbitrary. All the same, to those who know them, the leading symptoms are very helpful.

In his criticism of "isopathy" (pp. 399-401) de Jongh has in mind a method of using products of germs and of diseased organs (usually called "nosodes"). According to de Jongh isopathy is this kind of practice only when it is used by homœopaths and when the products are potentised. It must thus not be confused with "legitimate" vaccinotherapy (or active immunotherapy) nor with organotherapy. Otherwise this therapy would apparently not be so ridiculous as it is in the hands of homœopaths. Nevertheless, as a zealot of pure homœopathy, de Jongh points out that this isotherapy has nothing to do with proper homœopathy. In so far as the preparations have not been tested on healthy persons, he is, of course, right. But some of them, like Tuberculinum, have been tested, and for them, at least, de Jongh might relax his ban. It must, however, be admitted that many of these preparations are used on superficial grounds, based on disease-diagnosis; but that is not a homœopathic privilege. Nor shall the unreasonable extension of nosode-preparations be defended, as it is mostly due to lack of knowledge of the existing homœopathic *materia medica*. In theory vaccinotherapy or active immunotherapy is, however, not quite so remote from homœopathy as it seems to de Jongh. As I have tried to explain this relation several times (e.g., Immunotherapy and Homœopathy D.Z.f.H. 1922 and Süddeutsche Monatshefte, Feb., 1932)—I may be excused from doing so again. After all, the potential actions of some bacteria are not quite unknown and may be assumed to be not so unlike those of their products. Thus there is a symptomatology to go on, but it cannot be said to be pure homœopathy as long as the particular preparations have not been tested. It goes without saying that passive immunotherapy is a substitutive method and as such outside homœopathy

There is no point in following up in detail de Jongh's arguments against the psora-theory (pp. 402-407). My own objections to Hahnemann's attempt to solve the problems of chronically alternating diseases by three "miasms" are not assuaged by any modern attempts to substitute one or three or even all the infections in the place of the three miasms. All the same. I accept the testimony of experience that sometimes the struggle of an organism with infections over a long period results in changing the inner conditions for the worse, i.e. renders a person more prone to disorders (on the other hand the result of a fully overcome infection is sometimes seen to have a favourable constitutional effect); but there are many other factors also found capable of deteriorating the constitution of a person so as to make him prone to a series of vicarious and alternating ailments. Sudden suppression of secretions and excretions appears one of the most important events in the sequence of such processes. Mental shock may sometimes be found to be the chief cause, in other cases it happens to be infection. The problem of chronic diseases is therefore mainly concerned with the alterations of the personal constitution. Hence I cannot subscribe to any causal generalisations as an adequate solution of this empirical problem. de Jongh, however, fights as usual. in the thin air of abstractions, because he does not even see the actual problem, but only the unsatisfactory hypothetical attempts to solve it.

As to the constitution problem itself de Jongh (pp. 407-416) is still so immerged in conceptual difficulties that he cannot find a simple and comprehensive approach which would permit him to come to grips with the concrete empirical issue. Had he thought that it concerns the inner conditions which determine a person's disease and the behaviour of the organism in disease, which have to be reckoned with in every case, he might have found a way out of the academic unreality in which he fences with scholastic arguments. The glimpse he seemed to have (p. 148) of this fundamental problem of Medicine and of the new possibilities of tackling it from the homœopathic approach by person-diagnosis has now vanished altogether.

First he tries to establish that the constitution of a patient can be widely disregarded in certain fields of successful therapy: in substitutive methods, where the deficient or, in chemotherapy, where the germicidal substance can be administered schematically. That this is done cannot be denied and it need not be debated whether the results would not be better, if a little more attention were paid to the patient as a whole even in those cases where a distinct insufficiency of a hormone or a vitamin, etc., can be met or where the life of germs can be impaired without great risk to the patient. Such a debate of border-line problems is out of place here, because these methods of therapy have no bearing on the issue. We are dealing here with the vast majority of cases for medicinal treatment where neither a definite substance deficiency nor a distinct germ can be tackled.

A nice scholastic exercise follows (pp. 410-411): according to de Jongh, "it is not comprehensible how a particular therapeutic 'system' (still not method!) could in principle be more suitable for treating on constitutional lines than any other system. Such a therapy would be not merely the best constitutional therapy, it would be simply the best therapy in every respect." de Jongh is therefore convinced "that a therapeutic 'theory' possessing special superiority as constitutional therapy cannot exist." Must it be said that a therapeutic method based on diagnoses of diseased persons is more constitutional than another which bases treatment on general disease-type-diagnoses? Which of the two is likely to be superior? But to conclude that such a method would be *universally* superior is presuming too much: Nature, in her unscholastic ways, presents us with cases in which the inner conditions of a diseased person are in practice unalterable or are such that they have second place, while the environmental conditions offer better access for intervention.

Then de Jongh finds that the homœopaths have made two errors of thinking: firstly, they believe (!) roughly, that a therapeutic theory (!), merely because it reckons with the constitutional issue, is superior to another one (which one?); secondly, they entertain the faulty conception of a theory which by its peculiar character must possess a specific superiority as a constitutional therapy. These errors spring, according to de Jongh, from a faulty interpretation of the constitution-concept. He wants to replace constitution by personality, individuality. Well, the homœopaths do not raise any objection to this illuminating piece of *quid pro quo.* They have always talked about the diseased individual person and constitution was only a reminder that the constituent structure-functions have something to do with the person as a whole. What they claim is that a method which implies treatment on person-diagnosis is superior to one which implies treatment on disease-diagnosis; therefore they will go on using it regardless of whether de Jongh finds their conception of the peculiar character of their method faulty or not.

Then de Jongh struggles with his own misconception of clinical constitution, taken as the sum of unknown factors of disease, and, on top of it, interpreted as static. That is just the kind of sterile notion which the primitive homœopaths have outgrown and left to scientific thinkers. The homœopaths, as it happens, are concerned with the knowledgeable details of the reactions of a person. But, being homœopaths, they have, of course, the wrong conception of constitution. And to make quite sure that there is nothing in it, de Jongh suggests that the term constitution be banished from Medicine altogether.

de Jongh does not spare his readers any of his mental exercises and labours. He argues: "Because constitutional therapy also has to generalise about the details (symptoms) it does not follow that its final result achieves a greater differentiation than a therapy which neglects these details. The constitution-therapeutist generalises only according to other criteria than does the non-constitutionalist." (A primitive thinker would interject: is it not of some importance what is to be generalised? Does it not make all the difference whether one uses or neglects the constitutional details?) de Jongh goes on: "If one takes chiefly 'more or less individualisation' as a gauge of the constitutional treatment by this method, one already commits an error." (No, the error is his; there are two methods to be compared, one with more the other with less individualisation!) Further: "Though the homœopath treats different patients with the same disease with different remedies he has not considered that he often employs the same remedy for sufferers from different diseases." de Jongh cannot get over the fixed association: one disease—one remedy, name against name. Has he never considered that a medicinal substance may have many potential actions which manifest themselves in different directions, make different syndromes, just because of the different inner conditions of the reactors? May not arsenic-poisoning produce gastro-enteritis in one man, in a second nephritis, in a third neuritis and so on *ad lib.*? And cannot sublimate mercury do the same? What a terrible confusion! The crux of the matter is: Just to the degree to which the homœopath is able to take the constitutional details into account, he becomes independent of disease-diagnoses. Lastly: "We, the allopaths, have at any rate many more medicines than the homœopath so that we should be much better able to individualise than he." Firstly, it is not true that the allopath has more remedies at his disposal than the homœopath, as a comparison of any official pharmacopœias of the two schools shows. Besides, neither school can monopolise any remedial substances. If the thousands of unofficial preparations on both sides were counted, a record not very gratifying to either party would be revealed. Even if the allopath had more remedies, his method of using them for differentiating and individualising would in no way be comparable to the homœopathic method. That is just the point.

de Jongh further assures us that the homœopath cannot and does not have any constitutional treatment; that the claim is nothing but a false pretext used to defend a treatment which does not accord with the simile-principle. And the reason? Well, in the constitutional drug pictures of the homœopath are features which cannot have been produced in the provings of these drugs. Hence the homœopath does not comply with de Jongh's strict canon of homœopathy, his constitutional therapy is not homœopathy at all. It has sufficiently been pointed out, how the useful addenda about habitus, complexion and all the clinical, biochemical and other constitution-types have come into the drug pictures, viz. as a description of those types which have shown themselves particularly susceptible to that drug. There is not the slightest reason to abandon this experience in order to please the ultra-homœopathy of de Jongh; I am afraid, once he grasps the whole idea he will become an extremist homœopath. I can only warn him.

The idea that constitutional disorders can be traced to chemical structures constituting the organism is fantastic to de Jongh; that it should be possible to diagnose the disorder of such constituents qualitatively, even more so. He cannot discuss it seriously, before efforts have been made to substantiate this special kind of constitution-theory. A pity that all the efforts of biochemistry, endocrinology, etc., have been lost on him.

Conclusion: homœopathy has no legitimate constitutional therapy; but as homœopathy has already been shown to have no legitimate existence it does not matter much. Things look bad for homœopathy. In theory it is dead. But de Jongh must still "*servare mentem rebus in arduis,*" i.e., he still must stick to his "objective" mentality, which has animated him from the beginning of his work, up to the very last, in case the theoretically dead delinquent should still be alive in practice.

THE HOMŒOPATHIC PRACTICE.

Alas, what does de Jongh find in the practical field of homœopathy? There is no such thing as homœopathy, there are only physicians who call themselves homœopaths and who treat their patients in various ways, seemingly alike, but in essence different (p. 424).

Now we can at last see what de Jongh has been struggling with all the time: an ideal system of speculations, a ghost freely suspended in mid-ether. It does not exist. It is a *fata morgana* of de Jongh's. There are only a queer sort of doctors who practise, in preference to others a medicinal method based on certain experimental-empirical lines unknown to and neglected by 99 per cent. of their colleagues.

What they practise is not homœopathy according to de Jongh, because actually it is not congruent with his ideal postulate. The homœpaths are an inconsistent, arbitrary lot; every one uses his own judgment, they sometimes take their clue from characteristic psychical symptoms, sometimes from modalities qualifying the symptoms present, sometimes even from organ affinities and *horribile dictu*, are, moreover, possibly influenced in their choice of the remedy by quite ordinary diagnoses such as thyreotoxicosis, nephritis, etc. There is no relying on these people! Verily, it is not fair of Nature to present us with such a variety of situations and patients, and even less with such a diversity of medical brains, of which one centre of practical judgment is, alas, tainted with the homœopathic stain!

The homœopathic *materia medica* has already been declared *urbi et orbi* as unreliable and impracticable by de Jongh; hence it is another proof of grave inconsistency on the part of these homœopaths to use it all the same to the best of their ability and to consider other indications for treatment if the situation should so require. Therefore while they may aver having used such a *materia medica,* their casuistics must be viewed with the greatest mistrust (p. 425/6).

Equally inconsistent are the homœopaths in applying such a wide range of doses, mostly too small but sometimes also too large for de Jongh's rules. They have no right to defy de Jongh's rulings and to adjust the doses and preparations as well as their poor judgment permits.

In brief, homœopathy should either conform to de Jongh's ideals or cease to exist; it should not try to grapple with the actual conditions of diseased persons.

The pragmatic approach to homœopathy is given remarkably little space by de Jongh (pp. 428-437). He examines the evidence of homœopathic casuistics from the detached position of an outsider. The proof of the pudding is in the eating. This commonplace advice is, however, just what de Jongh does not want to follow. If, after acquiring sufficient knowledge of *materia medica,* he had tried for himself, he could at least have put down his own cases as proof for his refusal to use the method any further. His personal conviction would then have a support as solid as his cases. As it is, he must base his verdict on weighing the evidence of casuistics of both schools as laid down in publications. This is clearly beyond the power of even the most voracious critic. First a common denominator would have to be found and, for the sake of getting any further, the homœopath would gladly agree to compare cases on the disease-diagnosis basis. Of course, for every period the then prevailing standard of diagnostics would have to be applied. As this again is beyond any feasible demands on the faculties of a contemporary impartial critic, one would have to select a definite period of literature, say the last 20 years. Then the difficulties of the umpire would mount again. Should he exclude all cases where the diagnoses are not confirmed by clinical standards? Then, of course, a ratio of the available homœopathic as against allopathic hospitals would have to be introduced, say for each homœopathic case which passes the test, 1,000 allopathic cases of the same diagnosis and so on. So the difficulties would grow *ad infinitum.* If, instead, one wishes to come to a provisional fair opinion one has no choice but to assume honesty, goodwill and the same average of critical faculty on both sides. To pick out a few uncritical case reports and to omit reliable reports of clearly diagnosed cases is singularly unfair.

As to the very limited value of statistics in forming an impartial judgment I can entirely agree with de Jongh. Indiscriminating statistics, with only a general disease diagnosis as their common denominator merely mean multiplying the factors of error by the same number as the factor of evidence. An adequate impartial investigation of claim against claim is still far away; it would need an amount of good will to cooperate which, so far, has not been forthcoming from the official side.

Meanwhile, de Jongh is unfair in running down, for instance, the reports of a number of polyarthritis rheumatica acuta cases of recent homœopathic literature (p. 433). The data are to him insufficient. The choice of the fitting remedy will, of course, remain to him a secret which cannot be illuminated until he has studied the relevant *materia medica.* And that is de Jongh's main trouble vis-à-vis all homœopathic case reports. "He does not speak to me," a "real primitive" would say before an effigy which is unlike his idols. Such short records of practitioners, e.g., by J. T. Wouters (p. 274) are informative to homœopathic adepts; to "scientific" outsiders they may appear pointless. However, by a little more searching de Jongh could have found some recent casuistic complying with the clinical requirements of to-day. No doubt he would find reasons for discrediting each and any, as he has done, to his obvious satisfaction, with everything in homœopathy which might be valuable for the advance of Medicine. But then there is no obligation for homœopaths to convince de Jongh or anybody else who shows no interest. On one page de Jongh belittles homœopathic statistics where they are given, on another (p. 433) he complains that there are none on tuberculosis, cancer, etc.

It must be awkward for de Jongh to hear that Guttentag (p. 435) who could not convince himself sufficiently by clinical trials in co-operation with Schier and at the time thought the term "similar" too vague, nevertheless convinced himself later; de Jongh would say, became uncritical afterwards. Terrible to think what the future may hold for our critic de Jongh.

Of Bier's position regarding homœopathy enough has been said; but de Jongh's assertion that sulphur usually does not produce furuncles as Martini's few experiments are alleged to show, seems not altogether the full truth. Apart from the prejudiced homœopaths including Bier, such unsuspect pharmacologists like Lewin also state that sulphur frequently does produce acne and furunculosis.

Kisskalt's negative results in trying to cure tetanus-infected rabbits with strychnine (p. 435-6) become no better clinical criteria from the fact that Wapler hails this sort of experiment as the only possible method of proving the truth of the homœopathic method. This is simply a retrogressive error on the part of the all too conciliatory Wapler.

When Gessler (p. 436) in his hospital work obtains good results with the homœopathic method he becomes uncritical, and when he states the homœopathic principles as evident to him he utters "enormities" which obviously are refuted by what de Jongh has so studiously piled up against these principles. It may not have been quite so easy for the chief of a big hospital to arrive even at his cautious conclusions. He had, besides the literature, only the occasional advice of a former assistant, who had convinced himself at the Stuttgart Homœopathic Hospital of the usefulness of homœopathic treatment. Will de Jongh set his own bungling with the homœopathic *materia medica* and his few (two or three?) trials against the earnest efforts and honest conclusions of an unbiassed clinician? The poor uncritical homœopaths, who have convinced themselves through decades of experience, naturally are annihilated by the magnificent aplomb of de Jongh.

Concerning the analogies of homœopathy with other stimulative methods (vaccinotherapy and certain psychotherapeutic methods) de Jongh is again referred to "Grundlagen der Heilkunde."

In the chapter on "homœopathia involuntaria" a precious insight into de Jongh's pharmacological water-tight compartments is offered to us by his ipecacuanha-asthma example of alleged homœopathia involuntaria. He dictates: "Ipecacuanha is not an anti-asthmaticum, only an expectorant and only for this reason is it sometimes used for asthmatics." Then he goes on: "Incidentally it happens that some people are qualitatively oversenstive to ipecacuanha, so that they react with an asthma-attack to its presence, but the asthma-patients to whom the allopath occasionally gives ipecacuanha, are not those who normally react with an asthma-attack to this substance; hence there is no question of homœopathia involuntaria." This is indeed an ingenious interpretation of the simile-principle! It would imply that an agent must be able to show in the same person when healthy, the full-fledged disease which it is supposed to cure in that person. It is not difficult to prove beyond doubt that homœopathy, be it intentional or unintentional cannot exist! Admirable is the rigidity of de Jongh's text-book-mind by which he so categorically can separate "expectorantia" from "anti-asthmatica." As ipecacuanha comes under the one heading, it must not be thought of under another. And these awkward incidents in Nature which is otherwise so strict in conforming to our scientific "laws"! Why should one state simply that ipecacuanha in some people provokes asthma-attacks and is sometimes seen to improve asthma, if one can complicate the matter in order to achieve complete confusion?

Similarly de Jongh deals with the cinchona-fever example. Regarding the arsenic treatment of skin disorders, de Jongh denies that the skin manifestations from arsenic are similar to those treated with arsenic by the dermatologist. I wonder whether the dermatologists are so sure about the differential diagnosis of cases of eczema, acne and parakeratotic dermatoses that they can exclude arsenic as a cause simply by the appearance and without reference to anamnesis; de Jongh is sure of it, so new characteristics have apparently been detected since I last heard of it.

Colchicine-gout is indeed not a good example of homœopathia involuntaria. The inflammatory swelling after subcutaneous injection is uncharacteristic, and swelling of joints as a sequel of Colchicum poisoning is, as far as I know, mentioned only in one case (Lewin).

Secale in central nervous system degenerations admittedly is a poor example because its use on the suggestion of Strümpell has given only transient or doubtful results.

The X-ray treatment in degenerative blood diseases (and in cancer, not mentioned by de Jongh) falls short as evidence also, because the curative results are so poor, but not because of any hypothesis on the modes of action. It is not yet clear to de Jongh that the designation "homœopathic" implies only similarity of manifestations but nothing regarding the mechanism of actions and processes behind them.

With regard to the therapy of desensibilising with the same kind of substance against which a patient shows allergic reactions, de Jongh, only at the end, begins to realise that homœopathy is a special method of a wider field of therapy based on similarity. But if some homœopaths call the over-sensitiveness observed in provings "allergic" it is not a "wild hypothesis" as de Jongh says, but at its worst a misnomer Whether the application is oral or parenteral, affects the theoretical issue very little.

In remedies with circumscribed organ-affinity such as Digitalis, Strophanthus (and dozens more could be given) nobody will assert that similarity of symptoms is of a high degree; the homœopathicity of the ordinary use of these organ-remedies is on a low level, but it is there all the same.

As to the similarity of Iodides and organic arsenicalia (and why omit mercury?) to syphilitic manifestations, de Jongh would judge perhaps a little more cautiously if he had considered the different phases of syphilis for which each of the agents proves effective.

(I would refer him to the chapter on syphilis in my "Homœopathy and Chemotherapy," at present in the press.) If de Jongh brings in the big doses as an argument against homœopathicity, he reveals once more that he has not yet rid himself of the popular style of thinking chiefly in quantities, of the bogey of "homœopathic dosis."

Why are such examples as Iodine in Graves' disease not considered?

The whole chapter on "homœopathia involuntaria" is, however, superfluous and ill-conceived, if de Jongh intended to argue against the supposition that examples of unplanned application of the principle of similarity serve to prove or to support the homœopathic case. It would indeed be a strange undertaking to find support for an elaborately planned method from examples of crude, unintentional and unplanned application. No, such examples can serve no other purpose than to set someone or other thinking who is both willing and capable of grasping the possibilities of the new approach.

It appears an utter waste of time to have paid any attention to an author who on page 441 of his book answers the question: what is homœotherapy? in such a childish way as de Jongh. It begins with: "homœotherapy is not the therapy which is based on the homœopathic simile-principle." *Lucus a non lucendo*! One characteristic of this obscure mixture of many queer ways of treatment is "that certain obscure remedies are used." Obscure to whom? To those who have taken the trouble of experimenting with these remedies and to study the results over many years? Or to de Jongh?

Though de Jongh has made it abundantly clear that homœopathic therapy is good for nothing, he still invokes the testimony of the "scientific-critical" homœopaths to show that nothing or at least very little can be achieved by homœotherapy (p. 442). He says: "The more scientific a homœopath is, the more he treats cases which really matter in the allopathic way and the more he reserves homœopathy for cases where it does not matter so much; thus the more he abandons his homœopathic ideal." Clearly, that depends on what one calls scientific. If it is scientific in the old school terms, this glorious deduction boils down to the truism that the more a homœopath leans towards the old school the more he will use the school-therapy. As I do not lay any claim on their brand of "scientific-critical" qualification, I hold no brief for this group of "assimilants"; but what I have seen of them has convinced me that they, too, call themselves homœopaths *a fortiori*, because, in the vast majority of their medicinally treated cases, they choose the remedy according to the simile-principle, by applying their knowledge of the homœopathic *materia medica*.

What anybody calls a "miracle healing" is obviously relative; the "miracle" in it being in direct proportion to his ignorance. The better the knowledge of *materia medica* the less miraculous are successful cures from applying this knowledge.

An equally easy evasion is to attribute to suggestion what one fails to understand.

The homœopath derives his special designation from additional knowledge of *materia medica* which he can put to good use by his special method: and de Jongh's opinion whether this designation is justified or a false aureole is not of the slightest moment, as he lacks just this knowledge and cannot, therefore, apply it.

According to de Jongh who, at this stage (p. 443), feels justified in dropping the last show of reserve (there was not much of it anyhow): If a homœopath uses other methods of treatment as well, he works under false pretences; if he refuses to give his patients treatment other than homœopathic he is a danger to public health. Thus whatever he does he is wrong.

Well, this is on p. 443 and this fact may serve as a plea for attenuating circumstances for the critic; but is there to be none for his patient readers, if they still haven't dropped something else besides their last reserve?

CONCLUSIONS.

It remains to pin down some terminal ejaculations of our troubled critic without comment: One can attach any medical significance to this system of homœopathy only on unreal grounds not in accord with sound reason and science. Homœopathy is an unsuccessful attempt to let therapy follow a very small number of definite rules. The homœopath is certainly not a scientific thinker. Neither is homœopathy an art, because therapy is not an art. The "idiographic" method is an atavism in the present stage of our science as applied to Medicine. The whimsical character of the homœopathic way of choosing the remedy is not a sign that the homœopath uses intuition but comes from pure arbitrariness in his therapy. The homœopath must be unscientific in the sense that he accepts certain thoughts as unquestionable truth, as a dogma. As a homœopath, he need only accept a small number of definite rules and to learn a great number of symptoms by heart and then need not think again for all his lifetime (be it then that he practises allopathy in between).

(N.B.—Parenthesis of de Jongh and apparently not meant as a joke ! O.L.)

The homœopath through his peculiar way of thinking is attached to homœopathy on account of its character as a theory (pp. 445-449).

Finally de Jongh applauds in three languages* his performance as a critic who has found absolutely nothing in homœopathy. A reconciliation between homœopathy and allopathy is out of the question, because they are not equal partners. The dispute between the two exists only in the imagination of the homœopath and it will exist there as long as there still remain representatives of the autistic-undisciplined thinking of homœopathy. Thus speaks de Jongh: "It seemed to us desirable, nay necessary, to clear up matters completely as regards homœopathy. We thought we might be allowed to take upon ourselves this modest task" (pp. 451-458).

It would, however, be cruel to withhold de Jongh's final theses from an appreciative posterity:

"(i) The theory of homœopathy consists of a heterogeneous complex of untenable, inaccurate and improbable assertions.

(ii) There is a deep gulf between the theory and practice of homœopathy.

(iii) The practice of the homœopaths is a conglomerate of divergent actions that cannot be placed under one real common point of view, and the efficacy of which must be considered as very improbable.

(iv) Homœopathy viewed as a whole is an unsuccessful endeavour to let therapy take its course according to a definite scheme, based on the old similarity-thoughts, for which thought there is no room in present-day medicine.

* In the following quotations of de J's English text I have taken the liberty of altering a few words and phrases which do not convey his meaning according to the Dutch text. O.L.

(v) A medical man with a scientific turn cannot be a convinced adherent of the homœopathic principles.

(vi) Seen from an objective point of view it would be better for homœopathy to disappear from the medical scene."

Nay, is it not dead and ripe for burial?

E PUR SI MUOVE!