Homeopathy (2009) 98, 122–124

doi:10.1016/j.homp.2009.02.003, available online at http://www.sciencedirect.com

SOCIAL AND HISTORICAL

20 years ago: *The British Homoeopathic Journal*, April 1989

ST Land*

8 High Bungay Road, Loddon, Norwich NR14 6JT, UK

History of potentizing machines

'A brief history of potentizing machines' is by Julian Winston. From 1850 to 1920, coinciding with the rise of the science and technology of the industrial revolution, a number of homeopaths attempted to construct labour-saving machines to create high potencies. Hahnemann gave a clear outline of the methodology of attenuation in Paragraphs 267–271 of the fifth edition of the *Organon*, but spoke only of the 30th centesimal. When his pupil Boenninghausen made a preparation of the 200th, and Von Korsakoff introduced new methods for higher attenuations, Hahnemann corresponded with the latter, but only suggested that, for the sake of 'uniform results', it would be best to stay with the 30th. It was another early experimenter, Julius Casper Jenichen, who believed that it was the *succussion* which gave the drug its strength; and introduced the concept of *potency* rather than simply dilution.

The quest began in earnest for mechanized forms of potentization after Hahnemann's death, all the experimenters believing that they were following him to the letter 'because all the essential points are most scrupulously observed and greatly improved upon, whilst time is economized, and error is next to an impossibility, so perfect are the methods used', as Thomas Skinner wrote. Eight pages are devoted to detailed descriptions of the machines, with many quotations from their originators, giving detailed outlines of the methodology used. There are seven figures of drawings by the designers. Carroll Dunham was one of the first to attempt to mechanize the process, but the result was rather unwieldy, with rather more force used than was necessary. The potencies were given to Smith's Pharmacy in New York City; some of which still exist marked '200D'. Francis Boericke and Adolph Tafel established the still then extant firm of Boericke and Tafel, selling homeopathic medicines; while Boericke developed his own machine. Little is known of its fate and that of the possible potencies produced.

Bernard Fincke was the prime mover of efforts to find less labour-intensive ways of producing high potencies. He experimented with several methods and wrote a small volume *On High Potencies*, published by Tafel. He was granted a patent for a new process, 'fluxion', which involved a continuous water flow. There was much debate about the method, but Thomas Skinner, a convert to homeopathy, developed the Skinner Fluxion Centesimal Attenuator, which was much praised by J T Kent and was still in use at Boericke and Tafel in Philadelphia. A single-vial Skinner machine, made by Skinner himself, was on display at the Faculty of Homeopathy in London. Kent developed his own machine, and many of his potencies were still in use. It is interesting that Skinner completely changed his view about succussion, stating: 'It is now my firm conviction, founded on experience, that it is attenuation, and attenuation only, which is the agent or factor in the dynamization of homoeopathic medicine! and that *succussion has nothing whatever to do with it!*'

With the decline of homeopathy in the United States came a parallel decline in the demand for high potencies. Winston quoted Jeremy Collier from 1941 'We must not let go manifest truths because we cannot answer all the questions about them. It is evidently in just this spirit that a small group of physicians have firmly held to the tremendous therapeutic power of the high attenuation. Relegated to comparative obscurity it has long remained a sleeping giant who should be awakened'. The author considered that, 47 years later, it could still be done.[1]

Potency theory

'Potentization and the peripheral forces of nature' is the reprint of a lecture given by George Adams to the British Homoeopathic Congress in London in 1961. In an introduction, Peter Fisher said that he made no apology for reprinting, from time to time, classical but neglected pieces of work. This one, which aroused great interest at the time, certainly had important implications for the nature of extreme dilutions; implications not widely recognized and not developed, but which instead had fallen victim to our collective short-term memory loss.

*Correspondence: ST Land, 8 High Bungay Road, Loddon, Norwich NR14 6JT, UK.
E-mail: derek.land@electramail.co.uk
Received 17 February 2009; accepted 17 February 2009

There are 10 pages of rather dense philosophical exploration of the phenomenon of potentization. The author's main theme is the need to look beyond the ancient Euclidean way of thinking about space and the reality it contains, to the school of geometry variously known as projective, modern synthetic, or the geometry of position. Although first apprehended by pure mathematicians from the 17th century, it really began to blossom forth in the early 19th: 'Occasionally, scientists of the 19th century – W K Clifford, for example – reflected that cosmic space might after all be 'non-Euclidian', its structure differing from the Euclidian to so slight an extent as to escape our instruments of measurements. But neither this nor Einstein's four-dimensional space–time did more than modify the profoundly Euclidean – I might also call it earthly – way of thinking about space and the realities it contains. This is so taken for granted as to be difficult to describe; few people realize that there is any other way'.

The author proposed that: 'the forces of nature, manifesting in the world of space and time, are not only centric; there are peripheral forces also'. He referred to the medical work of Rudolph Steiner *Fundamentals of Therapy*, written in conjunction with Ita Wegman, in which he described the ethereal formative forces of the human and other living organisms as in their essence 'peripheral forces'. He distinguished between the forces – manifested above all in the lifeless realm – emanating from material centres, and another kind of force, working not outward from any earthly centre but inward from the periphery, generally from the surrounding cosmos. In spatial character he described them succinctly as 'forces which have not a centre but a periphery'. Adams developed his theme thus: 'The ethereal or peripheral forces, in the nature of the case, have more to do with living growth and development, with the 'becoming' of things. If there were only rigid and finished forms the old Euclidian geometry might suffice us. To understand the genesis and metamorphosis of living forms we need a more mobile thinking, and one that reveals the balance between the central and peripheral, architectural and plastic aspects'. In summing up, the author stated: 'The seemingly unsurmountable division between an orthodox scientific outlook and realms of human skill and experience which find no place in the accepted system of the day, is overcome without injustice to either party when a fresh aspect springs into focus'. He believed this was about to happen, and that in it, homeopathy would find new life and vindication.[2]

Choice of potency

'Choice of potency homoeopathic prescribing' is a five-page translation of a German text which Adolf Voegeli had published in 1988 in his 90th year. There is much useful advice on the conduct of both acute and chronic conditions, with very specific indications of potencies required. It is given in a rather quaint style, with reference to Korsakovian, Hahnemannian and 'q' (LM) potencies (ones which permit homeopathic medicines to be repeated more frequently and over a longer period of time than is possible with Korsakovian and Hahnemannian potencies). There is a list of the circumstances in which 'q' potencies are particularly indicated.[3]

Report on homeopathic research

'Report on the status of research on homoeopathy with recommendations for future research' is a comprehensive 10-page assessment of the subject by Beverley Rubik, Research Director of the Foundation for Homoeopathic Education and Research, California. The author stressed the importance of preparation and quality control of homeopathic medicines, and the need for quality assurance procedures on water and containers used. She suggested that *The United States Pharmacopoeia* should be consulted, stating: 'One possible reason for lack of replicability in homoeopathic research studies may be slight differences between homoeopathic preparations due to a lack of complete standardization. A protocol of the steps and processes used in the experimental preparations or supplied by pharmaceutical companies should be elaborated in the research report with sufficient detail so that replication by others is possible'. Two pages discuss possible methods for investigation of potency by studying solvent vibratory energy, with either infrared or NMR spectroscopy; while there was hope in developments of quantum theory, and other developments such as non-equilibrium thermodynamics and the physics of synergetics. These ideas were discussed in the 1987 book *Scientific Foundations of Homoeopathy* by Resch and Gutmann.

The remainder of the report cites all the main studies performed in the following areas: enzymes to demonstrate signal transmission from drugs; yeast, bacteria and blood as target systems; multi-cellular organism studies; and clinical studies. The various experiments conducted in these areas are discussed and assessed. Recent reviews of the literature by Kollerstrom and Scofield had concluded that very few studies could survive critical assessment. The author outlined five important experimental design criteria, and stated that all the criteria could be met by using micro-organisms which could be easily, quickly, and inexpensively grown, manipulated to be generically identical, and studied in large numbers such as millions per millilitre of culture medium. She referred at length to the studies on yeast by Jones and Jenkins, replicated by Steffen, then Baker and Smith; stressing again how strict attention to details was required for a true replication.

Two of the multi-cellular organism studies were singled out: the study by Singh and Gupta on chick embryos showing possible antiviral activity by homeopathic drugs held great promise, and Khanna and Chandra's drug control of fungal blight in wheat could lead to novel applications in agriculture. These would warrant careful replication. Other important areas showing some success were the studies on diabetes in mice, and the effects of remedies on poisoning with heavy metals. However, most success seemed to have been had in clinical studies. Replication by other scientists was important to increase confidence in these results.

In conclusion, the author recommended new research work on the succussion effects on solvent; on developing a rapid and sensitive microbial or cell culture system; and on clinically well-defined acute conditions in animals or humans that would respond rapidly to treatment. Finally, replication of some of the key studies mentioned herein with attention to strict controls would be highly recommended as well.[4]

Homeopathy in Leningrad

'A visitor's view on homoeopathy in Leningrad, USSR' is a glimpse of homeopathy in Russia by Mervi Moisander-Webster. Homeopathy was much in evidence on the busiest central street, Nevsky Prospect, with a bright blue 'Homoeopathic Pharmacy' sign. There were five large homeopathic polyclinics in the area (some as large as hospitals), but the relationship with the state seems, to have been a strange one. Homeopathy flourished before the 1917 Revolution, enjoying special protection and with a large homeopathic hospital. On requesting homeopathic books, the author was asked 'Do you wish books before or after the Revolution?' Older books were available, after special permission, at a smaller library, showing the hospital with its various clinics and laboratories; books which seem to have been in good use before the Revolution. The reader was met with patience and interest, so unusual in customer–client contacts in the Soviet Union. Requests for 'free' information in the clinics were met with astonishment, where even the things that you pay for were often scarce.

Over the years, attempts were made to form homeopathic medical societies, but they had checquered careers, with eventually in 1978 a clampdown on such associations, along with homeopathic publications. However, the author did discover D A Kogan's *Homoeopathy and Modern Medicine* (1964), and a brand new book by T D Popova *Essays in Homoeopahy*: 'an excellent book for both the doctor and patient, with humour and deep understanding of human sensitivity and individuality'. There were no official bilateral agreements with the Soviet Union in place, although it was hoped for soon; but it was the author's impression that contacts on a personal level were very much sought after: 'How very interesting it would have been to have someone medically qualified from Great Britain talking to us here and exchanging views with us!'

The author found it difficult to judge whether the homeopathic atmosphere was easing up or not, but a lot was going on in Soviet society in general, and the 70 homeopathic physicians at Praga Street were looking forward to the future with new courage and 'new wind under their wings' as the Polyclinic prepared to celebrate its 120th anniversary. How quickly things might have opened out with the momentous events occurring in the Soviet Union so soon after this article was written![5]

References

1 Winston J. A brief history of potentizing machines. *Br Hom J* 1989; **78**: 57–68.
2 Adams G. Potentization and the peripheral forces of nature. *Br Hom J* 1989; **78**: 69–79.
3 Voegeli A. Choice of potency homoeopathic prescribing. *Br Hom J* 1989; **78**: 80–85.
4 Rubik B. Report on the status of research on homoeopathy with recommendations for future research. *Br Hom J* 1989; **78**: 86–96.
5 Moisander-Webster M. A visitor's view on homoeopathy in Leningrad, USSR. *Br Hom J* 1989; **78**: 106–108.

Homeopathy (2009) 98, 125–126
© 2009 The Faculty of Homeopathy
doi:10.1016/j.homp.2009.02.005, available online at http://www.sciencedirect.com

REVIEWS AND ABSTRACTS

International press abstracts

Cina

Jean Alaerts

The author describes the materia medica of this well-known remedy. The patient is often a child who is touchy, does not want to be cuddled, touched or looked at. He wants to be carried over the shoulder, but unlike *Chamomilla*, carrying brings no relief. He throws things at you (Chamomilla: everywhere) and unlike *Chamomilla*, tries to hit.

The main physical symptoms are headaches, hypersensitivity of scalp, yellow or green vision, pruritus of nose, twitching of muscles of the face, refuse mother's milk, intolerance of milk, great hunger or capricious appetite, troubles from worms, cough or convulsions from being scolded or punished or during fever, hypersensitivity to pain.

Alaerts J. Matière médicale de *Cina*. *Revue Belge d'Homoeopathie* Juin 2008; **2**: 76–85.

Gentiana cruciata

Marie-Luc Fayeton

This paper starts with the story of a young boy, 29 months old, who did not talk, and who still was not toilet trained. He liked milk and chocolate, but detested fruits. He wanted attention and bit his brother. His appetite was improved following an infusion of gentian. "Disinclination to talk" led to *Gentiana cruciata*, which was prescribed in 1 MK then 10 MK with good results.

The main symptoms are (Clarke): weeping mood with disinclination to talk, apprehensiveness, difficult swallowing, constant hawking of tenacious mucus, sensation of movement in the stomach and abdomen, disappearing on walking; sensation of protrusion in right inguinal region, drawing in cervical muscles; difficult walking.

Fayeton M-L. *Revue Belge d'Homoeopathie* 2008; **4**: 13–15.

Iridium metallicum

Patricia Le Roux

A clinical case of a young boy, 10 years old, is given in the introduction. The boy had testicular pains, and was worried about his father, a famous cook, whose restaurant was threatened. He was at the summit, but he had to suppress a last obstacle to his continuation of his job. This was the theme of *Iridium*, which cured this case.

The materia medica of this remedy is summarized: affectionate, anxiety as if something going to happen, hurry, feeling of being separated, feeling of having two wills, children growing too fast, anaemia, numbness, watery or purulent coryza, hoarse cough, pain with swelling in ovaries or testes, weakness of cervical region, dislocated feeling in left hip.

Leroux P. *Cahiers du Groupement Hahnemannien* 2008; **2–3**: 159–168.

Biographie de Madeleine Bastide

Jean-Marie Bastide

In a special issue devoted to Professor Madeleine Bastide, her husband recounts her life history and the work of this outstanding woman. She was born in 1935 and died in 2007. She was professor of immunology in Montpellier University of Pharmacology, and did research in immunology, mycology, and in homeopathy. She founded GIRI (Groupe International de Recherche sur l'Infinitésimal) with Dr René Halm in 1987. This group now consists of more than hundred researchers from 22 different countries. Concerning homeopathy, we must quote the following works: effects of embryonic bursectomy and in-ovo administration of highly diluted bursin on adrenocorticotropic and immune responses of chickens.[1] Specific abolition reversal of pituitary-adrenal activity and control of the humoral immunity in bursectomized chickens through highly dilute bursin,[2] and the effects of low doses and homeopathic potencies on lymphocytes.[3]

References

1 Youbicer-Simon BJ, Boudart F, Mekaouche M, Bastide M, Baylé JD. *Int J Immunother* 1993; **9**: 169–180.
2 Youbicer-Simon BJ, Boudart F, Mekaouche M, Bastide M, Baylé JD. *Int J Immunopathol Pharmacol* 1997; **9**: 43–51.
3 Walchli C, Baumgartner S, Bastide M. *J Altern Complement Med* 2006; **12**: 42–67.

Bastide JM. *Cahiers du Groupement Hahnemannien* 2008; **2–3**: 67–75.

Beryllium

Dominique Viola

Berylium is fully discussed in this article. *Berylium* is toxic and can provoke syndromes similar to sarcoidosis, pneumonia or lung cancer. The main psychic symptoms are: sensitivity to critic, irresolution and passivity, yielding, self - protection by being unobtrusive and unassuming. Other symptoms include: stooped head and shoulders, dryness of skin and mucous membranes, cancer (bones, lungs), recurrent infections (kidneys, lungs), blocked nose, nosebleed, thyroid affections, cardiovascular diseases (angina pectoris, feeling there is not enough room for the heart), itching of the back, formication, retarded growth or arrested development.

Viola D. *Revue Belge d'Homoeopathie* 2007; **2**: 54–65.

Collinsonia canadensis

Yves Faignaert

By way of introduction the author describes this plant and its herbal use (for digestive, urinary and respiratory problems). Then, four clinical cases are reported: a child suffering from constipation, a woman with a meniscal lesion, a pregnant woman with piles, and another woman with colitis.

The article concludes with the materia medica of this medicine. The main psychic symptoms are: melancholy, irritability, irresolution, need for protection, feeling of culpability, of being forsaken, cannot talk. Other symptoms include: feeling of enlargement of various parts and organs of the body, constipation with rectal pains, hemorroïds, and pelvic congestion. Also speaker's sore throat, cough from excessive use of voice, heart disease and hot flushes both with piles.

Faingnaert Y. *Revue Belge d'Homoeopathie* 2007; **2**: 66–111.

Colchicum

Philippe Marchat

This article starts by recalling the main symptoms of *Colchicum*: digestive troubles, such as pains in the transverse colon and distension of abdomen, autumnal diarrhoea, gout and erratic joint pains or joint inflammations, the patient cannot bear to have them touched or moved, sensitivity to the cold and to the dampness. There is great hypersensitivity to odors, to pain, and some psychic problems, particularly a fear to be excluded, and consequently the need to keep one's place.

A clinical case illustrates this remedy: a woman who felt rejected both by her husband and by her employers.

Marchat P. *L'Homéopathie Européenne* 2007; **2**: 17–19.

The betrayal of *Radium Bromatum*

Philippe Colin

At the 1997 British Homeopathic Congress, Jonathan Shore illustrated *Radium Bromatum* with several clinical cases. This article recalls the materia medica and the main themes of this medicine.

The most important somatic symptom are dermatological and rheumatological: acne rosacea, naevi, psoriasis, sclerodema, epithelioma. Characteristic symptoms include: itching and burning all over body; pains in cervical, lumbar and sacral spine, pains in arms and legs, as though they would break on moving, sensation of weakness between the scapulae and the lumbo-sacral region, a sensation of general heaviness, especially in arms and legs. There is aggravation by motion, and improvement in the open air, from continued motion and hot bathing.

The themes of this medicine, according to Jonathan Shore, are a feeling of general heaviness, and above all of being stabbed in the back. I have used *Radium Bromatum* on these indications in my practice, and found these indications reliable. Another use is for patients having radiotherapy, to alleviate side effects, or allowing other medicines to act in patients who have been treated by radiotherapy.

Colin P. *L'Homéopathie Européenne* 2007; **2**: 21–24.

French – language journals
reviewed by Philippe Colin

Homeopathy (2009) 98, 127–133

doi:10.1016/j.homp.2008.12.003, available online at http://www.sciencedirect.com

LETTERS TO THE EDITOR

Analysis of a re-analysis of a meta-analysis: in defence of Shang *et al.*

Rutten and Stolper[1] re-analysed the data used in the landmark Lancet meta-analysis[2] of trials of homeopathy and conventional medicine. However, their approach to this work seems to have been influenced by a belief that the Shang analysis was deliberately skewed against homeopathy, and in favour of conventional medicine. I argue here that the evidence does not support that contention, and that the re-analysis by Rutten and Stolper does not show that the Shang *et al.* study was invalid.

In the abstract of their paper, Rutten and Stolper state *"There is a discrepancy between the outcome of a meta-analysis published in 1997 of 89 trials of homeopathy by Linde et al. and an analysis of 110 trials by Shang et al. published in 2005, these reached opposite conclusions"*, and on page 170 they write *"The contradiction between Linde's conclusion based on 89 trials, and Shang et al.'s conclusion, based on 110 trials seems odd"*. But there is nothing particularly surprising about this discrepancy. The Linde paper referred to was published in the Lancet in 1997.[3] The same team re-analysed the data in a paper published in 1999.[4] They concluded that because trials of higher methodological quality had smaller effect sizes, and that because a number of newly published high-quality trials showed negative results for homeopathy, their meta-analysis had over-estimated the effectiveness of homeopathy. Hence there is no reason to see the discrepancy between Shang *et al.* and Linde *et al.* as being particularly "odd".

Rutten and Stolper make statements about the "pre-specified hypotheses" of the Shang *et al.* study, but these are not consistent through the paper. In the introduction, they state:

> *"The hypotheses predefined mentioned in the introduction of Shang et al's paper were: 'Bias in conduct and reporting of trials is a possible explanation for positive findings of placebo-controlled trials of both homeopathy and allopathy (conventional medicine)'; and: 'These biases are more likely to affect small than large studies; the smaller a study, the larger the treatment effect necessary for the results to be statistically significant, whereas large studies are more likely to be of high methodological quality and published even if their results are negative'".*

Yet, in Rutter and Stolper's section on "Pre-specified hypotheses" they include *"quality in homeopathy is worse than in conventional medicine"* as a hypothesis of Shang *et al.*, and say that this hypothesis was falsified in the Shang *et al.* study. This is a straw man: it is not a hypothesis that was discussed in the Shang *et al.* paper, and Rutten and Stolper have missed the point of including a matched set of trials of conventional medicine. As Rutten and Stolper state (p. 170) *"Pooling of results is...questionable if homeopathy works for some condition and not for others"*. This is a reasonable point. However, it is clear that some experimental conventional treatments work and some do not. The results of the analysis of conventional medicine were not consistent with the placebo hypothesis, showing that it is possible to obtain a positive result using the methods of Shang *et al.*, even there is considerable heterogeneity in the results.[5]

Rutten and Stolper claim that the sub-sets of larger, higher quality studies were chosen *post hoc*, presumably to make homeopathy appear less effective than it really is. In their paper, Rutten and Stolper state [pp. 172–173]:

> *"Cut-off values for sample size were not mentioned or explained in Shang el al.'s analysis. Why were eight homeopathy trials compared with six conventional trials? Was this choice predefined or post-hoc? Post-publication data showed that cut-off values for larger higher quality studies differed between the two groups. In the homeopathy group the cut-off value was n = 98, including eight trials (38% of the higher quality trials). The cut-off value for larger conventional studies in this analysis was n = 146, including six trials (66% of the higher quality trials). These cut-off values were considerably above the median sample size of 65. There were 31 homeopathy trials larger than the homeopathy cut-off value and 24 conventional trials larger than the conventional cut-off value. We can think of no criterion that could be common to the two cut-off values. This suggests that this choice was post-hoc".*

The first thing to note is that it is not true that cut-off values for sample size were not mentioned or explained in the Shang *et al.* analysis. In the original Shang paper, on page 728, it is stated that *"Trials with SE* [standard error] *in the lowest quartile were defined as larger trials"*. In other words, the cut-off was not defined in terms of numbers of subjects, but in terms of standard error. It might be argued that this is a strange way of defining "larger" trials (and perhaps it should have been phrased as "lower standard error"). But it makes sense when criteria must be stated *a priori*. If a number of subjects were stated as a cut-off value, there would be no way of knowing how many studies would meet that criterion before looking at the data. You might find that a very large or very small number of studies met the criterion, making further analysis difficult. So, there is no mystery as to why the "cut-off values" were different between trials of homeopathy and trials of conventional medicine: it is because the distribution of standard errors is different between the two populations. This could be discovered simply by reading the original paper, and the conclusion that the groups were chosen *post hoc* cannot be sustained.

A further point here is that the group of "larger" homeopathy trials contains smaller trials that would not have made the cut for "larger" trials in the conventional medicine group. Those smaller trials are more likely to show spurious positive results. It follows that had the authors engineered the groups to get the result they wanted, they had engineered them in favour of homeopathy.

Another paragraph in Rutten and Stolper states "We did not further investigate possible selection bias by excluding trials, but we were surprised by the exclusion of Wiesenauer's trial on chronic polyarthritis. This was a larger trial ($n = 176$), of good quality according to Linde, with positive results. This trial would have contributed positively to the outcome of the larger higher quality trials. Shang excluded this trial because no matching trial could be found" (page 171). Since the trial was excluded on the basis of the clearly stated, pre-specified exclusion criteria, what is surprising about it having been excluded? Including it would have made a nonsense of the design of the study and violated the pre-specified exclusion criteria, and therefore would have been a gross error.

Rutten and Stolper conduct a sensitivity analysis, but, as they note, the decisions they make in this analysis are highly subjective. They decide to exclude all trials of homeopathy for muscle soreness,[6–9] on the grounds that "*treatment of healthy individuals is very rare in homeopathic practice* [and] *this outcome has low external validity to judge the effect of homeopathy as a method*" (page 173). Yet, the trials were conducted with the participation of prominent homeopaths, and some were published in homeopathic or alternative medicine journals,[8,9] so at least some homeopaths seem to be of the opinion that there is enough external validity for it to be worth conducting a trial. So how can the external validity of the trials be judged in a transparent way? In a meta-analysis based on clear, pre-specified criteria, there could be no justification for omitting the studies.

It is also notable that one of the authors was a co-author of another re-analysis published in the *Journal of Clinical Epidemiology*.[10] That analysis showed that if random-effects meta-analysis is used, it is possible to add smaller trials to Shang's set of "larger, higher quality" trials of homeopathy, and get a statistically significant (although clinically unimpressive) benefit for homeopathy. All this really shows is that a finding in favour of homeopathy is not robust, and as Shang *et al.* showed, including smaller trials also decreases the reliability of the findings. The re-analysis also showed that the benefit for homeopathy was statistically insignificant when a meta-regression analysis was used: this negative finding was strangely not mentioned in the *Homeopathy* paper. Because the results differed between meta-regression and random-effects analyses, and because Shang *et al.* showed highly significant evidence of funnel-plot asymmetry in their complete dataset of 110 trials of homeopathy, it is arguable that meta-regression analysis is a more appropriate choice.

Overall, it is clear that "another outcome" (i.e. one favourable to homeopathy) is possible, as long as negative studies are excluded without good reason, smaller and less reliable studies are included, and a particular method of statistical analysis is used. In a paper that (wrongly) criticises a study for analysing data based on criteria established *post hoc*, this seems like an odd point to make.

The analysis by Rutten and Stolper contains misconceptions concerning Shang *et al.*, some important errors, and does not show that it was an invalid analysis. In particular, there is no evidence that the Shang *et al.* study involved *post hoc* choice of subgroups. The results of meta-analyses can be debated, but scientists should not be accused of research misconduct on the basis of no evidence, or on the basis of having failed to read their work properly.

References

1 Rutten ALB, Stolper CF. The 2005 meta-analysis of homeopathy: the importance of post-publication data. *Homeopathy* 2008; **97**: 169–177.
2 Shang A, Huwiler-Müntener K, Nartey L, *et al.* Are the clinical effects of homeopathy placebo effects? Comparative study of placebo-controlled trials of homeopathy and allopathy. *Lancet* 2005; **366**: 726–732.
3 Linde K, Clausius N, Ramirez G, *et al.* Are the clinical effects of homeopathy placebo effects? A meta-analysis of placebo-controlled trials. *Lancet* 1997; **350**: 834–843.
4 Linde K, Scholz M, Ramirez G, Clausius N, Melchart D, Jonas WB. Impact of study quality on outcome in placebo-controlled trials of homeopathy. *J Clin Epidemiol* 1999; **52**: 631–636.
5 Shang A, Jüni P, Sterne JAC, Huwiler-Müntener K, Egger M. Are the clinical effects of homeopathy placebo effects? A meta-analysis of placebo-controlled trials: author's reply. *Lancet* 2005; **366**: 2083–2084.
6 Vickers AJ, Fisher P, Wyllie SE, Rees R. Homeopathic Arnica 30X is ineffective for muscle soreness after long-distance running – a randomized, double-blind, placebo-controlled trial. *Clin J Pain* 1998; **14**: 227–231.
7 Vickers AJ, Fisher P, Smith C, Wyllie SE, Lewith GT. Homoeopathy for delayed onset muscle soreness – a randomised double blind placebo controlled trial. *Brit J Sports Med* 1997; **31**: 304–307.
8 Jawara N, Lewith GT, Vickers AJ, Mullee MA, Smith C. Homoeopathic Arnica and Rhus toxicodendron for delayed onset muscle soreness – a pilot for a randomized, double-blind, placebo-controlled trial. *Br Homeopath J* 1997; **86**: 10–15.
9 Tveiten D, Bruset S, Borchgrevink CF, Norseth J. Effects of the homeopathic remedy Arnica D30 on marathon runners: a randomized, double-blind study during the 1995 Oslo Marathon. *Complement Ther Med* 1998; **6**(2): 71–74.
10 Lüdtke R, Rutten ALB. The conclusions on the effectiveness of homeopathy highly depend on the set of analyzed trials. *J Clin Epidemiol* 2008; **61**: 1197–1204.

Paul Wilson
University of Manchester,
Manchester, UK
E-mail: paul.wilson@manchester.ac.uk

Reply to Wilson

Wilson's main objection is our suspicion of post-hoc hypothesising by Shang *et al.* Wilson states that larger trials were defined by Shang as "*Trials with SE* [standard error] *in the lowest quartile were defined as larger trials*". According to Wilson this was done to predefine 'larger trials'. We agree with Wilson that this is indeed a strange way of defining 'larger trials', but it is perfectly possible to simply define larger studies a priori according to sample size in terms like 'above median' as we suggested in our paper. Shang *et al* did not mention the sensitivity of the result to this choice of cut-off value: if median sample size (including 14 trials) is chosen homeopathy has the best (significantly positive) result, if eight trials are selected homeopathy has the worst result. In the post-publication data they mentioned sample sizes but not Standard Errors. Isn't it odd that the authors did not mention the fact that homeopathy is effective based on a fully plausible definition of 'larger' trials, but stated that it is not effective based on a strange definition of 'larger', but that this was not apparent because of missing data?

We questioned the possibility of true pre-specification of hypotheses regarding homeopathy in our introduction. The director of Shang *et al.'s* analysis, Egger, analysed Linde's data in a paper published in 2001, introducing the hypothesis that low quality, especially in smaller trials, could bias results in homeopathy.[1] On the other hand, smaller good quality trials can produce stronger effects by better selection of patients. In that case the asymmetry of the funnel plot is not caused by bias and extrapolation towards the largest trials by meta-regression analysis is questionable. We think that the authors should have referenced to this paper in the introduction instead of in the methods section, because this was the logical-starting-point for Shang's analysis.

We were indeed amazed that no matching trial could be found for a homeopathic trial on chronic polyarthritis by Wiesenauer. Shang did not specify criteria for matching of trials. We would expect the authors to explain this exclusion because Wiesenauer's trial would have made a difference in meta-regression analysis and possibly also in the selection of the eight larger good quality trials.

Wilson's remark about prominent homeopaths choosing muscle soreness as indication is not relevant. Using a marathon as starting point for a trial is understandable from an organisational point of view, although doubt is possible about external validity. Publishing negative trials in alternative medicine journals is correct behaviour. There is, however, strong evidence that homeopathic Arnica is not effective after long distance running and homeopathy as a method should not be judged by that outcome.[2]

In the Homeopathy paper we did mention that meta-regression analysis showed no difference between homeopathy and placebo and referred to our objections against this method of meta-regression in the Journal of Clinical Epidemiology,[3] which were: "*First, the asymmetry of funnel-plots is not necessarily a result of bias. It can also occur when smaller studies show larger effect just because they were done in a condition with high treatment effects, and thus requiring smaller patient numbers. Moreover, meta-regression predicts the OR at an extreme value (the minimum standard deviation observed).*

It is well known from mathematical statistics, that these predictions are imprecise, especially when the number of observations is small and the estimate of the regression line is unstable. This is the case in our analyses. For example, the funnel plot of the four largest trials was negatively skewed (AC = 0.13), whereas it was positively skewed for the five largest trials (AC = 1.97). Thus, our meta-regression analyses generally suffer from low statistical power".

Furthermore, Shang *et al.* disregarded safety. The fact that meta-regression in conventional medicine did not extrapolate to ineffectiveness was much influenced by three therapies that are not available because of serious adverse effects. As stated above, the comparative meta-regression analysis was questionable because of the difference in quality in smaller trials between homeopathy and conventional medicine. This difference was not mentioned in Shang's paper.

As stated in our paper, the difference in quality and sample size regarding the homeopathy and conventional trials on muscle soreness was an important factor in the different outcome for homeopathy and conventional medicine. One ineffective indication with four trials was included in homeopathy and excluded in conventional medicine. Further influences on the different outcome are different quality, different safety and different publication bias.

The hypotheses stated by Shang *et al.* in their introduction were not as clear as the hypothesis posed by Sterne, Egger and Davey-Smith in 2001.[1] What other hypothesis could be proven by this comparison with a matched set of conventional trials than that quality in homeopathy trials is worse than in conventional trials? We showed that matching was lost if this hypothesis about quality was abandoned to compare effects. The conclusion that homeopathy is a placebo effect and that conventional medicine is not was not based on a comparative analysis of carefully matched trials, as stated by the authors.

References

1 Sterne JAC, Egger M, Smith GD. Investigating and dealing with publication and other biases in meta-analysis. *BMJ* 2001; **323**: 101–105.

2 Ernst E, Barnes J. Are homoeopathic remedies effective for delayed onset muscle soreness? A systematic review of placebo-controlled trials. *Perfusion* 1998; **11**: 4–8.

3 Lüdtke R, Rutten AL. The conclusions on the effectiveness of homeopathy highly depend on the set of analyzed trials. *J Clin Epidemiol* 2008; **61**: 1197–1204. doi:10.1016/j.jclinepi.2008.06.015.

ALB Rutten
Breda, The Netherlands
E-mail: lexrtn@concepts.nl

CF Stolper
Heerde, The Netherlands

Problematic statistical analyses in Zacharias *et al.*

In *Homeopathy* July 2008, Zacharias *et al.* evaluate the effects of homeopathic treatment on sheep carrying the parasite *Haemonchus contortus*.[1] Their paper's Table 1 comprises 3 correlation matrices, each showing 37 correlation coefficients that quantify the correlations between assorted physiological variables of the sheep. They use some of these correlations to try to attribute positive effects to the homeopathic treatment.

Unfortunately, their attempt is stymied by several problems: (1) transcription errors; (2) dubious significance testing of correlations; (3) failure to account for doing multiple significance tests on correlations derived from the same dataset; and (4) comparison of the treatment group's correlations with zero and not the corresponding correlations in the other groups.

For two correlation coefficients, Zacharias *et al.* give two different values, one in their main text and the other in its Table 1:

- The homeopathy group's FEC (fecal egg count)–Hb correlation is −0.893 in the main text, but in Table 1 it's −0.834. (A neighbouring cell in the table displays an Hct–Hb correlation of 0.893; Zacharias *et al.* appear to have quoted this latter number instead, mistakenly prefixing it with a minus sign).
- The anthelminthic group's FEC–Hb correlation is −0.039 in the main text and −0.399 in Table 1.

The inconsistencies render the correlation coefficients' true values uncertain. For the purposes of this reanalysis, I took those presented in Zacharias *et al.*'s Table 1 as correct, and ignore those in the paper's main text.

Several times, the Zacharias *et al.* paper explicitly claims that a treatment group's correlation coefficient is statistically significant, or treats a correlation coefficient as significant by saying it 'suggest[s]' a causal effect.

Where correlations are taken as evidence of real treatment effects, it should be on the basis of rigorous significance testing. Consequently, to verify Zacharias *et al.*'s correlation-based claims of the homeopathy's efficacy, I attempted to replicate their significance tests.

I consulted the statistical analysis subsection of their paper's materials and methods section, which mentions that "*[c]orrelation between values from different parameters was calculated for each group using Pearson's correlation coefficient*". It gives no other details regarding the correlations, so it's unclear which significance test they used. (I have emailed Dr Mendonça-Lima, the corresponding author, to ask how significance was tested, but have not received a reply).

The format of Zacharias *et al.*'s Table 1 suggests that they tested each correlation individually for whether it differed significantly from zero. There are two standard procedures for carrying out this test on a Pearson correlation coefficient r, given the number of sample pairs N. One can transform r to a t-value $\left(t = r\sqrt{N-2}/\sqrt{1-r^2}\right)$ and, taking $N-2$ as the number of degrees of freedom, test that t-value for significance.[2] Alternatively, one can use the Fisher transformation to obtain a z-value ($z = \operatorname{arctanh} r$), take $(N-3)^{-1/2}$ as its standard error, and perform a z-test.[3] As I didn't know which (if either) procedure Zacharias *et al.* used, I calculated both t- and z-values for the correlations that Zacharias *et al.* interpret as it significant (Table 1).

As my Table 1 shows, Zacharias *et al.*'s reported p-values for the homeopathy and control groups' FEC–haematocrit and FEC–Hb correlations seem not to have come from a standard test. For the homeopathy group, the correct p-values are not less than 0.01, but are all at least 0.016. For the control group, they are unambiguously insignificant, with p-values of 0.401 or 0.432 (Zacharias *et al.* claim '$p < 0.05$') and 0.185 or 0.205 (Zacharias *et al.* claim '$p < 0.01$'). The correlation cited as 'suggesting' that the homeopathy "*stimulated the production of circulating eosinophils* (EOS)" is insignificant ($p > 0.4$), as is the correlation cited as 'suggesting' a superior immune response in the homeopathy group ($p > 0.1$).

Using one-tailed or two-tailed tests makes little difference. Switching to a one-tailed test simply halves all of the p-values, which brings the FEC–Hb correlation in the homeopathy group into line with Zacharias *et al.*'s statement that it's less than 0.01. All of the other p-values, however, remain greater than 0.05.

There is another oddity. For the homeopathy group, the FEC–whole proteins correlation is −0.475, which is

Table 1 r-values reported in Zacharias *et al.*'s Table 1, their equivalent t- and z-values, and the p-values that result from performing two-tailed t- and z-tests on those values

Group	*Variables*	r	t	z	p_t	p_z
Homeopathy	FEC, haematocrit	−0.656	−1.943	−0.786	0.110	0.116
Homeopathy	FEC, Hb	−0.834	−3.380	−1.201	0.020	0.016
Control	FEC, haematocrit	−0.425	−0.939	−0.454	0.401	0.432
Control	FEC, Hb	−0.624	−1.597	−0.732	0.185	0.205
Anthelmintic	FEC, haematocrit	−0.412	−1.011	−0.438	0.358	0.381
Anthelmintic	FEC, Hb	−0.399	−0.973	−0.422	0.375	0.398
Homeopathy	EOS, FEC	−0.344	−0.819	−0.359	0.450	0.473
Homeopathy	Globulin, IgG	0.610	1.721	0.709	0.146	0.156
Control	Globulin, IgG	0.239	0.492	0.244	0.648	0.673
Anthelmintic	Globulin, IgG	0.255	0.590	0.261	0.581	0.602

Table 2 Comparison of sample correlations from Zacharias *et al.*: r_h, r_c, and r_a are the correlations in the homeopathy group, control group, and anthelmintic group respectively, and z_h, z_c, and z_a their Fisher transformations. p_{hc} and p_{ha} are *p*-values for two-tailed tests of whether the homeopathy group's correlation differs significantly from the corresponding correlation in, respectively, the control group or the anthelmintic group

Variables	r_h	r_c	r_a	$\lvert z_h - z_c \rvert$	$\lvert z_h - z_a \rvert$	p_{hc}	p_{ha}
FEC, haematocrit	−0.656	−0.425	−0.412	0.332	0.348	0.66	0.62
FEC, Hb	−0.834	−0.624	−0.399	0.470	0.779	0.54	0.27
EOS, FEC	−0.344	−0.132	−0.142	0.226	0.216	0.77	0.76
Globulin, IgG	0.610	0.239	0.255	0.465	0.448	0.54	0.53

marked insignificant. For the anthelminthic group, the whole proteins–Hb correlation is 0.457, which is marked significant. But both groups are the same size, and so if a correlation in one group is significant, a correlation in the other group with a higher absolute value should also be significant. This isn't the case for the two correlations quoted: if the anthelmintic group's 0.457 correlation is significant, the homeopathy group's −0.475 correlation is also, but the paper says otherwise.

No standard test of individual Pearson correlations would have significance levels that shift in such a way or the deflated *p*-values that Zacharias *et al.* reported. At best, they have used an unusual significance test with a high type I error rate.

So far I, like Zacharias *et al.*, have used a significance level of 0.05 when testing correlations. This significance level is in fact too high, as both Zacharias *et al.* and I are performing repeated significance tests on the same dataset, which inflates the risk of type I error far beyond the nominal 0.05 level. With 111 significance tests (one for each of their reported correlations), a nominal significance level of 0.05 means a more than 99% chance of at least one test giving a *p*-value below 0.05, even with all 111 correlations being zero in the population. Under the null hypotheses, to maintain a real significance level of 0.05 over all 111 significance tests, a nominal significance level of 0.00046 is necessary. None of the correlations come close to reaching this.

Being less stringent hardly helps. If I consider only the five correlations that Zacharias *et al.* allege are significant (the FEC–haematocrit, FEC–Hb, and globulin–IgG correlations in the homeopathy group and the FEC–haematocrit and FEC–Hb correlations in the control group) and put aside the other 106, the nominal significance level need only be reduced to 0.01 instead of 0.00046. None of the five correlations are significant at the 0.01 level.

Often, when a correlation is calculated for a study, the question of interest is whether the correlation is statistically distinguishable from zero or not. However, Zacharias *et al.* try to use correlations to determine whether homeopathic treatment is more effective than no treatment and an anthelmintic, and what the homeopathy's unique physiological effects are. This implies that they wish to compare the homeopathy group to the other two groups, not to imaginary benchmarks of zero correlations. This means they tested the wrong null hypotheses: the pertinent null hypotheses are whether the homeopathy group's correlations differ from *those of the other two groups*, not whether they differ from *zero*.

Testing the correct null hypotheses reveals that the homeopathy group correlations that Zacharias *et al.* treat as significant are less significant than Table 1's *p*-values suggest. Table 2 shows that none of them differ at all significantly from either of the other groups' correlations.

Appropriate analyses demonstrate that all of Zacharias *et al.*'s relevant correlations are insignificant, leaving the inferences they draw from them without basis. It is not true that the correlations "*indicat[e] that the homeopathic medicine improved vital functions*" [p. 145], "*demonstrate the different responses to the homeopathic and antihelminthic treatments*" [p. 149], or "*[suggest] that the immune response was better in the homeopathy group*" [p. 150]. Their speculations that "*the homeopathic medicine acts by stimulating the entire body … This led to a better recovery from haematopoiesis*" [p. 149] and that "*the homeopathic medicine stimulated the production of circulating eosinophils*" [p. 150] remain unfounded.

It's worth briefly noting that Zacharias *et al.*'s statistical peccadilloes aren't confined to their handling of correlation coefficients. For instance, in the space of one page (p. 150), they observe that the sheep treated with homeopathy gained more weight than the other sheep, go on to conclude that "*these results would have a considerable impact in sheep flocks bred on a commercial scale*", and that "*cost benefit analysis, showed a favourable trend to homeopathy compared to other groups*". Also on page 150 is their Figure 2 and its caption, which states that the "*weight gain comparison did not show statistic significance*"! It is a bad idea to make sweeping veterinary recommendations on the basis of a result that could well be a fluke.

Ultimately, Zacharias *et al.*'s overenthusiastic interpretation of insignificant results, flawed significance testing, and numeric inconsistencies lead me to decide that most of their conclusions still await experimental confirmation.

The authors have not responded.

References

1 Zacharias F, Guimaraes JE, Araújo RR, *et al.* Effect of homeopathic medicines on helminth parasitism and resistance of *Haemonchus contortus* infected sheep. *Homeopathy* 2008; **97**: 145–151.
2 Samiuddin M. On a test for an assigned value of correlation in a bivariate normal distribution. *Biometrika* 1970; **57**: 461–464.
3 Fisher RA. On the "probable error" of a coefficient of correlation deduced from a small sample. *Metron* 1921; **1**: 3–32.

Drew M Thomas
Imperial College London,
London, UK
E-mail: dmt107@imperial.ac.uk

Reply to Thomas

We thank Drew Thomas for his comments on our paper on the effects of homeopathic treatment on sheep parasitized with *Haemonchus contortus*.[1] We respond below.

The fecal egg count (FEC) and larvae per gram of feces (LPGF) were log $10(x+1)$ transformed to normalize variance. The means of the treatment groups were compared by analysis of variance by the SAS general linear models (GLM) procedure, using Software Statistical Package for the Social Science (SPSS) (version 12). Follow-up comparisons were performed by the Tukey test. Correlation between values from different parameters was calculated within each group using Pearson's correlation coefficient. The Tukey test compares any two means: a trial with 05 treatments and experimental groups yields 10 contrasts. The Tukey test generally is applied at the level of 5% of probability and our study had values of <0.05 for LPGF favouring homeopathy for this important measure of parasite load.

The Pearson correlation coefficient measures the degree of correlation (and its direction—positive or negative) between two variables. This coefficient, usually represented by "r" is always between -1 and $+1$. ≥ 0.70 (positive or negative) indicates a strong correlation. 0.30–0.7 is considered a moderate correlation and <0.30 is considered a weak correlation. We observed that the experimental group treated with homeopathy had for the variables FEC:haematocrit $r = 0.656$, FEC:Hb $r = 0.834$, Globulin:IgG $r = 0.610$, indicating moderate to strong correlation between haematological, immunological and parasitological parameters in animals infected with *Haemonchus contortus*. We described the results of the statistical analysis, and reported when there was or was not statistical significance in these correlations.

In any scientific research, the general procedure is to formulate hypotheses and verify them, directly or through its consequences, statistical analysis of the observations is essential to allow for the effects of unmeasured factors that cause the variation. These effects, may be unknown individually and tend to mask or skew the effect of the treatment on study. However, the ability to interpret the information (social, economic and environmental impacts) should be considered subject to partial and limited judgments.

The greatest challenge today in the control of gastrointestinal parasites of goats and sheep is the emergence of resistance to antihelminthic drugs. Homeopathy is a promising alternative, along with other lines of research, including herbal products, pastures management and breeding of animals resistant to parasitism. The World Health Organisation considers veterinary products important vehicles of contamination of the food chain which may contaminate meat, eggs and milk. Some veterinary products, in feces are lethal to insects and microorganism responsible for their decomposition. Homeopathic products being highly diluted do not generate residues in the food chain or pollute the environment. The ease of use of homeopathic medication prevents stress provoked by the containment of the animals when treated with other medicines. Veterinary homeopathy therefore meets the criteria of sustainability and the result of our work shows that it can enhance parasitic control. Two recent studies have described the effect of the homeopathy in immunomodulation.[2,3]

We thank Drew Thomas for his comments, but we did not find anything of major relevance or significance to correct. It is true that there was an error in transcription of the tables to the text concerning the correlations of Hb:FEC correlation: it should have been 0.834, not 0.893 but both are in the same range of strong correlation, so this does not change the outcome of our work.

A growing number of sheep and goat farmers using the products tested in this study in our state of Bahia and in Brazil as a whole, with good results. We recognize the limitations of our study and will bear Drew Thomas's remarks in mind in our future publications and will be happy to exchange experience and technical information on parasite control in animals with him.

References

1 Zacharias F, Guimarães JE, Araújo RR, *et al.* Effect of homeopathic medicines on helminth parasitism and resistance of *Haemonchus contortus* infected sheep. *Homeopathy* 2008; **97**: 145–151.
2 Lopes L, Godoy LMF, de Oliveira CC, Gabardo J, Schadeck RJ, de Freitas Buchi. Phagocytosis, endosomal/lysosomal system and other cellular aspects of macrophage activation by Canova medication. *Micron* 2006; **37**: 277–287.
3 Sato D, Wal R, de Oliveira CC, *et al.* Histopathological and immunophenotyping studies on normal and sarcoma 180-bearing mice treated with a complex homeopathic medication. *Homeopathy* 2005; **94**: 26–32.

Farouk Zacharias
Fernanda W de Mendonça-Lima
Serviço de Imunologia Doenças Infecciosas, Universidade Federal da Bahia, Salvador, Brazil
E-mail: mlima@ufba.br

Homeopathic treatment of weaned piglets

Sir,

Soto *et al.*'s[1] paper in *Homeopathy*'s October 2008 issue concludes that giving newborn piglets homeopathic supplements could reduce their postnatal diarrhoea-induced weight loss. It describes a trial comparing a control group of piglets fed no sucrose saline to 24 piglets fed with plain sucrose saline and 24 other piglets fed with sucrose saline and a homeopathic complex. Analyses of the piglets' subsequent weight loss revealed that the piglets fed homeopathically treated sucrose lost less weight than the other piglets; this difference was statistically significant.

Soto *et al.* suggest that this result was an effect of the homeopathy. Examining their results more closely, however, indicates a more likely explanation: baseline differences in the piglets' weights.

The homeopathy group piglets weighed about 400 g less than the other piglets before the trial began. Soto *et al.* mention this, and also point out (correctly) that this difference was statistically insignificant. Soto *et al.* do not mention that the initial weights of the homeopathy group piglets also had less variance than those of the other piglets: the homeopathy group piglets' original weights had a standard deviation of 0.69 kg, as compared to 1.08 kg in both the other groups. These differ significantly ($F = 2.45$, $p = 0.036$). This suggests that the homeopathy group piglets' initial weights came from a different statistical population to the control piglets' weights, and that an assumption of the analyses of variance and Tukey tests that Soto *et al.* performed – namely, homogeneity of variance – was violated.

It is also significant that the homeopathy group piglets lost less weight than the other piglets despite eating no more than them and suffering as much diarrhoea. This result in particular is hard to reconcile with the hypothesis that the homeopathy group piglets lost less weight because the homeopathy stanched their diarrhoea.

If, as these facts imply, Soto *et al.*'s positive results are due merely to pre-trial differences, their view that "[t]hese data suggest that the supplementation…with added homeopathic medicine…may be an useful option to reduce weight loss in weaned piglets" is difficult to sustain.

Soto *et al.* have declined to respond.

Reference

1 Soto FRM, Vuaden ER, Coelho CdP, Benites NR, Bonamin LV, de Azevedo SS. A randomized controlled trial of homeopathic treatment of weaned piglets in a commercial swine herd. *Homeopathy* 2008; **97**: 202–205.

Drew M Thomas
Imperial College London, London, UK
E-mail: dmt107@imperial.ac.uk

Homeopathy (2009) 98, 134
doi:10.1016/j.homp.2009.02.006, available online at http://www.sciencedirect.com

OBITUARY

William Herbert Tankard-Hahnemann 12 January 1922–12 January 2009: great great great grandson of Samuel Hahnemann

It is with great sadness we report that Mr. William Tankard-Hahnemann passed away peacefully at his home in Crowborough, East Sussex on 12th January 2009, his 87th birthday.

As a young boy, Mr. Hahnemann remembered his mother telling him of her visits to 'Uncle Leo' at Ventnor, Isle of Wight. He later understood that this he was Dr. Leopold Suss-Hahnemann, Dr Samuel Hahnemann's grandson, the only son of his favourite daughter, Amelie (1789–1881). Dr. Suss-Hahnemann was the only member of the Hahnemann family to be present at Samuel Hahnemann's funeral, apart from Hahnemann's second wife Melanie, in Paris in 1843 and at his subsequent re-burial in the Perè Lachaise Cemetery in east Paris. Subsequently Leopold emigrated to England, when he practised homeopathy in London. He retired to the Isle of Wight and died there at the outbreak of World War I in 1914.

Dr Leopold Suss-Hahnemann's youngest daughter, Amelie had two children, Winifred (born in 1898) and Herbert. William Herbert Tankard-Hahnemann was Winifred's son.

After serving as a Major in the British Army during World War II, Mr. Hahnemann had a distinguished career in the city of London. He was honoured by his appointment as a Freeman of the City of London. On his retirement to Crowborough, he became the patron of the British Institute of Homeopathy in 1987. Since then he has given his time generously to promoting classical and modern homeopathy in the United Kingdom and around the world. Ambassadorial tact and diplomacy, his equanimity and gentlemanly behaviour were legendary and he never baulked at anything he was asked to do in the service of homeopathy.

I recall his escorted tour of an ancient fort in Lahore in a temperature of 100°F, when he scaled the highest tower unflinchingly, whilst covered in sweat, leaving his hosts far behind. It was also in Pakistan that he chaired an International Congress attended by 720 delegates. He bestowed an Honorary Fellowship to Mrs Benazir Bhutto (then Prime Minister) who stated that "the organisers and Mr Hahnemann's role in the welfare of suffering humanity was most laudable". We were then invited to dinner at the official residence of the Governor of Punjab, Ch. Altaf Hussain.

Mr Hahnemann also attended many homeopathic events in the United Kingdom including, as a Life Fellow, an Annual Congress of the Homeopathic Medical Association and award ceremonies of the British Institute, including events at Los Angeles, Toronto, New Delhi, Paris and the birth place of his great, great, great grandfather, Samuel Hahnemann, Meissen.

In October 2008, William Tankard-Hahnemann stood down as patron owing to ill health. He was succeeded as patron of the British Institute of Homeopathy by his son, Charles Tankard-Hahnemann.

The condolences of the entire homeopathic community go to William's wife, Beth (sadly it would have been their 60th wedding anniversary next month), and the Hahnemann family. We offer our respect and gratitude for his incalculable contribution to the progress of Samuel Hahmemann's Healing Art over the past 22 years.

Trevor M Cook
British Institute of Homeopathy, Surrey, UK
E-mail: info@britinsthom.com; http://www.britinsthom.com