

LETTERS TO THE EDITOR

The continuing mystery of the Memory of Water

Sir,

I read with a great interest the special issue about the Memory of Water. I began to work on that subject with Dr Benveniste in 1981 and I cooperated with him until 1991. To complement Thomas's article,¹ I think that some details connected with this story can be also important for future of the research on the high dilutions effect and homeopathy.

The first detail seems without importance, but it may be more significant than it appears: it concerns the origin of the expression 'memory of water'.² It was not published first in *Le Monde*, but in another French daily paper, 'l'Humanité', which first used this metaphor the 30th May 1988 'L'homéopathie a peut-être une base scientifique: la mémoire de l'eau'. The title of the article published in *Le Monde* the 30th June 1988, just before the publication of *Nature* the 1st July 1988 was 'La mémoire de la matière' (the memory of matter). It is interesting to note this detail because several of the hypotheses proposed in this Special Issue of Homeopathy concern the role of components other than water which are present during the process of dilution and agitation of high dilutions, such as silica, or dissolved gases.

Other details relative to the history, to the replicability and to the hypothesis may be interesting to complement the articles published in the special issue.

1. *About the history of memory of water, the origin of the work is complex, both before the publication in Nature in 1988 and before the beginning of digital biology:*

In 1980, Jacques Benveniste was the Director of INSERM (Unit 200), whose the theme of research was the Immunopharmacology of allergy and inflammation. He was well known for his work on paf-acether. In 1980, I wrote my thesis, as a student in Immunology, in this unit. I worked on the effect of paf-acether on free radicals production by the polymorphonuclear neutrophils³ (PMN). At the same time, I studied Homeopathy at the *Centre Homéopathique de France* and I had begun to test the effect of medicines such *Apis mel.*, *Belladonna*, *Ferrum Phos.* on PMN activity in another laboratory while studying for a diploma of the Pasteur Institute in 1979. I showed Benveniste some preliminary results in 1981. I was at the same time in contact with Dr Michel Aubin, Director of Research of the Laboratoires Homéopathiques de France (LHF). We organised a meeting at the beginning of 1982 and decided to study the effect of some homeopathic medicines on experimental models of inflammation and allergy. The same year, Benveniste, who was official advisor of the new minister of research, was contacted by the

Dr Belon, on behalf of the Boiron company. In 1983, contracts were signed between INSERM and two homeopathic companies:

- Boiron: Study of the high dilutions of histamine and paf-acether on basophils and mast cells.
- LHF: Study of the effects of various homeopathic medicines on inflammatory and allergic processes, including neutrophil activation, metabolism of macrophages, basophil degranulation and other models such as platelet activation.

At the end of 1983, the results were sent as reports or submitted for presentation to congresses:

- In February 1984, I sent Boiron a report about the effect on basophil activation of a mixture Histamine-Paf-acether.
- In September 1984, the results of two studies were presented at a congress (Forum des Jeunes Chercheurs), about the inhibitory effect of *Apis mel.* on basophil activation⁴ and of *Belladonna* and *Ferrum phos.* on oxygen radical production by PMN.⁵ Then, between 1986 and 1988, three publications about basophil activation^{6,7} and metabolism of macrophage⁸ were obtained in peer reviewed journals, as Thomas mentions.

In 2002, we synthesised the studies⁹ conducted at INSERM U200 from 1983 to 1988 (Table 1).^a

So, it is clear that the work about 'memory of water' was born of the will to study the biological effect of homeopathic medicines. Besides, the publication of *Nature* in 1988¹⁰ was dedicated to Dr Aubin.

The article sent to *Nature* in 1987, and eventually published the following year concerns the direct activation of basophils by anti-IgE high dilutions, not the inhibitory effect of histamine or *Apis mel.* on basophil activation. The reasons for this choice are complex: technical (it seems easier), strategic (the influence of *Nature* but also of the journal *Le Monde* was important), and psychological. In 1987, an article on the inhibitory effect of high dilutions of histamine had also been submitted to *Nature*, but was not accepted. The studies about histamine effect, conducted by Sainte-Laudy and Belon, were published in other scientific journals; they are well known in the homeopathic community.

Regarding the machines for digital biology, we were in touch in June 1988 with Dr Attias at INSERM U

^aRapport quadriennal d'activité 1982–1985 (p 13 et 94) et 1985–1998 (pp 57–59) de l'INSERM U 200.

Table 1 Effect of various homeopathic medicines on inflammatory cells: basophils, mast cells, macrophages, PMN, platelets

Basophils; mast cells	<i>Apis mel.</i> , <i>Poumon Histamine</i> , histamine, phospholipases
Lymphocytes	<i>Mercurius cor.</i> , <i>Phytolacca</i> : no positive results.
Macrophages	<i>Silicea</i> , <i>Tuberculinum</i>
Platelets	<i>Crotalus</i> , <i>Lachesis</i> : no positive results
PMN	<i>Apis mel.</i> , <i>Belladonna</i> , <i>Bryonia</i> , <i>Ferrum phos.</i> , <i>Silicea</i>

(Contracts INSERM-LHF 1983–1988).

200 and later at his office, he demonstrated the MORA machine. Later, Citro's machine, (Citro was a physician from Turin), was used at the Benveniste' laboratory (see reference 11). With regard to the contact with physicists, Jacques Benveniste met Del Giudice in March 1988, in Bermuda, at a scientific meeting where E. Davenas and I were also present. In the last months of 1988, we had a meeting with Preparata and Cyril Smith. I was also involved in a project of research with Prof. Ludwig in Germany and tried to develop cooperation with Ludwig, Del Giudice, Smith and some French physicists. But the management of the Boiron company decided in July 1989 to stop all funding of fundamental research projects on high dilutions. Fortunately, it was possible to carry on the cooperation with Demangeat in MNR studies starting in 1986.

2. *The problem of the 'scientific committee' of Nature and the problem of the replication of the results published in Nature are complex. In May 1988, when Jacques Benveniste announced to us that Nature accepted the publication, a central question was raised: why Nature decided to send a committee after the publication, not before?*

I have copies of all the letters exchanged between Nature and Benveniste in 1987 and 1988. It appears very clearly that Maddox was annoyed with Benveniste and that he tried to 'trap' him. About this very complex story, I can only briefly testify about the facts in which I participated directly.

It is certain that the deputation sent by Nature was not scientifically correct, although Sir John Maddox was very polite and sometimes sympathetic. But it is also true that there were difficulties in reproducing the activation by anti-IgE high dilutions before the publication and after. For instance, in July 1988 during the week which followed the visit of the scientific committee, Davenas worked on the system with Benveniste and me; we quickly realized that she could not reproduce the positive experiments and that there were technical problems. I told Jacques that it was better to work with discretion and Elisabeth Davenas was of the same opinion.

During the two years which followed, some positive results were obtained by Davenas, but other experimenters did not obtain the same positive results on anti-IgE activation (see reference 12). This fact, not reported in the version published in the *Comptes Rendu d'Académie des Sciences*,¹³ had been made clear in the articles sent in 1990 to Nature and Science (and refused

by these journals). Such an operator effect seems not have been observed during the study of inhibitory effect by *Apis mel* or histamine. It is not possible to explain this fact. There was no fraud at INSERM U 200 and Elisabeth Davenas was a very conscientious experimenter. I wrote an article on that subject,¹⁴ after the publications of a series of articles in Le Monde in 1997. I had also some exchanges about this problem with Michel De Pracontal who wrote in 1990 a book about the memory of water: 'Les Mystères de la mémoire de l'eau'.¹⁵ In this book, he was rather in favour of the effect of the high dilutions, but with the evolution of controversy he became increasingly skeptical and wrote, ten years later, about a 'scientific imposture'.¹⁶ I tried to demonstrate that an experimenter could obtain some non reproducible results without fraud.

To sum up, I am convinced, when I read all the results obtained from 1986 to 1991, that the activation of basophils by anti-IgE high dilutions was dependent on the origin of the blood and of the immunological state of the donor. We were not sufficiently rigorous on this point in 1988. But I agree with Yolène Thomas on the role of subtle human operator's effects, suggested in 1991 and observed recently in the study of Jonas about digital biology.¹⁷

Are there any simple solutions? It is scientifically possible to try to replicate the experiments conducted with anti-IgE and *Apis mel.*, as published in 1988 and 1991 with the new method of flow cytometry. It is also possible to replicate the protocol used in digital biology on activation of human neutrophils.¹⁸ But, at least in France, it is not politically easy.

More generally, some interesting hypotheses are developed in this Special Issue of *Homeopathy*. We discussed the possible role of silica in 1987, and also the role of free radicals and hydrogen bonds.¹⁹ Some new studies, coming perhaps from physicists, will shed some complementary light on the very mysterious story of memory of water, story which began before the Nature publication and which will surely continue a long time after.

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Memory of water and blinding

Sir,

As one of the co-authors of the "famous" article in *Nature* in 1988¹ on high dilutions and a former

member of the Benveniste's team, I would like to comment the recent article "The history of the Memory of Water" by Yolène Thomas.² She reports some of the problems with reproducibility encountered during the "memory of water" experiments, and suggests that uncontrolled parameters (eg electromagnetic pollution or quality of water) were most probably responsible when poor results were obtained. I do not fully agree with this presentation of the events. Indeed, the difficulties of reproducibility were quite atypical and did not appear to result from a weak "signal" among a noisy background. This was obvious with the experiments on isolated heart and with the coagulation experiments. The main issue was that in some circumstances, "effect" and "no effect" were randomly distributed regardless their origin (negative or positive samples).

The strangeness of these disturbances was particularly highlighted during the demonstrations that J. Benveniste organized regularly with the isolated heart system to convince other scientists of the reality of the phenomenon. These demonstrations were generally performed in two steps. In a first step, negative and positive samples were produced (high dilutions, samples of "informed water" or digital files) and were blinded with a code by an observer not belonging to the Benveniste's team. Some negative and positive samples were kept unblinded. In a second step, Benveniste's team tested all samples (blinded and unblinded). When all measurements were complete, the results were sent to the observer and the code was broken.

In these demonstrations, the biological effects (and absence of effect) were usually clear-cut. However, the results of blinded samples were almost always at random and did not fit the expected results: some "controls" were active and some "active" samples were without effect on the biological system. We could indeed hypothesize that active samples had been "erased" by external influence. It is however more difficult to explain how inactive samples had been transformed into "active samples". And we are unable to explain why the open samples (positive and negative samples), prepared and tested at the same time as blinded samples, gave systematically correct (ie expected) results.

It is difficult to summarize these numerous and disturbing experiments in a few lines, but I have described them in details in a recent book that tells the whole "memory of water" story.³ This can be read free on Internet (www.mille-mondes.fr): despite the successive technical improvements of the different experimental systems, the weirdness persisted. Taking these experiments as a whole, it appears that the results reflected more the expectations of the experimenters (and of the lab team) than supposed properties of the samples.

These strange results culminated with the DARPA experiments performed in 2001 on the coagulation

model using an automatic robot analyzer. Again the importance of the experimenter was confirmed, but the experts could not conclude that an effect related to "digital biology" had been evidenced. Interestingly, the experts of the team commissioned by DARPA concluded⁴ that unknown "experimenter effects" could explain these odd results, but that a theoretical framework was necessary to comprehend them; and they added: "Without such a framework, continued research on this approach to digital biology would be at worst an endless pursuit without likely conclusion, or at best premature."

I fully agree with this conclusion. Indeed, if the presence of certain people is necessary to obtain a biological effect whereas other people "block" it, are we talking about water properties? We all know that water is fascinating, but perhaps it is time now to ask whether water is really involved in the biological effects of the "high dilutions" and "digital biology". The fact that a simple blinding of the experimental samples induced such trouble is, in my opinion, the key to understand what occurred during the "memory of water" story and the "high dilutions" experiments reported by other teams. The early experiments with basophils were also not free from blinding disturbances. Indeed, the usual large and regular waves of degranulation (or inhibition of degranulation) routinely obtained by some teams became unnoticeable during large-scale blind experiments.^{5,6}

In conclusion, I propose that systematic assessment of blinding is no blinding should be performed by authors investigating "high dilutions" (or related) effects. Of course blinding of the samples should be performed by an independent scientist who does not take part in the experimental process. But perhaps some will prefer an "endless pursuit".

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Comment on "The Memory of Water: an overview"

Sir,

Martin Chaplin presents an interesting overview on the structure of water. Disappointingly, though, it seems to contain no useful evidence of a 'water memory' effect that would be relevant to the efficacy or otherwise of homeopathic treatments.

As is well known, the probability of any dilution beyond about 12c (a dilution factor of 1×10^{24}) containing a single molecule of the 'mother tincture' used to prepare the remedy is very low. So homeopaths require that water (or water/ethanol mixtures) somehow structurally 'remember' the mother tincture, and it is this structure that is responsible for any effect of homeopathic preparations. But Chaplin¹ appears to be talking about an entirely different effect when he states that "If there is evidence that the history of a sample of water affects its properties, then the 'memory of water' concept is proven without the need for a rationale for its action" (p. 146). This apparently broad interpretation explains some of the examples given as "Evidence for the memory of water", which otherwise appear to have little to do with homeopathic remedies, where the mother tincture must continue to influence water structure even when absent.

For example, Chaplin¹ (p. 146) states that "human taste is quite capable of telling the difference between two glasses of water, processed in different ways (e.g. one fresh and one undrunk for several days)". There is nothing mysterious about this. Depending on the circumstances, this can be due to outgassing of Cl_2 and/or absorption of atmospheric CO_2 . This has nothing to do with how a mother tincture diluted out of existence has any effect on water structure. In the homeopathic context, what would be really interesting is if it were possible to taste the difference, say, between two 30c dilutions made from different mother tinctures prepared under identical conditions.

Chaplin also refers to the Vybiral and Voráček paper in the special issue of *Homeopathy*,² stating that the authors "have shown that water changes its properties with time and its previous history"¹ (p. 146). There is no doubt that this is an interesting paper, but the authors specifically conclude that their results are a consequence of ions dissolved in the water, as the effect they observe is not present when deionised water is used. Again, it is unclear how this is relevant to cases where mother tinctures are diluted out of existence.

What Chaplin seems to be talking about in his paper is how impurities might affect water structure, as illustrated when he states "The water used for dilution is not pure relative to the putative concentration of the

'active' ingredient, with even the purest water considered grossly contaminated compared with the theoretical homeopathic dilution levels. This contamination may well have a major influence, and itself be influenced by the structuring in the water it encounters"¹ (p. 148). Since the concentration of the mother tincture will always be dwarfed by the concentration of impurities, it is difficult to see why the mother tincture should have an effect more important than the impurities on the water structure. Homeopathic remedies are not 'just water': they contain significant amounts of impurities, which might create interesting structures in the water. But how this would make them significantly different from any sample of water is not clear.

I am in agreement with Chaplin when he states that "simply proving that water does have a memory does not prove that homeopathic medicines work". The best evidence as to whether homeopathic medicines work or not is to be found in randomised placebo-controlled trials of the medicines. A recent meta-analysis³ has demonstrated that the best-conducted trials show no effect for homeopathic remedies beyond placebo. The structure of water is certainly a fascinating subject, as Chaplin shows, but all the best evidence shows that homeopathy has no effect that requires an explanation.

Competing interests

None declared.

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Reply to Comment on "The Memory of Water: an overview"

Sir,

Paul Wilson¹ argues that my overview² did not contain evidence of a water memory of relevance to homeopathic treatment. His argument fails on two

levels; firstly such an argument was not the primary purpose of my review and secondly, in fairness to its inclusion in the journal 'Homeopathy', I did present such an argument (e.g. Table 1, p. 149). Clearly, I was only able to present the available evidence and Wilson's letter does not direct me to any I missed.

The 'memory of water' debate has moved away from homeopathy, as evidenced by the recent paper in *Nature*,³ to considering whether water can show any changes in properties due to its processing history. Although I came down heavily in favour of such an effect in my review, there is still much debate within the water research area as to whether such a memory is evident. My paper primarily sets out the currently available evidence for such a memory for water. It should be noted that, although the processing history may involve changes in solutes, as described in my review, these were somewhat unexpected (except in retrospect) and due almost entirely to the particularly unusual properties of liquid water itself. On the basis of the available evidence and in contrast to the view expressed in Wilson's letter, I believe that the presence of such solutes can cause great differences in the biological effects of the water (although again, it was no part in my review to give the evidence for this, which could form a complete paper in itself). However, as an example, a recent paper has shown how tiny concentrations of hydrogen peroxide can have major effects on cellular behaviour;⁴ hydrogen peroxide being one of those solutes that arises naturally in even the purest water in concentrations that depend on physical agitation such as succussion.⁵

It is clear from the final paragraph of the letter that its author has his own preconceptions concerning homeopathy and that he was disappointed that I was not able to reinforce these. I should point out, however, that the paper cited in support of these preconceptions⁶ does not demonstrate (as stated) 'that the best-conducted trials show no effect for homeopathic remedies beyond placebo'. Examination of the data in this paper beyond its abstracted 'Interpretation' clearly demonstrates the opposite view to that proposed in the 'Comment'; a fact that I posted on my website⁷ within days of its publication and which has been subsequently argued elsewhere.⁸

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Comment on "The defining role of structure (including epitaxy) in the plausibility of homeopathy"

Sir,

We wish to draw attention to serious anomalies and incongruities in the UV absorption data presented in the paper by Rao *et al.*, published in the July 2007 issue of *Homeopathy*.¹ In a study of this nature, which examines multiple samples of ethanol, there must be absolute uniformity in the source of the solvent. For the data to be valid, all ethanol used must be sourced from the same stock bottle. However, the authors fail to mention of this point, and it is clear from the results presented that the source of ethanol in this investigation was not uniform.

The most striking anomaly is the UV spectrum presented for "plain ethanol", a single trace repeated three times in Figure 3 of Rao *et al.* (p. 179). The provenance of this sample is not recorded. This trace reveals extremely high absorbance (greater than 0.8 absorbance units) at 250 nm, falling off steeply towards 400 nm but still above 0.4 units by 350 nm, and demonstrating an absorbance peak of 0.65 units with a lambda-max of about 330 nm. It is impossible to represent this trace as being ethanol of any recognised degree of purity. Spectroscopic grade ethanol has an absorbance of less than 0.05 units between 250 and 400 nm,² and even USP/NF pharmaceutical grade ethanol has an absorbance of less than 0.3 units at 250 nm, falling off to less than 0.1 units by 270 nm.³ If the substance measured by the authors as "plain ethanol" was indeed ethanol at all, it is clear that it contained extremely high levels of impurities, possibly including acetone.

In contrast, the spectra of the samples which were diluted and succussed (*Nat mur.*, *Nux vomica* and the "succussed ethanol" with no mother tincture), and which were presumably all supplied by Hahnemann Laboratories, demonstrate substantially lower levels of impurities. While still not being spectroscopic grade ethanol, these samples could well represent ordinary pharmaceutical grade ethanol. The authors claim these samples are "different", however the evidence presented for this is weak to nonexistent.

Figure 1 presents one trace each for *Nat mur* and *Nux vomica*, each at 6c, 12c and 30c potencies. The traces are said to be "representative", however with no information on repeatability or how the "representative" traces were selected, it is impossible to say whether there is any real difference between any of the six spectra.

Figure 2 purports to address this point, but fails to present the necessary data. The legend declares that 10 samples of each of the six remedy preparations were analysed. The accepted way to present such data would be as mean absorbance \pm standard deviation for each wavelength point, or at least for a representative selection of wave length points. Statistical analysis could then be used to demonstrate whether or not there was a real difference between any of the remedies or potencies. However, the authors have instead chosen to present only two traces for each preparation, as "envelopes of differences". The derivation of these traces is not explained, although we surmise that "extreme" high and low traces for each preparation were chosen to provide an impression of the range of results obtained. This is not an appropriate method of handling data of this nature, as most of the information is lost and statistical analysis is rendered impossible.

A further difficulty with Figure 2 is that the upper (open circles) trace in the top graph of Figure 2a (30c *Nat mur*) appears to be a duplicate of the upper (filled circles) trace in the top graph of Figure 2b (30c *Nux vom*). Comparison with other traces of the two remedies indicates that this trace is really one of *Nux vom*, which has been duplicated into the *Nat mur* graph in error. In conclusion, paucity of data, ambiguity of presentation and lack of statistical analysis prevent any conclusions being drawn from the information in Figure 2.

Comparison of Figure 2 with Figure 1 reveals that all six traces presented in Figure 1 are taken from Figure 2, in each case the filled-circles traces. If indeed the traces in Figure 2 represent the extreme range of results obtained, this is startling, as the traces in Figure 1 are stated to be "representative". In addition, while it does appear that the *Nux vom* samples tended to demonstrate higher absorbances than the *Nat mur* samples (excluding the obvious mistake noted above), in two out of the three potencies the higher *Nux vom* trace from Figure 2 has been chosen for inclusion in Figure 1, thus exaggerating the apparent difference.

Figure 3(b) and (c) again repeats the same six traces as Figure 1, this time grouped by remedy. Presented in this way, it is clear that there is absolutely no difference between the three potencies of *Nat mur*, and that while variation between the *Nux vom* potencies is a little more pronounced, again all three appear to come from the same population. The same is true of the three potencies of "succussed ethanol" presented in Figure 3a.

On visual inspection it does appear that there may be genuine differences between the three remedies (although no statistics are presented to allow this to be tested), with the *Nat mur* showing the lowest absorption and the *Nux vom* the highest, with the succussed ethanol lying somewhere between. Nevertheless, these differences are entirely consistent with small differences in purity of the ethanol stock used for preparation of the three remedies - small, that is, relative to the very high level of impurity evident in the "plain ethanol" sample presented alongside. This degree of variation in UV absorbance is entirely to be expected between different batches of pharmaceutical grade ethanol, which is not prepared with spectroscopic analysis in mind. The authors make no mention of having stipulated to Hahnemann Laboratories that all material sent to them should be prepared from the same stock bottle, and the data presented indicate that the different remedies, possibly prepared at different times, simply came from different bottles of ethanol.

It is clear that the data presented are wholly inadequate to support the authors' assertion that UV spectroscopy can differentiate between the two remedies, and between different potencies of the remedies. If the authors wish to test their assertion it will be necessary to repeat the work from the beginning, ensuring that all samples used in the study are sourced from the same bottle of stock solvent, that all duplicate preparations for precision assessment are separately prepared *de novo* from the mother tinctures, and that sufficient data are generated to allow robust and valid statistical analysis of the results.

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Authors' reply to Kerr *et al*

Sir,

We refer to the letter by Kerr *et al* in response to our article in *Homeopathy*.¹ Simple processes are capable of producing large pressure changes, in the 10kb range at least, and hence major structural effects in condensed matter.^{2,3} This is a key point in our thesis: the classical view of "molecular" level structure for water (molecules, not condensed matter), may be misleading. There are many different structures in ordinary liquid water, in the pressure-temperature ranges of simple technologies, see Roy *et al* for detailed arguments.⁴ The thesis of our paper was that common structural tools may be used to demonstrate whether or not, after the complex, ill-defined processes of succussion, the original solvent without and the solvent with addition of a remedy are identical. It is structure not composition that controls properties most profoundly. The question is can structure be changed by the homeopathic process?

We showed that epitaxy, pressure and nanobubbles (the latter two created in the process of succussion) plausibly could cause changes in the structure and hence necessarily the properties of such fluids. Being neither champions nor detractors of homeopathy we obtained samples of homeopathic medicines as used in practice from a commercial supplier, and performed extensive spectroscopic analyses on the possible changes in the structure. This is the key point of our paper.

Kerr *et al*'s remarks concerning the probable contamination in the sample of the original solvent sent to us, although apparently not in any of the dilution materials (and reported accurately by us, we are inorganic materials scientists) illustrates one of the values of our work to homeopathy producers and users, and other health researchers. We have provided them with potential quality control tools, and a refining of the arguments away from incorrect generalizations.

All the analytical data shown in the paper are the result of reproducible analyses, although we appreciate

the suggestion of representing such data as an average with a standard deviation, we emphasize that our key identification by display of an envelope demonstrates that, there are indeed differences beyond the standard deviation range among individual homeopathic remedies, as used in practice.

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Going beyond the evidence

Sir,

Vybíral and Voráček¹ present some interesting, though incomplete, data on the bulk rheology of weak aqueous solutions of unidentified ions (referred to as “water” in the title of their paper). However, they draw conclusions that go far beyond what their data can support.

Most strikingly, the paper is replete with references to “clusters” but contains no evidence to support the existence of any particular structure. The authors’ own data are measurements of specific bulk rheological properties of the fluid samples. Therefore reference to “clusters” in this paper represents both speculation and assumption on the part of the authors.

One consequence of this is that they see conflict where none exists. For example, they state: “Currently two diametrically sets different of results supported by serious observations exist concerning the duration of structures in liquid water. According to one, molecular

clusters in water have a duration of less than one hundred femtoseconds. According to ours, clusters grow to webs on a time scale of days.” Here they show confusion about two particular issues. First they have no observations on the durations of any structures; just a time- (and shear history) dependent change in viscosity. Second, Cowan *et al.*² comments on hydrogen bonded structures in “pure” water rather than effects due to low concentrations of ions, as examined in this paper.

This confusion leads to a failure to compare like with like: “Moral: If two different observations seem to be mutually incompatible within the frame of an established theory, the most probable explanation is not that one of the observations is wrong, but that the theory is wrong or at least incomplete, and that the observations merely discovered that it was not self-consistent.” This is just muddled thinking; there is no incompatibility between their observations and those of Cowan *et al.*² The authors demonstrate that their observations are the result of low concentrations of ions present in the water. Whereas Cowan *et al.*² address the highly transient nature of hydrogen bonded structures in “pure” water.

Given the determining role of aqueous ions in their observations it is surprising that the obvious questions are not addressed, such as: What species of ions are responsible for this effect? How does this effect vary with ionic concentration? Where are these ions coming from (silica leaching from glassware or the decomposition of organic impurities in the water, for example)?

Finally, what emerges from the actual evidence contained in this paper is of little discernible relevance to either homeopathy or the idea of water memory. The effect measured depends on an actual concentration of ions and disappears when they do. The autothixotropy appears over time with no external input. Therefore, if it is possible to regard the time- (and shear history) dependent change in viscosity they observed as “information” (quite a stretch!), the “information” content of the “water” is increasing with time in the absence of any input. This is not a memory mechanism, but rather one of invention.

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Response to Adrian Gaylard: Going beyond the evidence

Our paper was a report on our results together with a possible explanation of the series of observed phenomena.¹ If our explanatory conjectures were impossible to check, then it would be proper to call them speculations. But, in fact, our conclusions are falsifiable; they can be tested and thus they fulfil the criteria for a hypothesis/theory as defined by Karl Popper. Since we observed an unknown phenomenon, we were justified to present a possible explanatory hypothesis as well.

Dr Gaylard claims that we see conflict where none exists. This appears to be a misunderstanding; if our text is read thoroughly it is clear that our point is just the opposite, in fact in accordance with what he claims. We wanted to point out that there is no conflict where others have previously claimed that conflict exists. This illusion of conflict is perhaps the reason why publication of our findings was refused many times on the grounds that "it is not possible," even when we omitted all comments to our observational results. We intended to say that the difference in the results is determined by the fact that it is necessary to differentiate between the "practically pure" and "very pure" water (in practice distilled and de-ionised water, respectively).

It is surprising that the source of unnecessary schisms and bitter attacks could be so trivial. The principal purpose of our article was to stop that unproductive conflict; both groups were right. It happens so often in science! It is, as well, the substance of the Moral in our paper. We do not claim "If two different observations are mutually incompatible ...", but "... observations seem to be mutually incompatible ...". The theory was indeed incomplete: it did not discriminate between "practically pure" and "very pure" water. Scientifically considered, "practically pure water" is not water. But then where does completely pure water exist in the real environment? The problem is beginning to be rather semantic.

The rest of Dr Gaylard's argument ("... there is no incompatibility between their observations and those of Cowan ...") is precisely our point, although "incompatibility" in the quoted sentence should be substituted by "inconsistency".

In our paper we never claimed anything about any relevance of our results either homeopathy nor to the idea of memory of water. Nonetheless, on being challenged by Dr Gaylard, we can state that our observations are not proof, but that they are compatible, not just consistent, with the mentioned doctrines.

Our results are in fact a spin-off from research on the theory of gravitation and we considered it inappropriate just to ignore them; they may be important for other scientists. As we do not have time, the economic means, nor the required skills, we performed this research to a limited extent. We will be satisfied if others continue and develop the research in spite of whether they will corroborate or disclaim our conclusions, or sustain or disprove the existence of the phenomena we observed. We look forward to hearing of further developments in the field.

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Straw men and black swans: the philosophy of contemporary science

Sir,

From the perspective of an ordinary practising scientist, Milgrom¹ represents the logical structure of science in a way that does not promote an accurate understanding of modern science. I would have expected that a paper from a senior researcher, who claims to have sought advice on philosophy, published in a peer-reviewed journal might have actually addressed the philosophy of science in a meaningful way.

Rather, the philosophical discussion takes aim at a "straw man" instead of real science. It criticises what it calls "science's primarily inductive logical structure". The example provided (white/black swans) is actually a common illustration of "naïve (or classical) inductivism" (see, for example, Ref.²). This philosophical approach had its limitations exposed a long time ago. No serious contemporary scientist would accept naïve inductivism as a realistic model for scientific endeavour. Therefore, it is not science's primary logical structure, as Milgrom contends.

The concept of falsification, originally formulated by Karl Popper,³ would make a more realistic candidate for modern science's primary logical structure. The "swan" example can be used to contrast these two approaches. Simply put, to "prove" the hypothesis that all swans are white the naïve inductivist has the impossible task of observing all swans through all time; however the observation of a single black swan would falsify this hypothesis. The move away from inductivism was driven, in large part, by Popper's massively influential text "The Logic of Scientific Discovery".³ This work is actually referenced in the paper [Ref. 14(a)], but as what seems to be an implied example of the Post-Modernist attack on logical positivism (Popper, of course, was not a Post-Modernist, but defined himself as a Critical Rationalist).

The author's apparent awareness of Popper's work makes it very peculiar that he continues to attack the "straw man" of naïve inductivism, rather than engaging with the more relevant (and stronger) falsificationist position generally aspired to in modern science. (For example, as part of my education as a scientist over 20 years ago, I was made to study Popper as part of my physics degree. This, naturally, included an exposé of inductivism along with subsequent criticisms of the falsificationist approach. I believe it was a second year topic.) Of course, this more realistic position makes a more difficult, but by no means unassailable, target (see, for example, Ref.⁴).

Similarly, when the author outlines his view of the unwillingness of scientists to reconcile themselves to new observations that do not fit into current theoretical models he neglects to mention the seminal analysis of this issue by Kuhn.⁵ It is from this work that we get the notion of "Paradigms" in science and "Paradigm Shift". Whether the author would agree with the analysis or not (and it does have notable critics among scientists), it is of such importance in context of the debate he frames that to omit it is a serious flaw. (This was, again, part of my undergraduate training as a physicist.)

More recent philosophical positions that do not figure in this assault on the "straw man" of naïve inductivism include the work of Imre Lakatos,⁶ who proposed that the object of evaluation should be whole "research programmes" rather than individual hypotheses, and Feyerabend^{7,8} who objected to any single prescriptive scientific method. It is not my contention that these approaches are complete or flawless descriptions of modern science, but that they are so influential that a discussion of the philosophy of contemporary science is incomplete without them. By extension, any critique of science that ignores the major philosophical analyses of science is fatally flawed.

The author then states: "Consequently, positive results from even the highest standard scientific trials are rejected by those who will not accept homeopathy's claim that remedies diluted out of molecular existence might have any effect. For black swans, read homeopathy." This is an odd assertion. First, the author goes to some lengths

to challenge the suitability of what would generally be considered to be "the highest standard scientific trials" (double blind RCTs) for assessing homeopathic interventions. If the outcomes of such trials are just being ignored this would seem to be unnecessary. Second, this very strong assertion is not backed up by any references. Finally, as previously discussed, the real scientists actually look for "black swans".

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Response to Adrian Gaylard: The dangerous swan song of the straw man

Sir,

Adrian Gaylard chides me for misrepresenting science's logical structure "in such a way as to not promote an accurate understanding" of it. I am also accused of mugging the "straw man" of naïve inductivism", when, instead, I should have taken a pop at his altogether tougher friends, Karl Popper and the falsificationists. Then, failing to mention Thomas Kuhn's seminal work on paradigms, and omitting both Imre Lakatos and Paul Feyerabend from my discussion, so "fatally flaws" my argument "no serious contemporary scientist would accept it." Finally, Gaylard states "real scientists actually look for "black swans"

In the context of my article,¹ Gaylard makes it trenchantly obvious what he thinks of my putative philosophical shortcomings. In fact, I could assist him further by pointing to other 'sins of omission'. For example, readers are invited to examine the work of Holmes *et al*, which appeals directly to the writings of Post-Modernist philosophers Michel Foucault² and Jacques Derrida,³ in order to deconstruct the 'fascist dictatorship' of evidence-based discourse in the health sciences⁴ (and yes, Popper was no Post-Modernist). I suspect practitioners of homeopathy and other forms of CAM, and their patients, might enjoy this refutation of naïve inductivism's dangerously⁵ exclusive hegemony. However, before this response descends into a 'who-can-name-drop-the-most-philosophers-in-a-sentence' competition, or indeed an up-dated version of Monty Python's *Philosopher's Drinking Song*,⁶ I should make it clear that I will not be throwing up my hands and admitting "It's a fair cop, guv!" just yet.

For there is a world of difference between what Gaylard believes somewhat idealistically scientists SHOULD do (and indeed the good ones probably do—in being open-minded enough to pursue 'black swans')—and what actually happens here, down on the ground, in real life. For example, consider this almost Churchillian utterance from the geneticist and science populariser Professor Steve Jones, "*Science is a broad church full of narrow minds trained to know even more about even less.*"⁷ The reasons for this might include education, peer pressure, and control and access to research funds, etc. Nevertheless, it is clear to Jones at least, that Russell's naïve inductivist 'turkey' is alive, well and strutting around the biomedical 'farmyard',^{7,8} regardless of Post-Modernism, Karl Popper, Latakos, Feyerabend, and the whole Philosophical Choir invisible still trying to wring its neck (I am deliberately equating narrow-mindedness in science with naïve inductivism, which is, of course, debatable). While Gaylard was no doubt fortunate to have received a tertiary physics education that included exposure to ideas from the philosophy of science, this is still a by no means common accoutrement of university courses,⁹ especially in the biochemical and bio-medical sciences (though such interdisciplinary research is now being encouraged at the post-doctoral level).¹⁰

Thus the *experience* (admittedly anecdotal) of those of us who argue the case for homeopathy and other forms of CAM, especially in the face of equivocal data from double-blind randomised controlled trials, is that many scientists and science writers appear to be, and most certainly can behave as, unreconstructed bloody-minded 'naïve inductivists',¹¹ ie, 'straw men'. I remember well a rather heated exchange I had with an ex-Astronomer Royal some years ago on the subject of homeopathy, which ended with him upbraiding me for being 'dangerous'. Later, I was left in no doubt that the bestowal of this 'cachet' owed more to his strong feelings of 'betrayal' at my combined interest in science

and homeopathy, than to any intellectual disagreements over the philosophy of science.

Of course, none of this detracts from Gaylard's 'naïve idealism', though I suspect it might well require some modification in the light of content to be found on the Internet in so-called sceptical web-sites. However, he needs to look no further than *The Times* newspaper, to see a particularly high-profile 'straw man' in action. This, from Edzard Ernst, the UK's first and only professor of CAM, "*I don't believe in anything I can't prove. My only true belief is in science and its ability to sort out belief from fact... My job is to establish whether or not they (CAMs) are evidence-based. There is no aspect of belief in this at all.*"¹² Yet, in the same article, gerontologist and philosopher, Professor Raymond Tallis refutes Ernst's naïve inductivism with, "*I believe so many things without proof that I am spoiled for choice. As Karl Popper pointed out, no belief can be legitimately placed beyond the reach of doubt. There is always the possibility of further observations that may prove it wrong.*"¹²

Ironically, given this contradiction, Tallis was a co-signatory with Ernst of the recent letter, leaked to *The Times*⁵ urging Health Trusts to ignore the 'false claims' of homeopathy and other forms of CAM. Since then, there have been reductions in NHS referrals to homeopathy, and the threats of closure to the NHS Homeopathic Hospitals. This illustrates the threat posed to patient's 'owning' their own health and expressing freedom of therapeutic choice within the NHS,¹³ by an unchecked and arrogantly applied naïve inductivism. Not before time then, resistance to this threat is beginning to grow.¹⁴

Clearly, as far as arguments over the efficacy of homeopathy/CAMs are concerned, it seems there will continue to be plenty of 'straw-men' to, as Gaylard puts it, "take aim at" (regardless of how many falsificationists they might hide behind), and that real 'black-swan' hunters appear dangerously thin on the ground.

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Comment on: “Conspicuous by its absence: the Memory of Water, macro-entanglement, and the possibility of homeopathy” and “The nature of the active ingredient in ultramolecular dilutions”

Sir,

The two central problems of homeopathy are the absence of studies that clearly demonstrate the superiority of homeopathic remedies over placebos in randomized, controlled double-blind trials (RCT) and the implausibility of the claim that dilutions beyond Avogadro's limit can have any specific effect linked to the properties of the original substance. While the first problem is located within the domain of the medical sciences, the second is much more fundamental and, if solved to the satisfaction of the adherents of homeopathy, probably will revolutionize physics. With this in

mind, the decision of the editor of “Homeopathy” to focus on the key issue of the *Memory of Water* and to invite contributions from proponents and critics of this idea has to be applauded.

Since it has been well established that neither classical nor quantum mechanics (QM) offer a plausible explanation for the alleged specific effects of high dilutions, it seems logical enough to look for alternative physical theories to explain this effect. Most prominent in this line of thought is “Weak Quantum Theory” (WQT).¹ Based on WQT, both Walach,² Weingärtner³ and Milgrom⁴ have developed models of homeopathy. I have criticized Walach's model previously⁵; Weingärtner's and Milgrom's models essentially have the same flaws as Walach's.

First and foremost, it is not at all clear whether there is something to explain. Walach, Weingärtner and Milgrom take the “fact” that high potencies have specific effects for granted. Thus, the real test of these models is not whether they explain previously known features of homeopathy, but whether they can be used to improve the design of experimental tests of homeopathy's core hypothesis that high dilutions are different from appropriately prepared placebos. Unfortunately, all three models fail this test. Walach argues that due to entanglement, “clinical trials [...] are a bad investment of time, money and effort”⁶; Milgrom asserts that “[...] the observational procedure of the RCT may ‘collapse’ the three-way entangled state, leading to the loss of the underlying homeopathic effect, a therapeutic equivalent of Heisenberg's Uncertainty Principle”.⁴ However, if the gold-standard of evidence-based medicine (randomized, double-blind trials) is rejected, another way to account for the (surprisingly powerful!) placebo effect needs to be proposed. None of the suggested methods – essentially, trials with lower standards of placebo control,^{2,6} would impress critics in the event of a positive outcome.

Second, while WQT itself is a legitimate mathematical framework for scientific theories, its application to homeopathy relies on leaps of faith and unconvincing analogies to proper quantum mechanics. The basic idea of WQT is to use mathematical models similar to algebraic quantum theory to describe a wide range of phenomena (not restricted to physics; see Ref.⁷ for an interesting example from the realm of psychology). But, as even one of the authors of the original WQT article concedes, “it has yet to be determined whether homeopathy is a good application of generalized quantum theory”.⁸ The most striking difference between the WQT models of homeopathy and proper QM concerns the use of mathematics. While QM has always relied heavily on mathematics and impressed even its most prominent critics by its ability to predict the results of experiments with unmatched precision, and while WQT is defined as a set of mathematical axioms, mathematics are *conspicuous by their absence* in the WQT models of homeopathy!

The crucial feature of quantum mechanics that needs to be generalized for the WQT models of homeopathy is *entanglement*. As there are no clear and unambiguous (mathematical) definitions of the relevant systems, states and operators, the proponents of these models speculate freely about the properties of their entangled states. The fact that entanglement is easily destroyed in proper quantum mechanics is used to explain the failure of homeopathy in RCTs, as the blinding procedure is thought to “[...] ‘collapse’ the three-way patient-practitioner-remedy entangled state in a way analogous to that by which observation collapses a particle’s wave function [...]”.⁴ The fragility of entangled states would seem to indicate that great care needs to be taken in the preparation, storage and administration of the homeopathic remedies, but this concern is only discussed in the case of the “homeopathic ritual”² (preparation). Finally, what is typically referred to as an entangled state in quantum mechanics is a maximally entangled state. If the homeopathic remedy and the set of symptoms are entangled, how is it that there are no limits on the potencies of homeopathic remedies? And how is it that two so fundamentally different concepts as a remedy (a material object) and a collection of symptoms (an abstract idea generalized from individual observations) can be entangled at all? Proper quantum entanglement is only possible if all parts of the system have common properties that can be described within the same mathematical framework.

Surprisingly, positive outcomes of RCTs can also be used to confirm the WQT models, as “[...] some trials of non-individualised homeopathic remedies have generated positive results [which] could be due to some surviving relic of entanglement from the production process [...]”.⁴

As a side note, the apparent understanding of quantum physics of Walach, Weingärtner or particularly Milgrom does not inspire much confidence. From various papers, Daniel Chrastina has compiled a list of errors and inaccuracies on his blog,⁹ some of which may be trivial, some of which would shame a second year physics student (such as the claim that quantum mechanics is non-deterministic or giving the units of Planck’s constant as [J/s] in Ref.¹⁰). Continuing in this vein, it should not go unnoticed that regarding WQT, Milgrom writes “Complementarity and indeterminacy are epistemological in origin not ontological”,⁴ which is a serious misquote of the original paper, where it says that “[...] there is no way to argue that complementarity and indeterminacy in weak quantum theory are of ontic rather than epistemic nature.[...] one would expect them to be of rather innocent epistemic origin in many cases.”¹ The difference between the two versions cannot be emphasized enough, as quantum effects such as entanglement are due to the ontic nature (ie not simply to our incomplete knowledge) of complementarity and indeterminacy! In classical physics or in daily life, there are trivial—

epistemological—examples of “entanglement”. For example, two identical candles being lit at the same time and then separated will still burn at the same rate. Thus, an observation of one candle also reveals the state of the other one. But this epistemological kind of entanglement is too trivial for Walach’s or Milgrom’s models of homeopathy, as it lacks many of the required features.

To summarize the above criticisms, it can be concluded that in their present states, the proposed applications of Weak Quantum Theory to the problem of ultra-molecular dilutions in homeopathy are not science, but rhetoric. There is simply no compelling evidence suggesting that a generalized form of quantum entanglement might be a useful concept in the discussion of the mode of action of homeopathic remedies. Unfortunately, with vocabulary borrowed from physics, referenced journal articles and scholarly discussions, journalists and lay readers may get the impression that there is a controversy about or even a cutting-edge-physics explanation of the mechanisms behind the action of ultra-high dilutions.

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Bayes and grade of membership analysis

Sir,

Rutten *et al's* papers on "Bayesian homeopathy" finally introduce some rational insight in how the homeopathic symptoms question could be analysed.^{1,2} I believe this approach should be related to another interesting paper by Davidson *et al* about the existence of the constitutional types and the technique of "grade of membership analysis".³ As Rutten states, "a symptom is an indication for a medicine when it occurs more frequently in the population cured by the medicine than in the rest of the population".¹ The authors propose a clear way to go back from clinical cases to the Materia Medica, with the aim of cleaning and making it more precise so to improve the homeopath's effectivity in daily practice.

It is apparent now that it would be impractical, problematic or impossible to redo the pathogenetic work as many homeopaths claimed during the past decade.⁴ In spite of talking about "cured cases" as the "gold standard" of this Bayesian approach, I would personally prefer to speak in terms of "good responders", "successful homeopathic prescription" or "clearly improved patients", although I am aware of the difficulties and flaws in defining these concepts and applying them objectively. But on the other hand, homeopathy has been mostly built on individual clinical cases with outstanding success.

In homeopathic semiology there are two quite different kinds of symptoms and signs: those which are real symptoms and signs, meaning manifestation of abnormality, and those which are not real symptoms but only "patients' peculiar and individual characteristics" known by different names and schools as "constitutional symptoms" or "sensitive type". Although there is some overlap between both categories, basically the real symptom can be cured or alleviated while the other ones are usually not modified by treatment and actually not intended to be modified but are only an indication of the reactivity of the patient to certain medicines.

In a "classical" model, which could be termed the "exclusive model", patients are seen as belonging to one medicine OR to another one (e.g. Sulphur OR Lycopodium). But very soon in practice, the homeopath encounters a Lycopodium-like patient with desire for salty food or a Sulphur-like patient with a dictatorial character. In order to save the exclusive model, two hypotheses arise: (1) Lycopodium patients could also have desire of salt and Sulphur patients could be dictatorial. (2) The Materia Medica is incomplete and there are unknown medicines matching these patients' pictures. Following the first hypothesis leads to endless adding of medicines to

rubrics and to data corruption. The second hypothesis is problematic because if we follow the individuality idea right to the end, there should be a different medicine for each person in the world which seems implausible.

I believe we should definitively switch towards an "inclusive model" where most of the patients are "impure" from the point of view of the Materia Medica and participate with different "grades of membership" to several medicines.³ This model explains easily why a patient may obtain relief from more than one medicine and gives a satisfactory explanation to the "dictatorial Sulphurs" and "salt craving Lycopodiums". Again, I find the "Bayesian method" proposed by Rutten *et al* the way to a reliably answer to the question whether a "constitutional characteristic" or a symptom should or should be not attributed to a medicine.

In the past few years I have been working with similar ideas to Rutten *et al*, though in not a fine and complete manner. With the data obtained from a previous work⁵ and with the intention to do a pilot proof, I identified 129 "successful homeopathic prescriptions" in 62 bronchial asthma cases, 50 Sulphur and 79 non-Sulphur, and compared the differences between both groups. Because of the small sample and its retrospective character the results don't have much reliability, but they are in a sense surprising and instructive. With the chi square method, symptoms with a significantly ($P < 0.05$) higher frequency of presentation were detected in each group and sensitivity and specificity were calculated for each one. Cumulative chance of being a Sulphur patient or a non-Sulphur patient was calculated with the Bayes formula.

Results are:

Symptoms significantly in favor of Sulphur patient (sensitivity/specificity):

- Fear of high places (14%/96.2%)
- Head perspiration (16%/94.9%)
- Worse from warm (42%/78.5%)
- Cumulative chance of being a Sulphur patient when these symptoms are present: 70%, 88.1%, 98.6%.
- Symptoms significantly in favor of being a non-Sulphur patient (sensitivity/specificity):
- Sympathetic (36.7%/88%)
- Malicious (15.2%/96%)
- Jealous (16.5%/96%)
- Lack of vital heat (43%/92%)
- Reserved (13.9%/98%)
- Cumulative chance of being a non-Sulphur patient when these symptoms are present: 82.9%, 94.8%, 98.7%, 99.8%, 100%.

No significantly difference between groups was found for well known Sulphur symptoms such as:

- Uncover feet
- Desire for open air

Desire for sweets
Aversion to fish
Desire of spicy food
Fear of thieves
Desire for fat

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National Policy on Integrative and Complementary Practices in the Brazilian Health System: steps towards the expansion of homeopathy

Sir,

Homeopathy was brought to Brazil in 1840 by the French doctor Benoit Mure. Since then, it has experienced times of social recognition and a repudiation. Homeopathy had important developments during the first decades of the 20th century, a significant fall in importance in the 1940s and 1950s and a rebirth in the 1970s. During the most recent decades it has increased in importance and recognition: it gained recognition as a medical speciality in 1980 and it was introduced officially as part of the core practices of the Brazilian Health System in 1999.

However, a fundamental step was taken in May 2006 when the *National Policy of Integrative and Complementary Practices* was published by Health Ministry.¹ This Policy embodies principles of the Brazilian health reform movement, underpinned by constitutional prin-

ciples (universality, comprehensiveness, equity, social control and popular participation) and organizational principles (hierarchy of services, decentralization of management and regionalization). Its objectives are to:

- (a) integrate homeopathy and other practices (Acupuncture, Thermalism and Phytotherapy) in the primary care of the whole Brazilian national health system, which serves about 150 million people;
- (b) increase access and contribute towards making the system more effective;
- (c) foment innovative practices that contribute to the sustainable development of communities; and
- (d) strengthen social participation and control.

Development is expected of homeopathy in different areas, including: *assistance*, integration at all levels of the system with emphasis on primary care; *economic support*, quality of medicines, information and good practice; *pharmacy*, creation of pharmacies to produce and distribute remedies; *continuing professional education*, different formats of courses for health professionals, system managers and the general population; *evaluation*, production of protocols and follow-up evaluation; *research*, to put homeopathy on a list of research priorities and support international collaboration.

The Policy was produced following the legal foundations of the Brazilian Constitution National Health System principles, professional and popular movements and WHO recommendations.² These factors give us confidence that homeopathy will grow substantially in importance in the coming years in Brazil.

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Clinical and pathogenetic symptoms

Sir,

I am happy that *Homeopathy* has published a debate on homeopathic pathogenetic trials (provings).¹ I have an observation about the long-standing question of the

clinical symptoms reported in *materias medicas* and repertories, as an argument to dismiss or refute provings.

If one examined the clinical files of homeopathic doctors, one might find thousands of clinical symptoms, and even syndromes or diseases, to add to repertories. These would be "clinical symptoms not present in the pathogenesis".

I believe that most such "clinical symptoms not present in the pathogenesis" arise from the fact that using HPTs original symptoms, homeopathic doctors have ameliorated or cured other symptoms and pathologies. It seems improbable, with perhaps the exception of a "last desperate try", that homeopathic medical doctors have given homeopathic medicine just to try if it works, without some clue arising from an HPT. So such "ab usum in morbi" symptoms are used operationally only in conjunction with the whole person symptoms present in a repertory or *materia medica*.

For these reasons I believe that controlled methods to capture the effect of substances on healthy subjects are the only logical approach to apply Homeopathy surely.

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OBITUARY

JBL Ainsworth

9 September 1919–31 August 2007



The Homeopathic community has lost a great supporter in the passing of JBL Ainsworth FRPharmS FFHom (Hon), who died peacefully in the early hours of Friday, 31 August 2007 at Kingsacre Nursing Home in Devon, the county in which he was born in 1919. He made an outstanding contribution to homeopathic pharmacy for more than 30 years.

John began reading for a degree in Chemistry at Kings College London and Bristol, but with the outbreak of the Second World War his studies were interrupted and he joined the Devon Regiment in Exeter. Commissioned in 1940 he moved to the Staffordshire Regiment and subsequently saw action in North Africa and Italy with the Eighth Army. He returned to the UK in January 1944 and was then posted to northwest Europe shortly after D Day. John

sustained a serious leg wound that was to trouble him for the rest of his life, and was finally invalided out the army during the following year.

John's wife Peggy, whom he married in 1942, had a family connection with Dudley Everitt, a Director of A Nelson and Company, homeopathic pharmacists of Duke Street, London and he secured employment there. He took a 2-year pharmacy course with the aid of a government grant for interrupted studies and joined the register on 22 July 1949, after completing the required pre-registration year at Nelson's. John stayed with the company for many years eventually becoming a Director. Following the tragic death of his two fellow Directors, Mr and Mrs Dudley Everitt in a plane crash in 1972, and the subsequent sale of Nelson's to the Truth Research Foundation John opened his own homeopathic pharmacy on 6 June 1978 in New Cavendish Street, London with a staff of 6. The grant of Royal Warrants to H.M. the Queen and H.M. Queen Elizabeth the Queen Mother soon followed.

From 1978 until John's retirement in April 1989 (when the staff had grown to 56), Ainsworth's Homeopathic Pharmacy prospered and enjoyed considerable professional and public support. The pharmacy served 3 Royal physicians, Dr Margery Blackie, Dr Charles Elliott and Dr Ronald Davey and a host of other influential clients and celebrities as well as members of the public in the UK and overseas.

John supported the work of the British Homeopathic Association enthusiastically throughout his professional life, joining its Council in 1955 and serving as Treasurer for many years, before being elected Life President in 1992. He was also a Council Member of the Homeopathic Trust. John was instrumental in promoting homeopathy at a time when consumers were beginning to ask questions about the safety of orthodox medicines. He organized courses for pharmacists through the BHA and encouraged the OTC supply of homeopathic medicines. John and Peggy contributed to BHA Roadshows around the country; I recall that the events were accompanied by much fun and laughter (for the presenters!) but were also very effective in spreading the idea of responsible and appropriate use of homeopathy. In Europe John was President of the International Homeopathic Pharmacists Committee (CIPH) for 3 consecutive years and UK member of CIPH Scientific Commission