What does homoeopathy do—and how?

Robert Buckman, George Lewith

See editorial by Oh

Robert Buckman, a medical oncologist, and George Lewith, a homoeopathic physician, met in Southampton while filming "Magic or Medicine?," a television series of four programmes about what can and might be learnt from the popularity of complementary medicine. In particular, the series showed how the effect of the patient's and the doctor's beliefs affected the relationship between them. The two doctors so enjoyed talking with each other that they carried on their debate in letters to each other over the next year. We think that their exchange merits a wider readership.

Dear George,

The profession-wide debate on homoeopathy seems to have polarised into a dispute about evidence and mechanism, and as a result we seem to have lost sight of a separate issue—namely, that of the benefit to the patient. I propose that we divide the debate into two distinct issues. One issue is whether highly diluted extracts prepared by succussion can have an effect on disease processes. The second issue is whether patients feel better when they take homoeopathic remedies. If they do, what does it mean and what should we do about it?

Let me start by describing what I saw when, with the film crew of *Magic or Medicine?*, I visited your practice in Southampton and watched you taking histories from, examining, talking with, and prescribing for your patients. I said to you at the time that your clinical skills—and particularly your interpersonal skills—were among the best I have ever seen. I also said (and it was not idle chatter) that a video of you in action should be shown to every medical student in the world as an example of what good doctoring looks like. It seemed to me that when I walked into your consulting room the most powerful medication in the room was sitting in your chair—in other words, you. I still believe that to be the case. Which still leaves us, I think, with two distinct issues.

The first question is this: is there a verifiable physical mechanism by which the homoeopathic remedies themselves affect disease? Here, I would claim that there have been no double blind placebo controlled studies which have been successfully repeated that show objective results. However, I would also add that this question is now so polarised that supporters of homoeopathy will never believe negative evidence, and sceptics believe there is no reproducible positive evidence.

So, the second question is: do homoeopathic remedies make patients feel better? My feeling is that they do—and that they improve symptoms in most cases. Which brings me to the question that I feel is central to the issue of patient care: what (if anything) should be done about the clinical side of things, while the evidence (one way or the other) is awaited? More particularly, how should we evaluate the clinical value or utility of belief in homoeopathy (whether or not there are or will be valid data to justify that belief)? In other words, if you, George, tell me that your belief in homoeopathy is what makes it possible for you to be such an empathic and supportive physician, must we then pursue the debate about mechanism to the (probably bitter) end? Would it not be possible to forgo the debate about mechanism completely and accept your patients' sense of wellbeing as an end point in itself? Would you be upset if that happened?

Dear Rob,

You raised three issues: the underlying mechanism of homoeopathy, its clinical effectiveness, and the effects of me as a therapist. The mechanism of homoeopathy is unknown, but conventional medicine frequently uses treatment for which it has no known mechanism, so I think that it is entirely appropriate to forgo the debate about an underlying mechanism and concentrate on the effectiveness of the treatment.

The second point you raised is about the clinical effectiveness of homoeopathy. A recent review of the clinical trials in homoeopathy came to a negative conclusion, but it included both good and bad homoeopathic studies.[1] If we look at three of these studies we find that there are good controlled trials which can provide convincing evidence for the clinical effectiveness of homoeopathy in hay fever,[2] migraine,[3] and fibromyalgia.[4]

These trials directly address the methodological problems that surround many of the poor studies on homoeopathy, and they also ask and answer the challenging questions of whether homoeopathy is purely a placebo effect. The evidence available indicates that under certain circumstances homoeopathy is not simply a placebo effect but provides therapeutic benefit greater than that expected from a placebo. This perhaps asks some incisive questions about its mechanism in the pharmaceutical and biochemically dominated world of conventional medicine.

The final issue is the placebo effect, which I believe you muddle a little with clinical effectiveness. Under the portmanteau of the placebo effect there is clearly some effectiveness to be gained from a positive clinical approach to the patient. I am flattered but not deluded by your compliments about my consultation technique. Yet, I believe in homoeopathy and I know that belief combined with a positive clinical approach to the patient can have a powerful therapeutic effect,[5] but this does not detract from the fact that homoeopathy does have proved therapeutic benefits (in some instances) greater than those expected from a placebo. The placebo effect itself is poorly understood and almost certainly contains elements of the doctor's approach to the patient, the natural resolution of the underlying illness, the effects of receiving treatment for an illness, and the belief system of both the patient and the consulting physician.

Belief, the placebo effect, and proved clinical

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effectiveness should not be muddled as they are in your arguments. I have changed and will change my treatment approaches according to sound clinical trial work, so on a personal basis I do not feel that my belief in homoeopathy will act to suspend my critical judgment.

Dear George,

I would like to respond to your statement that there is convincing evidence for the clinical effectiveness of homoeopathic remedies—but after that I will try to move the focus on to what we (as a medical profession) should do about any modality where the scientific jury is still out (or where there is a controversy about precisely where the jury is at the moment).

I discussed the most important randomised placebo controlled trials at greater length elsewhere,[6] but I can briefly summarise my position—which I would characterise as that of the thoughtful sceptic—on the three studies you referred to.

One of the most significant trials of homoeopathic remedies was the Italian trial by Brigo *et al*, who tested homoeopathic preparations against placebo in a double blind randomised trial for migraine headache.[3] The design of the study was superb and the results were dramatic. The results clearly showed that homoeopathic remedies were exceptionally active in migraine, producing considerable reductions in the number and the duration of headaches. If the results of that trial had turned out to be robust and reproducible there would have been very strong grounds for accepting homoeopathic remedies as active (at least in migraine) and for initiating widespread research into the mechanism. A group at the Charing Cross Hospital repeated the study, tightened up the entry criteria to make the study even more significant, and we filmed for *Magic or Medicine?* the moment that the trial was unblinded and the results analysed. As you know from the television programme, the study did show some dramatic responses—several patients having almost complete relief of symptoms—but those responses were unequivocally distributed evenly between the placebo and the homoeopathic periods of treatment. In other words, results of Brigo *et al* were not reproducible, and the Charing Cross study showed no difference between homoeopathy and placebo in these circumstances. Of course, in itself that does not (and cannot) prove that homoeopathy is ineffective, but it certainly supports the view that there is as yet no reproducible evidence of an effect of homoeopathy in migraine.

Similarly, the study you mention by David Reilly (with whom I spent a wonderful day in Glasgow) was also very interesting.[2] He and his colleagues showed that compared with placebo a homoeopathic pollen remedy used in hay fever produced a small but significant worsening of patients' symptoms during the two weeks of treatment, followed by a small but significant improvement over the two weeks after the treatment was completed. Again, if their study were to be repeated and the repeat study were to show the same alternating effect of exacerbation followed by amelioration over that time scale, then we would have all have to accept this as a proved phenomenon. However, Reilly *et al* completed their study in 1986, and despite its great importance in this debate it has never been repeated.

The study by Fisher *et al* showed a decrease in the number of tender spots and score on visual analogue scales (later incorporated into a single index of pain and sleep disturbance) in fibrositis.[4] As published, those data are rather sparse. Even so, if those results were shown to be reproducible, it would—at the very least—be interesting. Again, however, their study in 30 patients was published in 1989 and has not been repeated, although I would guess that it would not be a difficult study to repeat.

So at present, it is quite legitimate to maintain that there is no reproducible objective evidence that homoeopathy has an effect on symptoms. I have to admit that future studies may show a robust and reproducible effect, but none of the most interesting and well designed trials with positive results have yet been successfully repeated.

But that is almost a side issue. In all these studies (and in almost all studies of almost everything) placebo treatment produced a considerable decrease in symptoms. In conventional medicine we have always dismissed that as "just the placebo effect." Perhaps we should stop thinking that way. In every branch of conventional medicine we have a high percentage of patients whose symptoms we cannot cure. Perhaps we should move back towards the honest use of placebos, accepting that many practitioners believe that their own particular prescription is not a placebo. Perhaps—and this is just a stray idea of mine—we should have signs in our waiting rooms which say:

> As part of your treatment your doctor may prescribe certain drugs which have not—so far—been proved to have a specific action against diseases. Nevertheless, these drugs are completely safe and many patients find them beneficial. If your doctor thinks they may help you, she or he may recommend them to you.

Perhaps we could add a list of examples to include not only many complementary medicines but many conventional medicines as well and most borderline treatments such as vitamin supplements (when prescribed in the absence of proved vitamin deficiency). In this way, at least patients would be prepared for the fact that not every treatment is of proved value—and that would be an honest way of telling our patients that we will try to give them relief of symptoms whether or not there is scientific proof of efficacy. If such signs were printed, I would be happy to display one. Even in oncology, faith in the doctor plays an important part in the patient's perception of his or her symptoms. So my second question to you is this: if a sign like that were printed and I put one in our waiting room would you put one in yours?

Dear Rob,

The argument on clinical trials is complex. The study you refer to by Tom Whitmarsh from Charing Cross remains unpublished so I am unable to comment on it. Reilly *et al* have duplicated their hay fever study[2] using the asthma and house dust model.[7] This is a pilot study, but it shows the same significant trends as those of the hay fever study. We in Southampton are in the process of developing an asthma protocol, which will again examine the model proposed by Reilly *et al* in more depth and with larger numbers of patients. Another excellent study was that by Ferley *et al*, which looked at homoeopathic treatments for influenza.[8]

I think the debate on the effectiveness of homoeopathy could go on for ever. I feel, however, that my argument stands in that there are some good studies on homoeopathy with which it is difficult to argue on methodological or statistical grounds. Clearly, more work needs to be done in this subject, and, as I have mentioned, we are in the process of developing proposals that we hope will add to our understanding of homoeopathy.

I do not think that it is legitimate to claim that there is no reproducible evidence for homoeopathy. I think that it is legitimate to claim that the field is muddled; although there are some excellent studies, not enough

good research has been done to prove the clinical effectiveness of homoeopathy. To claim that there is no evidence is, in my view, academically unsustainable.

We have, of course, to set this against the background of our conventional knowledge. We know, for instance, that most treatments prescribed by conventional doctors are unproved. Some people have suggested that 85% of conventional medicine still needs to be properly evaluated. Attempts to correct this situation through detailed literature reviews from institutions such as the Cochrane Centre have all too frequently been ignored by many conventional doctors. I understand that most trials of non-steroidal anti-inflammatory agents look at improvement over the short term, yet we are encouraged to prescribe them on a long term basis to treat arthritis and pain. These inconsistencies in conventional medicine are reflected equally in homoeopathy, so I do not think that the conflicting evidence that exists within homoeopathy is in any way unique.

Placebo effect is enormously positive—here I agree with you totally. The doctor, through an as yet ill defined mechanism, is initiating a self healing response. This is a positive part of medical intervention. We should do our best to understand and amplify the placebo response in a responsible and professional manner. We should teach medical students about it so that they understand the limits of scientific medicine and the importance of their intervention. I totally agree with the notice you suggest should be pinned up in your waiting room—perhaps this article might be stimulus for a special offer and we could have the notice printed multilingually.

In the context of our debate about the evidence for homoeopathy I wonder why you are so resistant to looking positively at the evidence that does exist in what, after all, is a fairly underresearched field in which poor methodology and lack of finance have underpinned such limited achievements. I would also like to address the problem of the increasing numbers of individuals who seek homoeopathic medicine throughout the Western world—they, after all, must have some conviction that this is likely to be helpful and some experience that homoeopathy has been helpful to a friend or relative. Can such international enthusiasm really be sustained solely on the basis of a placebo response?

Dear George,

I think your last letter proves my point exactly. I am saying that there is no reproducible evidence that homoeopathy differs from placebo because that is the way things are at the moment. I am glad to hear that the study by Reilly *et al* has been carried out in another disease, asthma, and also that you are examining an asthma protocol. However, those are not confirmatory studies—and, as I say, there are no successful confirmatory studies in homoeopathy so far. Hence my assertion that the jury is still out is accurate.

Perhaps I can use a couple of illustrations from oncology. In 1975 Bonnadonna *et al* published the first study of cytotoxic polychemotherapy used post-operatively in women with lymph node positive breast cancer.[9] The initial report showed an improvement in disease free survival in premenopausal women.[9] The oncology community regarded it as an important study and result—and not unexpected since many observations in tumour biology could be adduced in support of a greater effect of chemotherapy when the tumour burden is low. However, the observation of Bonnadonna *et al* was not accepted as a proved fact until confirmatory studies were done (in the same disease, with identical entry criteria and treatment) which showed the same magnitude of effect over the same time course. Later, follow up reports showed not only improved disease free survival but also improved overall survival. As those results came in, oncologists started using cytotoxic polychemotherapy as the standard adjuvant treatment in node positive premenopausal breast cancer—a strategy then supported by a massive meta analysis which looked at the outcome of many thousands of women treated in randomised trials of the chemotherapy compared with no treatment.[10] So the effectiveness of cytotoxic polychemotherapy as adjuvant treatment is now an incontrovertible and undeniable fact and cannot be disputed: it has proved to be a robust and reproducible effect.

On the other hand, David Spiegel, a psychiatrist at Stamford, published with his colleagues a long term follow up of his study of group psychotherapy in patients with metastatic breast cancer.[11] Their paper showed that the patients who received psychotherapy survived longer than those who did not. They did not accept that single result as conclusive proof, any more than anyone else did, and they immediately set about organising a confirmatory study, which is now being carried out in many centres and will either confirm or contradict those initial results. So even according to the authors themselves, those initial results do not yet make a proved fact. At the moment it is a very interesting observation—and either it will be confirmed or it will not.

As I say—and as your letter also shows—in homoeopathy there are no confirmatory studies. This is not my being resistant to any concept or idea, it's just an accurate statement of the way things are at the moment. Of the three main randomised studies, two have not been repeated[2,4] and the last was repeated (although not yet published) and did not confirm the initial results.[3] Although, as you tell me, using Reilly's study design in asthma is laudable, it is certainly not a confirmatory study of the effect of homoeopathy in hay fever.

I think that you have to agree that at present there are no results of randomised studies which have been successfully repeated and which confirm the initial observations. So any clinical effect of homoeopathy over and above that of placebo is not yet proved. Which is why I am so interested in the question of what we (the whole medical profession) should do while we are waiting for more research and more studies.

Perhaps there is already an analogy in the way the medical profession works alongside the chaplaincy. Many hospitals have chapels, but that does not mean that the hospital or the health service officially accepts the existence of God as a proved fact. Speaking personally, I refer many of my patients to our clinic chaplain, with whom I have done joint counselling sessions and teaching sessions and written a book. Yet I do not share his belief in God at all—in fact, I'm something of a proselytising atheist. But that does not stop me acknowledging the benefit that the chaplain brings and the symptomatic relief that patients find in prayer. Perhaps that is the right model for cooperation between people who do not have the same beliefs but acknowledge the usefulness of what the other person does.

Finally, you did not quite answer my last question about informing patients about the use of drugs that have no proved effect (the operative word being proved). I am glad that you approve of my putting up a sign in my waiting room—but you did not say whether you would be prepared to put up a sign (multilingual, full colour, and illustrated) in yours. I think you are saying that you would—am I right?

Dear Rob,

If the level of proof that you require is repeated studies of the type mentioned, then clearly homoeopathy cannot be seen as a totally proved treatment. However, what remains fascinating is the number of studies with positive results, given the comparative infancy of research in this subject. Your illustration of cytotoxic polychemotherapy is a valid story, but one that I believe could be duplicated in only a few instances in conventional medicine. It is important to see such validation in context and not assume that all conventional medicines have been tested and evaluated in the manner suggested by you—in fact few have. Your assumption about my being prepared to put up your notice is quite correct. It is fair and I believe that it should be placed in every doctor's waiting room.

You fail to answer my final question about the West's increasing interest and commitment to complementary medicine. I think that medicine is not an absolute science but to a certain extent reflects—and sometimes leads—cultural change. The technocratic solutions promised by medicine in the 'fifties and 'sixties have not arrived, so people are looking for different ways of thinking to approach their chronic illness. Unfortunately, evaluative research has not kept pace, so we are rightly unsure about the clinical effectiveness of some of these approaches. However, social and cultural change are leading the way, and it is up to us as responsible physicians to respond coherently to this challenge.

We have glibly talked about homoeopathy being proved or disproved. It is important to remember that homoeopathy is a treatment for many diseases; it may be relevant in one set of circumstances while being irrelevant in another. The real challenge of homoeopathy, and I suspect the reason why many people resist it so forcefully, is not related to whether it benefits patients or indeed, whether it is effective. I have formed the impression that many conventional physicians do not really want to address the concept because it challenges fundamentally their narrow pharmacological and biochemical model: if homoeopathy does work, how do we explain it?

Running through our debate are fundamental philosophical questions about how medicine is changing. I see homoeopathy as one example of a different medical language. If doctors can learn the language of acupuncture and nutritional medicine, then they have a much larger breadth of medical models through which to approach a patient. Consequently, symptom patterns and poorly defined illnesses begin to have relevance and coherence, as well as a treatment—albeit in many instances unproved (at present) in relation to your stringent criteria. Our debate about the changing approach to the placebo response illustrates a similar shift in consciousness.

These old ideas combined with new technology can provide new, exciting, and innovative directions for conventional medicine. We are stuck in our biochemical model at the present, and many chronic illnesses are not really benefiting from our current treatments. There is, of course, the hope that molecular biology will provide these solutions, but it may again prove to be a false dawn. Rather than concentrating on the act of needling or a specific homoeopathic remedy, I think that it is important to approach these areas by looking at the underlying philosophies in order to expand our horizon conceptually and therapeutically.

Dear George,

This exchange has been great fun. I am sure neither of us thought when we started it that we would materially alter the other person's stance, but equally I am sure that we both hoped that the discussion would stimulate some thought and widen the debate on some important clinical issues. I think that we have done that—let us do it again in five years' time, when there will be new data and new issues to discuss.

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A PATIENT WHO CHANGED MY PRACTICE

Looking for petechiae

John was about 7 years old and came in with his mother. It was Friday evening and I was moving on to automatic pilot. He had a bit of headache, a sore throat, and his careful mother wanted him checked over "before the weekend, Doctor." His chest was clear, throat slightly injected, ears normal. Yes, paracetamol would be fine. They were going through the door, when I experienced a curious sense of unease and called them back. Listening again to John's chest, I realised that my subconscious anxiety had been due to a few petechial spots on his back. No neck stiffness, no significant headache, but a definite crop of petechiae.

His initial lumbar puncture was negative but, repeated next day, produced Gram negative diplococci. Blood culture on admission grew meningococci. He was treated and fully recovered.

I wish that I could report that this fortuitous early diagnosis and treatment of John's meningococcal septicaemia was followed by happy days. Shortly afterwards John's father died quickly from carcinoma of the stomach. His mother was sexually assaulted in their home, with grave psychological consequences. As a teenager, John drifted into substance abuse, truancy, and petty crime. He was in and out of care. When just 17 he was discovered dead in a fume filled car.

I never told John of the importance of my consultation with him. Perhaps I should have done. Now, however, I always actively look for petechial spots while listening to the chests of sick children. It takes no extra time and might save a life.