Halloween Science

The Truth about *Trick or Treatment?* by Simon Singh and Edzard Ernst

by

William Alderson RSHom LLSCH

on behalf of H:MC21

First Published April 2009
Homeopathy: Medicine for the 21st Century
Poppyseed Cottage, High Street, Stoke Ferry, Norfolk PE33 9SF

Homeopathy: Medicine for the 21st Century (H:MC21) is a registered charity, no. 1124711
Registered address: Poppyseed Cottage, High Street, Stoke Ferry, Norfolk PE33 9SF

Contents

A Preliminary Explanation

In *Trick or Treatment?* Edzard Ernst and Simon Singh have made very large claims, and we believe that these claims are damaging to the reputation of medicine – all medicine, orthodox and alternative – and to the reputation of science. Ernst and Singh have claimed about their book that "the key theme running throughout all six chapters is 'truth'" (p. 4[*]). This critique of their book will give readers another perspective on the truth: the truth about *Trick or Treatment?*. Beyond that it will only try to encourage open-mindedness and the recogniton that medicine has some way to go before it is a mature science like physics or chemistry.

Unlike Ernst and Singh, we shall not start by pretending to withhold our conclusions or by disguising them with claims of impartiality. We are homeopaths, and we have studied *Trick or Treatment?* using the knowledge we have as homeopaths as well as a wider knowledge of medicine, science and literary criticism. We are proud to present our conclusions immediately and to allow the evidence to follow in all its necessary detail. In summary then, we have found that:

1. **Ernst and Singh rely on unsupported evidence**
   Because there are virtually no references it is very difficult, or even impossible, to verify many of the figures, trials, events, quotations, statements, opinions and explanations presented in this book.

2. **Ernst and Singh present information out of context**
   For example: the financial strength of alternative medicine is not compared with that of orthodox medicine[1]; the actions of alternative therapists are not compared with those of orthodox practitioners; the evidence of effectiveness for alternative treatments is not compared with that for common orthodox treatments.

3. **Ernst and Singh use double-standards for evidence**
   They demand of supporters of alternative medicine replicated controlled trials, quotation supported by validated controlled trials, reports supported by validated controlled trials, examples supported by validated controlled trials, explanations supported by validated controlled trials, statements supported by validated controlled trials, and they reject

[*] All unattributed page references are to Simon Singh and Edzard Ernst, *Trick or Treatment? Alternative medicine on trial* (London: Bantam Press, 2008).

endorsements of alternative medicine by celebrities. Yet they justify their own arguments with single trials, selective quotation, anecdotal reports, *ad hominem* examples, hypothetical explanations, statements from supporters and the views of celebrities who endorse orthodox medicine.

4. **Ernst and Singh confuse absence of proof with proof of absence**
   They repeatedly use the lack of evidence for the success of alternative treatments or for the mechanisms of alternative therapies as the basis for claiming that they cannot work and that their mechanisms are non-existent.

5. **Ernst and Singh ignore the importance of theory**
   On the very first page, science is described in a way which excludes the concept of theory, and so is completely at odds with the general understanding of the nature of science. Throughout the book discussion of theory is minimal, and no arguments are oriented on testing theories.

6. **Ernst and Singh assume that orthodox medicine is scientific**
   No scientific justification of orthodox medicine is ever provided, and yet its approach is used as a standard against which to measure the scientific validity of alternative medicine.

7. **Ernst and Singh do not understand orthodox medicine**
   Examples include: claiming that no vitamins can be manufactured by the body; confusing chronic pancreatitis with acute pancreatitis; and failing to distinguish between treatment for deficiency diseases and treatment for infectious and chronic diseases.

8. **Ernst and Singh define alternative medicine in four different ways**
   The first two definitions contradict each other. The third definition establishes pharmacology as the measure of orthodoxy. The fourth definition establishes lack of proof of effectiveness in pharmacological trials as the test for alternative medicine, but not for orthodox treatments.

9. **Ernst and Singh fail to define significant terms**
   The most important terms which they fail to define are: science, disease, cure, effectiveness and orthodox medicine. They refer to some terms as “buzzwords”, such as ‘holistic’ and ‘individualisation’, and then use them themselves as legitimate terms. They

also refer to a question as 'fundamental' and then show that it comprises no less than 25,900 questions.

10. Ernst and Singh allow arguments to be based on readers' preconceptions

Because the authors fail to define their terms, readers will assume meanings they agree with, enhancing their acceptance of the conclusions. A key example of this is the term 'effective', which could mean a number of different things to different people.

11. Ernst and Singh fail to take different definitions of terms into account

Some alternative therapies have definitions of disease, cure and effectiveness which are significantly different from those used by orthodox medicine. These differences and their impact on clinical trials are not identified or discussed.

12. Ernst and Singh fail to present their main approach (EBM) accurately

The failure to describe the nature of evidence-based medicine either accurately or sufficiently means that readers are left unaware of the importance of clinical expertise based on experience to this approach. As a result the illusion is given that the RCT alone forms a sufficient basis for scientific examination of treatments.

13. Ernst and Singh fail to present homeopathy accurately

The failure to describe the nature and development of homeopathy either accurately or sufficiently means that the authors have created a fictitious model which they can and do use to mislead readers. This may also be true for other therapies they discuss, but we do not have the expertise to recognise if it such misrepresentation has occurred.

14. Ernst and Singh call into question the validity of orthodox drug therapy

By showing that the basic principles of drug research are flawed, they not only raise serious questions about the validity of such research as a comparator for alternative medicine in trials, but also undermine the rationale of orthodox drug therapy.

15. Ernst and Singh fail to show the validity of their main tool (the RCT)

They demonstrate that the randomised clinical trial is an appropriate tool for identifying harmful interventions, but that it is unable to identify beneficial interventions.

16. **Ernst and Singh fail to justify the meta-analysis as an objective tool**
    All their examples of meta-analyses demonstrate that this tool is subjective, and yet they base arguments on the conclusions of meta-analyses of trials. They even base arguments on the conclusions of secondary analyses of reviews of trials.

17. **Ernst and Singh rely on the scientifically inexplicable placebo effect**
    They allege that this key aspect of their analysis of therapeutic effects has no scientific basis. They indicate that it is unpredictably variable, individual, may be stimulated by completely opposite circumstances, and can lead to the belief that there has been improvement or to actual improvement without any explanation for these different consequences. They also fail to show that there is any consistent similarity between the placebo effect and the observed effects of alternative therapies.

18. **Ernst and Singh deny the importance of individuality**
    Even though they recognize that the curative process is individually determined, they insist that a scientific approach to medicine need not take this into account and, in fact, should actively try to minimise its influence on test results.

19. **Ernst and Singh deny the importance of clinical experience**
    In ignoring the fact that EBM takes clinical experience into account and in rejecting evidence from clinical experience, they are unable to consider objectively the problem of a discrepancy between experience and RCT results. Because of this they do not examine the possibility of errors in trial protocols, but only present multiple unverifiable explanations for the success of alternative medicine in clinical practice.

In brief, Ernst and Singh reveal a profound ignorance of their subject and a serious bias against alternative medicine. This bias is also seen in their choice of language, use of hyperbole and juxtaposition, and selection of sources for quotation.

These allegations against two “trained scientists” (p. 3) may seem extraordinary, but it is not our opinion we are presenting, but the opinions of Edzard Ernst and Simon Singh themselves. The attacks on their primary arguments are not made by contrasting their statements with those of other people, but by contrasting them with their own (often contradictory) statements. Occasionally we have supported the inevitable conclusions by reference to external sources, but those external

sources are not essential. On some occasions Ernst and Singh have omitted vital information, and then we have considered it important that this be introduced to enable readers to make an informed judgement. We have tried to refer to primary sources as far as possible, or to respected sources.

Details are important, since they are the basis of any claim to "an uparalleled level of rigour, authority and independence" (p. 3). For this reason we have quoted the authors' own words extensively and analysed their meaning carefully. In doing so we have often found that sentences have been constructed so that their apparent meaning in context is very different from their meaning when separated from that context. As a result we have had to constantly assess and re-assess what the authors have said, and pay attention to the details. In the end we have found that their lack of attention to detail runs right through to the final page.

Because Ernst and Singh use various terms for 'non-alternative' medicine, it was difficult to decide which of their terms would be appropriate at any given point. As a result we decided to use a single term and to define our reasons for using it. We have elected to use the term'orthodox medicine' throughout for the following reasons

1. No scientific foundations for this branch of medicine are provided in this text, implying that it has no objective truth;
2. The distinction between this branch of medicine and alternative medicine is said to be based on what ideas are acceptable or not, that is a doctrinal difference rather than an objective one;
3. The concepts of this branch of medicine are accepted without question by the authors indicating that their attitude is founded on a belief system rather than evidence.

By choosing a term which embraces these points we hope to keep in readers' minds the true basis of Ernst and Singh's approach and the extent to which it differs from that of science. Scientists should always be prepared to question accepted ideas in the face of anomalous evidence, but more importantly, their ideas should not be a barrier to recognising anomalous evidence in the first place.

The format of this critique reflects that of the book itself, looking at the introduction and each chapter in turn, in order to show the cumulative and interlocking effect of the authors' handling of their subject matter. We have not addressed every issue in each chapter, but only those which seemed the most important in terms of the main argument of the book. Necessarily the approach of

this critique in each chapter will reflect the nature of the original material in *Trick or Treatment?*, as well as the degree to which our argument can rely on what has already been shown. For the convenience of the general reader, the information for each chapter is presented under a series of headings.

Lastly, we wish to say that this work is intended not as a criticism of those committed people who work in the field of health care, whatever therapeutic practice they use, but as a criticism of the narrow-mindedness of an authority less concerned with others and more concerned with its own power. We remind readers that when it comes to the supporters of such authority

> The stuff they write is too evidently dictated by passion and too full of errors and falsehoods to impose on the public and induce them to regard such bunglers as good judges of ths important matter.
>
> The truth has already extended its rays too widely, and shines too brightly to admit of being eclipsed.[2]

This work is dedicated to three groups who have suffered from such authority and its supporters:

- all those who have been deprived of the treatment they needed,
- all those who have been deprived of the means they needed to help the sick, and
- all those who have been pilloried for standing up for what they know to be true.

Discussion of the Introduction

In the Introduction Ernst and Singh set out their aims and their views about the subject they are addressing:

> The contents of this book are guided entirely by a single pithy sentence, written over 2,000 years ago by Hippocrates of Cos. Recognised as the father of medicine, he stated:
>
> > *There are, in fact, two things, science and opinion; the former begets knowledge, the latter ignorance.* (p. 1)

In doing so, they exhibit two failings which are repeated throughout the book: lack of references and lack of clarity about definitions. No source is given either for the quotation or its translation, nor is there any explanation for the anachronistic use of the word 'science' in the translation. The meaning we attach to the word 'science' does not derive directly from Hippocrates, and

> 'Science', which meant merely 'learned knowledge' in the Middle Ages, only gradually took on its modern resonances.[3]

This casual approach to evidence and definitions is not consistent with the scientific "rigour" the authors are claiming for their work.

Since the 'Introduction' lays Ernst and Singh's foundations for their examination on the 'truth' about alternative medicine, it establishes the *principles* which guide their analysis, and such an opening statement is not a good omen. As we shall show, however, it is true to their approach in general. Ernst and Singh go on to construct their argument on precisely this basis of combining a failure to define terms clearly (if at all) with the use of partial evidence and even perjorative language. At best this leads to fallacious conclusions, but at worst it leads to pure propaganda.

The Nature of Science

The concept of science plays an important part in this book, and

> One of the key problems in philosophy of science is to understand how techniques such as experimentation, observation, and theory-construction have enabled scientists to unravel so many of nature's secrets.[4]

According to Ernst and Singh, however, (our emphases)

> Science employs experiments, observations, *trials, argument and discussion in order to arrive at an objective consensus on the truth*. (p. 1)

In this version the very important element of "theory-construction", of developing broad but detailed explanations capable of being used to design experiments, predict their outcome, and link their results to other areas of knowledge, has been replaced by "argument", "discussion" and "consensus". Nor does the reference to "objective consensus" help clarify their meaning, as Ernst and Singh immediately add that such consensus is nothing more than possibly mistaken proclamations:

> Even when a conclusion has been decided, science still probes and prods its own proclamations just in case it has made a mistake. (p. 1)

The significance of Ernst and Singh's version is that it claims that science is based on evidence and agreement about the importance of the evidence. In other words, evidence is absolute, though opinions about it may differ. This position is confirmed later when they state that (our emphases)

> Chapter 1 provides an introduction to the scientific method. It explains how scientists, *by experimenting and observing*, can determine whether or not a particular therapy is effective. Every conclusion we reach in the rest of this book depends on the scientific method and on *an unbiased analysis* of the best medical research available. (p. 4)

For Ernst and Singh the scientific method is about "experimenting and observing", not about experimenting, observing and *theory*. Futhermore, their conclusions are based on "unbiased analyses", despite the fact that bias is often a matter of opinion, as they will show in 'Chapter 3'.

Returning to their initial paragraph on the definition of science, Ernst and Singh's concluding sentence seems very strange in the light of their earlier statements:

> In contrast, opinions are subjective and conflicting, and whoever has the most persuasive PR campaign has the best chance of promoting their opinion, regardless of whether they are right or wrong. (p. 1)

Having removed theory from their definition, Ernst and Singh have presented science as a combination of evidence and opinion which produces an "objective consensus", yet they now claim that opinion itself is entirely subjective and dependent on "the most persuasive PR campaign". No explanation is given as to how an "objective consensus" is reached in these circumstances, and it is reasonable to question why this term replaces the word 'theory'.

One explanation for Ernst and Singh's choice of terminology may be the peculiarly variable nature of consensus in orthodox medicine, in that an "objective consensus" about treatment can change completely and then change back again all within a mere ten years (the treatment of secretory otitis media is just one example). In the sciences of chemistry and physics this sort of reversal does not occur, as changes in approach and practice are connected with *progressions* in theory. As a result, it appears that the authors' definition of science in terms of evidence and "objective consensus" is actually tailored to suit the weaknesses of orthodox medicine, rather than being one chosen to accord with more generally accepted standards. In the light of this, its qualifications as a basis for assessing other branches of medicine become highly suspect.

Another issue arising from the lack of any proper discussion of the importance of theory to science occurs when Ernst and Singh suggest that

> Medicine has never claimed to have all the answers, and over and over again there have been revolutions in our understanding of the human body. So will the next revolution lead to a discovery of the mechanisms underlying alternative medicine? (p. 2)

In physics, chemistry and biology it is the build-up of anomalous evidence and the discovery of new "mechanisms" which leads to a breakthrough in theory (a new understanding), but Ernst and Singh appear to be suggesting that in medicine the theoretical breakthrough has to *precede* the process of discovery. They offer no explanation of how this is possible. Furthermore, they will go on to suggest that anomalous evidence is inherently less valid than acceptable evidence, raising questions about what they mean by "unbiased".

Science and Opinion

An example of Ernst and Singh's confusion about their own definitions of 'science' and 'opinion' occurs only two paragraphs later when they state (our emphasis):

> Indeed, our definition of alternative medicine is any therapy that is *not accepted* by the majority of mainstream doctors, … (p. 1).

This is a clear acknowledgement of the division being one of opinion rather than of science, but they then go on to say:

> … and typically this also means that these alternative therapies have mechanisms that lie outside the current understanding of modern medicine. In the language of science, alternative therapies are said to be biologically implausible. (p. 1)

This appears to be a statement to the effect that the division between orthodox and alternative medicine is a scientific one, but it creates a paradox. If the division is scientific, then while some alternative therapies "typically … have mechanisms that lie outside the current understanding of modern medicine", others do not and so should not be classed as alternative. On the other hand, if the division is not scientific then the distinction between orthodox and alternative medicine has no scientific validity and is simply one of opinion. One effect of this paradox is that Ernst and Singh are compelled to redefine alternative medicine three times in ways incompatible with the definition given here.

This paradox is compounded by another one. If not all alternative therapies "have mechanisms that lie outside the current understanding of modern medicine", but all alternative therapies "are said to be biologically implausible", then some mechanisms within the current understanding of modern medicine must be considered "biologically implausible", and yet they are acceptable within orthodox medicine but not as part of alternative medicine. The effect of this paradox is that Ernst and Singh use a double-standard when assessing orthodox and alternative treatments, most clearly seen in their fourth definition of alternative medicine in 'Chapter 6'. An example of this double standard with respect to plausibility is not unique to this book, as the following remark from a study of clinical trials of homeopathy shows:

> Based on this evidence we would readily accept that homeopathy can be efficacious, if only the mechanism of action were more plausible.[5]

What is clear, is that Ernst and Singh wish to appear to have a scientific basis for distinguishing between orthodox and alternative medicine, but are actually demonstrating in practice the issues we have outlined above: they are using opinion rather than science, and they are devaluing the mechanisms of alternative therapies not because these have been proven wrong, but because they consider them unacceptable, that is, "outside the current understanding of modern medicine" or "implausible".

In distinguishing between orthodox medicine's "language of science" and the language of alternative medicine (described later), the authors are also implying that orthodox medicine has scientific validity and that alternative medicine does not, but they offer no evidence to support this distinction. Such an assumption is unacceptable in a book challenging the scientific validity of

alternative medicine, especially when the authors are redefining the term 'science' to suit their purposes. If orthodox medicine is scientific it should be demonstrated that this is the case, so that readers can compare its theoretical and experimental justifications with those for alternative therapies.

The Use of Evidence

The weakess of the authors' definition of science emerges almost immediately after this in a passage from which we have already quoted:

> 2. Or maybe alternative medicine is overwhelmingly effective. Perhaps the sceptics, including many doctors, have simply failed to recognise the benefits of a more holistic, natural, traditional and spiritual approach to health. Medicine has never claimed to have all the answers, and over and over again there have been revolutions in our understanding of the human body. So will the next revolution lead to a discovery of the mechanisms underlying alternative medicine? Or could there be darker forces at work? Could it be that the medical establishment wants to maintain its power and authority, and that doctors criticize alternative medicine in order to quash any rivals? Or might these self-same sceptics be puppets of the pharmaceutical corporations who merely want to hold on to their profits? (p. 2)

Assuming the premise that alternative medicine is overwhelmingly effective, Ernst and Singh offer two explanations for people failing to recognise the fact: deception (which is made to appear ridiculous by describing it in terms of "darker forces") or the inexplicability of the evidence. In latter case, this means that they are arguing that even if there is "overwhelming" evidence of effectiveness, scepticism is an reasonable response in the absence of an explanation (theory). Nonetheless, the authors will go on to reinforce the claim they have already made that evidence is the sole and sufficient foundation for determining what therapies work, and to promote (in 'Chapter 1') a very narrow expression of evidence-based medicine.

While any scientific argument does need to be based on evidence, this evidence needs to be verifiable, but the authors seem not to regard this as necessary in their own case. We have already shown that they are prepared to quote people without saying where the information comes from, but they also provide figures without any source for them. For example they refer to

> the current plethora of alternative treatments that are rapidly growing in popularity (p. 1)

and go on to remark that

> These treatments are piled high in every pharmacy, written about in every magazine, discussed on millions of web pages and used by billions of people, yet they are regarded with scepticism by many doctors. (p.1)

In a world with a population of less than 7 billion people, statements about "billions of people" need justification, and in the context of references to "every pharmacy" and "every magazine", it is reasonable to wonder whether this is a statement of fact at all, or merely a rhetorical figure aimed at suggesting the overwhelming influence of alternative medicine. Only three paragraphs later, we appear to get the answer when the authors continue:

> Indeed, it is estimated that the annual global spend on all alternative medicines is in the region of £40 billion, making it the fastest-growing area of medical spending. (p. 2)

Again the figure is unreferenced, and again it is used to justify claims about the scale of influence of alternative medicine and the rate of growth of this influence, but, whether it is true or not, it is also a serious misuse of evidence.

Ernst is a professor who "has spent fifteen years trying to work out which treatments work and which do not" (p. 3), largely based on randomised clinical trials involving statistical analysis; Singh "has spent almost two decades as a science journalist" (p. 3) and has written a book about a mathematician (*Fermat's Last Theorem*); and both of them are "trained scientists" (p. 3). Neither of them has any excuse, therefore, for not realising that two comparisons of expenditure, the first between different times and the second between different areas of medicine, require the support of a number of figures illustrating rates of growth for various defined fields. To provide as "evidence" only a single, estimated and unreferenced figure to justify their claim is incompetent and shows a contempt for rigour in their approach.

Manipulation

The explanation for the authors' attitudes to science and evidence can be found by exploring this particular evidence further. If we provide a context for the figure of "£40 billion", in 2006 the U.S.A. (a single country) spent $216.7 billion[6] (£145 billion) on orthodox prescription drugs, which is more than three times this alleged total alternative medicine spending for the whole globe. Globally drug sales in 2006 were $643 billion[7] (£435 billion), or more than 10 times Ernst and Singh's figure, and at an annual increase of 7% global sales will increase by over £30 billion per year. Clearly the influence of alternative medicine is nothing like as significant as is being implied, and is in fact dwarfed by that of the pharmaceutical companies.

This process of manipulating the evidence can actually be seen without quoting from outside Ernst and Singh's book. In the previous quotation "billions of people" were contrasted with "many doctors", but in the immediately following sentence the authors refer to alternative medicine as any therapy not accepted by "the majority of mainstream doctors" (p. 1): the tiny minority in respect of alleged general influence becomes the majority in respect of medical authority. At the same time, there is no evidence provided to support the claim that this view is a majority one, nor is the term "mainstream doctors" defined. While its implied meaning is orthodox medical practitioners, the authors state that "surveys show that in many countries over half the population use alternative medicine in one form or another" (p. 3), and that in India "there are 300,000 qualified homeopaths" (p. 93), so there is no certainty about what is really meant by the "majority of mainstream doctors".

Ernst and Singh's distortion of evidence is systematic. Just prior to their claims about the size and growth of the alternative medicine market discussed above, they have compared science and opinion (as we have already noted), stating that

> In contrast, opinions are subjective and conflicting, and whoever has the most persuasive PR campaign has the best chance of promoting their opinion, regardless of whether they are right or wrong. (p. 1)

In the context of this reference to the use of a "persuasive PR campaign", the inflated view of alternative medicine's economic power leads directly to the conclusion that its success is based entirely on powerful "PR", and this conclusion is actively encouraged by the passage which immediately follows the figures discussed (our emphasis):

> 1. Perhaps alternative medicine is entirely useless. Perhaps *persuasive marketing* has fooled us into believing that alternative medicine works. Alternative therapists might seem like nice people, talking as they do about such appealing concepts as 'nature's wonders' and 'ancient wisdom', but they might be misleading the public – or maybe they are even deluding themselves. They also use impressive buzzwords like holistic, meridians, self-healing and individualized. If we could see past the jargon, then would we realise that alternative medicine is just a scam? (p. 2)

In this way the seed of an idea is planted and then developed through other topics to become a specific attack. Such concealed arguments, based on suggestion and innuendo occur elsewhere in this book all too frequently.

Another example of this process is seen when we compare these remarks about "persuasive marketing" with the remarks (already quoted above) about deception by orthodox medicine:

> Or could there be darker forces at work? Could it be that the medical establishment wants to maintain its power and authority, and that doctors criticize alternative medicine in order to quash any rivals? Or might

> these self-same sceptics be puppets of the pharmaceutical corporations who merely want to hold on to their profits? (p. 2)

The idea that the "pharmaceutical corporations" and the orthodox "medical establishment" might have a vested interest or influence is made to look like something out of Harry Potter, a foolish belief in "darker forces", yet we have seen that their real economic power is more than ten times that of alternative medicine. Ernst and Singh have manipulated the information carefully in order to invert the reader's understanding of the true relationship between orthodox and alternative medicine. This is propaganda.

Propaganda

When we look more closely at the other reasons for the authors referring to "darker forces", we can identify another progression of ideas in the Introduction. This passage and the one preceding it are supposed to be replies to the following question:

> So who is right: the critic who thinks alternative medicine is akin to voodoo, or the mother who entrusts her child's health to alternative medicine? (p. 2)

In this context it can be seen that the authors are starting to equate alternative medicine with a mystical perspective, and orthodox medicine with a scientific perspective. Thus supporters of alternative medicine see the "pharmaceutical corporations" and orthodox "medical establishment" as "darker forces" whilst supporters of orthodox medicine see their opponents as having a "persuasive PR campaign". This is encouraged by comparing the "critic who thinks" with the "mother who entrusts", and further reinforced by the juxtaposition of a religion popularly associated with human sacrifice ("voodoo") with a mother offering up her child on trust to alternative medicine. The suggestion is being developed that alternative medicine is not safe but a dangerous belief.

This theme is returned to after a couple of paragraphs about the authors' dedication to the truth, including their claims to offer "rigour, authority and independence", to "examine the various alternative therapies in a scrupulous manner", and to "get closer to the truth than anybody else" p. 3). They then state that

> Our mission is to reveal the truth about the potions, lotions, pills, needles, pummelling and energizing that lie beyond the realms of conventional medicine, but which are becoming increasingly attractive for many patients. What works and what doesn't? What are the secrets and what are the lies? Who is

> trustworthy and who is ripping you off? Do today's doctors know what is best, or do the old wives' tales indeed tap into some ancient, superior wisdom? (p. 3)

By starting a list of treatment vehicles with the juxtaposition of "truth" and "potions", with all the latter's connotations of magic, the authors seek to devalue these mechanisms within alternative medicine, even though most of them are normal parts of practice in orthodox medicine too. The subsequent use of the words "secrets", "lies", "old wives' tales" and "ancient superior wisdom" continues to develop this theme, and to establish the image of alternative medicine as a form of witchcraft. This image is then contrasted with the next sentence, and the promise that

> All these questions and more will be answered in this book, the world's most honest and accurate examination of alternative medicine. (p. 3)

In less than three pages the authors have produced unsupported evidence, misrepresented facts, redefined science to suit their own needs, failed to use their own definitions consistently, implied that alternative medicine is a combination of a "persuasive PR campaign" and witchcraft, and told us that this is "the world's most honest and accurate examination of alternative medicine". However, before the end of this third page Ernst and Singh show again the extent to which they are prepared to depart not just from a scientific approach, but from the language of science.

Fundamental Questions

The authors state that

> In particular, we will answer the fundamental question: 'Is alternative medicine effective for treating disease?' (p. 3)

But they then go on to comment that

> Although a short and simple question, when unpacked it becomes somewhat complicated and has many answers depending on three key issues. First, which alternative therapy are we talking about? Second, which disease are we applying it to? Third, what is meant by effective? (p. 3)

In other words, two "trained scientists" (p. 3) are immediately admitting that a fundamental question is not fundamental at all. What is worse the degree to which this question is not fundamental is extraordinary even using the authors' own terms of reference.

In the first stage of 'unpacking' Ernst and Singh admit for the first time that alternative medicine is not a homogeneous entity but just a convenient group name for a range of very different

therapies, so by the count of this book alone the "fundamental question" is now 36 questions. In the second stage of 'unpacking' Ernst and Singh decide that the specific disease is important. They do not define disease, but if we use the orthodox medical view, *The Merck Manual of Medical Information*[8] lists about 240 *types* of illness, most of which have subcategories. Even if we ignore the subcategories, this would mean that the "fundamental question" is now 8,640 questions. In the third stage of 'unpacking' Ernst and Singh acknowledge that the term "effective" requires defining. Within orthodox medicine the term has an arbitrary definition, but this is not accepted by all forms of alternative medicine, and it may not accord with the views of patients themselves. However, if we accept that there are only three alternatives (those of orthodox medicine, homeopathy and the general public), this would still mean that the "fundamental question" is actually more than 25,920 questions.

It was irrational enough for Ernst and Singh to claim that something fundamental could be divided into three parts, but for it to be divisible into over 25,000 separate parts makes nonsense of the idea that their approach is scientific. In fact they have demonstrated that to answer their fundamental question "Is alternative medicine effective for treating disease?" on the basis of their methods, that is on the basis of what specific treatments are effective for which specific diseases, is to create a never ending problem. Indeed, part of the success of the scientific method is that it addresses this weakness of empiricism by relating the specific answers to a general understanding capable of application beyond the particular case, that is, by relating the evidence to theory. In order to successfully examine alternative medicine Ernst and Singh will have to take into account the role of theory in orthodox medicine and in each alternative therapy and the way this redefines disease and effectiveness. In fact, they fail to do this, and the next chapter will not only show why, but will expose their whole rationale as absurd.

Undefined Terms

In considering Ernst and Singh's "fundamental question" another point arises, namely the definition of terms. We have pointed out that their definition of alternative medicine is based on opinion and that their definition of science is idiosyncratic, but there is also a need to define disease and effectiveness since there are significant differences between the approaches of orthodox medicine and some alternative therapies. The authors touch on this by asking "which disease are we applying it to?" and "what is meant by effective?" but they never answer these questions. What is

more, they show so little concern for the meaning of terms that they state that alternative medicine uses

> impressive buzzwords like holistic, meridians, self-healing and individualized. If we could see past the jargon, then would we realise that alternative medicine is just a scam? (p. 2)

In fact all these words refer to specific concepts for which there is no other term, and for this reason Ernst and Singh will themselves use the words "holistic", "meridians" and "individualized" as legitimate terms later in their book, as well as using the concept of "self-healing". Furthermore, jargon is simply the specialist terminology in any field, such as quarks in physics with names like 'up', 'down', 'charm' and 'strange'. To categorize the terminology of a field as "impressive buzzwords" is hypocritical, especially when orthodox medicine itself (in English at least) uses an extensive jargon of Greek or Latin terms which were originally simply synonyms for quite ordinary expressions. In fact it could be argued that much of orthodox medical jargon was deliberately created to impress patients or to deny them access to information, whilst that of alternative medicine is largely functional. At the same time some alternative therapies, including homeopathy, have always insisted that everyday terms should be used as far as possible.

This attitude to jargon brings us to one last related point in the introduction. Although Ernst and Singh have elected to use the term "alternative medicine" throughout rather than the more cumbersome "complementary and alternative medicine" (CAM), by the end of the introduction they have referred to orthodox medicine as "mainstream doctors" once, "mainstream medicine" once, "the establishment" once, "the medical establishment" once, "modern medicine" once, "conventional medicine" twice, and "medicine" (by itself) twice (excluding its use in the phrase "father of medicine" when it genuinely applies to medicine as a whole). The importance of this is that while alternative therapies have principles or origins which define them and give them their names, in the eyes of these authors their competitor is simply defined by its official status. This perspective in which orthodox medicine is defined by its powerful position, rather than by any methodological or theoretical homogeneity, leads to a number of errors, because it means that conflicting evidence cannot be accepted as part of a process of change and development, but is seen as a challenge to authority. The authors will themselves demonstrate this problem in the history of medicine, but fail to acknowledge its significance to their own work, just as they are starting their book without acknowledging that they have no satisfactory definitions of science, disease, effectiveness, orthodox medicine or alternative medicine on which to base their examination.

Discussion of Chapter 1: How Do You Determine the Truth?

In the Introduction Ernst and Singh prepare readers for this chapter by saying that

> Chapter 1 provides an introduction to the scientific method. It explains how scientists, by experimenting and observing, can determine whether or not a particular therapy is effective. Every conclusion we reach in the rest of the book depends on the scientific method and on an unbiased analysis of the best medical research available. So, by first explaining how science works, we hope to increase your confidence in our subsequent conclusions. (p. 4)

However, they do not fulfil this promise because firstly, they do not explain the scientific method; secondly, they do not fully explain evidence-based medicine, the approach which they say "allows them to separate the effective from the ineffective" (p. 7); and thirdly, they do not define what they mean by "effective". They also have other problems:

• They continue to get facts wrong, as when they remark that

> The term 'vitamin' describes an organic nutrient that is vital for survival, but which the body cannot produce itself; so it has to be supplied through food. (p. 15)

'Vitamin' may be the name given to "an organic nutrient that is vital for survival" – it certainly does not describe one – but the body is quite capable of producing vitamins A, B3 (niacin), D and K.

• They continue to contradict themselves, as when they say that

> Despite being an undoubted force for good, evidence-based medicine is occasionally treated with suspicion. (p. 26)

Suspecting something necessarily means that you do not believe it to be an "undoubted force for good".

• They also continue to present unsupported figures which demand evidence, as in the following series of statements:

> … eradicating smallpox and saving literally millions of lives each year (p. 7)

> In turn this would allow doctors to save hundreds of millions of lives (p. 20),

and when referring to the list of "the fifteen greatest medical breakthroughs" since the foundation of the *British Medical Journal*:

> Oral hydration, which helps recovery from diarrhoea and which has saved 50 million children's lives in the last 25 years. The list also included antibiotics, germ theory and immunology, which together have helped to cure a whole range of diseases, thereby saving hundreds of millions of lives. Vaccines, of

> course, were on the list, because they have prevented many diseases from even occurring, thereby saving hundreds of millions more lives. And awareness of the risks of smoking has probably saved a similar number of lives. (p. 35)

In his seminal work on the reduction in mortality rates in England and Wales, Thomas McKeown stated that it was wrong to think that medical advances had been the main reason for this reduction, and that it is a conclusion which "results from failure to distinguish between the interests of the doctor and the interests of the patient, a common error in the interpretation of medical history".[9] He went on to conclude "that immunization and treatment contributed little to the reduction of deaths from infectious diseases before 1935, and over the whole period since cause of death was first registered (in 1838) they were much less important than other influences".[10] (As the medical historian Simon Szreter, pointed out the main influence was improved public health measures.[11]) The largest influence on mortality figures cited by McKeown is "vaccination against smallpox (which was associated with 1.6 per cent of the decline of the death rate from 1848-54 to 1971)".[12] However, if we take the US Census Bureau's estimations of global population between 1750 and 2008 and assume that every 50 years there is a wholly new population, the total population for this period would be 13,979,035,426. If we also assume that Ernst and Singh are referring to only 200 million lives saved in total, then they are claiming (without any evidence) that medicine has saved a staggering 1.43% of all lives throughout the world between 1750 and 2008.[13]

Evidence-based medicine

Ernst and Singh open Chapter 1 with the claim that

> This book is about establishing the truth in relation to alternative medicine. Which therapies work and which ones are useless? Which therapies are safe and which ones are dangerous? (p. 7)

They go on to say that the answer to these questions lies in the use of evidence-based medicine. We do not intend to discuss the merits of evidence-based medicine here, but only to look at Ernst and Singh's attitude to this approach and how they justify their version of it as a means of testing alternative medicine. To begin with they exaggerate its significance, as they state that (their emphasis)

> This approach, known as *evidence-based medicine*, has revolutionized medical practice, transforming it from an industry of charlatans and incompetents into a system of healthcare that can deliver such miracles as transplanting kidneys, removing cataracts, combating childhood diseases, eradicating smallpox and saving literally millions of lives each year. (p. 7)

In fact cataracts were being surgically removed in 1748 (and may have been removed in ancient times), whilst the authors' earliest example of randomised clinical trials is from 1747 (unpublished until 1753), so they provide no evidence that cataract surgery can be credited to evidence-based medicine.

Ernst and Singh's lack of rigour continues in the following definition of evidence-based medicine, attributed to David Sackett, but unreferenced:

> 'Evidence-based medicine is the conscientious, explicit, and judicious use of current best evidence in making decisions about the care of individual patients.' (p. 24)

Given that these very words are used by Sackett and others in an article in the *British Medical Journal*,[14] this may be the source Ernst and Singh used, but if so the quotation is only the beginning of two paragraphs which amplify exactly what is meant by this statement. In particular Sackett et al. explain the need for both research evidence and clinical experience in order to practice to the highest standard. As they point out,

> Without clinical expertise, practice risks becoming tyrannised by evidence, for even excellent external evidence may be inapplicable to or inappropriate for an individual patient.[15]

Blair Smith commented on the same issue in a response to this article, stating that

> Forms of evidence allowable in evidence based medicine, while now extending beyond the randomised controlled trial, remain heavily numerate. This encourages emphasis on the quantifiable and physical aspects of any clinical dilemma, which may be inappropriate. Denial of social and psychological aspects may be detrimental, and ignoring the less readily measured dimensions may be dangerous. Rich sources of evidence also include the anecdotal, which are so often slated, and the qualitative, which is not mentioned by this lobby yet has developed considerably and provides illuminative results.[16]

Ernst and Singh, however, not only promote the idea that (their emphases) "a *randomized controlled trial* (or RCT) or a *randomized clinical trial*" is "the gold standard for putting therapies to the test" (p. 22), but they also give no indication that any other method is needed, whereas the RCT is simply a single tool in the practice of evidence-based medicine, used to test the effects of particular interventions, but not sufficient in itself to validate the effectiveness of whole therapies.

The RCT

As part of their justification for using RCTs to test therapies, Ernst and Singh present four examples in this chapter, claiming that they show that this tool and the prospective cohort study can be used

> … to decide what works (lemons for scurvy), what does not work (bloodletting), what prevents disease (hygiene) and what triggers disease (smoking). (p. 36)

For their purposes, the crucial example is clearly that of "what works (lemons for scurvy)", but this claim is based on confusing two types of intervention. The authors repeatedly refer to the treatment of scurvy in terms of a *curative intervention* – "lemons were the key to curing scurvy" (p. 18); "the ability of oranges and lemons to cure scurvy" (p. 18) "vitamin C, the active ingredient that cured scurvy" (p. 18). In fact, we need to be clear that the condition which lemons, oranges or vitamin C are actually curing is the *absence* of vitamin C in the diet. In other words the treatment in this case is actually the *ending of a harmful intervention* (deprivation of vitamin C), and this harmful intervention is the one and only cause of the illness. In this respect dietary deficiency diseases and poisonings are totally different from infections or chronic diseases, where there are multiple causes. The point can be illustrated by reference to another of Ernst and Singh's examples: loss of blood as a result of bloodletting simply requires one to stop depriving the patient of blood, whereas a haemorrhage requires an active intervention to be initiated to solve the problem. Nobody would call the former action a 'cure', yet that is precisely what Ernst and Singh are doing in the case of scurvy.

In the light of this correction, it can be seen that all of Ernst and Singh's examples involve the use of the RCT (or prospective cohort study) to study a very specific type of illness, the consequences of a harmful human intervention: deprivation of vitamin C, deprivation of blood, exposure to high concentrations of pathogens and continual exposure to a toxic substance. In these cases the existing state is being compared with one in which the sole *causative* intervention is removed. What these examples do not show is that the RCT can test interventions where the condition is due to a cause other than the intervention, and the intervention is intended to be *curative*. In the former case continuing the causative intervention increases harm, whereas in the latter case continuing the curative intervention is supposed to produce benefit. Thus success in the former case is based on measuring the *harmful effects* of the causative intervention, whereas success in the latter case is based on measuring the *beneficial effects* of the curative intervention. In short, these examples support a claim that the RCT provides information about harmful interventions, but they do not support a claim that the RCT can prove the effectiveness of a curative intervention. The

failure of the authors, "both trained scientists" (p. 3), to recognise this indicates a remarkable lack of understanding of their subject.

The authors not only fail to provide evidence that the RCT can be used to test curative interventions, but their own statements support the argument that it cannot possibly be used for this purpose. If we look again at their four examples, in each case the harmful intervention would affect everybody if it were sufficiently intense or prolonged, and the outcome is easily determined, since it involves essentially the same pattern of response in everybody: an increase in illness or mortality, or a reduction in these if the harmful intervention is stopped. As a result trials show clear, even dramatic differences between continuing the intervention and stopping it: in the case of scurvy "the mortality rate was cut in half" (p. 19); in the case of bloodletting "the death rate for patients treated with bloodletting was ten times greater" (p. 21); in the case of hygiene "the death rate for all admitted soldiers was 43 per cent, but after her reforms it fell dramatically to just 2 per cent" (p. 27); and in the case of smoking "43 per cent of smokers compared with 15 per cent of non-smokers died between the ages of 35 and 69 years" (p. 34). In all these examples, the only significant difference between individuals is the *rate* at which they succumb to the harmful intervention, which is to say that while the *ill effects are common* to everyone, the *resistance to illness is individual*.

Since the resistance to illness is simply a facet of the ability to fight off illness (to get well and to stay well), this means that the curative response as a whole is individual, however common to everybody the pressure to illness may be. Ernst and Singh are aware of this, because although they show that the RCT is capable of determining general effects, that is those which harm, they explicitly state that it is incapable of providing evidence of what will help the individual:

> After the advent of the clinical trial, doctors could choose their treatment for a single patient by examining the evidence from several trials, perhaps involving thousands of patients. There was still no guarantee that a treatment that had succeeded during a set of trials would cure a particular patient … (p. 23)

This point was also made by both Sackett et al. and Smith in the quotations above, and it has enormous implications when testing interventions for curative effectiveness as opposed to possible harmful effects. In particular the authors' comments on the RCT raise questions about the relevance of the evidence it provides (which we will look at next) and about the need to match the clarity of illness and death as direct consequences of an intervention with an equal clarity in the meaning of cure or recovery and effectiveness at achieving these (which we shall look at later).

The Relevance of the Evidence

The final sentence of the previous quotation ends with the following comment:

> … but any doctor who adopted this approach was giving his patient the best possible chance of recovery. (p. 23)

This statement is echoed in the comments of Pierre Louis (unreferenced) that

> It was impossible to know whether or not a treatment might be safe and effective for the individual patient unless it had been demonstrated to be safe and effective for a large number of patients: 'A therapeutic agent cannot be employed with any discrimination or probability of success in a given case, unless its general efficacy, in analogous cases, has been previously ascertained' (p. 23);

in the comments of Alexander MacLean (unreferenced) that

> avoiding trials would mean that medicine would forever be nothing more than a collection of untested treatments, which might be wholly ineffective or dangerous. He described medicine practised without any evidence as 'a continued series of experiments upon the lives of our fellow creatures.' (p. 23);

and again on the following page, when the authors state that

> Evidence-based medicine empowers doctors by providing them with the most reliable information, and therefore it benefits patients by increasing the likelihood that they will receive the most appropriate treatment. (p. 24)

The problem is that the evidence being referred to is irrelevant to the purpose being discussed.

A simple illustration of this point is to imagine that a survey of shoe sizes reveals that one size is worn by a higher proportion of people than any other. This evidence may be extremely useful to manufacturers and retailers of shoes, but it is utterly useless to a purchaser of shoes. For the purchaser, it does not matter how many other people a pair of shoes may fit, the only evidence which has any importance to them is the size of their own feet and the size of the shoes they want to buy. In the same way no matter how many times a treatment is tested for its curative effects in a population, there can be no certainty that the evidence will be relevant to an individual so long as the curative process is individual.

Ernst and Singh blandly ignore this mismatch of evidence and purpose and tell us that

> From a twenty-first century perspective, it seems obvious that medical decisions should be based on evidence, typically from randomized clinical trials, but the emergence of evidence-based medicine marks a turning point in the history of medicine. (p. 24)

They clarify this "turning point" by claiming that, despite the fact that a treatment is now based on "probability", "likelihood" and "chance" (see above), evidence-based medicine

> transformed medicine from a dangerous lottery in the eighteenth century into a rational discipline in the twentieth century. (p. 24)

However great this transformation may be, the reality is that the NHS spends £8.2 billion on drugs[17] tested using RCTs, but is estimated to spend £2 billion on adverse reactions to those prescribed drugs.[18] The issue of the relevance of evidence is not just a philosophical problem but a very real one affecting people's health and lives, and for many individuals medicine could still be described as "a continued series of experiments upon the lives of our fellow creatures" (p. 23).

The Meaning of Effectiveness

The second problem with the RCT as a tool for testing treatments (let alone whole therapeutic systems) is the need for clarity about what is meant by effectiveness. The authors have pointed this out themselves in the Introduction, when they asked "what is meant by effective?" (p. 3), but the issue is clearer now as a result of their discussion of the value of the RCT in identifying the consequences of harmful interventions. Just as harmful interventions produce general and easily identifiable results, the effectiveness of *stopping* the intervention is also easily identified. In the case of *initiating* an intervention with the purpose of making someone better, success depends on the person's individuality because the curative response is individual. As a result, any definition of effectiveness must take this individuality into account, or include a justification of why it is not necessary to do so. In either case, there needs to be some objective measure of benefit with a scientific explanation of how it has been obtained.

The authors certainly believe that they have such an objective measure of benefit, as they state:

> Which therapies work and which ones are useless? Which therapies are safe and which ones are dangerous?
>
> These are questions that doctors have asked themselves for millennia in relation to all forms of medicine, and yet it is only comparatively recently that they have developed an approach that allows them to separate the effective from the ineffective, and the safe from the dangerous. (p. 7)

However, throughout their discussion of the only approaches they actually offer, the RCT and prospective cohort study, they never mention how they define the effectiveness of a curative

intervention. Clearly Ernst and Singh must have some definition of effectiveness which enables them to measure benefit, but they do not tell us, and they certainly do not provide evidence that it is a scientifically valid one.

If this is a problem when it comes to the credibility of the authors' assumptions about orthodox medicine, the problem is much greater when it comes to their assessment of alternative medicine. This is because some alternative therapies have their own definitions of effectiveness which form a crucial part of assessing the results of interventions and determining the course of treatment. As a result any scientific examination of these therapies requires a discussion of both the authors' definition and that used by each therapy, and an explanation of why one definition is being used in a trial as opposed to another. After all, without any knowledge of what definition of effectiveness is being used, it is not possible to be sure that the trials of these therapeutic interventions are using a measure at all appropriate to the therapy.

Closely related to this problem is the question of how disease is defined, but this is an issue which Ernst and Singh have not even acknowledged at this point. In the Introduction they point out the importance of knowing "which disease are we applying it to?" (p. 3) when assessing a treatment, but again some alternative therapies have their own definitions, which may not accord with the unstated one used by the authors. As a result any scientific examination of alternative therapies requires a discussion of both the authors' definition of disease and that used by each therapy, and again an explanation of why one definition is being used in a trial as opposed to another.

Theory

The final points which need to be made about this chapter involve the crucial importance of theory to the scientific method. Despite quoting the unreferenced remark by James Lind that

> he would 'propose nothing dictated merely by theory; but shall confirm all by experience and facts, the surest and most unerring guides' (p. 19),

Ernst and Singh themselves have confined themselves to "experience and facts" and failed to explain the theory which these are supposed to confirm. Indeed the scientific method developed because experience and facts *alone* (empiricism) are not "the surest and most unerring guides", any more than theory *alone* (rationalism). The scientific method merges the two approaches so that the

one can be used to inform the other. The fact that this chapter was supposed to provide "an introduction to the scientific method" (p. 4), but fails to place evidence in its proper relation to theory, raises the question about whether the authors actually have a theory of medicine which can enable their experience and facts to be integrated into a science. This is particularly important because if they are not basing their examination of alternative medicine on a science of medicine, then they have no credible basis for claiming to assess these therapies scientifically.

The nearest Ernst and Singh come to tackling this question is when they actually evade it by stating that

> More generally, well-designed scientific studies and trials are not engineered to achieve an expected outcome, but rather they should be transparent and fair, and those conducting the research should be open to whatever results emerge. (p. 34)

By referring to the need to be "transparent and fair" they shift attention towards the motives of researchers and away from the theoretical context within which a trial takes place. As Karl Popper has pointed out, however,

> … the belief that we can start with pure observation alone, without anything in the nature of a theory, is absurd … Observation is always selective. It needs a chosen object, a definite task, an interest, a point of view, a problem.[19]

Some expectation of outcome is needed in order for the trial to be constructed in the first place, and what determines this expectation is the context governing the researchers' ideas. By failing to provide this context for their own examination of alternative medicine, and by failing to consider this context when evaluating trials of alternative medicine, Ernst and Singh are attempting to remove evidence from its context and present it in the abstract as though it had some absolute truth of its own. In context, evidence does indeed provide "extraordinarily powerful and persuasive conclusions", but out of context it has no value whatsoever. In the following chapters Ernst and Singh will be examining four alternative therapies, but they will be doing so without theoretical principles, without defined terms and without a valid methodology, and so it is reasonable to expect that their conclusions will also be without scientific validity.

Discussion of Chapter 2: The Truth About Acupuncture

We do not pretend to know enough about acupuncture to respond to some of the details in this chapter concerning the therapy itself, but some of the statements made by the authors do not indicate that they are impartial in their approach. For example, they comment that "meridians or the flow of Ch'i" are concepts which "have no meaning in terms of biology, chemistry or physics" (p. 52), but whilst this may be true in respect of these fields at present, research has been conducted which might lead to a recognition of some validity for these concepts.[20] This aspect is not discussed at all by the authors, who depend entirely on RCTs for their 'evidence'.

Another curious point is that at the beginning of the chapter (pp. 55-6) Ernst and Singh accept that the tattoos on a 5,000 year old frozen corpse found in Europe are of meridians and acupuncture points. It is not clear why they consider the possibility of acupuncture originating outside China so important, but the tattoos do raise the interesting question as to how such very detailed information, transmitted for up to half that time largely by oral tradition, could remain consistent over 5,000 years and approximately 5,000 miles. Of course, if the knowledge of this therapy were being supported by the objective verification of people's experience, there would be no problem about an explanation, but if the action of the therapy were purely that of the so-called 'placebo effect', one might reasonably expect cumulative errors in the process of transmission to lead to significant change. This aspect is not discussed by the authors.

The Placebo Effect

The placebo effect is the most important concept discussed in this chapter, and it is interesting that Ernst and Singh apply the term to two forms of response. The first is changes imagined by the patient:

> It was not until 1832 that *placebo* took on its specific medical meaning, namely an insincere or ineffective treatment that can nevertheless be consoling. (p. 57)

The second is physiological changes observable by others:

> In fact scientists have observed real physiological changes in the body, suggesting that the placebo effect goes far beyond the patient's mind by also impacting directly on physiology. (p. 60)

However, they never discuss the significance of the difference between these two types of occurrence. In addition, they acknowledge that the mechanism of action of the placebo effect is unknown, noting, that

> While scientists strive to establish the scientific basis of the placebo effect, they have already been able to ascertain, by building on Haygarth's early work, how to maximise it. (p. 62)

In fact the reference to a single placebo effect may itself be inaccurate, since the evidence presented by the authors does not rule out the possibility that there may be a number of different unknown processes occurring and interacting with each other, but all gathered under the name of the placebo effect.

The lack of clarity about what is meant by the term in this book, together with the absence of a "scientific basis of the placebo effect", does not deter Ernst and Singh from depending on it heavily from this point onwards in their investigation of alternative medicine. Nor does it prevent them from expressing a hypothesis about its nature:

> … perhaps the placebo response is partly the consequence of an innate ability to block the acute phase response at a fundamental level, possibly by the power of expectation". (p. 62)

This exercise in pure jargon offers no more certainty than "perhaps" and "possibly", no more accuracy than "partly", and no more explanation than the use of undefined terms, such as "innate ability", "fundamental level", and "the power of expectation", and in consequence is merely a string of "impressive buzzwords" masquerading as science.

In trying to clarify what factors are significant for the placebo effect, they state that

> … among other things, the doctor's reputation, the cost of the treatment and its novelty could all boost the placebo effect. (p. 57)

They expain that this is because

> … the placebo effect arises out of the patient's confidence in the treatment. (p. 57)

They then clarify this further by saying that on the one hand it may depend on the conscious expectation of a particular result:

> Rather than treating a wounded soldier without morphine, he injected saline into the patient and suggested to the soldier that he was receiving a powerful painkiller. To Beecher's surprise, the patient relaxed immediately and showed no signs of pain, distress or shock. (p. 58)

On the other hand they say it may depend on unconscious conditioning to expect a particular result:

> Researchers worked with guinea pigs, which were known to develop a rash when injected with a certain mildly toxic substance. To see if the rash could be initiated through conditioning, they began lightly scratching the guinea pigs prior to giving an injection. Sure enough, they later discovered that merely scratching the skin and *not* giving the injection could stimulate the same redness and swelling. (p. 60)

These effects are illustrated further by noting the variation in responses as a result of differences in the delivery system, or the size and colour of pills (p. 62).

When Ernst and Singh come to discuss the relationship of acupuncture to the placebo effect, they state that

> … acupuncture has many of the attributes that would make it an ideal placebo treatment: needles, mild pain, the slightly invasive nature, exoticism, a basis in ancient wisdom and fantastic press-coverage. (p. 67)

This list of "attributes that would make it an ideal placebo treatment" has virtually no connection with the key factors mentioned earlier, "the doctor's reputation, the cost of the treatment and its novelty" (p. 57), or with the examples they themselves have provided, but no explanation is offered for this discrepancy. Furthermore, the authors do not explain how confidence in the treatment and expectation of a particular result play any part in the reaction to acupuncture. To conform with the authors' model of the placebo effect it would be necessary for the patient to be told what the specific reaction to treatment would be, but it is not made clear that this is what happens. Furthermore, in Chapter 6 they provide an example which appears to show that there is no significant conection between expectation and benefit in acupuncture.

Individuality

Ernst and Singh also remark that the placebo effect has a "potentially powerful effect on recovery" (p. 62), and that it is significantly related to the individuality of the patient, asserting that

> the actual placebo effect for a particular patient depends entirely on the belief system and personal experiences of that individual. (p. 62)

They do not explain how they arrive at the information that "the belief system and personal experiences" are so important. Nonetheless this confirms the authors' position in the previous chapter to the effect that the individuality of a patient is a significant factor in their resistance to and recovery from illness. Despite this further acknowledgement that the curative process is individual, however, Ernst and Singh persist in seeing individuality not as a factor which must be an integral part of a successful system of medicine but rather as a serious problem to be overcome in tests. Such perversity in the face of their own statements indicates that they do not have a scientific attitude to their subject but a prejudice which prevents them from rationally assessing the truth.

The Hawthorne Effect

Another effect discussed in this chapter is the Hawthorne effect, the hypothesis that

> the act of close monitoring can lead to a generally positive change in a person's health or performance. (p.65)

At the very best, the claims for the Hawthorne effect were not that the improvements were continuing ones, but that (our emphasis)

> Improved morale and increased productivity were observed, *temporarily*, in response to each of the interventions.[21]

However, in 1989, the most complete study of the research on the subject stated that

> The descriptive analysis of 86 studies revealed a diversity of practices designed to control one of three artifact variables: special attention, activity related to the experimental task, or awareness of participation in an experiment. The meta-analysis provided no evidence for a Hawthorne versus no-treatment control difference.[22]

The reason why the authors refer to this effect in this chapter is unclear, not just because of its questionable validity, but also because they do not overtly use it to explain or justify anything. Indeed the only relevance would appear to be in connection with a series of trials of acupuncture published in 2007:

> Germany's Federal Committee of Physicians and Health Insurers took a dramatic step and decided to initiate eight high-quality acupuncture trials, which would examine four ailments: migraine, tension-type headache, chronic low back pain and knee osteoarthritis. These trials were to involve more patients than any previous acupuncture trial, which is why they became known as mega-trials. (p. 82)

The initial conclusions of researchers in one of these trials (and the only one for which there is a reference – as part of the suggested Further Reading) appear to have been criticisd by Ernst, on the basis of the Hawthorne effect. The trial comprised

> A patient- and observer-blinded randomized controlled trial conducted in Germany involving 340 outpatient practices, including 1162 patients aged 18 to 86 years (mean ± SD age, 50 ± 15 years) with a history of chronic low back pain for a mean of 8 years. Patients underwent ten 30-minute sessions, generally 2 sessions per week, of verum acupuncture (n = 387) according to principles of traditional Chinese medicine; sham acupuncture (n = 387) consisting of superficial needling at nonacupuncture points; or conventional therapy, a combination of drugs, physical therapy, and exercise (n = 388).[23]

It produced the following result:

> At 6 months, response rate was 47.6% in the verum acupuncture group, 44.2% in the sham acupuncture group, and 27.4% in the conventional therapy group.

The researchers concluded that

> Low back pain improved after acupuncture treatment for at least 6 months. Effectiveness of acupuncture, either verum or sham, was almost twice that of conventional therapy.

Ernst's letter critcising the researchers' conclusions included the claim that

> The findings become a little less amazing if we consider the following: the 2 acupuncture groups were treated by their physician at least 10 times for 30 minutes with a "hands-on" intervention. The third group essentially received 10 sessions of physiotherapy or a normal consultation with their physician. "Hands-on" treatment by your physician is certainly unusual these days and therefore perhaps more prone than treatment by physiotherapists or normal physician consultations to promote patients' expectation.[24]

However, to maintain that such a sustained improvement by such a large proportion of the subjects is attributable to the attention paid to them requires a much stronger justification than can be provided by the Hawthorne effect (which is at best temporary). To maintain that it could be a product of the placebo effect depends on accepting Ernst and Singh's unexplained assertions about acupuncture being an "ideal placebo treatment", since it is not explicable on the basis of the evidence presented by them about this effect. Indeed, the placebo effect should have boosted the physiological effects of the orthodox treatment, since this carried all the authority of orthodox medicine. Nonetheless Ernst and Singh prefer to maintain that the "evidence seems largely to disprove acupuncture" (p. 84), which again raises questions about their impartiality.

The Validity of Trial Methodologies

The fact that there are problems when designing a trial for acupuncture is to some extent recognised by the authors:

> Ernst and his colleagues proposed the idea of a telescopic needle – that is, an acupuncture needle that looks as if it penetrates the skin, but which instead retracts into the upper handle part, rather like a theatrical dagger. (p. 80)

In this way a distinction could (theoretically) be made between actual needling and perceived needling, thus 'blinding' the trial more effectively whilst determining if the penetration of the needles has any significance. However,

> Unfortunately, it will never be possible to conduct a perfect acupuncture trial, because the ideal trial is double-blind, meaning that neither patient nor practitioner knows if real or placebo treatment is being given. In an acupuncture trial, the practitioner will always know if the treatment is real or placebo. (p. 81)

The importance of this involves another point, namely that the rationale behind the RCT is that *all* the factors which might influence the result should be equalised between the verum and placebo arms of the trial *except* for the one being tested. Such factors include the age and sex of the patient, the type and severity of the patient's condition, and the belief in the genuineness of the treatment. In this case the authors are acknowledging that they see no way of testing the needling in isolation from the practitioner-patient relationship, or without the practitioner being aware of the difference between the placebo and verum treatment.

In all RCTs, as Ernst and Singh's examples in Chapter 1 have shown, equalising relevant factors is an important aspect of a well-designed trial. One problem with this is that some of these factors will be unknown, and can only become known as a result of careful assessment and refining of the the test process. In the case of a therapy which uses an unknown mechanism of action, there is the added problem of determining which factors are critical to producing the outcome, and which are not.

The Effectiveness of Acupuncture

The 2007 trial discussed above is important because it showed that acupunture administered both 'correctly' and 'incorrectly' had substantial beneficial effects on the condition being studied. These results indicate that whatever mechanism is actually being employed, it was effective within the constraints of the trial. Given that there is no understanding of how the placebo effect works, it

cannot explain these results. On the other hand, confirmation that acupuncture (with a history of possibly 5,000 years of use) does work, even within a rigidly controlled trial setting, leads to the logical conclusion that whatever mechanism it uses may offer an explanation for at least part of the unknown placebo effect, which (as we have said) may be a result of many different processes interacting rather than of a single process.

In this context Ernst and Singh's following conclusions to the chapter are unfounded:

> 1. The traditional principles of acupuncture are deeply flawed, as there is no evidence at all to demonstrate the existence of Ch'i or meridians. (p. 83)

The authors have failed to explore the work being done in this area,[25] and they are also making the common mistake of assuming that *no current evidence* of "the existence of Ch'i or meridians" is the same as *evidence that they do not exist*.

> 2. Over the last three decades, a huge number of clinical trials have tested whether or not acupuncture is effective for treating a variety of disorders. Some of these trials have implied that acupunture is effective. Unfortunately, most of them have been without adequate placebo control groups and of poor quality – the majority of positive trials are therefore unreliable. (p. 83)

This conclusion does not take into account that the authors

1. Have failed to show that the RCT can assess curative interventions;
2. Have failed to provide and justify a definition of effectiveness;
3. Have failed to show that the trials used an appropriate method to test the therapeutic intervention, when they have themselves expressed doubts about the method;
4. Have failed to provide an objective measure against which the outcome can be assessed, since the placebo effect has no scientific explanation and may itself actually be (at least in part) a product of the mechanism used by acupuncture.

Therefore they have failed to provide proper justification for their claims about acupuncture trials.

> 3. By focussing on the increasing number of high-quality research papers, reliable conclusions from systematic reviews make it clear that acupuncture does not work for a whole range of conditions, except as a placebo. Hence, if you see acupuncture being advertised by a clinic, then you can assume that it does not really work, except possibly in the treatment of some types of pain and nausea. (p. 83)

The German trial mentioned above indicates that acupuncture can work, but that the assessment of its effectiveness requires a more sophisticated trial model and more investigation of its mechanisms

in order to develop such a trial model. The disparity between this trial and the trial mentioned in Chapter 6 tends to confirm this view.

> 4. There are some high-quality trials that support the use of acupuncture for some types of pain and nausea, but there are also high-quality trials that contradict this conclusion. In short, the evidence is neither consistent nor convincing – it is borderline. (p. 83)

This conclusion simply confirms that the trial model being used may not be appropriate, and that the confusing and contradictory results may be a consequence not of the failure of the effectiveness of acupuncture but of the effectiveness of the trial methodology. The fact that Ernst and Singh have already shown that the trial methodology is flawed lends credibility to this alternative explanation.

Ernst and Singh follow their conclusions with the "five main crtiticisms" allegedly offered by acupuncturists (without references). In the first example they refer to the need to "weigh up the evidence" ("much as a jury would do in a legal case") and "to decide which side of the argument is more convincing" (all p. 84). According to their own criteria, this is not the process of science but of opinion, and so they confirm (as has already been pointed out) their inability to distinguish between science and opinion. Furthermore, the evidence is being weighed up in the absence of any scientific theoretical understanding which can explain the results of the various trials.

In the second example, Ernst and Singh dismiss the issues of complexity and individualisation (regarded as an "impressive buzzword" in the Introduction, but used here by the authors as a legitimate term). They have not, however, explained how complexity and individualisation "can be (and often are) incorporated into the design of clinical trials" (p. 85), which is absolutely necessary if they are going to claim that these issues are genuinely taken into account.

In the third example they claim that

> Many acupuncturists claim that the underlying philosophy of their therapy is so at odds with conventional science that the clinical trial is inappropriate for testing its efficacy. But this accusation is irrelevant, because clinical trials have nothing to do with philosophy. Instead clinical trials are concerned solely with whether or not a treatment works. (p. 85)

Ernst and Singh's choice of words here is significant. Therapies which came to Europe before modern times often keep the old terminologies, so reference to the 'philosophy' of acupuncture is actually a reference to its theory, not to the modern discipine of philosophy. As such acupuncturists raising this issue are noting that the theory of acupuncture is at odds with theories which other people accept, including medical researchers. The authors have already acknowledged that the

principles of operation of acupuncture have to be taken into account in trials, if only in respect of the placing, depth of application or even use of needles, but they have not discussed whether there are any theoretical differences (such as definitions of effectiveness) which could affect the design of trials. The point has already been made in the notes on Chapter 1 that no trial can take place in a theoretical vacuum, so there needs to be clarity about the assumptions being made by those designing and conducting the trials, and about the relevance of these assumptions to the therapeutic intervetion being tested.

In the fourth example Ernst and Singh claim that

> Acupuncturists complain that the clinical trial is inappropriate for alternative therapies because the impact of the treatment is very subtle. (p. 85)

While a trial can be designed to test subtle effects, this is only possible if there is a definition of effectiveness and, furthermore, a definition which allows a high degree of accuracy in understanding those effects. The authors have not shown either of these to be true for the RCT.

In the fifth example they discuss the relationship of 'sham' acupuncture to real acupuncture. They remark that

> We have assumed so far that sham acupuncture is inert, except as a placebo, but is it conceivable that superficial and misplaced needling also somehow taps into the body's meridians? If this turns out to be true, then the entire philosophy of acupuncture falls apart – inserting a needle anywhere to any depth would have a therapeutic benefit, which seems highly unlikely. (p. 86)

This reasoning is simply replacing one assumption (placebo effect) with another (any needling is equally effective), and it shows the importance of theory and well-defined terms if one is to reach proper conclusions. Superficial and misplaced needling may tap into the body's meridians, but just because a trial indicates a similarity in the percentages reporting a specific benefit from real and 'sham' acupuncture, one cannot jump to the conclusion that the responses are the same in both cases (especially as there is no overlap in the percentages). Such trials also focus on a specific set of outcomes, but there may be other outcomes which are not being taken into account, and in addition there may be differences in the individual conditions affected by the two different interventions. As a result such trials are not a proof of the ineffectiveness of acupuncture, but proof of the need for more thorough investigation of the relationship of the trial results to the process employed.

In conclusion, Ernst and Singh have failed in this chapter to provide a secure basis for assessing acupuncture; they have themselves refuted some of their own conclusions and arguments; and they have shown a willingness to make unjustified assumptions critical of acupuncture. In short, their statement about the alleged claims of acupuncturists, that

> none of these criticisms stands up to proper scrutiny. They are the sort of flimsy arguments that one might expect from practitioners who instinctively want to protect a therapy in which they have both a professional and an emotional investment (p. 86),

would appear to be very much a description of the approach of Ernst and Singh themselves.

Discussion of Chapter 3: The Truth About Homeopathy

This chapter is notable for two major deficiencies in the information provided to the reader:

1. The authors ignore all the research conducted by homeopaths after Hahnemann hypothesized the homeopathic relationship of medicines to illnesses, leading to a gross misrepresentation of homeopathy. In particular the authors exclude any discussion of the relationship between theory and experiment in the origins and development of homepathy. As a result they are not examining a real therapy but a construct of their own.
2. The authors do not use primary sources to support their argument, let alone sources that demonstrably test the theoretical principles of homeopathy. Instead they use only secondary meta-analyses, which they acknowledge are based on original trials which did not necessarily take into account the theoretical principles on which homeopathic treatment is based, leading to erroneous results no matter how rigorously conducted the trials may be in other ways.

As a result the authors are depending not on factual evidence, but interpretations of inadequately gathered information

Hahnemann on Bloodletting

To begin with we need to clarify the significance of the statement that

> Having studied medicine in Leipzig, Vienna and Erlangen, Hahnemann earned a reputation as one of Europe's foremost intellectuals. He published widely on both medicine and chemistry, and used his knowledge of English, French, Italian, Greek, Latin, Arabic, Syriac, Chaldaic and Hebrew to translate numerous scholarly treatises. (p. 94)

Ernst and Singh do not explain that Hahnemann's learning gave him access to an extremely wide range of sources of information, which enabled him to reach some particular conclusions which differed from those of many doctors of his own time. Indeed, if we consider just three examples of medical issues referred to in this book (bloodletting, hygiene and cholera), we can see that he supported ideas which now are a normal part of orthodox medical understanding.

In the case of the first example, bloodletting, the authors note that Hahnemann

> … rarely bled his patients, even though his colleagues strongly advocated bloodletting. Moreover, he was an outspoken critic of those responsible for treating the Holy Roman Emperor Leopold of Austria, who was bled four times in the twenty-four hours immediately prior to his death in 1792. According to Hahnemann, Leopold's high fever and abdominal distension did not require such risky treatment. (p. 94)

Earlier Ernst and Singh commented on the attitude of those who used bloodletting, stating that

> … it would have ben easy for Rush to mistake the sedation caused by bloodletting for a genuine improvement, unaware that he was draining the life out of his patients (p. 25),

but they do not mention that Hahnemann himself provided precisely this analysis in the fifth edition of his *Organon of Medicine* (repeated in his final edition):

> the more frequently the patient was bled, the more leeches and cupping glasses sucked out the vital fluid (for the innocent irreplaceable blood was according to [Brouseau] responsible for almost all ailments). In the same proportion the patient lost strength to feel pain or to express his aggravated condition by violent complaint and gestures. The patient appears more quiet in proportion as he grows weaker, the bystanders rejoice in his apparent improvement, ready to return to the same measures on the renewal of his sufferings – be they spasms, suffocation, fears or pain, for they had so beautifully quieted him before and gave promise of further ease.[26]

Coincidentally, the fifth edition was published in 1833, a year when

> … many Eurpoean doctors continued to bleed their patients, so much so that France had to import 42 million leeches in 1833. But, as each decade passed, rationality began to take hold among doctors … (p. 23)

In Hahnemann's case rationality not only appears to have taken hold in 1792 (in respect of Leopold of Austria), but by 1833 he was describing bloodletting in terms familiar to modern medical writers.

Hahnemann on Hygiene

In the case of the second example, hygiene, the authors' statements about Florence Nightingale compare interestingly with those Hahnemann published in 1792 in *The Friend of Health*. Ernst and Singh state that

> … by November 1854 Florence was running the Scutari Hospital in Turkey, which was notorious for its filthy wards, dirty beds, blocked sewers and rotten food. It soon became clear to her that the main cause of death was not the wounds suffered by the soldiers, but rather the diseases that ran rife under such squalid conditions. (p. 25)

Sixty two years earlier, Hahnemann wrote in his chapter on 'Protection against infection in epidemic diseases':

> … it is quite another thing with the contagious exhalations from dangerous fevers and infectious diseases. … Most of them are not catching at the distance of a few paces in the open air, not even the plague of the East; but in close chambers these vapours exist in a concentrated form and then become injurious, dangerous, fatal, at a considerable distance from the patient.[27]

and again in his chapter on 'Things that spoil the air':

> … a person who from an idea of extreme convenience should, notwithstanding the vicinity of a water-closet, keep a night-chair in his sleeping apartment, should bear in mind that the disgusting exhalation from it spoils the air uncommonly, and renders the bed-chamber in which we pass a third of our life (if it be not very roomy) a very unwholesome place of abode.[28]

Similarly Ernst and Singh describe the comparison Nightingale made between field hospitals and Scutari:

> For example, to show that the filth at Scutari Hospital had been killing soldiers, she used her records to compare a group of soldiers treated at Scutari in the early unhygienic days with a control group of injured soldiers, who at the same time were being kept at their own army camp. If the camp-based control group fared better than the Scutari group, then this would indicate that the conditions that Nightingale encountered when she arrived at Scutari were indeed doing more harm than good. Sure enough, the camp-based soldiers had a mortality rate of 27 deaths per 1,000 compared with 427 per 1,000 at Scutari. This was only one set of statistics, but when put alongside other comparisons it helped Nightingale to win her argument about the importance of hygiene. (p. 28)

and Hahnemann, again in his chapter on 'Protection against infection in epidemic diseases', also noted such a difference in the risks of infection:

> The hospitals used by an army in a campaign, which are often established in churches, granaries, or airy sheds, are for that reason much less liable to propagate contagion, and also much more beneficial for the patients than the stationary hospitals, which are often built too close, low, and angular. In the latter, the nurses, physicians, and clergymen often run great risks.[29]

This concern for public health appears in several chapters of *The Friend of Health*, including advice on measures public officers could take to eradicate "a malignant fever"[30] and 'Suggestions for the prevention of epidemics in general, especially in towns'.[31] Such concerns are significant since the medical historian Simon Szreter identified

> … a primary role for those public health measures which combated the early nineteenth-century upsurge of diseases directly resulting from the defective and insanitary urban and domestic environments created in the course of industrialisation.[32]

In short, Hahnemann was advocating the type of measures which would later so significantly cut mortality rates during the period from 1850 to 1914.

Hahnemann on Cholera

The third example, cholera, is raised in this chapter when the authors state that

> During the 1849 epidemic the obstetrician Dr John Snow questioned the established theory that cholera was spread through the air by unknown poisonous vapours. He had been a pioneer of anaesthesia and had administered chloroform to Queen Victoria during the birth of Prince Leopold, so he knew exactly how gaseous poisons affected groups of people; if cholera was caused by a gas, then entire populations should be affected, but instead the disease seemed to be selective about its victims. (p. 112)

Yet already in 1831 Hahnemann (who was also a chemist) published the following comment on cholera:

> … On board ships – in those confined spaces, filled with mouldy watery vapours, the cholera-miasm finds a favourable element for its multiplication, and grows into an enormously increased brood of those excessively minute, invisible, living creatures, so inimical to human life, of which the contagious matter of the cholera most probably consists – on board these ships, I say, this concentrated aggravated miasm kills several of the crew; the others, however, being frequently exposed to the danger of the infection and thus gradually habituated to it, at length become fortified against it, and no longer liable to be infected.[33]

Clearly, more than sixty years before Koch identified the cholera bacterium, Hahnemann was basing his medical perspective on the concept of infectious micro-organisms.

In the light of the fact that homeopaths were among the first medical practitioners to take seriously the role of bacteria, it is obviously nonsense to claim, as Ernst and Singh do, that they

> … tend to reject many of the principles of conventional medicine, such as the role of bacteria as agents of disease. (p. 105)

But even if Ernst and Singh's claim were valid, it is wholly incompatible with the other allegation they quote, namely that

> 'Hahnemann and his writings are held in almost religious reverence by most homeopaths'. (p. 96)

This statement would lead one to expect a reverential acceptance by homeopaths of bacteria, not a rejection of the concept. The truth is that homeopaths from Hahnemann onwards have recognised the value of scientific knowledge and used it to question and develop their understanding of homeopathy. What Ernst and Singh need to explain is why they have failed to research their facts properly, and why they have accepted prejudicial comments uncritically.

These three examples show that Hahnemann, described by the authors as "a decent man, who combined intelligence with integrity" (p. 94), had a lot more in common with present medical

understanding than with the majority of doctors of his time. Further evidence of this will be pointed out later in addressing Ernst and Singh's examination of the medical system he developed.

The Development of Homeopathy

Hahnemann's biographers note that the first appearance of Hahnemann's comments on Cinchona appear in his translation of Dr William Cullen's *A Treatise on Materia Medica*, and indicate that he took the drug in order to test Cullen's hypothesis as to the reasons for its success with malaria,[34] but Ernst and Singh present the following (unreferenced) version of the origin of this therapy:

> [Hahnemann's] first step towards inventing homeopathy took place when he began experimenting on himself with the drug Cinchona, which is derived from the bark of a Peruvian tree. Cinchona contains quinine and was being used successfully in the treatment of malaria, but Hahnemann consumed it when he was healthy, perhaps in the hope that it might act as a general tonic for maintaining good health. To his surprise, however, his health began to deteriorate and he developed the sort of symptoms usually associated with malaria. In other words, here was a substance that was normally used for curing the fevers, shivering and sweating suffered by a malaria patient, which was now apparently generating the same symptoms in a healthy person. (p. 95)

One would expect a scientist to react to such an odd phenomenon by investigating it further in order to see if such a relationship existed for other successful treatments. According to Ernst and Singh Hahnemann did precisely this:

> He experimented with other treatments and obtained the same sort of results: substances used to treat particular symptoms in an unhealthy person seemed to generate those same symptoms when given to a healthy person. (p. 95)

At this point, one would expect a reasonable scientist to hypothesize that this relationship constituted a general principle, and according to Ernst and Singh this was precisely Hahnemann's next step (the quotation is unreferenced):

> … he proposed a universal principle, namely 'that which can produce a set of symptoms in a healthy individual, can treat a sick individual who is manifesting a similar set of symptoms.' (p. 95)

The inevitable action taken by a scientist after postulating a hypothesis is to test it, which in Hahnemann's case would mean researching the effects on healthy people of substances unknown as treatments, and by conducting experiments in order to see if these substances could have equal success on patients exhibiting similar symptoms. This is precisely what Hahnemann proceeded to do, but the authors acknowledge it only in part:

> Over the next few years, Hahnemann identified various homeopathic remedies by conducting experiments known as *provings*, from the German word *prüfen*, meaning to examine or test. (p. 96)

Instead something very interesting happens to Ernst and Singh's narrative of the development of homeopathy. Firstly, they have already claimed that the process outlined above is one of "inventing homeopathy" rather than of discovering its principles. Secondly, they now claim that Hahnemann arrived at this hypothesis "By reversing the logic …" (p. 95) of his observations, as if there were a distinction to be drawn between 'something being causative of symptoms in a healthy person which is curative of the same symptoms in a sick person' and 'something being curative of the symptoms in a sick person which is causative of same symptoms in a healthy person'. Thirdly, they stop discussing the theory of homeopathy and instead belittle it by comparing it to "the hair of the dog" (p. 96), by calling it "The gospel according to Hahnemann" (p. 97) and by wholly ignoring it when explaining the difference between homeopathy and herbalism (p. 97).

We have already pointed out, in our discussion of the Introduction, that the scientific method is based on the interaction of theory and experiment, and that the authors have chosen to define it without reference to theory. This refusal to accept the importance of theory now leads them to wholly ignore Hahnemann's experimental testing of his theory over a period of some forty years and that of other homeopaths during the following one hundred and eighty years. In doing so they fail to challenge the theory as a theory, but rather choose to challenge elements of homeopathic practice out of context. As a result the 'homeopathy' they are examining in this chapter bears no more resemblance to reality than a doodle. However, in order to understand the significance of the tests they refer to later, it is necessary to correct this deficiency and provide evidence of the theoretical principles which Ernst and Singh have left out. As far as possible we shall link this information to what they have included.

The Theory of Homeopathy

Hahnemann arrived at his hypothesis that 'like cures like' from observation, and he went on to provide extensive supporting evidence for his position: by gathering information from records of poisonings and comparing this information with what he gathered from provings;[35] by studying the work of Jenner and considering the relationship of inoculation to homeopathy;[36] by researching the way similar and dissimilar diseases interact in the same person;[37] and by researching the records of the effects of treatments (his own and others') on patients.[38] He even questioned his own approach

when he found that chronic conditions did not remain cured, leading to the first hypothesis of the origins and development of chronic diseases based on an analysis of evidence.[39]

Following on from this Constantine Hering, who became a homeopath after originally being commissioned to expose homeopathy as false, formulated a 'law of cure' based on the observations by homeopaths of both the progress of illness in individuals over time and the reaction to successful treatment:

> First, he observed that the human body seeks to externalize disease – to dislodge it from more serious internal levels to more superficial external levels. … Hering's second observation was that healing progresses from the top of the body to the bottom. … Hering's third observation was that healing procedes in the reverse order of the appearance of symptoms. Thus, the most recent symptoms will generally be the first to be healed. For this reason, in the process of cure a person may sometimes re-experience symptoms he or she previously suffered from (generally those symptoms that were suppressed or never really healed).[40]

His law was actually prefigured in Hahnemann's own observations of chronic diseases,[41] though Hahnemann never formulated it as such.

In addition to arguing his theory from observation and experiment, Hahnemann also argued it from principle. In section 3 of *The Organon of Medicine* he states that (his emphases)

> If the physician clearly perceives what is to be cured in diseases, that is to say, in every individual case of disease (*knowledge of disease, indication*), if he clearly perceives what is curative in medicines, that is to say, in each individual medicine (*knowledge of medicinal powers*), and if he knows how to adapt, according to clearly defined principles, what is curative in medicines to what he has discovered to be undoubtably morbid in the patient, so that recovery must ensue – to adapt it, as well in respect to the suitability of the medicine most appropriate according to its mode of action to the case before him (*choice of the remedy, the medicine indicated*), as also in respect to the exact mode of presentation and quantity of a required (proper *dose*), and the proper period for repeating the dose; – if , finally, he knows the obstacles to recovery in each case and is aware how to remove them, so that restoration may be permanent: *then he understands how to treat judiciously and rationally, and he is a true practitioner of the healing art*.[42]

In section 22 of *The Organon*, Hahnemann follows this up by stating that (his emphases)

> But as nothing is to be observed in diseases that must be removed in order to change them into health besides the totality of the signs and symptoms, and likewise medicines can show nothing curative besides their tendency to produce morbid symptoms in healthy persons and to remove them in diseased persons; it follows, on the one hand, that medicines only become remedies and capable of anihilating diseases, because the medicinal substance, by exciting certain effects and symptoms, that is to say, by producing a certain artificial morbid state, removes and abrogates the symptoms already present, to wit, the natural morbid state we wish to cure. On the other hand, it follows that, for the totality of the symptoms of the disease to be cured, that medicine must be sought which (according as experience shall prove whether the morbid symptoms are most readily, certainly, and permanently removed and changed into health by *similar* or *opposite* medicinal symptoms*) proved to have the greatest tendency to produce similar or opposite symptoms.

> * The other possible mode of employing medicines for diseases besides these two is the *allopathic method*, in which medicines are given, whose symptoms have no direct pathological relation to the morbid state, neither similar nor opposite, but heterogeneous to the symptoms of the disease. …

In other words, to practise medicine scientifically one needs to know as far as possible everything that is wrong with an individual patient, every aspect of health the individual medicines are capable of altering, and how to put these two sets of information together; and the only scientifically consistent curative relationship possible between a medicine and a particular case of illness must either be one in which the symptoms produced by the medicine are opposed to the symptoms of the illness, or be one in which the symptoms produced by the medicine are similar to the symptoms of the illness. This is the fundamental principle of *any* scientific approach to medicine, and it means that in order to disprove homeopathy Ernst and Singh only needed to *prove* that Hahnemann was wrong when he claimed that the correct relationship was one of similarity, and prove that the correct relationship was one of opposites. However, where Hahnemann produced the extensive evidence for his position noted above, Ernst and Singh merely produce the unsupported assertion that "After all, there is no logical reason why like should be guaranteed to cure like" (p. 105).

In fact, as many symptoms do not have opposites (a cough, for example, is either present or absent but has no opposite), it is impossible to base a medical system on treatment by use of opposites, so logically the only consistent relationship there can be between curative medicines and illnesses is that of similarity. Nor is this so strange a idea in the light of the modern concept of homeostasis, a fundamental aspect of living organisms known to Hahnemann.[43] Homeostasis is the continuous regulation of the organism's internal processes to keep them within a normal range, for example, changing the action of the heart in response to changes in bodily posture or activity. In the case of an excessive external stimulus the body will produce an opposing effect before returning to normal (such as when hands, chilled from playing in the snow, become hot during recovery). This means that it is entirely logical that any attempt to counteract symptoms will naturally lead to an opposing reaction, that is one increasing the symptoms (as is the case with the 'rebound effect'), whereas a stimulus similar to the symptoms will produce a reaction against the stimulus, and so necessarily against the symptoms too.

In short, whilst Hahnemann exhibits the whole scientific process of "paradigm articulation"[44] in his development of the principles of homeopathy, Ernst and Singh do not discuss any of this, but rather state that

> However, the sheer oddity of homeopathy's philosophy and practice does not necessarily mean that this approach to medicine should be rejected, because the critical test is not how bizarre it is, but whether or not it is effective. (p. 105)

The only oddity is that Ernst and Singh, "both trained scientists" (p. 3), consider a scientific process to be bizarre.

Scientific Standards

When it comes to Hahnemann's defence of scientific standards, the authors quote him (unreferenced) as insisting that

> 'He who does not walk exactly on the same line with me, who diverges, if it be but the breadth of a straw to the left or right, is a traitor and I will have nothing to do with him'. (p. 106)

At the same time, they themselves present three examples of such unjustified deviation from Hahnemann's principles, while claiming that

> … the process of finding the correct remedy can vary so much that it has led to the emergence of distinct schools of homeopathy. (p. 104)

Every one of these so-called "distinct schools" was opposed by Hahnemann himself either explicitly or implicitly, and they all represent a movement away from the scientific principles of homeopathy. Thus Ernst and Singh remark that (their emphasis)

> … *clinical homeopathy* simplifies matters by focussing only on the patient's main symptom and ignoring the more tangential aspects that would emerge during the usual homeopath's interview. (p. 104)

Hahnemann, on the other hand, constantly emphasized the need to address the totality of the symptoms (see above), and he also stated that (his emphases)

> … the *more striking, singular, uncommon and peculiar* (characteristic) signs and symptoms of the case of disease are chiefly and most solely to be kept in view; for it is *more particularly these that very similar ones in the list of symptoms of the selected medicine must correspond to*, in order to constitute it the most suitable one for effecting the cure.[45]

Again Ernst and Singh remark that (their emphasis)

> … *combination homeopathy* is interested only in the patient's main symptom, but it relies on mixtures of different remedies that all share the ability to treat this one outstanding symptom. (p. 104)

Hahnemann, on the other hand stated that only a single remedy should be used at one time because, among other reasons (his emphases),

> … even though the simple medicines were *thoroughly proved* with respect to their pure effects on the unimpaired healthy state of man, it is yet impossible to foresee *how* two and more medicinal substances might, when compounded, hinder and alter each other's action on the human body …[46]

Again Ernst and Singh remark that (their emphases)

> Another way to prescribe is according to the *doctrine of signatures*, which places less emphasis on the *Materia Medica* and instead looks for a clue, or signature, that indicates that a particular remedy is one which should be adopted. Therefore a walnut-based remedy would be appropriate for various mind-related disorders, such as stress, because the walnut resembles a brain. (p. 104)

Hahnemann, on the other hand, stated that (his emphases)

> I shall spare the ordinary medical school the humiliation of reminding it of the folly of those ancient physicians who, determining the medicinal powers of crude dugs from their *signature*, that is, from their colour and form, gave the testicle-shaped Orchis-root in order to restore manly vigour; the *phallus impudicus*, to strengthen weak erections; ascribed to the yellow turmeric the power of curing jaundice, and considered *hypericum perforatum*, whose yellow flowers on being crushed yield a red juice (*St. Johns's blood*) useful in haemorrhages and wounds, etc.[47]

Ernst and Singh have not only failed to treat the theory of homeopathy with "an unparallelled level of rigour, authority and independence" (p. 3), but have actually distorted it by including conflicting ideas and giving them undue weight, and then they have added insult to injury by holding Hahnemann up as dictatorial for opposing such distortions.

Potentisation

Another distortion in Ernst and Singh's approach to homeopathy is their treatment of potentisation. This process involves preparing medicinal substances by a combination of successive dilution in a mixture of water and alcohol and succussion – the shaking or banging of the container of diluted material. Hahnemann developed three scales of potentisation: the decimal (d), the centesimal (c) and the 50,000 (Q or LM) scale. The names are derived from the rate of dilution (1:10, 1:100, and 1:50,000 respectively), but each scale had different degrees of succussion too. Thus with the centesinmal scale "each succeeding phial is to be provided with one drop from the preceding phial (which has already been shaken twice) and is in its turn twice shaken",[48] whereas with the Q or LM scale the phial is "given 100 strong succussions [i.e.blows] against a hard but elastic body", "perhaps a leather bound book".[49] Hahnemann developed these different scales

because he was trying to minimize the negative effects of the medicines (or 'remedies') and produce a gentler action.

Just as Ernst and Singh fail to present the theory of homeopathy accurately and fully, so they fail to present potentisation accurately and fully. To begin with, they claim that

> Hahnemann went on to propose that he could improve the effect of his 'like cures like' remedies by diluting them. According to Hahnemann, and for reasons that continue to remain mysterious, diluting a remedy increased its power to cure, while reducing its potential to cause side-effects. (p. 95)

The authors do not produce any evidence of Hahnemann claiming that "diluting a remedy increased its power to cure", but, given that he was using as medicines substances known to cause harm, he necessarily would be interested, as are modern pharmacists, in discovering "doses so small that, without occasioning pain or weakening, they just suffice to remove the natural malady".[50] Ernst and Singh's version of events goes on to claim that (their emphases)

> … while carrying his remedies on board a horse-drawn carriage, Hahnemann made another breakthrough. He believed that the vigorous shaking of the vehicle had further increased the so-called *potency* of his homeopathic remedies, as a result of which he began to recommend that shaking, or *succussion*) should form part of the dilution process. The combination of dilution and shaking is known as *potentization*. (p. 96)

As regards 'potency', at least one biographer notes that "Hahnemann did not use this term until later editions of the *Organon*",[51] that is, not until after he had discovered the effects of succussion, so for the authors to claim that he applied the term to remedies prior to that time is inaccurate. This is, however, part of Ernst and Singh's continual confusion of the terms 'dilution' and 'potentisation' throughout this chapter (and indeed the rest of the book). It is the equivalent of confusing protons and atoms, the part and the whole, and indicates that these "trained scientists" (p. 3) have failed to understand the difference between the two processes. For example, they state that

> After the dilution, the mixture is vigorously shaken, which completes the potentization process. (p. 98)

Yet immediately afterwards they refer to (our emphasis) "Further dilution *and* potentization …" (p. 98).

Ernst and Singh's discussion of potentisation almost entirely revolves around the effects of dilution as if this were the only aspect of importance, yet Hahnemann himself drew the distinction between dilution and potentisation repeatedly, as in (his emphasis)

> We hear daily how homoeopathic medicinal potencies are called *mere dilutions*, when they are the very opposite, *i.e.* a true opening up of the natural substances bringing to light and revealing the hidden specific medicinal powers contained within and brought forth by rubbing and shaking. …
>
> Simple dilution, for instance, the solution of a grain of salt will become water, the grain of salt will disappear in the dilution with much water and will never develop into medicinal salt which by means of our well-prepared dynamization [an alternative term for potentisation], is raised to most marvellous power.[52]

So for Ernst and Singh to claim that "… Hahnemann believed that weaker solutions led to stronger remedies" (p. 98), is to misrepresent his views entirely.

The 'Active Ingredient'

The basis of Ernst and Singh's position is seen in two remarks. The first is that

> Herbal medicine, by contrast, follows the more commonsense rule that more concentrated doses lead to stronger remedies. (p. 98)

The second is that

> … herbal remedies will always have at least a small amount of active ingredient, whereas homeopathic remedies usually contain no active ingredient whatsoever. (p. 99)

In other words they are using the term "active ingredient" to mean a chemical. As has just been pointed out, Hahnemann, as a chemist, was explicit about there being a difference between chemical action and that of potentised remedies, and he remarked that (his emphasis)

> The homoeopathic system of medicine develops for its special use, to a hitherto unheard-of degree, the inner medicinal powers of the crude substances by means of a process peculiar to it and which has hitherto never been tried, whereby only they all become immeasurably and penetratingly efficacious and remedial, *even those that in the crude state give no evidence of the slightest medicinal power on the human body*.[53]

He added in a footnote that

> Long before this discovery of mine, experience had taught several changes which could be brought about in different natural substances by means of friction, for instance, warmth, heat, fire, development of odour in odourless bodies, magnetization of steel, and so forth.[54]

It has long been known that banging iron can cause reordering of the atoms to produce a magnet, and we now know that water molecules have polarity and form structures,[55] so it is entirely legitimate to maintain that the concept of an "active ingredient" needs to be extended out of the field of chemistry and into the field of physics.

However, after discussing *dilution* for nearly two pages, Ernst and Singh dismiss the relevance of physics to *potentisation* by reference to a small number of experiments. There is no discussion of magnetism, electrical charges, molecular structures, epitaxy, transmission of information, amplification or signal-to-noise ratios. They do not present the reader with evidence of any known law of physics being contradicted by homeopathy, despite the fact that few people are familiar with the esoteric nature of modern physics. Instead Ernst and Singh state that

> Homeopaths would argue that the remedy has some memory of the original ingredient, which somehow influences the body, but this makes no scientific sense. (p.100)

They do not offer a shred of evidence for their claim that "this makes no scientific sense". In fact the scientific means to investigate this aspect of remedies are still being developed, which would account for Ernst and Singh's remark that

> Unfortunately, a review of these studies published in the *Journal of Alternative and Complementary Medicine* in 2003 showed that these experiments were generally of poor quality and prone to errors. (p. 126)

Nonetheless research is being undertaken,[56] especially in the field of materials science.[57]

Jacques Benveniste

One piece of research which showed that potentised remedies have an effect on cellular material is discussed more fully by Ernst and Singh. This was performed by a French team under Jacques Benveniste (who coined the term "the memory of water"), and was published in *Nature* in 1988. Publication was followed by an investigation by "a team of experts" (p. 120) including the editor of *Nature* (John Maddox), a chemist (Walter Stewart), and a magician (James Randi). Ernst and Singh state that at the end of this investigation

> The results showed no evidence to support homeopathy, and instead they were in line with conventional scientific thinking and all the known laws of physics, chemistry and biology. (p. 122)

Even moderately close attention to their own report of this investigation, however, shows how compromised it was, and, therefore, how unreliable a basis it is for reaching such a conclusion.

To begin with, Ernst and Singh state that

> The experimental protocol seemed to be in order, but the claims in the paper were so extraordinary that Maddox [the editor of *Nature*] took the step of adding a disclaimer alongside the published paper. (p. 120)

They also state that

> The prize fund had steadily increased to $1 million by 1988, so if the team endorsed Benveniste's result then it would lead to Randi writing out a very large cheque to the Frenchman. (p. 120)

Finally they state that

> Maddox, Randi and Stewart went into a separate room, blanked out the windows with newspapers, removed the labels from the test tubes and replaced them with secret codes that they would later use to identify which samples had been treated with homeopathic solution and which had been treated with water. (p. 121)

The result of these individuals taking a role in the experiment which allowed them the crucial ability to affect the outcome was that (our emphasis)

> *This* time the results showed that the basophils in the homeopathically treated samples had not reacted differently from the control basophils treated with plain water. The experiment had failed to demonstrate the sort of effects that Benveniste had been finding for the last two years. (p. 122)

For Ernst and Singh to highlight suggestions that Benveniste and his team were incompetent, deluded and affected by vested interests in the context of such a sequence of events shows only how ready they are to attack one side of the case while accepting the other uncritically. This investigation of Benveniste's work by *Nature* and the subsequent attitude to his research have been analysed by Michel Schiff,[58] as an example of censorship in science, and his version of events makes an interesting contrast to that of Ernst and Singh. For example, they remark that

> In fact, within a year of Benveniste's original 1988 paper, *Nature* had published three papers by scientists who failed to reproduce the supposed effect of ultra-dilute solutions. (p. 125)

Schiff notes that

> Within weeks of the publication of experiments about high dilution effects on the staining of human basophils, *Nature* published four reports claiming that these effects could not be duplicated. As we shall see, the authors of these reports operated in such a way that their *a priori* chance of success was minimal. In each case they acted as if they desperately wanted to fail in their attempt to reproduce the results reported by Benveniste and his colleagues.[59]

Ernst and Singh also choose to support their position by referring to the failure of a U.S. Defense agency to replicate the results of a completely different experiment; the failure of the

BBC's *Horizon* programme to win James Randi's $1 million; and a study from 1999, in which (our emphasis)

> … Dr Andrew Vickers looked at 120 research papers related to Benveniste's work and *other* types of basic research into the actions of homeopathic remedies. (p. 125)

Despite providing no information about how many of these papers actually related to Benveniste's work, they comment that

> Vickers was struck by the failure of independent scientists to replicate any homeopathic effect: 'In the few instances where a research team has set out to replicate the work of another, either the results were negative or the methodology was questionable.' Independent replication is a vital part of how science progresses. One single set of experiments can be wrong for a range of reasons, such as lack of rigour, fraud or just bad luck, so independent replication is a way of checking (and re-checking) that the original discovery is genuine. Benveniste's research had failed this test. (p. 125)

In fact Benveniste's work was successfully replicated in 2004 by a multi-centre study, including four research centres in Europe, which confirmed the effect of potentised histamine inhibiting human basophil degranulation,[60] thus meeting the criteria for validating Benveniste's work.

The Placebo Effect

By ignoring the theory of homeopathy as well as both the circumstantial and actual evidence of the effects of potentised remedies, Ernst and Singh feel able to claim that

> From a scientific perspective, it is impossible to explain how a remedy that is devoid of any active ingredient can have any conceivable effect on any medical condition, apart from the obvious placebo effect. (p. 100)

Even so, this is demonstrably inadequate:

1. They have already pointed out that there is no scientific explanation for the placebo effect (p. 62), so it is extraordinary to offer it as a scientific explanation of homeopathy.
2. Hahnemann developed three scales of potentisation (x, c and Q or LM) in an attempt to eliminate the *negative* effects of homeopathic remedies. These negative effects cannot be explained by the placebo effect, even if the placebo effect were scientifically defined and, therefore, scientifically acceptable.
3. While the action of centessimal (c) potencies resembles that of decimal (x) potencies, it differs so markedly from the action of Q or LM potencies that the latter need to be

administered in a wholly different manner. Again this is inexplicable by reference to the placebo effect.

4. James Tyler Kent, a homeopath of the late nineteenth century, identified no less that twelve distinct reactions to a prescription of a remedy potentised on the centessimal scale, and detailed the significance of these as regards the pathology and prognosis of the patient's case.[61] The placebo effect, which Ernst and Singh point out is dependent on a patient's expectations, cannot possibly be used to explain such a sophisticated range of reactions.
5. The placebo effect is irrelevant to provings, where a potentised remedy is given to healthy people and the symptoms it produces are recorded. In this case there are no expectations or knowledge of what the effects will be, either on the part those taking the remedy to be proved, or on the part of those supervising the proving.

Provings

Ernst and Singh note that provings

> … involve giving daily doses of a homeopathic remedy to several healthy people and then asking them to keep a detailed diary of any symptoms that might emerge over the course of a few weeks. (p. 96)

This is not actually true, because on the one hand the remedy is given *only* until symptoms start to appear, and on the other hand the prover will be supervised and asked questions about the symptoms. Indeed the conduct of a proving involves close attention to detail both as regards the information gathered and the protocol of the trial.[62]

For example, Ernst and Singh remark that

> Because poorer-quality trials are more likely to produce misleading results, researchers have developed techniques for assessing quality and weeding out those trials that should not be taken seriously. For example, the Oxford quality scoring system, developed in 1996 by Alexandro Jadad and his colleagues at Oxford University can be used to give a trial a score between 0 points (very poor) and 5 points (rigorous). … the trial can score a point if the paper describes how the researchers dealt with data from patients who dropped out from the trial. If the researchers have thought about this in detail and bothered to describe it in their research paper, then it is a good indication of their overall level of rigour. (p. 134)

Just such a rigorous approach was used by Hahnemann in his tests some two hundred years ago:

> As regards my own experiments and those of my disciples every possible care was taken to ensure their purity, in order that the true powers of each medicinal subsance might be clearly expressed in the

> observed effects. They were performed on persons as healthy as possible, and under regulated external conditions as nearly as possible alike.
>
> But if during the experiment some extaordinary circumstance from without happened which might even be supposed to be capable of altering the result – for example, a shock, vexation, a fright, an external injury of considerable severity, dissipation or over-indulgence in something or other. Or any other circumstances of importance – from that time no symptom that occurred in the experiment was registered; they were all rejected, so that the observation should contain nought that had a suspicion of impurity about it.
>
> If some little circumstance happened during the experiment, which could hardly be expected to interfere with the effects of the medicinal action, the symptoms subsequently noticed were inclosed within brackets as not certainly pure.[63]

Hahnemann also explained why it is necessary to use healthy people in order to discover the effects of substances on the human body, stating that (his emphasis)

> If, in order to ascertain this, medicines be given to *sick* persons only, even though they be administered singly and alone, then little or nothing precise is seen of their true effects, as those peculiar alterations of the health to be expected from the medicine are mixed up with the symptoms of the disease and can seldom be distinctly observed.[64]

The significance of this is connected also with the issue of people's individuality of response to the same substance.

Individuality

With reference to orthodox medicine, Ernst and Singh have repeatedly noted the crucial importance of the individuality of patient responses to medicines (Chapter 2). It was not until the advent of the wide-spread use of RCTs to test drugs in the middle of the twentieth century that this could really be demonstrated as a fact. However, over one hundred years earlier Hahnemann had observed this fact, noting that

> Some symptoms are produced by the medicines more frequently - that is to say, in many individuals, others more rarely or in few persons, some only in very few healthy bodies.[65]

As a result it is advantageous to prove a remedy by using a wide cross-section of people, whereas RCTs are constantly trying to balance the contradictory needs for homogeneity (the similarity of the cases used in the test) and generalisability (the relevance of those cases used in the test to the diversity of the population as a whole). In other words, homeopathy does not need to cancel out the individuality of response but instead uses it to develop the fullest possible picture of how a substance acts on the human organism.

In the same way Hahnemann's approach to recording the details of the patient's case involved detailing that person's individual peculiarities,[66] and then finding not "… the remedy that offers a perfect match with the patient's symptoms" (p. 101), but the remedy with a matching *individuality*. A perfect match is impossible as all remedies are likely to have symptoms which cannot coexist in one person at one time. For example, alcohol can produce in the same person happiness and melancholy, increased liveliness and unconsciousness, but at different points of inebriation. In the same way, a remedy may have several symptoms relating to a part of the body which cannot occur simultaneously.

In the example presented by Ernst and Singh, they have simply copied out (inaccurately) two summaries of symptoms (p. 101) from Boericke's *Pocket Manual of Homoeopathic Materia Medica and Repertory*, without taking into account the fact that the different types of stool cannot occur at the same time, and turning the three symptoms relating to the face (plus a variant) into a single symptom. The correct versions are given below, with the individual symptoms correctly separated by full stops:

> **Face**.–Distortion of mouth. Trembling of jaw. *Facial paralysis;* more left side.
>
> **Stool**.–Bloody, black, and offensive. Gelatinous, yellowish green; semi-fluid, with urinary suppression.[67]

Ernst and Singh claim that "Cadmium sulphate is the perfect remedy" (p. 101) if a person exhibits all these symptoms, but it is impossible for anyone to do this. On the other hand, were a patient to have the combination of gelatinous, yellowish green stools with either trembling of the jaw or left-sided facial paralysis, these symptoms would indicate cadmium sulphate alone.[68] Such stools are more characteristic of the remedy and more peculiar generally (appearing in only 8 remedies) as they indicate a more complex digestive disturbance. The symptom of black and bloody stools (indicating internal bleeding in the digestive tract and rectum) is much more common, appearing in over a hundred remedies, seventy-two of which include the offensive odour. Even when combined with the individual facial symptoms between four and sixteen remedies are indicated, and this is reduced to two remedies only if all the facial symptoms are combined.

In short, homeopaths do not treat symptoms in the abstract way presented by Ernst and Singh. Matching the individuality of the remedy to that of the patient requires an awareness of the pattern of symptomatology produced by the remedy; the significance of symptoms in relation to Hering's 'law of cure'; their peculiarity in the context of anatomy, physiology and pathology; their

relationship to the patient's medical history; and the extent to which the patient is affected by the symptoms. Ernst and Singh have simply demonstrated their own lack of understanding of the process of finding a homeopathic remedy for a case, and when this degree of ignorance is applied to testing homeopathy or to judging such tests, it is unsurprising that the conclusions are deeply flawed.

Epidemics

Whilst Ernst and Singh note that homeopaths "often point to the successes they had achieved in dealing with major epidemics" (p. 107), these successes are treated in a puzzling way. For example, the authors state that

> This success was repeated during a cholera epidemic in London in 1854, when patients at the London Homoeopathic Hospital had a survival rate of 84 per cent, compared to just 47 per cent for patients receiving more conventional treatment at the nearby Middlesex Hospital. (p. 107)

Converting these figures to mortality rates we get 16% of patients dying at the homeopathic hospital and 53% dying at the conventional hospital. Ernst and Singh go on to argue that this difference could be attributed to "three major reasons" other than that homeopathy was effective.

> Taking the last reason first, the authors maintain that
>
> Third, perhaps the higher survival rate at the London Homoeopathic Hospital was not indicative of the success of homeopathy, but rather of the failure of conventional medicine. Indeed, medical historians suspect that patients who received no medical care would probably have fared better than those who received the conventional medications given at the time. (p. 108)

This may be true, but the authors offer us a direct comparison with another example of hospital mortality from cholera (and malaria) from the very next year. Nightingale's statistics showed that (our emphasis)

> … the death rate for *all admitted soldiers* was 43 per cent, but after her reforms it fell to just 2 per cent … (p.27)

Compared with this a mortality rate of 53% of those *who have already contracted malaria* prior to admission is not at all unexpected, and modern medical texts confirm this:

> Untreated cholera frequently results in high (50-60%) mortality rates.[69]
>
> More than 50 percent of untreated people with severe cholera die.[70]

In other words Ernst and Singh's conjectured reason is unsupported by the evidence.

Looking now at the first reason offered by Ernst and Singh, they claim that

> It could be, for instance, that the patients who attended the London Homoeopathic Hospital were wealthier, which would mean that they were in a better state of health before catching cholera and were better fed and cared for after leaving hospital – all of this, rather than the homeopathic treatment itself, might account for the higher success rate. (p. 107)

This ignores the fact that death from cholera is rapid and results from extreme dehydration, so anyone who left hospital alive would have survived, so subsequent care is not really relevant. In fact, in orthodox medical terms the only factor stated to have a significant impact on survival is rehydration:

> Rapid replacement of lost body fluids, salts, and minerals is central to treatment.[71]

Even if we assume that all the patients at the homeopathic hospital were significantly wealthier than all the patients at the Middlesex hospital, the authors would still be claiming that being wealthy can reduce the mortality rate among those who have *contracted* cholera by nearly 70%. It is true that poverty can increase the mortality rate, but there is no evidence that wealth can produce such a dramatic reduction in the rate. Furthermore, at that time the wealthy were generally more likely to have their health undermined by medical treatment because they could afford more treatment. This point is made by Ernst and Singh themselves when they state that

> The richest patients were the most heroic of all, because they endured the most severe treatments. (p. 108)

Is also made by Hahnemann in 1835, who refers to patients

> … too much run down and spoiled by allopathic [orthodox] treatment, as was unfortunately too often the case where the patient had any money to spend.[72]

In other words, it is arguable that an increase in the number of wealthy patients at the Middlesex Hospital would have been likely to entail an increase in the mortality rate rather than a reduction, and so increased the disparity between the hospitals.

The last reason offered is that

> The two hospitals may have differed in other important ways. For instance, the London Homoeopathic Hospital might have had a higher standard of hygiene than the Middlesex Hospital, which could easily

> explain its superior survival rate. After all, we are dealing with an infectious disease, so clean wards, uncontaminated food and safe water were of the utmost importance. (p. 108)

Again the assumption is that the chance of survival from this rapidly fatal illness could be improved by circumstances largely unable to influence its progress. Death without treatment occurs at the level recorded at the Middlesex Hospital, and the absence of clean wards, uncontaminated food and safe water might increase this mortality rate, but there is no evidence that their presence can reduce it. In short this reason, like all the others has no evidence to support it.

More importantly, this epidemic was only *one* of many for which figures were published, enabling comparison between homeopathic and orthodox treatment.[73] These show that the figures in Ernst and Singh's example are consistent with the others in showing significantly better survival rates under homeopathic treatment. Particularly remarkable examples include Austria in 1831, where

> Statistics show that those with cholera who tried homeoapthy had a mortality rate between 2.4 and 2.1 percent, whereas over 50 percent of those with cholera under conventional medical care died.[74]

Similarly,

> In 1849, the homeopaths of Cincinnati claimed that in over a thousand cases of cholera only 3 percent of the patients died. To substantiate their results, they even printed in a newspaper the names and addresses of patients who died or survived.[75]

In short Ernst and Singh have not given readers a true picture of the evidence relating to the success of homeopathic treatment during epidemics, and neither have they discussed the evidence seriously. Instead they have offered merely a collection of unsupported conjectures.

The Trials of Homeopathy

Finally we come to the issue of clinical trials to test the effectiveness of homeopathy. In order to understand the practical and theoretical context in which these need to take place, we have had to explain homeopathy rather more fully than Ernst and Singh have, despite their claims to "an unparalleled level of rigour, authority and independence" (p. 3). Where Ernst and Singh have left out information crucial to understanding factors which trials need to take into account, we have tried to provide this information, so that it is possible to assess more accurately the quality of what they propose as a satisfactory trial:

> A homeopathic trial would require the random assignment of patients into two groups, namely a group treated homeopathically and a placebo control group. The patients would not be told to which group they had been assigned. Both groups would receive an empathic encounter with a homeopath, who would also be blinded, inasmuch as he or she would not know which patients belonged to which group. Researchers would then create two batches of pills that were identical, except that one batch would have been treated with a drop of homeopathic solution and the other would remain plain. The treatment group would receive the homeopathic pill and the control group would receive the palin pill. Patients in both groups should experience some improvement, simply due to the placebo effect. The critical question is this: does the treatment group on average show significant improvements over and above the control group? If the answer is 'yes', then this would clearly indicate that homeopathy is genuinely effective. If, however, the answer is 'no' and each group shows a similar response, then homeopathy would be exposed as having nothing more than placebo effect. (p. 130)

In fact, the *only* difference between this trial format and a trial to test an orthodox medical drug is that the latter is recognised as only testing the drug, whereas this claims to be able to test a whole therapy.

There are substantial issues which Ernst and Singh's proposed trial format fails to take into account, and we have listed some of these (with explanations) below:

1. **A definition of what is being treated, or as Ernst and Singh put it, "which disease are we applying it to?" (p. 3).**
   Homeopathy defines what is being treated as a composite of everything which the person themselves, those near to them and the homeopath identify as being wrong. On the other hand orthodox medicine has a definition which Ernst and Singh have not yet supplied, but which they imply is the generalised categories of diseases, syndromes and conditions. Failure to use the homeopathic definition would mean that the trial would be based on inappropriate criteria, and successes in the verum arm of the trial (i.e. in the group that received the homeopathic medicine) would only occur in those cases where the two definitions effectively overlapped.

2. **Criteria defining the relationship of the symptoms being treated to the medical history.**
   Homeopathy treats symptoms not in themselves or as a state but as part of a process, recognised formally in Hering's 'law of cure'. As a result the sequence of recovery depends on the medical history, not on the practitioner's intent. A trial which demanded a different sequence of recovery in some cases would be inappropriate, and would necessarily lead to failures in the verum arm of the trial.

3. **Criteria defining the patients to be selected for the trial.**
   The likelihood of successful treatment increases with the individuality of the patient's symptoms, but this individuality can be reduced as a result of factors, such as the use of some prescription and non-prescription drugs or serious pathology. For example, corticosteroids suppress the immune response, addictions produce symptoms of toxicity, or a large tumour will produce symptoms of mechanical damage. Not only does this make the appropriate remedy harder to determine, but it indicates that the patient's ability to recover is compromised. Failure to take these factors into account will distort the level of success of the verum arm of the trial.

4. **A definition of what is meant by "significant improvement", that is, "what is meant by effective?" (p. 3).**
   Ernst and Singh have not yet supplied a definition of effectiveness, and so the term "significant improvement" is ambiguous, as it may refer to a predetermined set of symptoms or to a general improvement in well-being. Homeopathy defines improvement in relation to Hering's 'law of cure', and a significant improvement as part of the process of recovery may involve a change in symptoms, rather than a simple reduction in symptoms. Failure to use the homeopathic definition of improvement would mean that the trial would be using an inappropriate measure, leading to a distortion of the results of the verum arm of the trial.

5. **An explanation of why "significant improvement" is to be adopted as an outcome as opposed to assessing effectiveness on an ongoing basis.**
   Since the effects of remedies may vary, and since the assessment of the effects of remedies is a crucial part of the process of treatment, an explanation is required as to why this form of outcome has been selected. Since the trial is supposed to be one of the therapy and not of a specific medicine, the success of the therapeutic process should be being measured rather than some arbitrary aspect of improvement. For example, some reactions to remedies are 'negative', but are still indicative of the activity of homeopathic treatment, such as the homeopathic aggravation. An inappropriate measure of outcome will distort the results of the verum arm of the trial.

6. **An explanation of what time-scale is to be used in the trial when measuring "significant improvement".**
   The rate of any improvement in a particular case will vary according to the medical history of the patient and other factors, such as stress and current or previous use of some prescription or non-prescription drugs (including the oral contraceptive pill). Using an inappropriate time-scale will affect the results of the verum arm of the trial.

7. **An explanation of how researchers would manage issues which could affect the course of the trial.**
   Concomittent drug therapy, emotional shocks (both positive and negative), accidents and other factors could affect the course of treatment by delaying or altering the sequence of recovery. Failure to take these into account could affect the results of the verum arm of the trial.

8. **An explanation of how the trial would manage the use of a range of remedies or potencies in some cases.**
   For homeopathic treatment to be successful more than one remedy and potency may be required over the period of the trial. Restriction of a trial to the use of a single remedy and potency would distort the results of the verum arm of the trial.

9. **Criteria for determining a consistent standard of practice among the homeopaths taking part in the trial.**
   Homeopathy involves a continuous process of decision-making about the remedy to be prescribed, so a trial would need to establish criteria for a minimum level of competence. Failure to take practitioner competence into account could lead to un-recognised distortions in the results of the verum arm of the trial.

10. **An explanation of how the researchers would manage the "empathic encounter with a homeopath" in the context of a placebo controlled trial.**
    A failure to see any response to the prescription affects the conduct of treatment. In particular the remedy or potency will be changed by the practitioner in order to correct what they will perceive as a mistake in the remedy selection. In the proposed trial context repeated failure would lead a practitioner in either arm of the trial to assume that they are part of the placebo arm. Where the practitioner is in fact in the verum arm, they will be

acting on a mistaken assumption and failing to treat the information of non-reaction as important data. Failure to manage this issue appropriately would affect the conduct of the placebo arm of the trial, and could affect the results of the verum arm.

11. An explanation of how the researchers would manage the possibility of drop-out among patients in the placebo arm of the trial.

In some cases treatment may require a time-scale incompatible with maintaining a placebo arm of the trial, a problem which inhibits any long-term trial. As a result a trial would either have to include only cases manageable within a short time-scale, in which case researchers would need to establish criteria for identifying such cases, or there would need to be criteria for handling possible high levels of drop-outs in the placebo arm so as not to invalidate the trial.

It is clear from the above points that there are numerous potential errors which can compromise the results in the verum arm of a trial of homeopathy. Where these errors alter the intervention being tested, 'true' interventions will be mixed with 'false' interventions, leading to false results. Where these errors involve the inappropriate measurement of results, true results will be mixed with false results. In either case results in the verum arm will be compromised by false results, and the greater the number of errors the less true the results will be as a whole. Paolo Bellavite and Andrea Signorini, in *The Emerging Science of Homeopathy*, provide an example of how serious this problem can be by referring to a trial of Arnica montana in 1978, when "an analysis of the results in a highly critical and objective review of homeopathic research" showed that "only one patient presented typical Arnica symptoms and was included in the placebo group!"[76] That Ernst and Singh are aware of some of these issues is shown when they note (in relation to the meta-analysis by Shang et al.) that

> Indeed, most trials have not been individualized, but there have been trials in which patients were given detailed consultations and either individualized homeopathic prescriptions or placebo. (p. 138)

What is unforgivable in two "trained scientists" (p. 3) is they have not considered the implications of these issues in respect of the validity of trials. They do not even refer to any of them when they present two trials to illustrate that individualisation does not guarantee the success of homeopathy.

In the first case, a trial of "ninety-eight patients with chronic headaches over the course of twelve weeks" (p. 138), there is no information about: why there was an expectation of success in

twelve weeks; what constituted a chronic headache; how long the headaches had been experienced prior to the trial; what relationship the headaches had to the patients' medical histories as a whole; what measure of success was being used; and so on. In the second case, a trial of "ninety-six children with asthma [which] looked at their progress after twelve months of receiving individualized homeopathy or a placebo as an adjunct to their conventional treatment" (p. 138), there is no information about: how long the children had had asthma; what its relationship was to the patients' medical histories; what conventional treatments were being prescribed; what were the possible effects of interaction between the conventional and homeopathic treatment; what measure of success was being used; and so on. This trial did, apparently, take into account the need for "experienced homeopathic practitioners" (p. 139).

In short, Ernst and Singh admit the inadequacy of most trials and fail completely to demonstrate the adequacy of their examples of good trials. As such they confirm the lack of rigour in their approach to tests of homeopathic treatment which their proposed trial format indicated. In this context they render meaningless their claim that

> … hundreds of trials have failed to deliver significant or convincing evidence to support the use of homeopathy for the treatment of any particular ailment. (p. 139)

As far as the evidence they have presented goes, these are merely hundreds of inadequately designed trials with seriously questionable validity. The fact that Ernst and Singh also stoop to using the comments of a Nazi researcher about a trial for which there is not the slightest documentation (pp. 114-116) only goes to show the poverty of both their case and their critical faculties.

Bristol Homeopathic Hospital

A study castigated by Ernst and Singh is that

> … by the Bristol Homoeopathic Hospital in 2005. The hospital tracked 6,500 patients during a six year study and observed that 70 per cent of those suffering with chronic diseases reported positive health changes after homeopathic treatment. (p. 140)

Their criticisms of it are, to say the least, bizarre. To begin with they maintain that

> As far as the public was concerned, this appeared to be an extraordinarily positive result. (p. 140)

In other words they are noting that the general perception about chronic diseases is that 70% of people do not get better without medical intervention, while implying that health professionals would consider it perfectly normal for this to happen. This is a point they reinforce by quoting a "science writer Timandra Harkness" (unreferenced) who stated that

> 'It's as if you had a theory that feeding children nothing but cheese made them grow taller, so you fed all your children cheese, measured them after a year and said *There – all of them have grown taller – proof that cheese works!*' (p. 140)

This begs the question that if a 70% rate of positive health changes in chronic diseases without medical intervention is normal, then why does the NHS treat so many people for chronic diseases?

Ernst and Singh's answer to this is that

> The 70 per cent improvement rate could have been due to any number of factors, including natural healing processes, or patients being reluctant to disappoint whoever was interviewing them, or the placebo effect, or any other treatments that these patients may have been using. (p. 140)

If we consider these factors carefully, it becomes obvious that they are as absurd as the ones presented to explain the success of homeopathy in treating cholera. As in that case, it is useful to ignore the authors' sequence and to start with the final reason, "other treatments". Within the NHS referral to a homeopathic hospital is not a common occurrence, indeed there is even an acronym for the circumstances in which referral is often made: TEETH (tried everything else, try homeopathy). Put simply, patients are most likely to have been referred to the Bristol Homeopathic Hospital because other treatments have failed, not because those treatments were proving successful. This reason for improvement is, therefore, either marginal at most or an indication that homeopathic treatment had improved the patients' response to other treatments, which would actually indicate the effectiveness of homeopathy.

The placebo effect must also be a marginal factor in generating this rate of improvement despite the fact that Ernst and Singh show that they consider this to be almost entirely responsible:

> We suggest that you ignore the occasional media hype and instead rely on our conclusion, because it is based on examining all the reliable evidence – and the evidence suggests that homeopathy acts as nothing more than placebo. (p. 140)

As the authors themselves point out later in this book, the placebo effect would have applied to orthodox treatments at least as powerfully as to homeopathy:

> Whenever a doctor prescribes a proven treatment, then the patient hopefully experiences a biochemical and physiological benefit. However, it is important to remember that the impact of a proven treatment is always enhanced by the placebo effect. Not only will the treatment deliver a standard benefit, but it should also deliver an added benefit because the patient has an expectation that the treatment will be effective. In other words, patients receivng proven treatments already receive the placebo effect as a free bonus, so why on Earth would a patient take a placebo on its own which delivers only a placebo effect? (p. 248)

In other words, those patients most susceptible to the placebo effect would not have been referred to the homeopathic hospital. So if the placebo effect were a major factor, such a success rate among those not readily susceptible to the placebo effect would indicate an urgent need to investigate the effectiveness of homeopathy in generating such success other than by the means it claims. Furthermore, if homeopathic remedies are merely placebo, and it was something else in the therapeutic process that caused the benefit, then every RCT of homeopathy would have shown this high rate of improvement in both arms of the trial.

Of the other two factors, "natural healing processes" – in the sense of 'unassisted healing processes' – could also only be marginal, otherwise there would be no need to treat chronic diseases, let alone refer then to an unorthodox therapy. Finally, therefore, the only explanation for the majority of this improvement in health is "patients being reluctant to disappoint whoever was interviewing them", not just one or two patients but around 4,500 out of the 6,500. In practice this means that when Ernst and Singh state that "the Bristol study was largely meaningless" (p. 140), they are really saying that 4,500 people out of 6,500 lied.

Meta-analyses

Little of the evidence offered by Ernst and Singh in this chapter is actually primary material from trials, since they have chosen to support their argument principally by reference to meta-analyses, and they illustrate the method used by two examples. In the first example, a hypothetical test of astrology, they

> Imagine that a series of five experiments is conducted around the world by rival research groups. In each case, the same astrologer is simply asked to identify correctly a person's star sign based on a five-minute converstaion. The experiments range in size from 20 to 290 participants, but the protocol is the same in each case. (p. 131)

By adding up the total number of participants and the total number of successful identifications, an average result can be obtained, evening out variations due to trial size. In the second example, an actual analysis of drug trials, researchers wanted to test

> … if corticosteroid medication could help reduce respiratory problems in premature babies. They designed a trial which involved giving the treatment to pregnant women likely to have premature babies. (p. 132)

The problem was that

> Ideally, the researchers would have conducted one trial in a single hospital with a large number of cases, but it was only possible to identify a few suitable cases each year per hospital, so it would have taken several years to accumulate sufficient data in this manner. Instead, the researchers conducted several trials across several hospitals. (p. 132)

Again, by pooling the results a more accurate general result could be determined than could be obtained from any individual trial.

Ernst and Singh acknowledge that, as with their hypothetical study,

> The meta-analysis in the premature baby study was fairly straightforward, because the individual trials were similar to each other and so they could be merged easily. (p. 133)

However, in the case of Klaus Linde's meta-analysis of homeopathic trials in 1997, they state that

> … the meta-analysis was particularly problematic. In order to draw a conclusion about the efficacy of homeopathy, Linde was attempting to include homeopathy trials investigating a huge variety of remedies, across a range of potencies, being used to treat a wide range of conditions, such as asthma and minor burns. (p.133)

According to Ernst and Singh, in setting "certain basic conditions" (p. 133) for sorting out which trials to include and which to exclude, Linde and his colleagues took randomisation and the use of a placebo control as the primary factors, *not* the rigour with which they met criteria appropriate to testing homeopathy. Furthermore, according to Ernst and Singh the same approach governed criticisms of the meta-analysis. Firstly,

> Not surprisingly, Linde's conclusion was questioned by opponents of homeopathy. Critics argued that his meta-analysis had been too lax, inasmuch as it had included too many trials of relatively poor quality, and they feared that these might have biased the overall conclusion in favour of homeopathy. (p. 134)

Secondly,

> Critics pointed out that sixty-eight out of the eighty-nine trials in Linde's meta-analysis scored only 3 or less on the Oxford scale, which meant that three-quarters of the trials were substandard. (p. 134).

The Oxford (or Jadad) scale assesses three criteria relevant to an orthodox drug trial: randomisation, blinding, and handling of "data from patients who dropped out from the trial" (p. 134), but it was

not designed to assess the rigour with which a trial meets criteria appropriate to testing homeopathy, and it has also been criticised as more generally inadequate.[77]

Clearly, in terms of their examination of homeopathy, Ernst and Singh have shown that a meta-analysis is not only a secondary source of information, but it is based on interpretation of primary information according to arbitrary criteria, and so is open to debate as to its accuracy or even its validity. As such, a meta-analysis is an example of just that conflict of subjective opinions they set in opposition to science at the very beginning of their book, and so it can have no credibility as a *sole* guide to the efficacy of a controversial therapy, yet this is precisely what Ernst and Singh claim it does have.

The second meta-analysis Ernst and Singh refer to is that by Aijing Shang and others:[78]

> Hopes were high that Shang would at last be able to deliver a reliable conclusion. Indeed, after two centuries of bitter dispute between homeopaths and mainstream medics, Shang's meta-analysis was destined to decide, at last, who was right and who was wrong. (p. 135)

In this case

> Shang was ruthless in his demand for quality, which meant that his meta-analysis included only those trials with large numbers of participants, decent blinding and proper randomization. In the end, he was left with only eight homeopathy trials. (p. 136)

His demands may have been ruthless, but the meta-analysis provoked an enormous amount of debate, including criticism from Klaus Linde (author of the 1997 meta-analysis) and Wayne Jonas that

> … Shang and colleagues do not follow accepted and published guidelines for reporting meta-analyses.[79]

They amplified this by stating that

> Shang and colleagues do not report the trials excluded from the review, the quality assessments and odds ratios of all trials included in the review, nor which eight trials were included in the final meta-analysis. This lack of detail is unacceptable in a paper drawing a strong clinical conclusion.[80]

Others too raised objections about the lack of information as to the criteria for selecting the final eight homeopathy trials and six drug trials (out of 110 'matched pairs' of trials),[81] since this made it impossible to verify the validity of the analysis. Subsequently sufficient detail has been published to enable a reconstruction both of which trials were analysed and of the various

assumptions made about the data, and two recently published scientific papers based on such a reconstruction challenge the Shang analysis.[82]

The fact that there have been huge arguments about meta-analyses of homeopathy serves to show that this approach cannot be relied on absolutely, especially as these arguments revolve around basic issues, such as the assumptions used, the interpretation of data, the selection criteria, and the rigour and validity of the source trials from both homeopathic and orthodox perspectives. As a result, Ernst and Singh's claim that "Shang's meta-analysis was destined to decide, at last, who was right and who was wrong" (p. 135) is unjustified. Furthermore, their additional claim that its conclusions are "backed up by the Cochrane Collaboration" (p. 139) is a blatant misrepresentation of the facts. In each of their quotations from Cochrane Collaboration reviews, it is insufficiency of evidence which is the theme not proof of ineffectiveness. Again they have confused 'lack of proof that something is true' with 'proof that it is not true'.

Conclusion

Ernst and Singh begin their conclusion to this chapter with the words:

> It has taken several thousand words to review the history of homeopathy and to survey the various attempts to test its efficacy, but the conclusion is simple … (p. 139)

As we have shown, very little of this chapter has been of any use in reaching a reliable conclusion about homeopathy. What is more, we have only briefly referred to the two pages devoted to orthodox medicine and cholera and the two pages devoted to the missing documents of Nazi research. We have ignored the page belittling homeopathy in India and the two pages or so promoting jokes, doggerel and the unsupported arguments of nineteenth century opponents of homeopathy who rehash the dilution argument refuted by Hahnemann in 1827.[83] Nor have we mentioned the way that the authors attempt to belittle those who do not oppose homeopathy, or the extraordinary proposal that 30c potencies might be prepared by trituration (a process of rendering insoluble minerals soluble, and usually applied only up to the 3c potency, which takes at least 3 hours), or other minor absurdities. All of these matters have filled space the authors could have devoted to truly reviewing the history and nature of homeopathy.

What we have done is show that Hahnemann used the scientific method in his development of homeopathy. We have outlined enough of the theory of homeopathy to at least give a firmer basis

for understanding the issues relating to RCTs. We have also shown that he incorporated into his approach to medicine concepts entirely familiar to orthodox medicine today, including germ theory, public health and hygiene, individuality and homeostasis, but did so many decades before orthodox medicine. What has been implicit in our comments, but should be made explicit, is that there is no argument used against homeopathy today which was not used in Hahnemann's own lifetime. In other words all the advances in science of the last two hundred years have not managed to produce new understandings which conflict with homeopathy. Instead, as we have also pointed out, the science to explain his discoveries appears to finally be emerging, though it is still developing.

Lastly, we have shown that Ernst and Singh have not only failed to explain homeopathy properly or trace its theoretical development, but they have also failed to provide the necessary scientific context for understanding homeopathy in relation to modern knowledge of medicine, biology and physics. At the end of this third chapter, Ernst and Singh have still not defined the most basic of the terms they are using, let alone compared their scientific validity with those of homeopathy. As a result readers are operating in a vacuum in which they are left to define these terms as they think fit, allowing the authors to build an argument on preconceptions and prejudice. Indeed, Ernst and Singh have been prepared to set the purest unsupported conjecture above the facts, finally relying on second-hand interpretation rather than a scientific examination of their subject.

What is surprising is the authors' admission in their concluding section that

> … one of us has had a considerable amount of experience in homeopathy and has even spent some time practising as a homeopath. (p. 141)

How Ernst, with the experience they claim, could have failed to explain homeopathy properly and failed to identify the weaknessses of trials and meta-analyses is a mystery. In the end this chapter offers no evidence at all to support Ernst and Singh's statements that

> … we have come to our conclusions about homeopathy based on a fair, thorough, scientific assessment of the evidence. (p. 141)

Instead, it destroys entirely Ernst and Singh's credibility as a reliable source of information about at least one of the therapies they discuss in detail, and this renders highly questionable their reliability as a source of information about all the other therapies.

Discussion of Chapter 4: The Truth About Chiropractic Therapy

As was the case with acupuncture, we do not pretend to know enough about chiropractic therapy to respond to some of the details in this chapter concerning the therapy itself or its history, so we comment only on the nature of Ernst and Singh's handling of their examination of this therapy. This in itself leaves a lot to be desired, as there is repeated use of innuendo, double standards and even the substitution of opinion for evidence.

Ernst and Singh also make general statements about clinical trials in this chapter, and these throw yet more light on the deficiencies of their chosen model for testing therapeutic effectiveness. By the end of the chapter they have shown not only that the RCT cannot predict the appropriateness of a drug in a specific case, but that it can even fail to provide adequate information as to the safety of a drug. Indeed they admit that a drug tested by RCT may even have to be withdrawn as a result of the harm revealed by its use in clinical practice.

In addition they make further comments on homeopathy and reveal how inadequately they described the therapy in Chapter 3, by referring in this chapter to important concepts not mentioned there. At the same time they appear to be developing an approach to homeopathy which involves the substitution of erroneous terms and concepts for actual ones. Thus they continue their practice in the last chapter of referring to dilution when they mean potentisation, and they set up the idea that the homeopathic aggravation is the same as a deterioration in the patient's condition, in preparation for an argument in Chapter 5.

Finally they accuse alternative therapists of being dangerous on the grounds they do not agree that patients should be treated by orthodox medicine instead. However, Ernst and Singh's own statements about orthodox medicine's methods of testing treatments have undermined the credibility of those methods. As a result, it is appearing more and more reasonable for both readers and alternative therapists to disagree that orthodox drugs are the best treatment, and to question other orthodox treatments . Nonetheless, Ernst and Singh try to incorporate such disagreement into a new meaning for 'side-effects', claiming that oposition to certain orthodox treatments (such as immunisation") is a side-effect of alternative medicine.

Clinical Trials

Ernst and Singh may have had some inkling that their chosen tool (the RCT) has proven unsatisfactory in supporting their argument so far, as they remark that

> Some readers may start to suspect that evidence-based medicine is somehow biased against alternative medicine. Perhaps acupuncture and homeopathy are actually valid therapies, and instead maybe it is in fact the clinical trial that is at fault? Perhaps the clinical trial is part of an establishment conspiracy cooked up by doctors and scientists to protect themselves from the interference of meddling outsiders? (p. 148)

Shortly afterwards they comment that

> Despite its sheer simplicity and powerful ability to get to the truth, some alternative therapists argue that the clinical trial is a harsh test, which is somehow biased against their treatments. But that sort of attitude betrays a skewed understanding of the clinical trial, which merely seeks to establish the truth, regardless of the type of treatment being examined. In fact, the clinical trial provides a wholly unbiased and truly fair test of any medical treatment, either conventional or alternative. The unbiased nature of the clinical trial is demonstrated by the fact that history of mainstream medicine is littered with apparently good ideas from conventional doctors that clinical trials proved to be useless or harmful. (p. 150)

What they fail to take into account is the fact that the "powerful ability to get to the truth" of a clinical trial depends on another factor which they mention in passing only a few pages later, when they state that (our emphasis)

> Half a century later, today's doctors are much more accustomed to the concept of evidence-based medicine, and most accept that *a well-designed randomized clinical trial* is crucial for deciding what works and what does not. (p. 153)

The RCT is only as good as its design, and the "sheer simplicity" of the trial in practice may require great complexity in its design. It is relatively easy, therefore, for opponents of alternative medicine to design trials which appear to be suitable, but which actually yield meaningless results. We have, for example, already demonstrated in our discussion of Chapter 3 how design failures can wholly invalidate a trial of homeopathy.

Ernst and Singh also show in this chapter that their approach to RCTs involves a double standard. In a section where they discuss "the level of risk associated with chiropractic neck manipulation" (p. 176), they allege that

> Chiropractors have been oblivious to the damage that they might have been causing, because there is often a delay between vertebral dissection and the blockage of blood to the brain. Hence the link between chiropractic therapy and strokes went unnoticed for decades. (p.175)

In other words the authors are acknowledging that too narrow a view of the effect of a therapeutic intervention may mean that information about other events associated with that treatment are not noticed. A broader view, as regards both time-scale and the range of subsequent events taken into account, can provide information leading to very different conclusions.

Yet when Ernst and Singh discuss the trials of corticosteroids in relation to respiratory distress in premature babies in the previous chapter (pp. 132-133), they completely ignore this issue and its implications for the RCT. There the authors refer only to the narrowest of outcomes (the incidence of respiratory distress syndrome in actually premature babies), but they never mention other aspects of importance, such as the long term impact of the drug on the health of the mothers, on the health of the premature children, and on the health of those children not born prematurely but whose mothers have received treatment. In addition the figures they provide to demonstrate the success of this treatment in practice are wholly inappropriate, since they are taken from the early 1950s (thirty years before the trial) and from the present (twenty years after it), during which time a number of other factors could have changed.

Side-effects

At the same time Ernst and Singh show that they are well aware of the importance of the need for a broader view of effectiveness since they discuss side-effects, claiming that

> Every medical treatment should offer the likelihood of benefit, but it will also, almost inevitably, carry a likelihood of side-effects. The key issue for patients is simple: does the likely extent of the benefit outweigh the likely extent of the adverse side-effects, and how does this risk-benefit analysis compare to other treatments? As we shall discuss below, the dangers of chiropractic therapy can be serious and in some cases life-threatening. (p. 171)

They do not provide any scientific explanation for side-effects nor any justification of the assertion that “Every medical treatment … will also, almost inevitably, carry a likelihood of side-effects.” If side-effects are inevitable, they must occur as a result of some fundamental principle, which a scientific medical system should be able to explain. If side-effects are not inevitable, the authors should explain why they occur in some cases and not in others; whether they are produced by all alternative therapies; and why orthodox medicine uses a system which produces them rather than one which does not.

In fact, in their discussion of side-effects Ernst and Singh reveal to an even greater extent the inadequacy of the RCT as a means of testing curative interventions when they remark that

> The approach of the chiropractic profession stands in stark contrast to the conventional medical establishment, which rigorously assesses the safety of drugs before they are made publicly available. Even when a drug is made available for prescription, doctors are encouraged to continue to monitor and report any adverse incidents in order to identify any rare side-effects. In Britain, this programme of ongoing vigilance is called the Yellow Card Scheme and it is administered by the Medical Healthcare Products Regulatory Agency (MHRA). This and other methods are why we can, if risks emerge, withdraw a drug. Nothing remotely similar exists in the world of chiropractic. (p. 178)

Having already pointed out that there is "no guarantee that a treatment that had succeeded during a set of trials would cure a particular patient" (p. 23), they are now acknowledging that the harmful effects of such a drug may not be revealed until it is in use. What is more, they are accepting that the degree of these harmful effects may be so great as to require the withdrawal of the drug.

Claims for the "sheer simplicity and powerful ability to get to the truth" of the RCT clearly need to be taken in context. Whilst the RCT can identify whether a defined outcome occurs in one arm of the trial or not, failure to design this definition of outcome appropriately can destroy a trial's "powerful ability to get to the truth" altogether. As a result it may produce seriously incomplete information about the overall effects of a treatment (the range of harm and benefit). On this basis, doubts about the validity of trials for testing alternative medicine are not at all unreasonable.

The Homeopathic Aggravation

Another example of Ernst and Singh's failure to take context into account is their disussion of what they call (their emphasis) "a particularly strange facet of many alternative therapies, a phenomenon known as the *healing crisis*" (p. 186). This term is one used by modern practitioners usually in reference to homeopathic aggravations. According to Ernst and Singh,

> This means that an expected part of the healing process is that the therapy might cause symptoms to deteriorate before they improve – this is supposedly due to the body fighting back or toxins being expelled. (p. 186)

Unfortunately this explanation is wholly inadequate because the three most important terms ('symptoms', 'deteriorate' and 'improve') have not been defined. Ernst and Singh provide the following example to illustrate their point, but this is not referenced and is so lacking in information as to be seriously misleading (their emphasis):

> In one case, a patient being treated for pancreatitis (a life-threatening condition) was given a homeopathic remedy with a label advising that abdominal pain was part of the healing crisis, otherwise known as a *homeopathic aggravation*. So, just when the panceatitis might be worsening and the patient ought to be seeking urgent medical attention, the homeopathic advice would be that the patient should relax because everything is progressing as expected. (p. 186)

As the *The Merck Manual of Medical Information* points out, there is a significant difference between acute and chronic pancreatitis, and it is the acute form which "may be mild or life-threatening"[84] rather than the chronic, though chronic pancreatitis may have flare-ups. "Almost everyone with acute pancreatitis suffers severe abdominal pain"[85] and "most people with [acute] pancreatitis are hospitalized",[86] so the reference to *possible* abdominal pain suggests that this is non-life-threatening chronic pancreatitis, in which case

> During an attack, avoiding alcohol is essential. Avoiding all food and receiving only intravenous fluids can rest the pancreas and intestine and may relieve painful flare-ups. … If pain continues, a doctor searches for complications … If the person has continuing pain and no complications, usually a doctor injects the nerves from the pancreas to block pain impulses from reaching the brain.[87]

In other words, far from the patient needing to seek "urgent medical attention", it is only in the case of continuing pain that orthodox medicine considers that there is a need for further investigations.

Ernst and Singh also fail to explain the nature of the homeopathic aggravation, which varies in both nature and degree according to circumstances. As Kent points out,

> A disease ought always to be well considered as to whether it is acute or chronic. Where there are no tissue changes, where no ultimates are present, then you may expect the remedy to cure the patient without any serious aggravation, or without any sharp suffering, for there is no necessity of reacting from a serious structural change. Where there is a deep-seated septic condition, where pyaemia must be the result, you will sometimes find vomiting and purging.[88]

Indeed, the nature of the reaction to a potentised remedy is highly informative about the state of the patient's health, and Kent (as we have already pointed out) identified no less that twelve distinct reactions to a prescription of a remedy potentised on the centessimal scale.[89] Furthermore, though it is true that in chronic conditions of tissue damage, the homeopathic aggravation may be prolonged, there is a major difference between the homeopathic aggravation and a deterioration of the patient's condition:

> Among the commonest things that remedies do is to aggravate or ameliorate. The aggravation is of two kinds: we may have an aggravation which is an aggravation of the disease, in which the patient is growing worse, or we may have an aggravation of the symptoms, in which the patient is growing better. An aggravation of the disease means that the patient is growing weaker, the symptoms are growing stronger; but the homeopathic aggravation, which is the aggravation of the symptoms of the patient while the patient is growing better, is something the physician observes after a true homeopathic prescription. The true homeopathic aggravation, I say, is when the symptoms are worse, but the patient says, "I feel better."[90]

Because Ernst and Singh have failed to explain homeopathy properly, they have failed to explain the difference between the homeopathic understanding of symptoms and that of orthodox medicine,

and they have failed to acknowledge the difference between 'aggravation' and 'deterioration' and the relationship between 'amelioration' and 'improvement'.

In homeopathy symptoms are either a direct consequence of mechanical causes (such as a narrowing of the pancreatic duct in chronic pancreatitis) or the chemical action of a poison (such as alcoholism in chronic pancreatitis), or are the body's process of managing pathological stimuli, since

> Cells can adapt to damaging stimuli, becoming modified to achieve a new, steady state of metabolism and structure that better equips them for survival in the abnormal environment.[91]

Thus chronic pancreatitis may occur for entirely unknown reasons as a protective reaction. In each case the curative reaction to treatment will reflect the causative process, but in the case of a chronic illness as a result of a protective reaction, homeopathy recognises that the chronic symptoms are a positive activity of the body, a frustrated attempt at a curative response. In this case a brief aggravation accompanied by an increased sense of well-being is therefore not a deterioration but an improvement (the visible sign of "the body fighting back"), and totally different from a genuine deterioration in the patient's condition, of a reduced ability to recover.

Distortion of Terms

We have repeatedly shown how Ernst and Singh confuse or fail to define their terms, but in this chapter they actually redefine what they mean by alternative medicine. Thus, in the Introduction they stated that

> … our definition of alternative medicine is any therapy that is not accepted by the majority of mainstream doctors, … (p. 1)

They continued, stating that

> typically this also means that these alternative therapies have mechanisms that lie outside the current understanding of modern medicine. (p. 1)

However, in this chapter they completely invert this definition, and maintain that chiropactic therapy is an alternative therapy because it makes no scientific sense even though it is accepted by "mainstream" medicine:

> Perhaps the most significant indication that chiropractors have become part of the medical mainstream is that they are licensed in all fifty US states, and they also have legal recognition in many other countries. For example, chiropractors in the United Kingdom are regulated by statute, which means that they have similar standing to that of doctors and nurses. (p. 147)

They then go on to add that

> The concepts of innate intelligence and subluxations are as mystical and as baffling as the concepts of Ch'i in acupuncture or extreme dilution in homeopathy, which means it makes no sense at all from a modern scientific point of view. That is why chiropractic treatment is still considered by many as an alternative medicine – despite its current popularity. (p. 147)

If Ernst and Singh cannot even be consistent about their own definitions, they have no basis for claiming "an unparalleled level of rigour, authority and independence" (p.3) for their book.

Ernst and Singh's lack of rigour is also seen in their unwillingness to accept the difference between dilution (which they have no problem with) and potentisation (which they claim makes no scientific sense). Their use of the wrong term in the context above, where the right term would be much more expected, suggests that they are attempting to substitute an inappropriate term for the correct one in readers' minds, and this tends to be confirmed by a later statement that

> … the remedies were so diluted that none of them contained any active ingredient and all were equally useless. (p. 188)

In this case the insistence on dilution enables Ernst and Singh to repeat a conclusion, which they have no real basis for reaching. In the same way their reason for mentioning the homeopathic aggravation in *this* chapter is twofold. Firstly, by not mentioning it in the chapter on homeopathy, they avoid explaining its true relationship to homeopathic theory, and secondly, by misrepresenting it here they can later claim (erroneously) that

> Even if the start of treatment coincides with a decline in the patient's condition, then this can be excused by the so-called 'healing crisis' or 'aggravation', already discussed in Chapter 4. (p. 234)

Just as their insistance on substituting the term 'dilution' for 'potentisation' allows the authors to claim that there is no active ingredient, so their lack of rigour in explaining homeopathic aggravation enables them to represent it as a deterioration in the patient's health.

Ernst and Singh are happy to distort the use of other terms. For example, they state that

> Unfortunately, homeopathy can have surprising and dangerous side-effects (p. 184),

and then go on to explain that they do not mean side-effects in the sense it usually has within medicine, but in an analogous sense, since

> These have nothing to do directly with any particular homeopathic remedy, but rather they are an indirect result of what happens when homeopaths replace doctors as sources of medical advice. (p. 184)

In fact, these "side-effects" are nothing of the kind, even by analogy, and the term is simply being used in an attempt to denigrate homeopathy.

Immunisation

The first example Ernst and Singh offer as an illustration of "dangerous side-effects" is immunisation, stating that

> many homeopaths have a negative attitude towards immunization, so parents who are in regular contact with a homeopath may be less likely to immunize their child. (p. 184)

The only evidence they provide is a survey of 168 homeopaths, resulting in 77 responses, but the survey was not of the homeopaths' attitude to immunisation. Researchers

> effectively pretended to be a mother asking for advice about whether or not to vaccinate her one-year-old child against measles, mumps and rubella (MMR). This was in 2002 when the controversy over MMR was subsiding and the scientific evidence was clearly in favour of vaccination. (p. 185)

Reactions to such a specific question cannot be taken to be true in general for immunisation, since doubts about the MMR vaccine have not entirely subsided, even now and even in the population in general. For example, the US Court of Ferderal Claims has ruled in a case of MMR vaccination that

> Bailey's ADEM was caused-in-fact and proximately caused by his vaccination. It is well- understood that the vaccination at issue can cause ADEM, and the Court finds, on the record filed herein, that it did actually cause the ADEM.[92]

In fact it is an issue about which there is a lot of uncertainty, not least because over the last hundred years both homeopaths and non-homeopaths have raised serious questions about the long-term effects of this form of treatment.[93] In other words, as for homeopaths,

> The key issue for patients is simple: does the likely extent of the benefit outweigh the likely extent of the adverse side-effects, and how does this risk-benefit analysis compare to other treatments? (p. 171)

However, Ernst and Singh claim that

> The truth is that immunization is arguably the single most important discovery in the history of medicine (p. 186),

and they justify themselves by stating that

> Thankfully these diseases are now rare in the developed world, but this means that it is easy to forget their potentially devastating impact – we no longer appreciate why we used to fear them. If, however, we look beyond the developed world then we can be reminded of the dangers of childhood diseases and the value of vaccination. (p. 186)

In fact, as we have already noted in our discussion of Chapter 1, the major contributor to reduced mortality rates in the developed world was not vaccination at all but public health measures. So while vaccination may be reducing deaths from childhood diseases in Africa, this result could be achieved by "other treatments", such as improvements in living standards and public health measures. At the same time, the possible long-term effects of immunisation are unknown.

Relations with Orthodox Medicine

The second example of these "side-effects" offered by Ernst and Singh is not even based on research among homeopaths, but is a claim that

> … alternative therapists sometimes meddle with a patient's conventional drug-treatment programme, even though they are not qualified to advise about a patient's prescription. A 2004 survey of UK-based acupuncturists showed that 3 per cent of patients received advice about their prescriptions, some of whom suffered adverse consequences. (p. 186)

However, the authors have failed to provide a reference for this study, or even the figures or other information which would allow it to be put into context. For example, they could be referring to anything from 1% to less than 0.1% of patients suffering adverse consequences; they fail to include information about the patients who received advice but did not suffer adverse consequences, and who may, therefore, have benefited; they fail to explain what these "adverse consequences" were, and how they related to the advice; and they do not mention what training the acupuncturists had had, so some of them may have been qualified doctors, and some of these may have had patients who suffered adverse consequences.

Furthermore, Ernst and Singh's criticism of this advice also appears to be a result of using double standards, since they complain later that

> … ten out of ten homeopaths were willing to advise homeopathic protection against malaria instead of conventional treatment… (p. 187)

If, as they themselves have asserted, alternative therapists "are not qualified to advise about a patient's prescription", then these therapists are certainly not qualified to *recommend* a drug. In fact, *recommendation* of a drug requires specific knowledge of its action and of the relationship of this action to the patient's medical condition, and alternative therapists do not usually have the qualifications for this. On the other hand, even unqualified people (such as patients themselves) are quite capable of *observing the effects* of drugs and reaching conclusions as a result of those effects, and alternative therapists are likely to have a greater ability to recognise and understand these effects. In other words, Ernst and Singh are requiring alternative practitioners to act in way directly contradictory to their competence: to recommend drugs (which they are not qualified to do) but not to respond to the effects of these drugs (which they may be entirely qualified to do).

The third example of "side-effects" offered by Ernst and Singh also relates to orthodox medicine, when they claim that

> Perhaps the greatest danger in the way alternative therapists behave is simply the promotion of their own treatments when patients should be in the care of a conventional doctor. (p. 186)

This statement relies entirely on the assumption that a conventional doctor can provide better care than an alternative therapist, so it exhibits exactly the attitude that it is condemning, in that it is the promotion of one treatment over any other. Until the authors have proved that orthodox medicine is scientific and alternative medicine is not, such a demand is unjustified. In addition, the unreferenced evidence Ernst and Singh present to support their argument is a gross example of the use of double standards. They claim that

> There are numerous reports of patients with serious conditions (e.g. diabetes, cancer, AIDS) suffering harm after following irresponsible advice from alternative practitioners instead of following the advice of a doctor. (p. 186)

Yet according to their *own* criteria, such reports need to be verified by specific comparison with controls as the alleged harm might have happened anyway, as they said about the Bristol study:

> The study had no control group, so it was impossible to determine whether these patients would have improved without any homeopathic treatment. (p. 140)

In other words, information from a major study opposed to their argument is not valid, but they consider entirely valid these unreferenced and anonymous "reports" which support their argument while also affecting readers on an emotional level.

Even if these reports were accurate, generalising from them to all alternative practitioners is scientifically unacceptable. After all, there are documented reports of orthodox doctors killing people, but nobody would claim from this that murder is a dangerous side-effect of orthodox medicine.

The very real danger that these examples highlight is the hazard to patients resulting from their orthodox and alternative practitioners being unable to work together, and this hazard is only increased by books which misrepresent alternative therapies, evidence, and even science. That Ernst and Singh are aware of this is shown in this chapter, when they remark that

> … the AMA was forced to alter its attitude. For example, it could no longer discourage its members from collaborating with chiropractors. Although the medical establishment had fought against this move, it had to acknowledge that it resulted in two undoubtably positive outcomes. First, those doctors who collaborated with chiropractors persuaded many of them to be more sympathetic to the idea of conventional medicine. Second, it also encouraged many chiropractors to rethink their attitude to their own chiropractic therapy. (p. 165)

Whilst ill-informed attacks on alternative medicine lead to an increased likelihood that alternative and orthodox practitioners have to work in isolation and opposition, the encouragement of co-operation between them would lead to less risk of harm for patients and increased understanding of which approach is best in any given case at a given time.

The Law

The way in which chiropractic therapy is presented in this chapter illustrates the reverse process at work. Ernst and Singh continually mention legal battles, creating a perception that the therapy is highly questionable, yet time and again they have to state that the law came down on the side of chiropractic therapy. For example, they note that in the 1920s

> An attorney acting on behalf of one of his disgruntled customers attempted to sue Palmer: 'In all our experience as practising attorneys, nothing more closely resembling a fraud and a swindle has ever been brought to our personal attention than this proposition which your school is submitting to its graduates.' (p. 162)

Describing this as an attempt implies that the case was lost, so Ernst and Singh are either claiming that the judgement was wrong (in which case they should provide supporting evidence), or attempting to denigrate chiropractic therapy by quoting the opinions of those with an opposing vested interest.

Similarly, they state that

> Meanwhile, back in America, chiropractors were coming under increasing pressure from the medical establishment, which disapproved of their philosophy and methods. Doctors continued to encourage the arrest of chiropractors for practising medicine without a licence, and by 1940 there had been over 15,000 prosecutions. Palmer strongly endorsed the Universal Chiropractic Association's policy of covering legal expenses and supporting members who had been arrested, which resulted in 80 per cent of chiropractors walking free from court. (p. 163)

In this example the therapy was not in question but only the right to recognition, and again the legal battles arose from vested interests. Even so chiropractors were largely winning their cases. In fact, the authors finally acknowledge that underhand methods were deliberately used to attack chiropractic therapy in the U.S.A., noting that

> 'Sore Throat', an anonymous source within the AMA [American Medical Association], leaked material that revealed the details and extent of the AMA's campaign, which prompted Chester A. Wilk, a chiropractor from Chicago, to file an anti-trust lawsuit against the AMA. Wilk was arguing that the AMA's campaign against chiropractors amounted to anti-competitive behaviour, and that the medical establishment was merely trying to corner the market in treating patients. (p. 165)
>
> After dragging on for over a decade, the lawsuit eventually ended in 1987. Judge Susan Getzendanner, who had presided over the case, ruled that the AMA had indeed acted unfairly against chiropractors:
>
> > Evidence at the trial showed that the defendants took active steps, often covert, to undermine chiropractic educational institutions, conceal evidence of the usefulness of chiropractic care, undercut insurance programs for patients of chiropractors, subvert government inquiries into the efficacy of chiropractic, engage in a massive disinformation campaign to discredit and destabilize the chiropractic profession and engage in numerous other activities to maintain a medical physician monopoly over healthcare in this country.
>
> The AMA took the decision to the Supreme Court, but the appeal failed in 1990 and thereafter the AMA was forced to alter its attitude. (p. 165)

In this context and whatever the merits or faults of this therapy, Ernst and Singh do not help their argument when they make claims such as that

> D. D. Palmer died just a few weeks later [in 1913] – officially the cause of death was recorded as typhoid, but it seems more likely that his death was a direct result of injuries caused by his son. Indeed, there is speculation that this was not an accident, but rather a case of patricide. (p. 161)

By rejecting the official record (presumably a death certificate signed by an orthodox doctor), and by speculating about the facts, they are displaying a partiality in their approach which can only lead readers to question the validity of this examination of chiropractic therapy.

Lack of Balance

This apparent lack of balance on the part of the authors tends to be confirmed by their justifications of the AMA's attitude to chiropractic therapy, as when they remark that

> This antagonism might seem unreasonable, but remember that the medical establishment had several reasons for despising chiropractors. These included their belief in the unscientific notion of innate intelligence, their denial that bacteria and viruses cause many diseases, and their conviction that realigning a patient's spine could cure every ailment. On top of all this, conventional doctors were shocked by the fact that many chiropractors were fond of the *E-meter*, another bizarre diagnostic gadget. (p. 163)

While reference is made in this chapter to the "beliefs" of chiropractors, Ernst and Singh do not explain clearly what chiropractors mean by "innate intelligence". Nor do they clarify whether chiropractors deny any role for bacteria and viruses as causes of disease, or only deny that they are the entire cause (which orthodox medicine also denies). Furthermore, the authors do not make clear what the scientific or evidence-based grounds were in the 1960s for these "reasons for despising chiropractors". In the absence of such grounds opposition is simply the expression of an opinion, and (according to Ernst and Singh themselves) unscientific and unjustified.

Similarly, it is unhelpful that the authors constantly imply that the popularity of chiropractic therapy is due to reasons other than its therapeutic success, such as when they claim that

> Thanks to his radio station and other clever marketing techniques, Palmer oversaw the growth of the chiropractic movement over the next few decades, not just in America but also in Europe. (p. 163)

Even a "pioneering radio station" did not reach the whole of America in 1922, let alone Europe as well. Futhermore, orthodox drug companies also have access to the media which they use to promote their views and products, so any implication that such access is a significant factor needs to be balanced by a comparison with the access available to other approaches and their relative popularity.

Finally, Ernst and Singh's criticism of the therapy itself relies on doubly interpreted results, since they comment that

> In fact, there have been so many systematic reviews that in 2006 Edzard Ernst and Peter Canter at Exeter University decided to take all the current ones into account in order to arrive at the most up-to-date and accurate evaluation of chiropractic therapy. Published in the *Journal of the Royal Society of Medicine*, their paper was entitled 'A systematic review of systematic reviews of spinal manipulation'. (p. 154)

As the distance from the primary evidence increases it becomes harder to identify possible methodological flaws in the original research. In the case of homeopathy we have shown that such flaws may invalidate the results in individual trials, which are nonetheless incorporated into meta-analyses, fatally compromising their conclusions. At the same time there is a degree of subjectivity in the process of constructing meta-analyses of trials, and necessarily subsequent analyses of meta-analyses will have a tendency to increased unreliability. In addition, if readers of this book are to trust the analyses of analyses which the authors are using as a basis for assessing chiropractic therapy, they must be able to trust the competence of the researchers at each stage, but one of these researchers (Edzard Ernst) has shown himself to be highly unreliable, so his research is not an appropriate basis for drawing conclusions about the therapy.

Discussion of Chapter 5: The Truth About Herbal Medicine

The discussion of herbalism as an alternative therapy offers an interesting problem for Ernst and Singh because, according to their first definition,

> … alternative medicine is any therapy that is not accepted by the majority of mainstream doctors, and typically this also means that these alternative therapies have mechanisms that lie outside the current understanding of modern medicine. (p. 1)

According to their second definition of alternative medicine, a therapy *can* be "part of the medical mainstream" (p. 147), the decisive criterion being if

> … it makes no sense at all from a modern scientific point of view. That is why chiropractic treatment is still considered by many as an alternative medicine … (p. 147)

However, in the case of herbalism they note that

> It is already becoming quite clear that this chapter on herbal medicine will be very different from the previous chapters on acupuncture, homeopathy and chiropractic manipulation. These other therapies have struggled to be accepted by mainstream medicine, partly because their underlying philosophies conflict with our scientific understanding of anatomy, physiology and pathology. (p. 196)

They go on to add that

> By contrast, plants contain a complex cocktail of pharmacologically active chemicals, so it is not surprising that some of them can impact on our wellbeing. Consequently, herbal medicine has been embraced by science to a far greater extent than the other treatments above. (p. 196)

They even remark that

> Indeed, there is general agreement that much of modern pharmacology has evolved out of the herbal tradition. (p. 196)

Quite simply, neither of the definitions of alternative medicine offered in this book up to this point justifies the inclusion of herbalism, and yet the authors maintain that

> Despite all these examples, which demonstrate that numerous herbs have become part of mainstream medicine, it is important to stress that much of herbal medicine is still considered alternative. (p. 197)

As a result Ernst and Singh have to create their *third* definition of alternative medicine, in a book supposedly devoted to taking "a scientific look at the current plethora of alternative treatments

..." (p. 1). In Ernst and Singh's growing plethora of definitions of alternative medicine, the nearest this third one comes to being expressed explicitly is when Ernst and Singh state that

> Crucially, the scientists attempted to evaluate the impact of their treatments on patients to find out which herbal extracts were safe and effective, and which were dangerous or ineffective. The treatments that emerged from this scientific approach to herbal medicine are so utterly mainstream that they are no longer labelled herbal medicines, but rather they are simply incorporated within the realm of modern pharmacology. (p. 197)

In other words, alternative medicine is actually anything outside "the realm of modern pharmacology".

This explanation makes clearer a lot of things that were puzzling before. Because pharmacology is essentially a branch of chemistry, the authors have shown no difficulty with explanations based on chemical activity (herbalism), but any suggestion that activity may not be taking place on the chemical level (acupuncture, homeopathy) or at least on a gross anatomical level (chiropractic therapy) is anathema to them. Had Ernst and Singh started their book with this definition, readers would have had no illusions about what the authors meant when they stated

> All these questions and more will be answered in this book, the world's most honest and accurate examination of alternative medicine. (p. 3)

Readers would have been able to add the necessary proviso "… according to the opinions and practices of modern pharmacology", and been able to readily identify the authors' attempts to constrain acupuncture, homeopathy and chiropractic therapy to levels of action "acceptable" to pharmacology. The failure of these therapies to pass the tests of pharmacology would also have raised questions about the appropriateness of the theoretical perspective within which the tests were conducted. In short, had Ernst and Singh been honest, the book could not have been mistaken for exhibiting "an unparalleled level of rigour, authority and independence" (p. 3), but would have been instantly recognised as flawed, compromised and prejudiced.

The Whole and the Part

Looking at some of the details of this chapter, there are yet again serious problems with the scientific validity of some of Ernst and Singh's arguments. For example, they remark (our emphases) that

> In short, alternative herbal therapists continue to *believe* that Mother Nature knows best and that the whole plant provides the ideal medicine, whereas scientists *believe* that nature is just a starting point and that the most potent medicines are derived from identifying (and sometimes manipulating) key components of a plant. (p. 197)

According to their own words, Ernst and Singh are discussing two beliefs at this point, but when they assert that "The scientists wanted to identify the active ingredient of each plant and isolate it" (p. 197), the belief has been assigned the weight of a fact, without any scientific grounds being given for the assumption that the action of herbal medicines is solely the result of an "active ingredient". Indeed, the idea that a whole plant, a whole part of a plant and a specific chemical extract of a plant will all have have the same action on the human body requires substantial evidence in its support. In the case of poison nut (nux vomica), St Ignatius bean (Ignatia) and strychnine, these respectively contain greater amounts of strychnine, but they do not produce identical symptoms in human beings. In fact Ernst and Singh themselves confirm that the action of a plant differs from that of its component chemicals when they note that

> There have been attempts to isolate the key active ingredient in St John's wort, thought to be either *hyperforin* or *hypericin*, but when these have been tested, however, it appears that they are not as effective as the plant itself. In this particular instance, the herbalist's view appears correct. In other words, it seems that the benefits of St John's wort are due to a combination of chemicals, each one working to enhance the effect of the others. (p. 200)

Furthermore, this difference in action has important implications, since it indicates that out of the *total* effects of the herbal material a *specific* effect is being sought when scientists (that is, pharmaceutical researchers) "endeavour[ed] to improve on nature by manipulating the molecules of the original ingredient" (p. 197). In other words, when Ernst and Singh describe chemical extracts as "not as effective" or as "the most potent medicines", they are not referring simply to a quantitative difference but also to a qualitative difference in the definition of effectiveness. That is, they are changing the meaning of effectiveness from one relating to the *actual effects* of the herbal material to one relating to the *desired effects* of the drug. Ernst and Singh never define what they mean by effectiveness, and they do not address this important distinction.

At the same time, Ernst and Singh do indicate that a narrowly defined 'desired effectiveness' is unachievable. For example, they comment on the proposition 'first do no harm' that (our emphasis)

> Modern medicine interprets this edict in terms of benefit versus risk, because we now accept that *almost every medical intervention carries a risk of side-effects*. (p. 205)

We have already discussed the issue of side-effects in our notes on the previous chapter, but in this context Ernst and Singh are confirming that the whole strategy of trying "to identify the active ingredient of each plant and isolate it", the rationale behind the development from herbalism to drug therapy, is flawed. The refinement of using specific chemicals rather than plants did not eliminate the problem of side-effects, because the *actual effects* of a substance cannot be reduced to a specific *desired effect*. Hence any definition of effectiveness must take into account the whole action of a medicinal substance and its relationship to the symptoms as a whole. Indeed, Bellavite and Signorini make it clear that as our understanding of how drugs act in the body has increased, so has our recognition of their unpredictability of action at a cellular level:

> Receptor dynamics vary in physiology and pathology and are so complex that a given substance can behave as an activator or an inhibitor on the same cell or on the same organism, according to the doses of the substance itself in relation to the receptor sensitivity at a given time.[94]

In short, side-effects show that the homeopathic approach of holistically relating treatment to symptoms has a better rationale, and is more scientific.

Risks and Benefits

Ernst and Singh provide us with an example of how drug therapy failed to reduce actual effects to only desired effects when they discuss Aspirin, which is derived from willow bark. They state that chemists

> … successfully identified the active ingredient, this time naming it *salicin*, based on *salix*, the Latin word for willow. In this case, however, chemists took nature's drug and attempted to modify and improve it, driven by the knowledge that salicin was toxic. Taken in either its pure form or in willow bark, salicin was known to cause particularly harmful gastric problems, but chemists realized that they could largely remove this side-effect by transforming salicin into another closely related molecule known as acetylsalicylic acid. (p. 195)

This chemical was marketed under the name 'Aspirin' and Ernst and Singh go on to state that

> On the negative side, scientific investigations have also revealed that aspirin can lead to stomach bleeding in 3 out of every 1,000 people and can increase risk of asthma attacks. Moreover, aspirin is not recommended for children under twelve years of age. (p. 196)

Clearly, there continued to be risks with the drug as there had been with the bark, and it should also be remembered that these risks were discovered and investigated as a result of clinical use of the drug, much as the risks of the bark had been identified through its clinical use. It is also pointed out that other uses for Aspirin have been found

> … and it has become far more than the painkiller it was first believed to be. Clinical trials have shown that it can reduce the risk of heart attack, stroke and many types of cancer. (p. 196)

Again, it should be remembered that these applications were discovered and investigated as a result of its use in clinical practice, not during its initial research.

In addition Ernst and Singh identify a commercial benefit derived from making drugs as opposed to using plants. They note that pharmaceutical researchers, having identified an "active ingredient",

> … then sought to synthesize it industrially, in order to mass produce it at low cost. (p. 197)

In the case of Aspirin this meant great commercial success for the company which had developed the drug, Bayer (now the third largest chemical company in the world[95]), since,

> Thanks to the scientific approach, aspirin has gone from strength to strength. It is now the cheapest and biggest-selling drug in the world, … (p. 196)

At the start of this book (p. 2) the authors mentioned "the annual global spend on all alternative medicines", "the fastest-growing area of medical spending" and "persuasive marketing" as if they were problems. Similarly, in Chapter 4 they were critical of the commercialism of the founders of chiropractic therapy, but here they have nothing but praise for how (our emphases)

> The Bayer Company in Germany started marketing this *new wonder drug* under the name of aspirin in 1899, and kicked off its promotional campaign by writing to 30,000 doctors across Europe in *the first mass mailing in pharmaceutical history*. Aspirin was an immediate success and there were *numerous celebrity endorsements* – Franz Kafka said to his fiancée that it eased the unbearable pain of being. (p. 196)

This is another example of Ernst and Singh's double standards, since, despite the fact that many of the effects of this "wonder drug" were unknown when it was launched, they have no objection to the "persuasive marketing" used or to "celebrity endorsements", though these are condemned at length in the next chapter when used to support alternative medicine.

Trial and Error

Ernst and Singh's argument about effectiveness, benefit and risk centres on their claim that drug therapy differs from herbalism in that

> Crucially, the scientists attempted to evaluate the impact of their treatments on patients to find out which herbal extracts were safe and effective, and which were dangerous or ineffective. (p. 197)

At the beginning of the first chapter they claimed that

> … it is only comparatively recently that [doctors] have developed an approach that allows them to separate the effective from the ineffective, and the safe from the dangerous (p. 7),

and yet in this chapter they acknowledge that such an approach actually existed thousands of years ago, since

> Societies around the world used trial and error to develop their own bodies of medical knowledge based on local plants, with the tribal healer acting as the expert database and provider of medicines. (p. 193)

Indeed, they note that willow bark "had been used to reduce pain and fevers for thousands of years" (p. 195), and that cinchona bark "had long been used by the Peruvian Indians to treat malaria" (p. 194).

When it comes to the discovery of the uses of drugs, Aspirin was developed prior to the extensive use of the RCT to test drugs, and, as we have pointed out, a fuller evaluation of its uses and deficiencies arose from observation of its effects in clincal practice, that is, through trial and error. Furthermore, Ernst and Singh have pointed out in the previous chapter that it is important that drugs tested using the RCT be monitored in clinical practice, so that "we can, if risks emerge, withdraw a drug" (p. 178). In other words, determining effectiveness and safety still involve a degree of trial and error.

Even the discovery of drugs may involve "lucky discoveries", as Ernst and Singh point out in another case:

> Viagra, one of the most successful drug discoveries in recent years, was originally developed to treat angina, but a pilot study showed that it did little to alleviate this condition. However, when researchers decided to stop the trial early and recall any unused pills, they were perplexed by the reluctance of the trial volunteers to return them. Subsequent interviews revealed that Viagra had an unexpected and desirable side-effect. Further trials and safety tests have resulted in Viagra's current widespread availability for the treatment of impotence. (p. 225)

Apart from the fact that this shows the interchangeability of side-effects and "desirable" effects, this example makes it clear that trial and error are still as integral a part of the discovery of drugs as they were part of the discovery of herbal medicines. Clearly the development of drugs has involved only a faster development of the process of trial and error, rather than a shift to a different approach.

Drug Theory

Despite the absence of a definition of effectiveness Ernst and Singh assert that

> We know that scientific plant-based pharmaceuticals are effective, but the key issue in the context of this book is whether or not alternative complete-plant herbal medicines actually work. (p. 198)

Given that they also claim that "numerous herbs have become part of mainstream medicine" (p. 197) as a result of their use as treatments, and that "scientists wanted to identify the active ingredient of each plant and isolate it" (p. 197), there can surely be no question in their minds that "complete-plant herbal medicines actually work". Such is the dogmatism of their approach, however, that they will not accept that any treatment is effective unless it has been tested by RCT, even though they have shown that the RCT is deficient as tool for testing curative interventions. The implication is that orthodox pharmacology itself rests not on foundations of a scientifically justified theory, but on the use of a single inadequate tool.

In this context, another herbal treatment mentioned by Ernst and Singh has important implications, since they comment about cinchona that

> It was this powder that inspired Samuel Hahnemann to invent homeopathy … (p. 195)

In other words, for Hahnemann cinchona proved the starting point of a wholly new and systematic approach to medicine, whereas

> Scientists, however, took the herbal remedy in quite a different direction and ultimately maximized its potential. Speculating that it was only one component of the bark that was medically active, they attempted to isolate that component and then deliver it in a more concentrated and potent manner. (p. 195)

In the case of these scientists, not only has their speculation still not produced a drug from cinchona free of side-effects, but these side-effects can bear a remarkable similarity to malaria:

> Within 24 hours of taking the first dose of quinine 260 mg for leg cramps, a 57-year-old Native American female presented to the hospital with symptoms of nausea, vomiting, generalized myalgia, headache, fever, chills, and rigor. … Following discontinuation of quinine, the patient's symptoms resolved within 48 hours.[96]

Certainly they have not succeeded in producing any qualitative advance towards a general theory of medicine.

The reason for this difference between Hahnemann's conclusions from studying cinchona and the conclusions reached by other scientists is revealed by Ernst and Singh in their discussion of the study of digitalis:

> Withering's career in medicine combined with his interest in science resulted in a major investigation into the medical benefits of the foxglove plant, also called *digitalis*. It had long been known that digitalis could be used to treat dropsy, a swelling associated with congestive heart failure, but Withering spent nine years meticulously documenting its impact on a total of 156 patients. (p. 194)

It should be pointed out, before going any further, that the authors never acknowledge the forty years spent by Hahnemann meticulously documenting his clinical, academic and experimental research, indicating once again the double standards Ernst and Singh, "both trained scientists" (p. 3) use when judging those who support or oppose alternatives to orthodox medicine. Having said that, these authors are making clear a primary tenet of orthodox drug therapy and of this book, namely that tests of drugs should be conducted on sick people, whereas Hahnemann argued that accurate information could only be obtained from tests on subjects who were as healthy as possible. His reasoning was in agreement with that of *The Merck Manual of Medical Information*, which notes that "many factors influence drug response" and lists twenty-six examples, significantly including "disease" among them.[97]

Ernst and Singh go on to say of Withering that

> His report highlighted his rigorous and impartial approach to analysing digitalis:
>
> > "It would have been an easy task to have given select cases, whose successful treatment would have spoken strongly in favour of the medicine, and perhaps been flattering to my own reputation. But Truth and Science would condemn the procedure. I have therefore mentioned every case … proper or improper, successful or otherwise" (p. 194)

In doing so, they make clear a second tenet of orthodox drug therapy, namely that drugs should be tested for their effect on a condition, rather than for all their possible effects:

> … without decent trials it is impossible to give an indication of whether or not a particular treatment is effective for a particular condition. (p. 204)

The fact that Ernst and Singh demonstrate in Chapter 1 that this approach is fundamentally flawed is compounded by the fact that we also know that the total range of effects of digitalis are paradoxical since it can both produce heart failure and be used to treat heart failure. As Bellavite and Signorini point out:

> Digitalis, which is regarded today as a fully fledged hormone probably produced by the adrenal glands [*Lancet* editorial, 1991], causes depression of cardiac function in healthy subjects when administered in pharmacological doses, whereas it has a positive inotropic effect in heart failure.[98]

In other words, Hahnemann was correct in emphasizing the need to discover all the symptoms a substance can produce, and it is misleading for Ernst and Singh to claim that

> Withering's research marks a turning point in the history of herbal medicine, from its haphazard ancient roots towards a more systematic and scientific attitude. (p. 194)

In fact, Withering's research simply represented an increase in the detail with which scientists were studying medicines, not a change of perspective, since his work was based on two assumptions which have been a part of medicine for thousands of years and which have become the basis of pharmacology wthout any scientific justification of their validity. At almost exactly the same time another scientist (Hahnemann) was demonstrating that these assumptions were erroneous, and that there was a wholly different approach which could revolutionise medicine.

Ernst and Singh's failure to discuss these issues, or even to mention the paradoxical nature of their chosen examples of pharmacological success, seriously undermines the credibility of their book. It shows that they have so little confidence in their standard for measuring medical success, that they dare not scrutinise it.

Fallacies

In announcing the conclusions they have reached, Ernst and Singh maintain that

> We have drawn upon the results of hundreds of scientific papers in order to examine the four major strands of alternative medicine: acupuncture, homeopathy, chiropractic therapy and herbal medicine. (p. 219)

This is true, but they have also repeatedly used double standards when assessing these papers, and in addition

1. They have defined alternative medicine in three different ways to suit their needs;
2. They have failed to define what they mean by disease or effectiveness;
3. They have failed in at least one case to explain an alternative therapy sufficiently or accurately;

4. They have covertly used the perspective of pharmacology to judge scientific validity;
5. They have failed to show that their principal tool (the RCT) is adequate for the task; and
6. They have shown that the secondary analyses they have used are inherently subjective.

Nonetheless, they ignore the fact that their own conclusions are based on inadequate evidence in order to accuse alternative therapists of nine so-called "fallacies".

These "fallacies" appear to be arbitrarily selected (there are three sets of three), and not only is no evidence provided as to their significance, but many of them are presented in a bizarre form. As regards the first three, the authors allege that (their emphases)

> The initial reasons why people find alternative medicine appealing are often related to the three core principles that underlie so many of the therapies – they are said to be based on a more *natural*, *traditional*, and *holistic* approach to healthcare. Advocates of alternative medicine repeatedly cite these principles as strong grounds for adopting alternative medicine, but in fact it is easy to show that they are nothing more than clever and misleading marketing ploys. (p. 222)

Curiously, in their discussion of four major alternative therapies, Ernst and Singh have not seemed to think that these "core principles" are important, since this is the first time they have been identified as such, and they do not explain their importance now either. Indeed the authors do not even explain what they or the alleged "advocates of alternative medicine" actually mean by "natural". On the other hand, they present some unsupported justifications for selecting the other "principles", such as that

> The notion that traditional is a good quality helps many alternative therapists because it means that the placebo effect is reinforced by a dose of nostalgia. (p. 223)

This is a repetition of their claim in Chapter 2 that "a basis in ancient wisdom" (p. 67) could promote the placebo effect, despite the fact that the only examples they offer in that (or any other) chapter point to the opposite conclusion, namely that (our emphasis)

> … among other things, the doctor's reputation, the cost of the treatment and its *novelty* could all boost the placebo effect (p. 57).

They also overlook their earlier dismissal of "holistic" as only an "impressive buzzword" (p. 2) in order to claim that (our emphasis)

> If anything, conventional medicine takes a *more holistic* approach than alternative medicine. (p. 223)

They go on to justify this claim by the example of "the case of a student looking for advice about malaria prevention" (p. 223, referring to pp. 187-188) on the grounds that

> The conventional clinic offered a long consultation, covering not just the drug options, but also the use of insect repellent, appropriate clothing and the student's medical history. (p. 223)

In fact, this was only a case of a specialist travel clinic providing specialist travel advice. Another example they offer is that of GPs, on the basis that

> GPs consider a patient's lifestyle, diet, age, family history, medical background, genetic information and the results of a variety of tests. (p. 223)

In fact GPs will still use drugs to treat specific parts of the patient's condition and will refer patients to specialists in certain illnesses or parts of the body, rather than provide a single treatment for the whole patient.

Fallacies and Science

The next set of "fallacies" are based on the allegation that

> Alternative therapists are, of course, aware that scientists are largely critical of alternative treatments, so they attempt to undermine the scientific criticisms by questioning the credibility of science itself. (p. 223)

Ernst and Singh provide no evidence to justify the statement that "scientists are largely critical of alternative treatments", but their own conduct during this book goes a long way towards undermining the "credibility of science". They have shown that "trained scientists" (p. 3) are willing to redefine their terms (when they have defined any) and even science itself to suit their own purposes, and they have presented the RCT (which is merely a tool) as if it were capable of producing truth in the abstract, independently of any questions of theoretical context. Any 'scientist' who uses 'science' of this sort "cannot test alternative medicine", "does not understand alternative medicine" (or indeed any medicine), and is inherently "biased against alternative ideas" (all p. 224), and alternative therapists are justified in criticising them.

With the third set of "fallacies" the authors appear to be suggesting that alternative medicine should not use scientific explanations, products ("gadgets") or experiments, "to its own advantage whenever it is convenient" (p. 226), though why alternative medicine should be treated exceptionally in this respect is not explained. For example, the authors comment that

> … magnet therapists sometimes argue that magnets act on the iron component in our blood to restore the body's electromagnetic balance, but this makes no scientific sense. (p. 226),

yet, as part of their criticism of homeopathy, they also allege that

> Herbal medicine follows the more commonsense rule that more concentrated doses lead to stronger remedies. (p. 98)

As we noted above, Bellavite and Signorini point out, when discussing digitalis, that more concentrated doses can produce a reaction not stronger but entirely opposed to more dilute ones. In other words, Ernst and Singh are presenting an explanation as though it were scientifically valid, when in fact it is not.

Similarly, Ernst and Singh remark that

> Just because some alternative therapists employ gadgets that look impressive, it does not mean that they actually work. (p. 226)

This may be true, but again this is not a failing unique to alternative therapists. Examples include the unnecessary use by dentists of "an applicator generating ultrasound" (p. 58), and the misuse by doctors of antibiotics:

> Doctors are overprescribing antibiotics for common sinus infections and related conditions, possibly in the false belief they may help in cases where symptoms are protracted, researchers reported on Friday.
>
> Bacteria can cause rhinosinusitis – an inflammation of the sinuses – but a virus such as the common cold is often a more likely culprit so antibiotics seldom work, the researchers reported in the journal *Lancet*.[99]

More importantly, unlike the Aqua Detox footbath mentioned by Ernst and Singh, this misuse of a scientific product has serious consequences, since

> Moderating antibiotics, which are useless against viruses, is critical because overuse of drugs is contributing to the rapid rise of drug-resistant bacteria.[100]

In other words, the 'scientific gadget' fallacy within orthodox medicine has contributed to the development of MRSA and *c. difficile*.

Explaining the final "fallacy" (concerning clinical trials), Ernst and Singh state that

> We have stressed the vital role of clinical trials in determining the truth about a treatment … (p. 227)

As has been unwittingly shown by the authors themselves, this is itself a fallacy. While they have demonstrated that clinical trials can identify the harmful effects of a treatment, they have not provided evidence that it can prove that a treatment has beneficial effects. Indeed, they have shown that the best it can do is to identify a combination of beneficial and harmful effects, the significance of which may not become apparent until the treatment is in clinical practice. It is also misleading to claim that

> The problem here is that a single trial is not enough to demonstrate that a particular therapy works, because that particular trial might have been prone to error, the vagaries of chance or even fraud. That is why we have not based the conclusions of this book on individual pieces of research, but instead we have examined the broad consensus drawn from the totality of the reliable evidence. In particular, we have relied on meta-analyses and systematic reviews, in which a team of scientists has set itself the task of examining all the research in order to come to an over-arching conclusion. (p. 227)

As we have explained, if the design of the trials is inappropriate, the evidence gathered is unreliable, no matter how many trials are conducted, and any meta-analyses and systematic reviews based on these trials will also be inaccurate, and will in addition introduce a degree of subjectivity into the conclusions (as Ernst and Singh have made clear).

Personal Experience

Ernst and Singh sum up their 'examination' of acupuncture, homeopathy, chiropractic therapy and herbal medicine by stating that "with some important exceptions our conclusions about alternative medicine are largely negative" (p. 230), but then go on to acknowledge that,

> If we take homeopathy as an example, then millions of people are convinced that it is effective because of their own personal experience – they suffer various ailments, they consume homeopathic remedies and they feel better, so it is perfectly natural to assume that the homeopathic remedy was responsible for their recovery. (p. 231)

As they point out, while it is reasonable to ask

> If recovery from illness takes place after taking some homeopathic pills, then isn't it obvious that the homeopathic pills caused the recovery? If there is a correlation between two events, then isn't it common sense that one event caused the other? The answer is 'No'. (p. 231)

While Ernst and Singh's argument may be true for personal experience of a single occurrence, if the same relationship is observed between the pills and recovery again and again for a variety of different conditions and different people, then the likelihood of a correlation increases significantly. What proportion of Ernst and Singh's "millions of people" have experienced single occurrences,

and what proportion repeated occurrences, is something the authors do not tell us, despite its importance to their question of

> How do we resolve this conflict between personal experience and scientific research? (p. 231)

Ernst and Singh believe that they have used scientific methods to reach their conclusions, so they cannot question the validity of the "scientific research" and accept the validity of personal experience. However, we have shown, using mainly their own statements, that they have actually failed to provide a firm scientific basis for claiming that alternative medicine does not work, and in this context personal experience needs to be investigated seriously. Instead they "assume (for the time being, at least) that homeopathy is ineffective" (p. 231), and explain personal experience by a collection of coincidences, summing them up by noting that

> Many of the coincidences described so far are particularly likely to impress those patients who already have a strong belief in alternative medicine. (p. 234)

Coincidence

Some of these explanations relate to orthodox drugs and are remarkably far-fetched. The first of them is not only highly indefinite, in that (our emphases)

> … the patient *might* be taking conventional medicine that *might* coincidentally take effect around the time that he or she resorts to homeopathic pills (p. 232),

but also requires that we accept a contradictary response. Someone with "a strong belief in alternative medicine" would be unlikely to continue with orthodox medicine. On the other hand, if the patient "resorts" to homeopathy and continues with orthodox treatment, he or she clearly does not "have a strong belief in alternative medicine", and so he or she is much more likely to attribute any success to the orthodox treatment. In addition Ernst and Singh provide no evidence as to the actual proportion of those "millions of people" affected in this way. In short this is only an unsubstantiated opinion.

Similarly, the authors require us to accept an extraordinary set of possibilities when they suggest that

> We also have to consider the possibility that the homeopathic remedy is contaminated, perhaps with steroids or other conventional pharmaceuticals. (p. 232)

They require us firstly, to accept that this contaminant is *accidentally* the appropriate orthodox medication; or secondly, that it is *accurately prescribed* by a practitioner who, according to the authors, is "not qualified to advise about a patient's prescription" (p. 186). Thirdly, they require us to accept that the contamination produces a sufficiently high dose as to be effective in orthodox terms. Fourthly they require us to accept that the contaminant is capable of succeeding, often in one single dose in a month, when orthodox medicine would need to prescribe it in doses repeated once, twice or even three times per day for weeks! Unsurprisingly, Ernst and Singh have not provided any evidence that these circumstances have ever occurred, perhaps because, as they point out, "remarkable coincidences are rare" (p. 233).

The next set of explanations are based on the idea of "regression to the mean" (p. 233), the tendency for patients experiencing acute conditions or flare-ups in chronic conditions to return to "their average (or mean) state" (p. 233). Ernst and Singh's argument is that this process alone is responsible for a patient's return to health, unaided by any alternative medical treatment, claiming that

> … it is quite natural for symptoms to fluctuate, and it might be that the taking of a homeopathic pill coincides with an upswing in the patient's condition. (p. 233)

They point out that

> … many conditions have a limited natural duration, which means that the body heals itself given time. (p. 233)

They also suggest that

> Even if the start of treatment coincides with a decline in the patient's condition, then this can be excused by the so-called 'healing crisis' or 'aggravation' … (p. 234)

Finally they claim that

> … when recovery actually begins, for whatever reason, the alternative therapist is still in a position to take the credit. (p. 234)

As an example, they propose that

> Unexplained lower back pain significantly improves within six weeks for roughly 90 per cent of patients who receive no treatment, so any homeopath who can retain a patient for a couple of months is highly likely to see some sort of recovery within this period. (p. 233)

The problem with the whole argument is summed up by this example. Despite the statistical evidence, it is not possible to predict when an individual patient will recover, or, as Ernst and Singh put it when discussing drug trials:

> There was still no guarantee that a [drug] treatment that had succeeded during a set of trials would cure a particular patient … (p. 23)

In other words, if there is no means of identifying *in an individual case* the difference between the body healing itself regardless of treatment and because of it, this argument is true for all treatments, orthodox or alternative. That such means are absent in orthodox medicine is indicated by *The Merck Manual of Medical Information*, which notes (their emphasis) that

> Much of the mystery surrounding drug action can be cleared up by recognizing that drugs affect only the *rate* at which biologic functions proceed; they do not change the basic nature of existing processes or create new functions.[101]

In an individual case, orthodox treatment only changes the rate of recovery not the nature of the recovery, and so no difference can be identified. As a result, this argument is incapable of being proved or disproved for either orthodox or alternative medicine using the orthodox medical model, and so it has no scientific validity within that model. It is simply an opinion.

To explain why Ernst and Singh have failed to recognise the absurdity of these arguments, we have to bear in mind that they believe in orthodox medicine, and they themselves point out that (their emphasis)

> … believers are vulnerable to *confirmation bias*, which is the tendency to interpret events in a way that confirms preconceptions. In other words, believers will focus on information that supports prior beliefs and ignore information that contradicts those beliefs. (p. 234)

For the same reason they finally come back to

> … the phenomenon whereby a patient responds positively to a treatment simply because of a sincere belief that the treatment is effective. (p. 234)

Even though "scientists strive to establish the scientific basis of the placebo effect" (p. 62), Ernst and Singh are prepared to rely on it as their main explanation as to why the personal experience of "millions of people" contradicts their own belief in the "evidence" of clinical trials of homeopathy. Quite simply, they attack homeopathy (along with other alternative therapies) on the grounds that it

has no scientific explanation, while relying on the placebo effect which they acknowledge also has no scientific explanation. *That* is confirmation bias.

Discussion of Chapter 6: Does the Truth Matter?

Ernst and Singh have told us their conclusions, and in this chapter they make pronouncements based on these conclusions. Despite their triumphalist air, however, the foundations of these conclusions are far from secure. They have earlier pointed out that it is not possible to know in advance which orthodox drug will produce least side-effects in an individual patient, and that it is necessary to monitor drugs after licensing to check whether they are actually more harmful than beneficial. They have, therefore, recognised that an individual can fail to be benefited by treatments tested by RCT, and may even be harmed by such treatments, but they do not suggest that this might be the factor "most responsible for our increasing enthusiasm for alternative medicine" (p. 250). Instead they attack nine groups of people for "giving alternative medicine undue credibility" (p. 250), and claim that these people "can correct the prevailing overly optimistic, uncritical and misguided view of alternative medicine" (p. 250) if they would only do as they are told – by Ernst and Singh.

In all justice we feel that the hypocrisy of these attacks should be pointed out at the same time as discussing some of the fundamental issues raised by the authors in this chapter. These issues include effectiveness, the placebo effect, evidence, health spending, regulation and, amazingly, a *fourth* definition of alternative medicine.

Celebrities

In referring to supporters of alternative medicine Ernst and Singh mention eleven individual names and collectively eleven American presidents and seven popes. They then insist that

> All these uninformed or ill-informed celebrities would do the public a service if they stopped endorsing useless therapies. Better still, celebrities should arm themselves with the best available evidence and condemn faddish, flawed and dangerous treatments. (p. 251)

Ernst and Singh are assuming that these celebrities are ignorant of "the best available evidence" and of the scientific context of medicine, yet they acknowledge that, for example, sporting celebrities "… take special care of their health and have excellent advisors" (p. 251), and they go on to say that

> The truth is that wealthy sportsmen and their coaches can afford to waste money on extravagant placebos, while also spending large sums on the very best that conventional medicine has to offer. (p. 251)

Howver, they are ignoring the fact that high-level competitive sport demands results which have to be both rapid and lasting. Dr Jean Marcel Ferret, doctor to the French soccer team from 1993 to 2004 (the period which included their World Cup win) is presumably an "excellent advisor" armed "with the best available evidence", and he has remarked that

> As a sports doctor I quickly discovered that, except for anti-inflammatories and muscle relaxants, I was very limited in the care of athletes. I began to use homeopathy, first occasionally, and then more and more. At first the athletes were surprised and even wary. So I explained how and why homeopathy acts. The greatest value in sports? Its speed of action. I can use it directly on the soccer field, within seconds of the trauma, and note the results almost immediately.[102]

At the same time Ernst and Singh welcome celebrity support for their own position (for example, on pp. 196 and 251), without ever demonstrating the expertise or knowledge of medicine of these celebrities. In short, they have one standard for supporters and another for opponents of alternative medicine.

Medical researchers

Ernst and Singh next blame medical researchers on the grounds that

> There has been a general tendency for researchers to focus on their own speciality, perhaps developing new antibiotics, vaccines or surgical techniques, while ignoring the fact that alternative practitioners are often undermining their work by scaremongering about conventional medicine and overhyping their own alternative treatments. (p. 252)

While this acknowledgement of orthodox medicine's tendency to a specialist approach sits strangely with their assertion in Chapter 5 that "If anything, conventional medicine takes a more holistic approach than alternative medicine" (p. 223), the more important point is that they imply that criticism of orthodox treatments is unfounded and originates within alternative medicine. As we have seen, bloodletting was criticised by both orthodox doctors and homeopaths, but only the orthodox practitioners were listened to. The withdrawal of an orthodox drug does not occur because of "scaremongering" by alternative practitioners, but because orthodox monitoring of the effects in clinical pratice reveals problems. Even the idea that vaccination produces only beneficial effects has been questioned within both alternative and orthodox medicine. It would appear that medical researchers are much more aware of the need to check the validity of orthodox treatments than Ernst and Singh give them credit for.

The authors' further criticism that

> … too many medical researchers have stood by and silently watched the rise of alternative medicine and the crackpot theories behind them (p. 252),

requires medical researchers to do one of two things: either they must speak out against something they know nothing about, which would compromise their credibility as scientists; or they must remain true to their training and first research alternative approaches as thoroughly as they would orthodox ones. We hope that this critique of *Trick or Treatment?* will show how damaging it is to the credibility of science and "trained scientists" (p. 3) to present as scientific a biased and wholly inadequate examination of the facts. However, Ernst and Singh also offer a case of what they consider exemplary behaviour.

In 2006 a letter (often referred to as "the Baum letter") was sent to the chief executives of NHS Primary Care Trusts by

> a few shining examples of academics who have gone out of their way to highlight the contradictions, exaggerated claims and falsehoods within much of alternative medicine. (p. 252)

One of these "shining examples" was Ernst himself. In this letter they argued that

> the NHS should reserve its funds for treatments that had been shown to work. (p. 252)

The letter was followed up by another in 2007, about which complaints were made, leading to the Department of Health issuing the following statement:

> A document entitled "Homoeopathic Services" which was distributed to Directors of Commissioning earlier this year has caused some confusion because it carried the NHS logo. We would like to clarify that this document was not issued with the knowledge or approval of the Department of Health and that the use of the National Health Service logo was inappropriate in this instance.
>
> The document does not represent any central policy on the commissioning of homoeopathy and PCTs continue to be responsible for making the decisions on what services or treatments to commission to meet their community's health needs.[103]

In addition, these "shining examples" fail to mention that the *British Medical Journal*'s Clinical Evidence team (which advertises itself as "the international source of the best available evidence on the effects of common clinical interventions") reports about orthodox medicine that

> Of around 2500 [commonly used] treatments covered 13% are rated as beneficial, 23% likely to be beneficial, 8% as trade off between benefits and harms, 6% unlikely to be beneficial, 4% likely to be ineffective or harmful, and 46%, the largest proportion, as unknown effectiveness.[104]

On the basis of this information, Ernst and his co-signatories were calling for at least 64% of common orthodox treatments to be withdrawn, or even 87% if one were to accept only those proven to be beneficial. Unsurprisingly, the Baum letter, written by officious academics to NHS administrators and by-passing public consultation, does not seem to have resulted in wholesale reductions in orthodox medical treatments, but only in reduced availability of alternative medicine. In other words, Ernst and Singh are supporting the widespread exercising of double standards in medicine. It should be added that, in at least some cases, patients had alternative medical treatments withdrawn which had been the only treatments to have helped them.[105]

Universities

In their attack on universities, Ernst and Singh allege that there were forty-five BSc degree courses in Britain, and that

> Five of the BSc degrees specialize in homeopathy – this means that students spend three years studying a subject that we have demolished in this book in a single chapter. (p. 255)

The problem is that the authors have not even explained homeopathy adequately in this book (as we have shown), let alone demolished it. For example, nowhere in that chapter did they mention the concept of miasms, an important part of Hahnemann's theory of homeopathy, even though one of the authors is alleged to be a trained homeopath as well as a "trained scientist" (p. 3). Now, however, they claim that

> The completely crass nature of alternative-medicine degrees is easily demonstrated by a question posed in 2005 to students taking the 'Homeopathic Materia Medica 2A' examination at the University of Westminster, London: 'Psorinum and Sulphur are Psoric remedies. Discuss the way in which the symptoms of these remedies reflect their miasmatic nature.' This question is a throwback to the Dark Ages of medicine, when it was believed that disease was caused by *miasmas*, which were poisonous vapours – the idea became obsolete in the late nineteenth century when scientists developed the more accurate and useful germ theory of disease. (p. 255)

As we pointed out in our comments on Chapter 3, Hahnemann was the first person to integrate the germ theory of disease into a general medical theory (by 1831 at the latest). However, where orthodox medicine, "in the late nineteenth century", turned to an old word for seed as the name for these micro-organisms ('germs'), he turned to the term used at the time for bad air which was believed to make one sick ('miasma'), thus:

> the cholera-miasm finds a favourable element for its multiplication, and grows into an enormously increased brood of those excessively minute, invisible, living creatures, so inimical to human life, of which the contagious matter of the cholera most probably consists.[106]

In other words, this examination question is actually about the germ theory of disease and its relationship to the nature and treatment of chronic diseases.[107] Ernst and Singh's criticism of the question shows either a serious ignorance of their subject, or a wilful attempt to mislead readers. Either way, it is incompatible with thir claims to "an unparalleled level of rigour, authority and independence" (p. 3).

Alternative Gurus

The term 'gurus' in this context is clearly intended to exploit its perjorative overtones in English. In fact, the issue Ernst and Singh are discussing is the ability to win high levels of support from large numbers of people, which they maintain is achieved through "undeniable charisma, coupled with corporate professionalism" (p. 256). However, this does not explain how so-called "alternative gurus" can base their success on promoting alternative medicine, unless there already exists a profound popular discontent with orthodox medicine. Ernst and Singh themselves acknowledge this discontent later, noting that

> Surveys from across the world show that users of alternative medicine are motivated at least in part by their disappointment with conventional medicine. (p. 270)

Ernst and Singh, however, exonerate "conventional medicine" itself, claiming that "progress has been immense and continuous" (p. 288), that "we live longer and enjoy a better quality of life" (p. 288), and that "All of this is thanks to applying rational scientific thought to healthcare and medicine" (p. 288). Instead, as we shall discuss later, they blame doctors for the discrepancy between this alleged success of orthodox medicine and the public's "disappointment".

As we have shown, Ernst and Singh have actually provided the basis of a different explanation for the discrepancy between the view they express here and that of the public. They have noted that there is "still no guarantee that a treatment that had succeeded during a set of trials would cure a particular patient" (p. 23), and that "even when a drug is made available for prescription, doctors are encouraged to continue to monitor and report any adverse incidents" so that "we can, if risks emerge, withdraw a drug" (p. 178). They have, therefore, recognised that an individual can fail to be benefited by treatments tested by RCT, and may even be harmed by such treatments, problems which mean that doctors may have to experiment to find the best drug regime

for an individual patient. On the other hand Ernst and Singh comment about one of their "alternative gurus", Dr Andrew Weil, that

> He even suggests to patients that they should experiment with a range of alternative therapies and find out what works for them (p. 257),

and they go on to refer to this derisively as a "suck-it-and-see philosophy" (p. 257). Whatever one might think of the individuals discussed in this section, it would appear that the authors are employing double-standards in their approach to the discussion.

The Media

Determining the degree of balance in the media on any topic requires systematic research. In the two sections on the media Ernst and Singh provide four examples, but the research, as presented by them appears to be incorrectly reported, inadequately quoted, or inadequately undertaken. Nonetheless, they show no qualms about extrapolating from these examples to general conclusions and criticisms of the media. The first example is

> … a survey of Canadian print media by the Department of Community Health Sciences at the University of Calgary. Three researchers scanned nine publications for articles that appeared between 1990 and 2005, looking for any that linked CAM (complementary and alternative medicine) to cancer treatment. (p. 260)

Ernst and Singh claim that

> The main results confirmed previous, similar investigations:
>
> > CAM therapies were most often described in a positive fashion, and CAM use was most often (63%) described as a potential cure for cancer. The majority of articles did not present information on the risks, benefits, and costs of CAM use and few provided a recommendation to speak with a health care provider before use.
>
> In short, the print media in Canada (and elsewhere) tend to present an overly positive and simplistic view of alternative medicine. (p. 260)

However, the section they quote from the abstract continues as follows (our emphases):

> CONCLUSIONS: The results correspond with the *commercial interests* of media outlets, as *coverage appears to be focused around entertainment rather than information* provision. The media play a role in *introducing a range of treatment options* to cancer patients that may not be discussed by conventional health care providers; however, the information provided in media articles appears insufficient to assist patients with informed decision-making.[108]

In other words, the information was not presented in the media as a serious foundation for treatment decisions, but in the context of encouraging curiosity and discussion, and this approach was determined by the commercial interests of the journals. Ernst and Singh may not wish to encourage curiosity and wide-ranging discussion on the subject of medicine, but the public have the right to decide for themselves and to expect the print media to supply the stimulus for debate.

At the same time, Ernst and Singh make no mention of research on articles about orthodox treatments for cancer. It is highly probable that those treatments too would be "most often described in a positive fashion", and "described as a potential cure for cancer", but it is also highly probable that such articles would appear on the news and information pages of the journals, rather than being "focussed around entertainment". In other words, the Canadian research can only be fully understood in the context of an assessment of the relative weight of medical coverage, and to extrpolate the results of a one-sided study in a single country to the print media globally is misleading.

The second example (which is not referenced, unlike the Canadian one), is "a 1999 survey of British newspapers by Professor Edzard Ernst", in which he "sampled four broadsheet newspapers on eight separate days" (p. 265). Regardless of what results he may have obtained, without an explanation as to how these days were selected, there is no guarantee that the selection was not biased, especially as he claims to have found an average of eleven stories per day over the four newspapers. As in the case of the Canadian survey, Ernst and Singh also fail to include information about the context of the stories. Even so, they are confident enough to extrapolate this poor quality evidence into the statement that "it seems that alternative medicine is amost beyond criticism" (p. 265).

The third example concerns criticism of "scare stories" in the news about mercury fillings in teeth. Ernst and Singh note that

> In fact, a major study in 2006 confirmed numerous previous investigations showing that fears over mercury fillings were groundless. Researchers monitored the health of 1,000 children who had received either mercury fillings or mercury-free fillings. Over the course of several years there was no significant difference between the two groups in terms of their kidney function, memory, coordination, IQ and other qualities. (p. 265)

It is unfortunate that there is no reference for this research, as we have not been able to find any evidence of it. On the other hand we have found that in 2006 there were two separate studies published, one conducted over 5 years in New England, USA, and the other over 7 years in Lisbon,

Portugal, involving respectively 534 children of ages 6 to 10 years old and 507 children of ages 8 to 10 years old.[109,110] If these studies are combined, they closely resemble the authors' single "major study" in terms of numbers and results, though both reported a significantly higher mean urinary mercury level in children with mercury fillings, which Ernst and Singh do not mention.

In the fourth example they claim to show "how easy it is to scare the public" (p. 267) by referring to the publishing of a spoof story about "DiHydrogen MonOxide" (H_2O). Its author, Karl Kruszelnicki comments that

> 'You can give people this totally accurate (but emotionally laden, and sensationalist) information about water. When you then survey these people, about three quarters of them will willingly sign a petition to ban it.'(p. 267)

But Ernst and Singh do not mention whether this research has been replicated and confirmed, although they point out that "independent replication is a vital part of how science progresses." (p. 125). Nor do they provide any information about what medium was used to publish the article, what size of population was involved, how they were selected, what control was used, how the responses were surveyed, nor, crucially, what relationship the population sample has to the population using alternative medicine. In other words, it satisfies none of their own requirements for a valid trial, and so provides no evidence at all for their claim of "how easy it is to scaremonger" (p. 267).

If the lack of academic rigour in the authors' use of research is disturbing, their treatment of other issues is also questionable. For example, they offer a defence of the MMR vaccine based at least partly on exploiting the emotions. Thus they remark that

> Maurice Hilleman, for example, was born into a poor Montana family in 1919, living on a single meal a day and sleeping in a bunk ridden [sic] with bedbugs. He witnessed how childhood diseases had decimated his community, which later inspired him to develop eight of the fourteen vaccines routinely given to children, including MMR. (p. 266)

They then go on to provide categorical (and unreferenced) figures for the incidence of complications and mortality rates for measles, ignoring the fact that these vary according to social conditions, such poverty, poor housing, poor sanitation and poor nutrition:

> While measles is an inconvenience for most families, it will cause ear infections for 1 in 20 children, respiratory problems for 1 in 25, convulsions for 1 in 200, meningitis or encephalitis for 1 in 1,000, and death for 1 in 5,000 children. (p. 266)

They also offer no other context for these figures, such as an explanation of the connection between pre-existing problems in the affected children and the complications of ear infections, respiratory problems, convulsions, meningitis or encephalitis. Nor do they provide any evidence of research using a control group to assess the long-term effects of vaccination on health, even though, as they showed in the example of smoking, the effects of some interventions may not noticed without such a study.

Lastly, there is their example of the television show *The Wright Stuff* and "some of the products mentioned on the show by Jayney Goddard" (p. 261). They state that

> This is a conflict of interest, inasmuch as Goddard admits that she helped formulate a brand of supplements that were promoted on the programme and which are sold on the CMA's website. Such conflicts of interest turn out to be the rule and not the exception. (p. 265)

The correct procedure in cases of conflict of interest is to tell people about it so that they can base their decisions on the full facts, which is what Ernst and Singh say Goddard has done. On the other hand, by presenting this case in isolation, Ernst and Singh have not provided the full facts themselves. For example, they have failed to point out the much greater concerns about the pharmaceutical industry's potential for conflict of interest, as the *New York Times* pointed out in 2007:

> The drug companies ply doctors with a wide range of gifts, everything from free lunches for busy doctors and their staffs while sales representatives extol the virtues of their latest drugs to subsidized trips to vacation spots for conferences billed as educational events. The companies also pay large sums to doctors for consulting or for conducting research. These payments, which can mount into the hundreds of thousands of dollars over a period of years, look suspiciously like inducements to promote or prescribe the companies' drugs.[111]

As a whole, the criticisms Ernst and Singh level at the media in this section are heavily compromised by their own failure to abide by the standards they have demanded of others. Given that they claim to be "both trained scientists" (p. 3) and that they claim that their book shows "an unparalleled level of rigour, authority and independence" (p. 3), this contempt for the use of consistent standards raises serious questions about their competence and integrity. This is the more surprising as they show that they are aware of the hazards of such behaviour, offering

> A word of warning, however, because those who dare to question the value of alternative medicine can easily become the target of attacks on their reputation and integrity. (p. 254)

Doctors and the Placebo Effect

Rather than explain the global "disappointment with conventional medicine" (p. 270) as a consequence of the problems with orthodox medical treatments which they have identified in this book, Ernst and Singh claim that it is the fault of doctors. Thus,

> Doctors may well do a good job at getting the diagnosis and treatment right, but many patients feel that other, equally crucial, qualities of 'good doctoring' are missing. They feel that their doctor has too little time, sympathy and empathy for them, whereas survey data confirm that patients consulting an alternative practitioner are particularly keen on the time and understanding they often receive. In a way, it seems as though some doctors delegate empathy to alternative practitioners. (p. 270)

On this basis they argue that

> The message for mainstream medicine is clear: doctors need to spend more time with patients in order to develop better doctor-patient relationships (p. 270),

implying that the success of orthodox medicine is more dependent on the therapeutic relationship than on the treatments themselves. Certainly this is a point they make about alternative medicine:

> We believe that there is an important message here: alternative medicine is not so much about the treatments we discuss in this book, but about the therapeutic relationship. Many alternative practitioners develop an excellent relationship with their patients, which helps to maximise the placebo effect of an otherwise useless treatment. (p. 270)

However, Ernst and Singh refer to a trial of acupuncture published in 2007, involving advice and exercise, advice and exercise plus true acupuncture, and advice and exercise plus non-penetrating acupuncture. They do this in order to further support their argument that this therapy has no real benefit, but they overlook the serious implications this trial has for their arguments about the placebo effect. In the 'Results' section of the report, the trial authors state (our emphasis) that

> Participants receiving either acupuncture intervention were *significantly more confident* that treatment could help their knee problem than those receiving advice and exercise alone.[112]

In the 'Conclusions' section the trial authors state (our emphases) that

> The small additional benefits from acupuncture were *unlikely to be clinically significant*, were *limited to pain intensity and unpleasantness*, were *mostly short lived*, and could not be attributed to specific acupuncture needling effects. Further research is needed to investigate the possible mechanisms of acupuncture, particularly the role of expectancy effects.[113]

This means that, despite the patients' raised expectations, the placebo effects were minimal, which is wholly inconsistent with the argument on which Ernst and Singh have been relying. They have asserted that

> … acupuncture has many of the attributes that would make it an ideal placebo treatment: needles, mild pain, the slightly invasive nature, exoticism, a basis in ancient wisdom and fantastic press-coverage. (p. 67)

At the same time they have asserted that the placebo effect "… is potentially very powerful, providing everything from pain relief to boosting a patient's immune system" (p. 235), and that it is "… the phenomenon whereby a patient responds positively to a treatment simply because of a sincere belief that the treatment is effective" (p. 234). On this basis patients who "were significantly more confident that treatment could help", should have shown a significant placebo effect, and yet they did not.

Problems with the Placebo Effect

As we have shown, in the course of this book Ernst and Singh present an extraordinary number of factors which they claim can boost the placebo effect, some of them contradictory. In this chapter they also argue (our emphasis) that

> The term 'ineffective', however, does not mean that such remedies are of no benefit to patients, because there is always the placebo effect, which we know can offer *varying levels of relief*. (p. 244)

It could be suggested, therefore, that the trial discussed above represents one end of this scale of variation, the other end of which includes "real physiological changes in the body" (p. 60), though Ernst and Singh maintain that "of course, patients with life-threatening conditions cannot rely on the placebo effect to rescue them …" (p. 244). The problem is that there is no way of knowing what combination of factors will produce what degree of beneficial effect.

As we reach the end of their book, the concept of the placebo, as described by Ernst and Singh, is now so vague as to be applicable to almost any degree of benefit in almost any circumstances:

1. The causes of the placebo effect are allegedly numerous and contradictory and there is no explanation of how these causes may interact with each other.

2. The results of the placebo effect can allegedly range from a minimal increase in the perception of benefit to enormous increases in the perception of benefit, without any clear explanation for this variation.
3. The results of the placebo effect can allegedly also range from solely perceptual benefits to actual physiological changes without there being any explanation for the extent or causes of such very different outcomes.
4. There has been offered no means of assessing, even *after* the placebo effect has occurred, what specific causes have generated the effect and in what degree.
5. The mechanism of action of the placebo effect is entirely unknown.

In short the placebo effect is a wholly unscientific and unsafe explanation of outcomes.

In the context of this unpredictability, Ernst and Singh argue that

> … the best way to exploit the placebo effect is to lie excessively in order to make the treatment seem extra special (p. 245),

and that this conflicts with the need for "honesty between doctor and patient" (p. 245). Instead they suggest the bizarre idea of using the "impure placebo":

> By contrast, magnesium in the treatment of anxiety is a good example of an impure placebo. This is because magnesium cannot really treat straightforward anxiety, but it can successfully treat some very rare conditions that have symptoms similar to anxiety. Hence, a doctor who treats a patient complaining of anxiety with magnesium might conceivably be giving the perfect remedy, because the patient might have one of these rare conditions. In reality, however, it is much more likely that the magnesium will only alleviate the patient's anxiety through the placebo effect. (p. 269)

In a sense, this is no different from prescribing any drug, since Ernst and Singh have informed us that the effect of a drug on an individual is unpredictable. However, this approach involves a "dangerous lottery" (p. 24), since it means that doctors would prescribe "active ingredients" which they have no reason to believe would enable "the best possible chance of recovery" (p. 23), but which would still have all the risks of side-effects. At the same time, since the primary reason for the prescription is not the remote possibility that the drug might be effective but a hypothetical possibility that the patient will benefit from the placebo effect, doctors would need to encourage in the patient at least "a sincere belief that the treatment is effective", even though they themselves do not believe this to be the case. Of course, according to Ernst and Singh,

> … the best way to exploit the placebo effect is to lie excessively in order to make the treatment seem extra special. (p. 245)

In other words the success of the impure placebo would be dependent on the same approach as is needed for the pure placebo.

The distinction Ernst and Singh draw between these two approaches is not only a fascinating piece of sophistry, but is very revealing about their attitudes:

> This form of impure placebo is much more acceptable than pure placebo, because we are avoiding complete lies. On the other hand, we are still dealing in half-truths, as opposed to complete truths. (p. 269)

Throughout this book we have revealed Ernst and Singh's extensive use of "half-truths, as opposed to complete truths", and their use of the placebo effect is an example of this approach. Whatever validity the concept may have, as presented in this book it cannot explain alternative medicine, but all too often it has been used with the intention of producing a real, but erroneous, effect on the judgement of readers. In science, as in law, there is a reason for requiring people 'to tell the truth, the whole truth, and nothing but the truth'. It is clear that Ernst and Singh are aware of this, but willing to exploit the advantages of ignoring it.

Doctors and Alternative Medicine

In addition to blaming doctors for patients' "disappointment with conventional medicine" (p. 270) on the grounds of their failure to exploit the placebo effect, Ernst and Singh also accuse them of promoting alternative medicine. Thus they claim that

> So far we have two categories of problematic physician. First, there is the ignorant doctor who advises alternative medicine, but who is unaware that it does not really work. Second, there is the lazy doctor, who advises alternative medicine in order to satisfy patients with otherwise untreatable conditions. Both types actively steer some patients towards alternative medicine, but there is a third category – the inconsiderate doctor – who inadvertently frustrates patients so that they seek out alternative therapies. (p. 269)

In the UK the number of referrals to alternative therapists in the NHS is small, and the authors point out that "shining lights", including Ernst himself, have caused a significant reduction in these, so Ernst and Singh must be claiming that the UK's contribution to alternative medicine being "the fastest-growing area of medical spending" (p. 2) arises from a rapidly growing problem of incompetent (or at least "inconsiderate") doctors!

This attack on doctors is, as usual, based on woefully inadequate foundations, such as the claim that

> The numbers vary from country to country, but a reasonable ballpark figure is that roughly half of GPs refer patients to alternative therapists, and many more will respond positively to the idea of patients trying remedies from the alternative-health section of the local pharmacy or health-food store. (p. 268)

There is no reference for this statement, nor any information to support it, such as the number or type of countries being referred to; what is meant by a GP; what circumstances affect a GP's decision; or what degree of variation there is between different countries. Indeed it is difficult to understand how this claim should be considered reasonable by Ernst and Singh, whom we are assured are "both trained scientists" (p. 3). Furthermore, when their other remarks are taken into account, they are stating explicitly that half of GPs in these countries are either ignorant or lazy, and the rest include a number of "inconsiderate" doctors as well as

> a much rarer, but more serious problem. There are a few doctors who are genuinely convinced of the power of alternative medicine, despite all the lack of evidence. (p. 270)

One cannot help but ask the question: how do Ernst and Singh explain their alleged "immense and continuous" progress of orthodox medicine towards "a better quality of life", if they consider the majority of its front-line practitioners to be incapable of "applying rational scientific thought to healthcare and medicine" as a result of ignorance, laziness or conviction?

Alternative Medicine Societies

Ernst and Singh are very keen to urge organisations representing alternative therapists to be

> … a huge force for good, helping to establish high standards, promoting good practice and ensuring ethical principles. (p. 271)

However, they make a point of attacking an organisation which has aimed at meeting precisely these requirements:

> Worse still, when the Society of Homeopaths, based in Britain, was criticized for not taking a firm stand against inappropriate use of homeopathy, it decided to suppress criticism rather than to address the central issue. Andy Lewis, who runs a sceptical and satirical website (www.quackometer.net), had written about the Society and the issue of homeopathic malaria treatments, which resulted in the Society asking the company that hosts his website to remove the offending page. In our opinion, the Society needs to improve in three ways. First, it ought to police its practitioners more thoroughly. Second, it ought to act publicly and promptly when serious complaints are made. Third, it should listen to its critics rather than silence them.

Three of the trustees of H:MC21 are also members of the Society of Homeopaths, and we wish to repudiate Ernst and Singh's remarks using quotations from a letter sent by the Society to *The Guardian* newspaper about this matter.[114] We are proud of the fact that

> During the last 30 years, The Society has been working to create a robust system of voluntary self-regulation for homeopathy, establishing professional-level training standards (both private colleges and university degree courses) and a culture of research and critical reflection on practice. In that time, we have heavily invested our members' money and their time to create a responsible homeopathic profession. We always recommend that members of the public seek a qualified, registered and insured practitioner and we take our own responsibilities in this field very seriously.

We are also proud of the fact that the Society has been cited as a model of best practice in voluntary self-regulation by the House of Lords Select Committee on Science & Technology's Inquiry into complementary and alternative medicine.

> Of the three points raised by Ernst and Singh, all three are irrelevant. Firstly, the Society
>
> … contacted the programme makers directly to ask for their evidence that any Society members had given dangerous or misleading advice to members of the public. They were unable to provide a single example. The Society's professional conduct procedures cannot be invoked without a specific complaint, an alleged offender or any evidence. In these circumstances, The Society was unable to investigate a specific case.

In other words, no complaint was made against any member of the Society, and so there was nothing for the Society to "police". Secondly,

> … as a further precaution, [the Society] reissued [its] Guidelines on advice for the prevention of malaria and sent a copy to every member within a day of the programme being aired.

This constitutes acting "promptly when serious complaints are made", and there was no need to act more publicly as the matter only involved reminding members of their responsibilities. Thirdly,

> The Society instructed lawyers to write to the Internet Service Provider of Dr. Lewis' website because the content of his site was not merely critical but defamatory of The Society, with the effect that its reputation could have been lowered. Dr Lewis, in his article, stated as fact highly offensive comments about The Society and it is for that reason that The Society decided it had no option but to take action. The very crude abuse posted on various websites and e-mailed to The Society since our action suggests that these bloggers/authors are not people who are interested in a real debate on the basis of either science or the public good but who simply want to attack homeopathy, for the very sake of it.

It would appear that Ernst and Singh are either unable to distinguish between criticism and defamatory remarks, or else believe that organisations have no right to take action against defamatory remarks made in public.

Governments and Regulators

Ernst and Singh's attack on the regulation of alternative medicine shows a similar lack of thought. They state that

> The UK Medicines Act of 1968, for instance, was a direct consequence of the Thalidomide disaster.
>
> Alternative medicine, however, seems to have sidestepped these regulations. Buzzwords such as 'natural' and 'traditional' have allowed them to carry on largely unhindered in a parallel universe that is oblivious to safety issues. For example, in most countries, herbal remedies and other supplements can be marketed without rigorous proof of safety. The burden of proof is reversed: it is not the manufacturer who has to demonstrate that his product is harmless, but it is the regulator who has to prove that the product is harmful – only then can it be withdrawn from the market. This obviously is haphazard, as there are far too many products, so regulators react only when problems emerge. This is much like drug regulation before Thalidomide: a disaster (or several) waiting to happen. (p. 274)

Far from being haphazard the 1968 Act[115] was a legal document and needed to be precise and practical. One of the issues it addresses, ignored by Ernst and Singh, is the problem of drawing a distinction between foods and medicines. Thus in Part II, Subsection 12 it outlines the nature of exemptions for herbal remedies from the restrictions on the manufacture, sale and supply of medicines (Subsections 7 and 8):

> (2) Those restrictions also do not apply to the sale, supply, manufacture or assembly of any herbal remedy where the process to which the plant or plants are subjected in producing the remedy consists only of drying, crushing or comminuting, and the remedy is, or is to be, sold or supplied—
>
> (a) under a designation which only specifies the plant or plants and the process and does not apply any other name to the remedy, and
>
> (b) without any written recommendation (whether by means of a labelled container or package or a leaflet or in any other way) as to the use of the remedy.

From this it can be seen that the only 'manufacturing' allowed in preparing herbs for sale is no more than some foods undergo (such as spices), which means that the material sold is of a similar order to that obtainable from the environment by any individual, and a general prohibition of this sort of preparation would involve the prohibition of foods. As regards the sale of herbal remedies the Act states that the purchaser has to be told what they are buying and how it has been prepared, but not be given information about any medicinal properties. In other words herbal remedies are to be sold under this Act in the same way as foods, not in the way medicines are sold. Indeed, it is difficult to conceive of a way of distinguishing in law between selling chilli powder, foxglove powder or flour, other than by naming specifically either all those substances which cannot be sold or all those which can. As the authors point out, this would be ludicrous since "there are far too many products".

By contrast, Ernst and Singh have stated that drugs are highly processed to produce an "active ingredient", specifically in order "to isolate that component and then deliver it in a more concentrated and potent manner" (p. 195). Drugs can only be obtained by purchasing them from the manufacturer, not freely from the environment. They also have three names – their chemical name, their generic name and their trade name – and the knowledge of their chemical nature and effects is owned by the developer and may not be easily researchable by individuals. Lastly, drugs are manufactured and supplied solely for the purpose of modifying biological function in specific ways, incidentally modifying function in other ways as side-effects. Quite clearly herbal remedies and drugs are very different categories of product, requiring very different rules for their regulation. To argue that the same regulations should be applied to herbs as to drugs is effectively to demand that every food should be proved safe by clinical control trials before it can be sold, which is nonsensical.

The suggestion that the situation as regards the regulation of herbal remedies has any resemblance to "drug regulation before Thalidomide: a disaster (or several) waiting to happen" is an example of exactly the "scaremongering" (p. 252) about which Ernst and Singh complain. Firstly, they themselves have identified that the aim of drug manufacturing is to isolate the active ingredient "and then deliver it in a more concentrated and potent manner" (p. 195), which was the case with the drug Thalidomide. As such, they appear to be arguing that drugs have greater power to affect people than herbs while at the same time arguing that herbs have the same power as drugs. Secondly, the Thalidomide disaster occurred because pregnant women's healthy scepticism about medication was allayed by the 'scientific' credentials of the drug. Thirdly, as a result of the Thalidomide disaster practitioners and pregnant women are very much more concerned about the possible health risks of medication. Neither prescribers nor pregnant women are likely any longer to assume that a medication is safe during pregnancy, even if it has 'scientific' credentials.

Regulation of Practitioners

As regards the regulation of practitioners of alternative medicine, Ernst and Singh confuse two separate issues. Firstly, the lack of regulation may mean that

> … literally anyone reading this text in Britain could call themselves a homeopath, a naturopath, a herbalist, an aromatherapist, an acupuncturist, a reflexologist or an iridologist. You might have no training in conventional or alternative medicine, yet nobody could stop you nailing a sign to your front door and placing an advert in your local newspaper. It goes without saying that this situation is less than satisfactory. (p. 274)

It does not follow, however, that a person will turn to an alternative therapy without being concerned about the competence of the therapist. The existence of "about one hundred" societies in the UK representing at least the thirty-six therapies in this book is a measure of the recognition by these therapists that they need to establish some form of self-regulation. In this context, it is surprising that the authors have not mentioned the strenuous efforts made by the Society of Homeopaths to establish standards of training, codes of conduct and ethics and a single regulatory body for homeopathy. It is also worth noting that the first national medical association in the USA was the American Institue of Homeopathy,[116] and that the American Medical Association was only established afterwards in virulent opposition to homeopathy.[117] This was the same AMA which later

> … took active steps, often covert, to undermine chiropractic educational institutions, conceal evidence of the usefulness of chiropractic care, undercut insurance programs for patients of chiropractors, subvert government inquiries into the efficacy of chiropractic, engage in a massive disinformation campaign to discredit and destabilize the chiropractic profession and engage in numerous other activities to maintain a medical physician monopoly over healthcare in this country. (p. 165)

Such behaviour does not sit well with Ernst and Singhs requirements for representative organisations being

> … a huge force for good, helping to establish high standards, promoting good practice and ensuring ethical principles. (p. 271)

The second issue is the claim that

> There are, of course, considerable national differences, but in general alternative practitioners do not require any in-depth medical training or experience. (p. 274)

If there are "considerable national differences" such a generalisation is, of course, meaningless. At the same time, it is uncertain as to what is meant by "in depth medical training or experience". No practitioner has experience before they start practising, and Ernst and Singh offer no comparison of the nature of medical training required of different practitioners. Even within homeopathy in the UK, there are differences in training, but colleges accredited by Society of Homeopaths, for example, must include anatomy, physiology and pathology in the course. There is also no reasonable excuse for preventing orthodox and alternative practitioners from sharing tuition in these subjects at medical schools, even though they may diverge in their approaches to treatment, as occurs in India. On the other hand, it is not true that such training would eliminate the risk that

> Serious diagnoses can be missed, conditions that never existed can be diagnosed, ineffective or harmful treatments can be applied, wrong or dangerous advice can be issued, and patients can be ripped off – and all this without adequate control or recourse. (p. 274)

After all, these mistakes occur among orthodox practitioners, even to the extent that

> Studies of autopsies have shown that doctors seriously misdiagnose fatal illnesses about 20 percent of the time. So millions of patients are being treated for the wrong disease.[118]

It is the task of regulators to make sure that training is of a high enough standard and to act when mistakes are made, but training can only reduce the incidence of mistakes, not eliminate them. Furthermore, Ernst and Singh do not provide any evidence of the extent of this alleged problem in alternative or orthodox medicine, so it is simply conjecture.

World Health Organization

In discussing the last of their targets, Ernst and Singh argue that

> According to a report in the *Lancet*, the WHO is planning to publish a report on homeopathy, which will have much in common with its irresponsible report on acupuncture. In other words, it will be rose-tinted and lacking in rigour.
>
> Once again, practitioners will use the report to help validate invalid treatments. And, once again, patients will be persuaded that it is worth spending their money and risking their health on bogus treatments. (p. 278)

They justify this statement of opinion, by claiming that

> … those who have seen a preliminary version of the report state that the WHO views homeopathy as a valid form of treatment for diarrhoea. Globally, over a million children die each year of diarrhoeal diseases, and an increased use of homeopathy would only make the situation worse. India's National Rural Health Mission is already showing signs of advocating homeopathy to treat diarrhoea, and the WHO report would only give credibility to this foolhardy policy. (p. 278)

But Ernst and Singh appear to make the same mistake as they made over the UK Medicines Act 1968, in that legislators and the World Health Organization both have to relate to what actually happens in the world when planning and making decisions, rather than to some idealised model. Futhermore, in addition to their earlier failure to look at decades of remarkably consistent figures for homeopathic success in treating cholera[119], Ernst and Singh have also omitted important information about India.

The assumptions underpinning their criticism are firstly, that India's National Rural Health Mission has no experience of the facts about treating children with diarrhoea, secondly, that it has

no evidence of the effectiveness of homeopathy, and thirdly that the WHO has reached a wholly abstract view about homeopathic treatment of diarrhoea, which it will then foist on the world. However, it takes very little thought to realise that India's National Rural Health Mission will have substantial knowledge of treating children with diarrhoea. India also uses homeopathy extensively, integrated with orthodox medical training, so it probably has a greater pool of clinical experience of the combination of orthodox medicine and homeopathy than any other country in the world. Furthermore, Ernst and Singh have shown that the WHO draw heavily on the experience of member states (such as on Chinese research in the case of acupuncture), which suggests that the WHO would base its report (at least in part) on clinical evidence from India, in addition to evidence from Brazil and Mexico, where homeopathy is also widely used. It is, then, more likely that evidence from India will be used to give credibility to the WHO's report, than that Indian doctors will rely on the WHO to give credibility to their medical policy.

Given their concentration on evidence from Europe and North America throughout this book, their criticism of evidence obtained from China (in chapters 2 and 5), and their devaluing of medical practice in India (in Chapter 3 and at various points in this chapter), the omission at this point of such important facts about India has unpleasant overtones. In addition, both Ernst and Singh (in this book) and Ernst and his other "shining examples" (p. 252) elsewhere have exhibited a willingness to use "partial truths" and misrepresentation to gain their ends. Given the choice, most people would trust the World Health Organization to reflect genuine global experience, rather than trust the parochial interests of a few academics and journalists.

The Cost of Health Care – Small Scale

Again and again Ernst and Singh have presented arguments in this book which seem perfectly valid because they have been divorced from their context. However, as soon as these arguments are placed back into context, a context almost always provided elsewhere by Ernst and Singh themselves, the arguments are seen to be scientifically invalid or unjustified. Occasionally, it is necessary to provide a proper context by introducing information from outside this book, and the issue of medical spending is an example. On both the small scale and the large scale Ernst and Singh make statements not borne out by the facts, and this omission enables them to create a highly distorted view of the financial power of alternative medicine.

To begin with they claim that

> … not only do alternative therapists offer us often ineffective and sometimes dangerous treatments, they also charge us heavily for these services and products. (p. 240)

They amplify this by claiming that

> Alternative therapists happily devote half an hour to each patient, because they are generally charging a great deal of money for their time. (p. 270)

They then quantify these charges as follows:

> Acupuncture sessions, chiropractic manipulations and homeopathic consultations can all cost upwards of £50 each and are often more than double this price. (p. 240)

No evidence is supplied to support these claims, but if we assume them to be correct for the time being, then an alternative therapist actually working an average of 40 hours per week, including time spent studying the cases (say an average of 10 minutes per consultation), keeping client and financial records (say 3 hours per week), taking holidays (say 4 weeks per year, plus bank holidays), etc, will theoretically earn from £126,900 per year, out of which they have to pay expenses, including the costs of premises, utilities, insurance, stationary, telephone, etc.

By comparison, the NHS reports that in 2006/07 the average income before tax in the UK for the 5,069 salaried GPs was between £49,779 and £52,328 plus an average of £6,190 for expenses (total: £55,969 to £58,518), whereas for the 33,887 contractor GPs the average income was between £99,580 and £135,546, plus an average of £139,694 for expenses (totals: £239,274 to £275, 240). [120] This difference between the levels of GP salaries and expenses depend partly on the type of contract, but also on the fact that

> Contractor GPs have additional responsibilities, covering clinical, organisational, operational, financial and personal responsibility for provision of GP services not borne by salaried GPs. On average, contractor GPs work more hours than salaried GPs. These factors are reflected in their average earnings.[121]

As has been pointed out, alternative therapists also have responsibilities and expenses, so Ernst and Singh's figures would place them on a par with GPs. However, this is only the theoretical income, and in fact, alternative therapists probably get nothing like this. For example, according to Prospects (which claims to be "the UK's official graduate careers website")

> [Homeopaths'] salaries vary widely across the UK, starting from around £5,000 per year (pro rata) and rising to £30,000 (salary data collected Jan 07 from the Society of Homeopaths).

> Income depends to some exent on local market conditions. Fees charged range from £30 - £100 an hour. Fees in London and the South East of England are likely to be higher than those in most other parts of the UK.[122]

The Cost of Health Care – Large Scale

If Ernst and Singh are misleading about the financial costs to patients of alternative medicine, they are no less misleading about spending on the larger scale. For example, without providing any reference for their figures, they claim that

> Surveys of the money spent on alternative medicine can give conflicting results, but the general trend has been inexorably upwards, and a recent extrapolation estimated that Britons currently spend £5 billion on alternative treatments – £4.5 billion by the public and the remaining £500 million by the National Health Service. (p. 240)

They then go on to use these figures to argue that alternative medicine is threatening the finances of the NHS:

> In terms of UK government spending, the alternative lobby might defend the £500 million bill by pointing out that it represents less than 1 per cent of the National Health Service budget, but £500 million spent on unproven or disproven therapies could instead pay for 20,000 more nurses. (p. 241)

They also argue that this growth in spending has given alternative medicine the financial power to affect governments:

> For some reason they seem frightened of confronting the multi-billion-dollar alternative medicine industry. Or perhaps they are more worried about millions of voters who currently use alternative medicine and who might be offended if their favourite herbalist or homeopath were forced to shut up shop. (p. 275)

At the heart of this argument there are three paradoxes.

The first paradox is that "the general trend [of the money spent on alternative medicine] has been inexorably upwards" when orthodox medical care allegedly works and is free at the point of delivery, whilst alternative medicine allegedly does not work and its practitioners "charge us heavily". Ernst and Singh attempt to sidestep this by laying the blame on various groups, as discussed above, and claiming that

> … the public is being misled over and over again, often by misguided therapists, sometimes by exploitative charlatans. (p. 288)

The second paradox involves the question of pressure on governments. If governments are "worried about millions of voters who currently use alternative medicine", then this would mean that not only are patients who turn to alternative medicine willing to pay extra money for their health care, but they are also willing to cast their votes in favour of these therapies. According to Ernst and Singh, however, these therapies have no beneficial effects, so it is inexplicable that they can inspire this level of confidence.

The third paradox again involves the question of pressure on governments, since Ernst and Singh claim that governments "seem frightened of confronting the multi-billion-dollar alternative medicine industry". At the beginning of the book they gave us an idea of the scale of the financial power of this industry (though they have not provided any source for their figure), stating that

> … it is estimated that the annual global spend on all alternative medicines is in the region of £40 billion, making it the fastest-growing area of medical spending. (p. 2)

What they did not do was provide any figure for the annual global spend on all medicine. This figure is illuminating. According to figures published by the WHO in 2007, total global expenditure for health annually is greater than $4.1 trillion (£2.8 trillion).[123] One does not need to have written a book about a mathematician to realise that if "the multi-billion-dollar alternative medicine industry" can put pressure on governments, then the multi-*trillion*-dollar orthodox medicine industry can put even more pressure on them.

If we look at orthodox drug companies alone, the WHO states that "Pharmaceuticals account for over 15% of measured global spending on health",[124] and, as we pointed out when discussing the Introduction, global pharmaceutical sales in 2006 are reported as being $643 billion[125] (£435 billion). Furthermore, according to *Fortune 500* the top twelve pharmaceutical companies accounted for nearly $416 billion (£281 billion) of this, with profits of $78.6 billion (£53 billion) between them.[126] Indeed the top seven pharmaceutical companies alone made $62 billion (£42 billion), making their *annual profits* greater than the whole of the annual spending on alternative medicine.

In the end, the important question is not

> So how did we get into a position whereby each year we are spending £40 billion globally on alternative therapies, most of which are as senseless as homeopathy, and many of which are a good deal more dangerous? (p. 250)

but 'why is spending of around $4 trillion on orthodox medicine delivering results which are so unsatisfactory that millions of people are turning to alternative medicine?'

The Truth about Effectiveness

As we have seen in the discussion of the previous four chapters , Ernst and Singh's claim to

> … have shown that the majority of alternative treatments are wholly or largely ineffective in treating the majority of conditions (p. 244)

is unjustified. From a purely mathematical perspective, with more than 240 groups of conditions alone (according to *The Merck Manual of Medical Information*) and 36 therapies (according to this book) they would need to have presented evidence for 2,299 combinations to justify the statement. In fact, they have not even been able to prove the scientific validity of their means of assessing these therapies (the RCT) or of their explanations for any therapeutic effects (the placebo effect), so they cannot claim to have proved anything about any alternative treatment. Nonetheless, if we were to accept that the RCT is an appropriate tool, there is still the following insuperable difficulty in accepting their evidence.

Almost at the beginning of this book Ernst and Singh stated:

> In particular, we will answer the fundamental question: 'Is alternative medicine effective for treating disease?' Although a short and simple question, when unpacked it becomes somewhat complicated and has many answers depending on three key issues. First, which alternative therapy are we talking about? Second, which disease are we applying it to? Third, what is meant by effective? (p. 3)

At no point have they answered the last question, and yet every conclusion they have reached depends on the answer. As we have pointed out in our discussion of the chapter on homeopathy (Chapter 3), if medical researchers are using the wrong definition when testing this therapy, then the results of trials will be at best ambiguous and at worst wholly wrong. That the results are proving ambiguous is something Ernst and Singh have stated repeatedly. Furthermore, if homeopathy's model for assessing effectiveness is actually correct, then it is a general principle, and this means that if it is not applied in other trials of alternative medicine, these trials too will suffer from ambiguity and errors. Again the ambiguity of the trial results is something Ernst and Singh have stated repeatedly.

On the other hand, if orthodox medicine is using the wrong definition, then we would expect its widespread use to result in an increase in health problems rather than a decrease. Ernst and Singh have assured us the latter is the case, but the evidence does not support them. Over 20 years ago Anthony Storr wrote in his introduction to *Philosophy of Medicine*, that

> In both Denmark and Great Britain, the health services have come to cost more and more in real terms. Consultations and hospital admissions have risen; but waiting lists have not been eliminated. …
>
> The idea that one can get reduced 'disease' by expanding the health services has proved illusory, and should perhaps lead to a new way of looking at what constitutes 'disease'.[127]

By 2005 the WHO pointed out that

> Globally, of the 58 million deaths in 2005, approximately 35 million will be as a result of chronic diseases. They are currently the major cause of death among adults in almost all countries and the toll is projected to increase by a further 17% in the next 10 years. At the same time, child overweight and obesity are increasing worldwide, and incidence of type 2 diabetes is growing.
>
> This is a very serious situation, both for public health and for the societies and economies affected.[128]

Futhermore,

> … for most of the post–World War II period, inflation-adjusted health care costs rose at a much faster rate than did GDP. To illustrate, between 1945 and 1998 the growth rate in real per capita national health care spending averaged 4.1 percent, compared with a 1.5 percent increase in GDP. Moreover, for every ten-year period between 1945 and 1998, spending on health care grew at a rate faster than that of income. Although some increase in health spending would be expected solely from the aging of the U.S. population, evidence suggests that historically, changing demographics have accounted for only a small fraction of the gap between the growth of real health care spending and GDP.[129]

In the light of these facts, the question of "what is meant by effective?" is not an academic problem to be ignored, but a very real issue, and the tendency for more and more people to turn to alternative medicine may well be an indicator that there are genuine reasons for their "disappointment with conventional medicine" (p. 270).

The Definition of Alternative Medicine

As Bellavite and Signorini show,[130] genuine scientific investigation of alternative therapies is valuable and illuminates not only alternative medicine, but also orthodox medicine. However, in this book Ernst and Singh have simply launched a wholesale attack on alternative therapies without providing even the minimum scientific foundations for their arguments. At the same time, they certainly recognise that a definition of effectiveness at least is essential, since this concept is central their fourth attempt to define alternative medicine:

> This brings us to an interesting situation: any provably safe and effective alternative medicine is not really an alternative medicine at all, but rather it becomes a conventional medicine. Therefore, alternative medicine, by definition, seems to consist of treatments that are untested, or unproven, or disproven, or unsafe, or placebos, or only marginally beneficial. (p. 287)

To understand this new definition, we need to start by being clear about what Ernst and Singh mean by "provably". Orthodox medicine cannot determine that a treatment is *able to be proved* "safe and effective" according to its own standards, but only that is *has been proved* so. Indeed, the only medical system capable of determining that its medicines are safe and effective before they are used is homeopathy. It would probably be fair, therefore, to assume that the authors meant to use the word "proven".

On this basis, we do indeed have "an interesting situation". Ernst and Singh are saying that "alternative medicine" comprises treatments "untested, or unproven, or disproven, or unsafe, or placebos, or only marginally beneficial" *according to criteria external to those therapies*. At the same time "conventional medicine" comprises treatments which may or may not be "untested, or unproven, or disproven, or unsafe, or placebos, or only marginally beneficial" *according to its own criteria*, since (as we have already pointed out) orthodox medicine includes 64% to 87% of commonly used treatments which have not been proven safe and effective according to the approach accepted by Ernst and Singh, that of evidence-based medicine. The logic of this argument, therefore, is that alternative medicine can reasonably claim that the standards used are not appropriate, but orthodox medicine has to explain its failure to apply its own standards consistently.

In this book Ernst and Singh have ignored the fact that in reality orthodox medicine uses a much greater range of criteria than the RCT alone when assessing treatments, including clinical experience in particular. They have also ignored the need for theory as a means of linking evidence together and giving it a coherent and scientific structure. They have ignored the need to define their terms, including the key one of effectiveness. As a result, they have not been able to consistently define the subject of their examination. Initially they provided a definition based on opinion:

> … alternative medicine is any therapy that is not accepted by the majority of mainstream doctors … (p. 1)

In Chapter 4 they amended this and based the definition on alleged "scientific" opinion:

> … it makes no sense at all from a modern scientific point of view. That is why chiropractic treatment is still considered by many as an alternative medicine ... (p. 147)

In Chapter 5 they amended this again and made acceptance by pharmacology the key criterion:

> The treatments that emerged from this scientific approach to herbal medicine are so utterly mainstream that they are no longer labelled herbal medicines, but rather they are simply incorporated within the realm of modern pharmacology. (p. 197)

Now they are claiming that alternative medicine is anything called 'alternative medicine' and not proven safe and effective, without explaining who decides on the application of this label.

The Level Playing Field

Having shown their enthusiasm for moving the goalposts when defining alternative medicine, Ernst and Singh have the nerve to call for

> … a level playing field, whereby alternative medicine has to maintain the same high standards required of conventional medicine. Regulation across the board would provide protection to all patients seeking any form of medical treatment. (p. 281)

Their model for such regulation is unsurprising:

> In particular, this would mean that each alternative treatment would have to be tested, and only if it were proved that it generated more good than harm would it be permitted. Most patients are unaware of the immense amount of testing undergone by conventional treatments, so it is worth quickly summarizing how pharmaceuticals are assessed and investigated in order to see the sort of scrutiny that we are also proposing for alternative treatments. (p. 282)

In other words, they are suggesting that alternative medicine be regulated using the methods designed to try and protect the public from the dangers of pharmaceuticals, because

> … this level of testing is essential if the public is to be protected from harmful and ineffective drugs. (p. 283)

In fact that the "high standards required of conventional medicine" still lead to serious problems in clinical practice. As we have said already (in discussing the Introduction), the reality of orthodox medicine is that the NHS spends £8.2 billion on drugs[131] tested using RCTs, but is estimated to spend £2 billion on adverse reactions to those prescribed drugs.[132]

When it comes to using these methods to test alternative therapies, there are critical problems which prevent there being anything like "a level playing field". Among these are:

1. The lack of an agreed scientific definition of effectiveness applicable to all therapies;
2. The lack of an agreed scientific definition of illness applicable to all therapies;
3. The lack of a valid means of testing for curative effects as opposed to harmful ones;

4. The lack of a practical definition of what constitutes a medicine – herbs, for example, may not differ from foods, while potentised remedies only become medicinal in a specific context;
5. The lack of equality of financial power, and hence political power, between pharmaceutical companies and alternative therapies;
6. The lack (for most alternative therapies) of any prospect of financing or recouping the costs of trials.

Finally, we have pointed out during the course of this critique that Ernst and Singh have presented only one mechanism for testing medical interventions, and that they themselves have explained why this is not appropriate: they have shown that the RCT can test for harmful effects, but cannot prove that a drug will have a curative effect, and they have shown that even so some harmful effects can go unnoticed until the drug is used in clinical practice. We have also pointed out that their argument has been based not on science, not on clearly defined terms justified by evidence, not on accurate presentation of the facts about therapies, not on an accurate outline of medical history, not on the full facts about economic power in medicine, nor on the truth, the whole truth and nothing but the truth. Instead their argument has been based on opinions, undefined terms, constantly redefined terms, contradictory statements, double-standards, misrepresentation, prejudiced sources, unreferenced information and propagandism.

It is, therefore, rather satisfying to end this work by quoting from the last paragraph but one of the main body of Ernst and Singh's book. As in their opening paragraph, they exhibit the same lack of attention to details that has undermined their argument the whole way through this book. They call for (our emphasis) "*scientific* standards, evaluation and regulation to be applied to all types of medicine", yet they have shown that they actually mean the "standards, evaluation and regulation" used for orthodox pharmaceuticals. They call for these "In the name of honesty, progress and good healthcare", when they have been dishonest, failed to recognise the potential for progress and ignored patients' decisions about what constitutes good healthcare. In the end the consequence of applying their distorted views will be precisely the mistake they present us with in this penultimate paragraph (our emphasis):

> In the name of honesty, progress and good healthcare, we call for scientific standards, evaluation and regulation to be applied to all types of medicine, so that patients can be confident that they are receiving treatments that demonstrably generate ***more harm than good*** [sic] (p. 288).

Bibliography

Journals

Adair, John G., Donald Sharpe and Cam-Loi Huynh, 'Hawthorne Control Procedures in Educational Experiments: A Reconsideration of Their Use and Effectiveness', *Review of Educational Research*, 59 (1989), 215-228 at <http://www.jstor.org/pss/1170415>, accessed 27 November 2008.

Bellinger, David C. et al., 'Neuropsychological and Renal Effects of Dental Amalgam in Children: A Randomized Clinical Trial', *JAMA*, 295 (2006), 1775-1783 at <http://jama.ama-assn.org/cgi/content/full/295/15/1775>, accessed 23 February 2009

Belon, P, J Cumps, M Ennis, P F Mannaioni, M Roberfroid, J Sainte-Laudy, F A C Wiegant, 'Histamine dilutions modulate basophil activation', *Inflamm. Res.*, 53 (2004), 181-188

Berger, Vance W, PhD, 'Is the Jadad Score the Proper Evaluation of Trials?' (letter to the editor), *J. Rheumatol.* (2006) at <http://www.jrheum.com/subscribers/06/08/1710-c.html>, accessed 3 March 2009

Boseley, Sarah, 'Adverse drug reactions cost NHS £2bn', *The Guardian*, 3 April 2008, <http://www.guardian.co.uk/society/2008/apr/03/nhs.drugsandalcohol>, accessed 14 November 2008

DeRouen, Timothy A, et al., 'Neurobehavioral Effects of Dental Amalgam in Children: A Randomized Clinical Trial' *JAMA*, 295 (2006), 1784-1792 at <http://jama.ama-assn.org/cgi/content/short/295/15/1784>, accessed 23 February 2009

Ernst, Edzard, 'Acupuncture Ineffective, Attention Effective?', *Arch. Intern. Med.*,168 (2008), 551 at <http://archinte.ama-assn.org/cgi/content/extract/168/5/551>, accessed 28 November 2008

Fisher, Peter, Brian Berman, Jonathan Davidson, David Reilly, Trevor Thompson and 29 others, Letter to the editor, *Lancet*, 9503 (2005) at <http://www.thelancet.com/journals/lancet/article/PIIS0140-6736(05)67879-8/fulltext>, accessed 3 March 2009

Foster, Nadine E, Elaine Thomas, Panos Barlas, Jonathan C Hill, Julie Young, Elizabeth Mason, Elaine M Hay, 'Acupuncture as an adjunct to exercise based physiotherapy for osteoarthritis of the knee: randomised controlled trial', *BMJ,* 335 (1 September 2007), p. 436 at <http://www.bmj.com/cgi/content/full/335/7617/436?ck=nck>

Haake, Michael, PhD MD; Hans-Helge Müller PhD, Carmen Schade-Brittinger, Heinz D. Basler PhD, Helmut Schäfer PhD, Christoph Maier PhD MD, Heinz G Endres MD, Hans J Trampisch PhD, and Albrecht Molsberger PhD MD, 'German Acupuncture Trials (GERAC) for Chronic Low Back Pain Randomized, Multicenter, Blinded, Parallel-Group Trial With 3 Groups', *Arch. Intern. Med.*, 167 (2007), 1892-1898 at <http://archinte.ama-assn.org/cgi/content/abstract/167/17/1892>, accessed 27 November 2008

Kahn, Michael, 'Antibiotics overprescribed for common viruses: study', *Reuters*, 14 March 2008 at <http://www.reuters.com/article/healthNews/idUSL1386500020080314>, accessed 4 February 2009

Kirk, Andy (Chair of The Society of Homeopaths), Letter sent to *The Guardian*, 22 October 2007

Kleijnen, J, P Knipschild, and Riet G Ter, 'Clinical trials of homoeopathy' *BMJ*, 302 (1991), 316-23.

Lawrence, Tom, 'Patients Picket for Homeopathy', *Harrow Observer*, 13 June 2007

Lee, Christopher, 'Medicare Helps Push Drug Spending Up', Washington Post, 8 January 2008 at <http://www.washingtonpost.com/wp-dyn/content/article/2008/01/07/AR2008010702943.html>, accessed 2 December 2008

Leonhardt, David, 'Why Doctors So Often Get it Wrong', *New York Times*, 22 February 2006 at <http://www.nytimes.com/2006/02/22/business/22leonhardt.html>, accessed 29 December 2007

Linde, Klaus, Wayne B Jonas, 'Meta-analysis of homoeopathy trials' (letter to the editor), *Lancet*, 9503 (2005) at <http://www.thelancet.com/journals/lancet/article/PIIS0140-6736(05)67878-6/fulltext>, accessed 3 March 2009.

Lüdtke, R and A L B Rutten, 'The conclusions on the effectiveness of homeopathy highly depend on the set of analyzed trials', *J. Clin. Epidemiol.*, (2008) at <http://www.aekh.at/fileadmin/Bilder/Hom_opathie_int/LuedtkeRuttenJCE08.pdf>, accessed 15 April 2009

New York Times, 'Is Your Doctor Tied to Drug Makers?' (editorial), *New York Times*, 2 July 2007 at <http://www.nytimes.com/2007/07/02/opinion/02mon2.html>, accessed 23 February 2009

Pharma & Bioingredients, 'Global Drug Sales Rise 7% in 2006', <http://www.pharmabioingredients.com/articles/2007/04/global-drug-sales-rise-7>, accessed 4 December 2008

Rogers, Jim, 'Homeopathic medicine: Can less really be more when it comes to treating sports injuries?', *Peak Performance: The research newsletter on stamina, strength and fitness*, 218 (2005), 1-4, p. 4, at <http://www.arcanum-utbildning.se/sidor/peak+performance+sept+2005.pdf>, accessed 23 February 2009

Rutten, A L B, and C F Stolper, 'The 2005 meta-analysis of homeopathy: the importance of post-publication data', *Homeopathy*, 2008 at <http://www.aekh.at/fileadmin/Bilder/Hom_opathie_int/RuttenStolperHomeopathyarticle.pdf>, accessed 15 April 2009

Sackett, David L, William M C Rosenberg, J A Muir Gray, R Brian Haynes, W Scott Richardson, 'Evidence based medicine: what it is and what it isn't', *BMJ*, 312 (1996), 71-72 (13 January), at <http://www.bmj.com/cgi/content/full/312/7023/71, accessed 6 December 2008

Shang, Aijing, MD, Karin Huwiler-Müntener MD, Linda Nartey MD, Peter Jüni MD, Stephan Dörig, Jonathan AC Sterne PhD, Daniel Pewsner MD, Prof Matthias Egger MD, 'Are the clinical effects of homoeopathy placebo effects? Comparative study of placebo-controlled trials of homoeopathy and allopathy', *The Lancet*, 366 (2005), 726-732

Shukla, Vijay K, Annie Bai, Sarah Milne and George Wells, 'Systematic Review of Quality Assessment Instruments for Randomized Control Trials: Selection of SIGN50 Methodological Checklist', *Cochrane Colloquium Abstracts* (The Cochrane Collaboration) at < http://www.imbi.uni-freiburg.de/OJS/cca/index.php/cca/article/view/5053>, accessed 3 March 2009

Smith, Blair H, 'Evidence based medicine', letter in *BMJ*, 313 (1996), 169 (20 July), at <http://www.bmj.com/cgi/content/full/313/7050/169/b>, accessed 6 December 2008

Weeks, L, M Verhoef and C Scott, 'Presenting the alternative: cancer and complementary and alternative medicine in the Canadian print media', *Support Care Cancer*, 15 (2007), 931-8, at *U.S. National Library of Medicine* website at <http://www.ncbi.nlm.nih.gov/pubmed/17624558>, accessed 23 February 2009

Websites

BBC, 'Call to curb rising NHS drug bill', 3 April 2008, at <http://news.bbc.co.uk/1/hi/health/7190267.stm>, accessed 27 November 2008

BMJ Clinical Evidence at <http://clinicalevidence.bmj.com/ceweb/about/knowledge.jsp>, accessed 23 February 2009

Doublecheckmd, 'Qualaquin (quinine) – Cinchonism', *Doublecheckmd* website at <http://doublecheckmd.com/EffectsDetail.do?dname=quinine&sid=12268&eid=4405>, accessed 1 February 2009

Fortune 500, '155. Bayer', at <http://money.cnn.com/magazines/fortune/global500/2008/snapshots/6254.html>, accessed 4 March 2009

Fortune 500, 'Global Listing for Pharmaceutical Industry', *Fortune 500*, 21 July 2008, at <http://money.cnn.com/magazines/fortune/global500/2008/industries/21/index.html>, accessed 21 February 2008

London South Bank University, Martin Chaplin BSc PhD CChem FRSC, 'Water structure and science', at: <http://www.lsbu.ac.uk/water/>, accessed 15 April 2009

NHS Information Centre for Health and Social Care, '*GP Earnings and Expenses Enquiry 2006/07: Initial Report*, October 2008, at <http://www.ic.nhs.uk/webfiles/publications/gpearnex0607a/GP%20Earnings%20and%20Expenses%20Enquiry%202006-07%20Initial%20Report.pdf>, accessed 19 February 2009

Peak Performance Online, Jim Rogers, 'Homeopathy In Sport: Can less really be more when it comes to treating sports injuries?', *Peak Performance Online* at <http://www.pponline.co.uk/encyc/homeopathy-in-sport.htm#ref>, accessed 23 February 2009.

Prospectus, 'Homeopath: Salary and conditions', at <http://www.prospects.ac.uk/p/types_of_job/homeopath_salary.jsp>, accessed 28 February 2009

Symposia, Jim Owens PMP, 'Hawthorne Effect, an examination', *Symposia: the online philosophy journal*, <http://journal.ilovephilosophy.com/Article/Hawthorne-Effect--an-examination/872>, accessed 27 November 2008.

Textbookofbacteriology, 'Vibrio cholerae and Asiatic Cholera (page 1)' at <http://www.textbookofbacteriology.net/cholera.html>, accessed 19 March 2009

UK Department of Health, 'Homoeopathic Services document' (Gateway ref. 8971), *Department of Health Weekly Bulletin*, 25 October 2007 at <http://www.dh.gov.uk/en/Publicationsandstatistics/Bulletins/theweek/DH_079859>, accessed 25 February 2008

UK Statute Law Database, 'Medicines Act 1968', *UK Statute Law Database* at <http://www.statutelaw.gov.uk/content.aspx?LegType=All+Primary&PageNumber=61&Na

vFrom=2&parentActiveTextDocId=1662209&ActiveTextDocId=1662226&filesize=3007>, accessed 20 February 2009

US Census Bureau's estimations of global population at <http://www.census.gov/ipc/www/worldhis.html>, accessed 24 November 2008

US Court of Federal Claims, Office of Special Masters, No. 02-0738V, Filed 20 July 2007, Proceedings p. 20 at <http://big.assets.huffingtonpost.com/BANKS_CASE.pdf>, accessed 3 March 2009

WHO, *Spending On Health: A Global Overview*, World Health Organization, 2007 at <http://www.who.int/mediacentre/factsheets/fs319/en/index.html>, accessed 21 February 2008

WHO, *The World Medicines Situation*, World Health Organization, 2004, <http://www.who.int/medicinedocs/en/d/Js6160e/7.html>, accessed 21 February 2008

Books

Bellavite, Paolo and Andrea Signorini, *The Emerging Science of Homeopathy: Complexity, biodynamics, and nanopharmacology* (Berkley: North Atlantic Books, 2002)

Berkow, Robert, MD (Ed. in Chief), *The Merck Manual of Medical Information: Home edition* (New York: Simon and Schuster Inc., 2000)

Boericke, William, MD, *Pocket Manual of Homoeopathic Materia Medica and Repertory*, 9th edn (Delhi: B. Jain Publishers, repr. edn 1990), p. 139.

Bradford, Thomas Lindsley, MD, *The Logic of Figures or Comparative Results of Homeopathic and Other Treatments* 1st edn (Philadelphia: Boericke and Tafel, 1900), (Kessinger Publishing: [United States], repr. edn [n.d.])

Burnett, J Compton, MD, *Vaccinosis and Its Cure by Thuja*, 1st edn 1884 (New Delhi: B. Jain Publisher, repr. edn 2001)

Chaitow, Leon, *Vaccination and Immunisation: Dangers, delusions and alternatives (What every parent should know)*, 1st edn 1987 (The C.W. Daniel Company: Saffron Waldon, rvd edn 1998)

Chernew, Michael E, Richard A Hirth, and David M Cutler, 'Increased Spending On Health Care: How Much Can The United States Afford?', *Health Affairs*, 22 (2003), 15-25, p. 15, also at <http://content.healthaffairs.org/cgi/reprint/22/4/15.pdf>, accessed 21 February 2009.

Cook, Trevor M, *Samuel Hahnemann: His Life and Times* (Staines: Homoeopathic Studies, 1993)

Coulter, Harris L and Barbara Loe Fisher, *A Shot in the Dark: Why the P in DPT vaccination may be hazardous to your child's health* (Avery: New York, 1991)

Coulter, Harris L, *Divided Legacy: The Conflict between Homoeopathy and the American Medical Association,* vol. III (Berkeley: North Atlantic Books, 1982)

Coulter, Harris L, *Vaccination, Social Violence, and Criminality: The Medical Assault on the American Brain* (Berkeley: North Atlantic Books, 1990)

Donegan, Dr Jayne L M, MBBS DRCOG DFFP DCH MRCGP MFHOM, *Vaccinatable Disease and Their Vaccines* (London: Jayne Donegan, [n.d.]) at <http://www.jayne-donegan.co.uk/LinkClick.aspx?fileticket=d%2ffky%2fIvwv4%3d&tabid=826>

Golden, Dr Isaac, PhD, *Vaccination & Homoeoprophylaxis? A review of risks and alternatives*, 6th edn (Daylesford: Isaaac Golden, 2005)

Hahnemann, Dr Samuel (trans. Prof. Louis H. Tafel), *The Chronic Diseases: Their Peculiar Nature and Their Homoeopathic Cure*, from the 2nd enlarged German edn (1835), 2 vols (Delhi: B. Jain Publishers, repr. edn 1995)

Hahnemann, Samuel (trans. R E Dudgeon MD and Richard Hughes LRCPE), *Materia Medica Pura*, 2 vols (New Delhi: B Jain Publishers, repr. edn 2002)

Hahnemann, Samuel (trans. R.E. Dudgeon), *The Organon of Medicine*, 5th edn, at <http://www.homeoint.org/books/hahorgan/organ260.htm#P270E5>, accessed 27 December 2008

Hahnemann, Samuel (trans. William Boericke MD), *The Organon of Medicine*, 6th edn, finished in 1842 (Calcutta: Roy Publishing House, repr. edn 1972)

Hahnemann, Samuel, (trans. R E Dudgeon MD), *The Lesser Writings of Samuel Hahnemann*, 1851 edn (New Delhi: B. Jain Publishers, repr. edn 2002)

Jong-wook, Lee (Director-General, World Health Organization), 'Introduction' to *Preventing Chronic Diseases: a vital investment: WHO global report* (Geneva: World Health Organization, 2005), p. vii, also at <http://www.who.int/chp/chronic_disease_report/full_report.pdf>, accessed 17 February 2009

Kent, James Tyler, AM MD, *Lectures on Homoeopathic Philosophy* , 5th edn (New Delhi: B. Jain Publishers, repr. edn 1993)

Kuhn, Thomas S, *The Structure of Scientific Revolutions*, 3rd edn (Chicago: University of Chicago Press, 1996)

Lawrence, I Conrad, Michael Neve, Vivian Nutton, Roy Porter and Andrew Wear, *The Western Medical Tradition* (Cambridge: Cambridge University Press, 1995), p. 5.

McKeown, Thomas, 'The medical contribution' in Davey, Basiro, Alastair Gray and Clive Seale (Eds), *Health and Disease: A reader* (Buckingham and Philadelphia: Open University Press, 1993)

Okasha, Samir, *Philosophy of Science: A very short introduction* (Oxford: Oxford University Press, 2002)

Popper, K R, *Conjectures and Refutations*, 2nd edn (London: Routledge and Kegan Paul, 1965)

Rao, M L, R Roy, I R Bell and R Hoover, 'The defining role of structure (including epitaxy) in the plausibility of homeopathy', *Homeopathy*, 96 (2007), 175-182

Scheibner, Viera, PdD, *Vaccination* (Blackheath, Australia: Viera Scheibner, 1997)

Scheibner, Viera, PhD, *Behavioural Problems in Childhood: The Link to Vaccination* (Blackheath, Australia: Viera Scheibner, 2000)

Schiff, Michel, *The Memory of Water: Homoeopathy and the battle of ideas in the new science* (London: Thorsens, 1994)

Schroyens, Dr Frederick (ed.), *Synthesis: Repertorium Homoeopathicum Syntheticum* (London: Homeopathic Book Publishers, 2001)

Singh, Simon, and Edzard Ernst, *Trick or Treatment? Alternative medicine on trial* (London: Bantam Press, 2008)

Stevens, Allen, MBBS FRCPath and James Lowe BmedSci BMBS, DM, FRCPath, *Pathology* (London: Harcourt Publishers, 2000)

Storr, Anthony, 'Introduction' to Henrik R Wulff, Stig Andur Pedersen and Raben Rosenberg, *Philosophy of medicine: an introduction* (Oxford: Blackwell Scientific Publications, 1986)

Szreter, Simon, 'The importance of social intervention in Britain's mortality decline *c*. 1850-1914: a re-interpretation of the role of public health' in Davey, Basiro, Alastair Gray and Clive Seale (Eds), *Health and Disease: A reader* (Buckingham and Philadelphia: Open University Press, 1993), pp. 191 - 199

Ullman, Dana, *Discovering Homeopathy: Your introduction to the science and art of homeopathic medicine*, rev. edn (Berkeley: North Atlantic Books, 1991)

Wulff, Henrik R, Stig Andur Pedersen and Raben Rosenberg, *Philosophy of Medicine: An introduction* (Oxford: Blackwell Scientific Publications, 1986)

Notes

[1] We shall explain later why we have chosen to use the term "orthodox medicine" throughout this critique.
[2] Samuel Hahnemann, Letter to Dr Stapf, 14 November 1825 in Thomas Lindsley Bradford MD, *The Life and Letters of Dr Samuel Hahnemann* (New Delhi: B Jain Publishers, repr. edn 1999), p. 162.
[3] I Conrad Lawrence, Michael Neve, Vivian Nutton, Roy Porter and Andrew Wear, *The Western Medical Tradition* (Cambridge: Cambridge University Press, 1995), p. 5.
[4] Samir Okasha, *Philosophy of Science: A very short introduction* (Oxford: Oxford University Press, 2002), p. 2.
[5] J Kleijnen, P Knipschild, and Riet G Ter, 'Clinical trials of homoeopathy' *BMJ*, 302 (1991), 316-23.
[6] Christopher Lee, 'Medicare Helps Push Drug Spending Up', *Washington Post*, 8 January 2008 at <http://www.washingtonpost.com/wp-dyn/content/article/2008/01/07/AR2008010702943.html>, accessed 2 December 2008.
[7] 'Global Drug Sales Rise 7% in 2006', Pharma & Bioingredients website at <http://www.pharmabioingredients.com/articles/2007/04/global-drug-sales-rise-7>, accessed 4 December 2008.
[8] Robert Berkow MD, (Editor in Chief), *The Merck Manual of Medical Information* (New York: Simon and Schuster Inc., 2000).
[9] Thomas McKeown, 'The medical contribution' in Davey, Basiro, Alastair Gray and Clive Seale (Eds), *Health and Disease: A reader* (Buckingham and Philadelphia: Open University Press, 1993), p. 182.
[10] Thomas McKeown, 'The medical contribution' in Davey, Basiro, Alastair Gray and Clive Seale (Eds), *Health and Disease: A reader* (Buckingham and Philadelphia: Open University Press, 1993), p. 189.
[11] Simon Szreter, 'The importance of social intervention in Britain's mortality decline *c*. 1850-1914: a re-interpretation of the role of public health' in Davey, Basiro, Alastair Gray and Clive Seale (Eds), *Health and Disease: A reader* (Buckingham and Philadelphia: Open University Press, 1993), pp. 191 - 199.
[12] Thomas McKeown, 'The medical contribution' in Davey, Basiro, Alastair Gray and Clive Seale (Eds), *Health and Disease: A reader* (Buckingham and Philadelphia: Open University Press, 1993), p. 189.
[13] US Census Bureau's estimations of global population at <http://www.census.gov/ipc/www/worldhis.html>, accessed 24 November 2008. The total is based on taking average of the maximum and minimum figures for each period.
[14] David L Sackett, William M C Rosenberg, J A Muir Gray, R Brian Haynes, W Scott Richardson, 'Evidence based medicine: what it is and what it isn't', *BMJ*, 312 (1996), 71-72 (13 January), at <http://www.bmj.com/cgi/content/full/312/7023/71, accessed 6 December 2008.
[15] David L Sackett, William M C Rosenberg, J A Muir Gray, R Brian Haynes, W Scott Richardson, 'Evidence based medicine: what it is and what it isn't', *BMJ*, 312 (1996), 71-72 (13 January), at <http://www.bmj.com/cgi/content/full/312/7023/71, accessed 6 December 2008.
[16] Blair H Smith, 'Evidence based medicine', letter in *BMJ*, 313 (1996), 169 (20 July), at <http://www.bmj.com/cgi/content/full/313/7050/169/b>, accessed 6 December 2008
[17] 'Call to curb rising NHS drug bill', 3 April 2008, <http://news.bbc.co.uk/1/hi/health/7190267.stm>, accessed 27 November 2008.
[18] Sarah Boseley, 'Adverse drug reactions cost NHS £2bn', *The Guardian*, 3 April 2008, <http://www.guardian.co.uk/society/2008/apr/03/nhs.drugsandalcohol>, accessed 14 November 2008.
[19] K R Popper, *Conjectures and Refutations*, 2nd edn (London: Routledge and Kegan Paul, 1965), p. 46, quoted in Henrik R Wulff, Stig Andur Pedersen and Raben Rosenberg, *Philosophy of Medicine: An introduction* (Oxford: Blackwell Scientific Publications, 1986), p. 22.
[20] See Paolo Bellavite and Andrea Signorini, *The Emerging Science of Homeopathy: Complexity, biodynamics, and nanopharmacology* (Berkley: North Atlantic Books, 2002), pp. 275 – 278.
[21] Jim Owens PMP, 'Hawthorne Effect, an examination', *Symposia: the online philosophy journal*, <http://journal.ilovephilosophy.com/Article/Hawthorne-Effect--an-examination/872>, accessed 27 November 2008.
[22] John G. Adair, Donald Sharpe and Cam-Loi Huynh, 'Hawthorne Control Procedures in Educational Experiments: A Reconsideration of Their Use and Effectiveness', *Review of Educational Research*, 59 (1989), 215-228 at <http://www.jstor.org/pss/1170415>, accessed 27 November 2008.
[23] Michael Haake PhD MD, Hans-Helge Müller PhD, Carmen Schade-Brittinger, Heinz D. Basler PhD, Helmut Schäfer PhD, Christoph Maier PhD MD, Heinz G Endres MD, Hans J Trampisch PhD, and Albrecht Molsberger PhD MD, 'German Acupuncture Trials (GERAC) for Chronic Low Back Pain Randomized, Multicenter, Blinded, Parallel-Group Trial With 3 Groups', *Arch. Intern. Med.*, 167 (2007), 1892-1898 at <http://archinte.ama-assn.org/cgi/content/abstract/167/17/1892>, accessed 27 November 2008.

[24] Edzard Ernst, 'Acupuncture Ineffective, Attention Effective?', *Arch. Intern. Med.*,168 (2008), 551 at <http://archinte.ama-assn.org/cgi/content/extract/168/5/551>, accessed 28 November 2008.
[25] See Paolo Bellavite and Andrea Signorini, *The Emerging Science of Homeopathy: Complexity, biodynamics, and nanopharmacology* (Berkley: North Atlantic Books, 2002), pp. 275-279.
[26] Samuel Hahnemann (trans. William Boericke MD), *The Organon of Medicine*, 6th edn, finished in 1842 (Calcutta: Roy Publishing House, repr. edn 1972), § 60 n. 66 cont., p. 139.
[27] Samuel Hahnemann, 'Protection against infection in epidemic diseases' from *The Friend of Health: Part I* (Frankfurt am Main: [n. p.], 1792) in Samuel Hahnemann (trans. R E Dudgeon MD), *The Lesser Writings of Samuel Hahnemann*, 1851 edn (New Delhi: B. Jain Publishers, repr. edn 2002), p. 168.
[28] Samuel Hahnemann, 'Things that spoil the air' from *The Friend of Health: Part I* (Frankfurt am Main: [n. p.], 1792) in Samuel Hahnemann (trans. R E Dudgeon MD), *The Lesser Writings of Samuel Hahnemann*, 1851 edn (New Delhi: B. Jain Publishers, repr. edn 2002), p. 178.
[29] Samuel Hahnemann, 'Protection against infection in epidemic diseases' from *The Friend of Health: Part I* (Frankfurt am Main: [n. p.], 1792) in Samuel Hahnemann (trans. R E Dudgeon MD), *The Lesser Writings of Samuel Hahnemann*, 1851 edn (New Delhi: B. Jain Publishers, repr. edn 2002), p. 168.
[30] Samuel Hahnemann, *The Friend of Health: Part II* (Frankfurt am Main: [n. p.], 1795) in Samuel Hahnemann (trans. R E Dudgeon MD), *The Lesser Writings of Samuel Hahnemann*, 1851 edn (New Delhi: B. Jain Publishers, repr. edn 2002), pp. 203-212.
[31] Samuel Hahnemann, *The Friend of Health: Part II* (Frankfurt am Main: [n. p.], 1795) in Samuel Hahnemann (trans. R E Dudgeon MD), *The Lesser Writings of Samuel Hahnemann*, 1851 edn (New Delhi: B. Jain Publishers, repr. edn 2002), pp. 212-227.
[32] Simon Szreter, 'The importance of social intervention in Britain's mortality decline *c*. 1850-1914' in Davey, Basiro, Alastair Gray and Clive Seale (Eds), *Health and Disease: A reader* (Buckingham and Philadelphia: Open University Press, 1993), p. 199.
[33] Samuel Hahnemann, 'Appeal to Thnking Philanthropists Respecting the Mode of Propagation of the Asiatic Cholera', (Leipzic: the author, 1831) in Samuel Hahnemann (trans. R E Dudgeon MD), *The Lesser Writings of Samuel Hahnemann*, 1851 edn (New Delhi: B. Jain Publishers, repr. edn 2002), p. 758.
[34] For example, Trevor M Cook, *Samuel Hahnemann: His Life and Times* (Staines: Homoeopathic Studies, 1993), p. 58.
[35] Samuel Hahnemann (trans. William Boericke MD), *The Organon of Medicine*, 6th edn, finished in 1842 (Calcutta: Roy Publishing House, repr. edn 1972), § 110, p. 180.
[36] Samuel Hahnemann (trans. William Boericke MD), *The Organon of Medicine*, 6th edn, finished in 1842 (Calcutta: Roy Publishing House, repr. edn 1972), § 36, p. 113; § 38, p. 115; § 40, p. 120; and § 46, p. 123.
[37] Samuel Hahnemann (trans. William Boericke MD), *The Organon of Medicine*, 6th edn, finished in 1842 (Calcutta: Roy Publishing House, repr. edn 1972), §§ 34-50, pp. 111-128.
[38] See particularly Samuel Hahnemann (trans. William Boericke MD), *The Organon of Medicine*, 6th edn, finished in 1842 (Calcutta: Roy Publishing House, repr. edn 1972) and Dr Samuel Hahnemann (trans. Prof. Louis H. Tafel), *The Chronic Diseases: Their Peculiar Nature and Their Homoeopathic Cure*, from the 2nd enlarged German edn (1835), 2 vols (Delhi: B. Jain Publishers, repr. edn 1995).
[39] Dr Samuel Hahnemann (trans. Prof. Louis H. Tafel), *The Chronic Diseases: Their Peculiar Nature and Their Homoeopathic Cure*, from the 2nd enlarged German edn (1835), 2 vols (Delhi: B. Jain Publishers, repr. edn 1995).
[40] Dana Ullman, *Discovering Homeopathy: Your introduction to the science and art of homeopathic medicine*, rev edn (Berkeley: North Atlantic Books, 1991), p. 17.
[41] See Samuel Hahnemann (trans. William Boericke MD), *The Organon of Medicine*, 6th edn, finished in 1842 (Calcuta: Roy Publishing House, repr. edn 1972), §§ 201-204 and 214-220, pp. 225-228 and 234-237.
[42] Samuel Hahnemann (trans. William Boericke MD), *The Organon of Medicine*, 6th edn, finished in 1842 (Calcutta: Roy Publishing House, repr. edn 1972), § 3, p. 90.
[43] Samuel Hahnemann (trans. William Boericke MD), *The Organon of Medicine*, 6th edn, finished in 1842 (Calcutta: Roy Publishing House, repr. edn 1972), § 63-69, pp. 142 - 148. Note that Hahnemann uses the contemporary terminology for homeostatic reactions of 'primary and secondary action'.
[44] Thomas S Kuhn, *The Structure of Scientific Revolutions*, 3rd edn (Chicago: University of Chicago Press, 1996).
[45] Samuel Hahnemann (trans. William Boericke MD), *The Organon of Medicine*, 6th edn, finished in 1842 (Calcutta: Roy Publishing House, repr. edn 1972), § 153, p. 204.
[46] Samuel Hahnemann (trans. William Boericke MD), *The Organon of Medicine*, 6th edn, finished in 1842 (Calcutta: Roy Publishing House, repr. edn 1972), § 274, p. 277.
[47] 'Examination of the Sources of the Common Materia Medica', from vol. III, 2nd edn (1825), in Samuel Hahnemann (trans. R E Dudgeon MD and Richard Hughes LRCPE), *Materia Medica Pura* (New Delhi: B Jain Publishers, repr. edn 2002), vol. II, p.10
[48] Samuel Hahnemann (trans. R.E. Dudgeon), *The Organon of Medicine*, 5th edn, § 270 at <http://www.homeoint.org/books/hahorgan/organ260.htm#P270E5>, accessed 27 December 2008.

[49] Samuel Hahnemann (trans. William Boericke MD), *The Organon of Medicine*, 6th edn, finished in 1842 (Calcutta: Roy Publishing House, repr. edn 1972), § 270, p. 272 and note 152.
[50] 'Preface' to Samuel Hahnemann (trans. William Boericke MD), *The Organon of Medicine*, 6th edn, finished in 1842 (Calcutta: Roy Publishing House, repr. edn 1972), p. 21.
[51] Trevor M Cook, *Samuel Hahnemann: His Life and Times* (Staines: Homoeopathic Studies, 1993), p. 97.
[52] Samuel Hahnemann (trans. William Boericke MD), *The Organon of Medicine*, 6th edn, finished in 1842 (Calcutta: Roy Publishing House, repr. edn 1972), § 269 n.149, p. 270.
[53] Samuel Hahnemann (trans. William Boericke MD), *The Organon of Medicine*, 6th edn, finished in 1842 (Calcutta: Roy Publishing House, repr. edn 1972), § 269, p. 268.
[54] Samuel Hahnemann (trans. William Boericke MD), *The Organon of Medicine*, 6th edn, finished in 1842 (Calcutta: Roy Publishing House, repr. edn 1972), § 269 n. 146, p. 268.
[55] See Martin Chaplin BSc PhD CChem FRSC, 'Water structure and science', London South Bank University website at: <http://www.lsbu.ac.uk/water/>, accessed 15 April 2009.
[56] See Bellavite, Paolo and Andrea Signorini, *The Emerging Science of Homeopathy: Complexity, biodynamics, and nanopharmacology* (Berkley: North Atlantic Books, 2002) and M L Rao, R Roy, I R Bell and R Hoover, 'The defining role of structure (including epitaxy) in the plausibility of homeopathy', *Homeopathy*, 96 (2007), 175-182.
[57] See M L Rao, R Roy, I R Bell and R Hoover, 'The defining role of structure (including epitaxy) in the plausibility of homeopathy', *Homeopathy*, 96 (2007), 175-182.
[58] Michel Schiff, *The Memory of Water: Homoeopathy and the battle of ideas in the new science* (London: Thorsens, 1994).
[59] Michel Schiff, *The Memory of Water: Homoeopathy and the battle of ideas in the new science* (London: Thorsens, 1994), p. 85.
[60] P Belon, J Cumps, M Ennis, P F Mannaioni, M Roberfroid, J Sainte-Laudy, F A C Wiegant, 'Histamine dilutions modulate basophil activation', *Inflamm. Res.*, 53 (2004), 181-188.
[61] James Tyler Kent, *Lectures on Homoeopathic Philosophy* , 5th edn (New Delhi: B. Jain Publishers, repr. edn 1993), pp. 225-234.
[62] See Samuel Hahnemann (trans. William Boericke MD), *The Organon of Medicine*, 6th edn, finished in 1842 (Calcutta: Roy Publishing House, repr. edn 1972), §105-45, pp. 177-200; and Jeremy Sherr, The Dynamics and Methodology of Homoeopathic Provings, 2nd edn (Malvern: Dynamis Books, 1994).
[63] 'Author's Preface' from vol. I, 3rd edn in Samuel Hahnemann (trans. R E Dudgeon MD and Richard Hughes LRCPE), *Materia Medica Pura*, vol. I (New Delhi: B Jain Publishers, repr. edn 2002), p. 2.
[64] Samuel Hahnemann (trans. William Boericke MD), *The Organon of Medicine*, 6th edn, finished in 1842 (Calcutta: Roy Publishing House, repr. edn 1972), §107, p. 178.
[65] Samuel Hahnemann (trans. William Boericke MD), *The Organon of Medicine*, 6th edn, finished in 1842 (Calcutta: Roy Publishing House, repr. edn 1972), §116, p. 183.
[66] Samuel Hahnemann (trans. William Boericke MD), *The Organon of Medicine*, 6th edn, finished in 1842 (Calcutta: Roy Publishing House, repr. edn 1972), §§ 82-104, pp. 162-177.
[67] William Boericke MD, *Pocket Manual of Homoeopathic Materia Medica and Repertory*, 9th edn (Delhi: B. Jain Publishers, repr. edn 1990), p. 139.
[68] This and the following analyses are based on a larger modern repertory: Dr Frederick Schroyens (ed.), *Synthesis: Repertorium Homoeopathicum Syntheticum* (London: Homeopathic Book Publishers, 2001).
[69] See 'Vibrio cholerae and Asiatic Cholera (page 1)', Introduction, Cholera, para. 3 at <http://www.textbookofbacteriology.net/cholera.html>, accessed 19 March 2009.
[70] Robert Berkow MD (Ed. in Chief), *The Merck Manual of Medical Information: Home edition* (New York: Simon and Schuster Inc., 2000), p. 869.
[71] Robert Berkow MD (Ed. in Chief), *The Merck Manual of Medical Information: Home edition* (New York: Simon and Schuster Inc., 2000), p. 869.
[72] Dr Samuel Hahnemann (trans. Prof. Louis H. Tafel), *The Chronic Diseases: Their Peculiar Nature and Their Homoeopathic Cure*, from the 2nd enlarged German edn (1835), 2 vols (Delhi: B. Jain Publishers, repr. edn 1995), p. 2.
[73] See Thomas Lindsley Bradford MD, *The Logic of Figures or Comparative Results of Homeopathic and Other Treatments* 1st edn (Philadelphia: Boericke and Tafel, 1900), (Kessinger Publishing: [United States], repr. edn [n.d.]), pp. 115-146.
[74] Dana Ullman, *Discovering Homeopathy: Your introduction to the science and art of homeopathic medicine*, rev. edn (Berkeley: North Atlantic Books, 1991), p. 40.
[75] Dana Ullman, *Discovering Homeopathy: Your introduction to the science and art of homeopathic medicine*, rev. edn (Berkeley: North Atlantic Books, 1991), p. 126.

[76] Paolo Bellavite and Andrea Signorini, *The Emerging Science of Homeopathy: Complexity, biodynamics, and nanopharmacology* (Berkley: North Atlantic Books, 2002), p. 45. They refer to R.H. Savage and P.F. Roe, 'A further double-blind trial to assess the benefit of Arnica montana in acute stroke illness', *Brit. Hom. J.*, 67 (1978), p. 210 and A.M. Scofield, 'Experimental research in homeopathy: A critical review', 2 parts, *Brit. Hom. J.*, 73 (1984), p. 161.

[77] Vance W Berger PhD, 'Is the Jadad Score the Proper Evaluation of Trials?' (letter to the editor), *J. Rheumatol.* (2006) at <http://www.jrheum.com/subscribers/06/08/1710-c.html> and Vijay K Shukla, Annie Bai, Sarah Milne and George Wells, 'Systematic Review of Quality Assessment Instruments for Randomized Control Trials: Selection of SIGN50 Methodological Checklist', *Cochrane Colloquium Abstracts* (The Cochrane Collaboration) at < http://www.imbi.uni-freiburg.de/OJS/cca/index.php/cca/article/view/5053>, both accessed 3 March 2009.

[78] Aijing Shang MD, Karin Huwiler-Müntener MD, Linda Nartey MD, Peter Jüni MD, Stephan Dörig, Jonathan AC Sterne PhD, Daniel Pewsner MD, Prof Matthias Egger MD, 'Are the clinical effects of homoeopathy placebo effects? Comparative study of placebo-controlled trials of homoeopathy and allopathy', *The Lancet*, 366 (2005), 726-732.

[79] Klaus Linde, Wayne B Jonas, 'Meta-analysis of homoeopathy trials' (letter to the editor), *Lancet*, 9503 (2005) at <http://www.thelancet.com/journals/lancet/article/PIIS0140-6736(05)67878-6/fulltext>, accessed 3 March 2009.

[80] Klaus Linde, Wayne B Jonas, 'Meta-analysis of homoeopathy trials' (letter to the editor), *Lancet*, 9503 (2005) at <http://www.thelancet.com/journals/lancet/article/PIIS0140-6736(05)67878-6/fulltext>, accessed 3 March 2009.

[81] Peter Fisher, Brian Berman, Jonathan Davidson, David Reilly, Trevor Thompson and 29 others, Letter to the editor, *Lancet*, 9503 (2005) at <http://www.thelancet.com/journals/lancet/article/PIIS0140-6736(05)67879-8/fulltext>, accessed 3 March 2009.

[82] R Lüdtke and A L B Rutten, 'The conclusions on the effectiveness of homeopathy highly depend on the set of analyzed trials', *J. Clin. Epidemiol.*, (2008) at <http://www.aekh.at/fileadmin/Bilder/Hom_opathie_int/LuedtkeRuttenJCE08.pdf>; and A L B Rutten and C F Stolper, 'The 2005 meta-analysis of homeopathy: the importance of post-publication data', *Homeopathy*, 2008 at <http://www.aekh.at/fileadmin/Bilder/Hom_opathie_int/RuttenStolperHomeopathyarticle.pdf>, both accessed 15 April 2009.

[83] Samuel Hahnemann, 'How can small doses of such very attenuated medicine as homoeopathy employs still possess great power?' from *Reine Artzneimittellehre*, pt vi, ([n. p.]: [n. pub.], 1st edn 1827) in Samuel Hahnemann (trans. R E Dudgeon MD), *The Lesser Writings of Samuel Hahnemann*, 1851 edn (New Delhi: B. Jain Publishers, repr. edn 2002), pp. 728-734.

[84] Robert Berkow MD (Editor in Chief), *The Merck Manual of Medical Information: Home edition* (New York: Simon and Schuster Inc., 2000), p. 504.

[85] Robert Berkow MD (Editor in Chief), *The Merck Manual of Medical Information: Home edition* (New York: Simon and Schuster Inc., 2000), p. 505.

[86] Robert Berkow MD (Editor in Chief), *The Merck Manual of Medical Information: Home edition* (New York: Simon and Schuster Inc., 2000), p. 506.

[87] Robert Berkow MD (Editor in Chief), *The Merck Manual of Medical Information: Home edition* (New York: Simon and Schuster Inc., 2000), p. 508.

[88] James Tyler Kent AM MD, *Lectures on Homoeopathic Philosophy*, 5th edn (New Delhi: B. Jain Publishers, repr. edn 1993), p. 215.

[89] James Tyler Kent AM MD, *Lectures on Homoeopathic Philosophy* , 5th edn (New Delhi: B. Jain Publishers, repr. edn 1993), p. 225-234.

[90] James Tyler Kent AM MD, *Lectures on Homoeopathic Philosophy*, 5th edn (New Delhi: B. Jain Publishers, repr. edn 1993), p. 225.

[91] Allen Stevens MBBS FRCPath and James Lowe BmedSci BMBS, DM, FRCPath, *Pathology* (London: Harcourt Publishers, 2000), p. 9.

[92] United States Court of Federal Claims, Office of Special Masters, No. 02-0738V, Filed 20 July 2007, Proceedings p. 20 at <http://big.assets.huffingtonpost.com/BANKS_CASE.pdf>, accessed 3 March 2009.

[93] J Compton Burnett MD, *Vaccinosis and Its Cure by Thuja*, 1st edn 1884 (New Delhi: B. Jain Publisher, repr. edn 2001); Harris L Coulter, *Vaccination, Social Violence, and Criminality: The Medical Assault on the American Brain* (Berkeley: North Atlantic Books, 1990); Harris L Coulter and Barbara Loe Fisher, *A Shot in the Dark: Why the P in DPT vaccination may be hazardous to your child's health* (Avery: New York, 1991); Leon Chaitow, *Vaccination and Immunisation: Dangers, delusions and alternatives (What every parent should know)*, 1st edn 1987 (The C.W. Daniel Company: Saffron Waldon, rvd edn 1998); Dr Jayne L M Donegan MBBS DRCOG DFFP DCH MRCGP MFHOM, *Vaccinatable Disease and Their Vaccines* (London: Jayne Donegan, [n.d.]); Dr Isaac Golden PhD, *Vaccination & Homoeoprophylaxis? A review of risks and alternatives*, 6th edn (Daylesford: Isaaac Golden, 2005); Viera Scheibner PdD, *Vaccination* (Blackheath, Australia: Viera Scheibner, 1997); Viera Scheibner PhD, *Behavioural Problems in Childhood: The Link to Vaccination* (Blackheath, Australia: Viera Scheibner, 2000).

[94] Paolo Bellavite MD and Andrea Signorini MD, *The Emerging Science of Homeopathy: Complexity, biodynamics, and nanopharmacology*, (Berkley: North Atlantic Books, 2002), p. 159, but see also Chapter 5 in general.
[95] '155. Bayer', *Fortune 500* ranking at <http://money.cnn.com/magazines/fortune/global500/2008/snapshots/6254.html>, accessed 4 March 2009.
[96] 'Qualaquin (quinine) – Cinchonism', *Doublecheckmd* website at <http://doublecheckmd.com/EffectsDetail.do?dname=quinine&sid=12268&eid=4405>, accessed 1 February 2009.
[97] Robert Berkow MD (Editor in Chief), *The Merck Manual of Medical Information: Home edition* (New York: Simon and Schuster Inc., 2000), p. 35.
[98] Paolo Bellavite MD and Andrea Signorini MD, *The Emerging Science of Homeopathy: Complexity, biodynamics, and nanopharmacology* (Berkley: North Atlantic Books, 2002), p. 141.
[99] Michael Kahn, 'Antibiotics overprescribed for common viruses: study', *Reuters*, 14 March 2008 at <http://www.reuters.com/article/healthNews/idUSL1386500020080314>, accessed 4 February 2009.
[100] Michael Kahn, 'Antibiotics overprescribed for common viruses: study', *Reuters*, 14 March 2008 at <http://www.reuters.com/article/healthNews/idUSL1386500020080314>, accessed 4 February 2009.
[101] Robert Berkow MD (Editor in Chief), *The Merck Manual of Medical Information: Home edition* (New York: Simon and Schuster Inc., 2000) p. 25.
[102] Quoted by Jim Rogers, 'Homeopathic medicine: Can less really be more when it comes to treating sports injuries?', *Peak Performance: The research newsletter on stamina, strength and fitness*, 218 (2005), 1-4, p. 4, at <http://www.arcanum-utbildning.se/sidor/peak+performance+sept+2005.pdf> or Jim Rogers, 'Homeopathy In Sport: Can less really be more when it comes to treating sports injuries?', *Peak Performance Online* at <http://www.pponline.co.uk/encyc/homeopathy-in-sport.htm#ref>, both accessed 23 February 2009.
[103] 'Homoeopathic Services document' (Gateway ref. 8971), *Department of Health Weekly Bulletin*, 25 October 2007 at <http://www.dh.gov.uk/en/Publicationsandstatistics/Bulletins/theweek/DH_079859>, accessed 25 February 2008.
[104] *BMJ Clinical Evidence* website, <http://clinicalevidence.bmj.com/ceweb/about/knowledge.jsp>, accessed 23 February 2009.
[105] Tom Lawrence, 'Patients Picket for Homeopathy', *Harrow Observer*, 13 June 2007.
[106] Samuel Hahnemann, 'Appeal to Thnking Philanthropists Respecting the Mode of Propagation of the Asiatic Cholera', (Leipzic: the author, 1831) in Samual Hahnemann (trans. R E Dudgeon MD), *The Lesser Writings of Samuel Hahnemann*, 1851 edn (New Delhi: B. Jain Publishers, repr. edn 2002), p. 758.
[107] Dr Samuel Hahnemann (trans. Prof. Louis H. Tafel), *The Chronic Diseases: Their Peculiar Nature and Their Homoeopathic Cure*, from the 2nd enlarged German edn (1835), 2 vols (Delhi: B. Jain Publishers, repr. edn 1995).
[108] L Weeks, M Verhoef and C Scott, 'Presenting the alternative: cancer and complementary and alternative medicine in the Canadian print media', *Support Care Cancer*, 15 (2007), 931-8, at *U.S. National Library of Medicine* website at <http://www.ncbi.nlm.nih.gov/pubmed/17624558>, accessed 23 February 2009.
[109] David C. Bellinger et al., 'Neuropsychological and Renal Effects of Dental Amalgam in Children: A Randomized Clinical Trial', *JAMA*, 295 (2006), 1775-1783 at <http://jama.ama-assn.org/cgi/content/full/295/15/1775>, accessed 23 February 2009.
[110] Timothy A DeRouen et al., 'Neurobehavioral Effects of Dental Amalgam in Children: A Randomized Clinical Trial' *JAMA*, 295 (2006), 1784-1792 at <http://jama.ama-assn.org/cgi/content/short/295/15/1784>, accessed 23 February 2009.
[111] 'Is Your Doctor Tied to Drug Makers?' (editorial), *New York Times*, 2 July 2007 at <http://www.nytimes.com/2007/07/02/opinion/02mon2.html>, accessed 23 February 2009.
[112] Nadine E Foster, Elaine Thomas, Panos Barlas, Jonathan C Hill, Julie Young, Elizabeth Mason, Elaine M Hay, 'Acupuncture as an adjunct to exercise based physiotherapy for osteoarthritis of the knee: randomised controlled trial', *BMJ,* 335 (1 September 2007), p. 436 at <http://www.bmj.com/cgi/content/full/335/7617/436?ck=nck >
[113] Nadine E Foster, Elaine Thomas, Panos Barlas, Jonathan C Hill, Julie Young, Elizabeth Mason, Elaine M Hay, 'Acupuncture as an adjunct to exercise based physiotherapy for osteoarthritis of the knee: randomised controlled trial', *BMJ,* 335 (1 September 2007), p. 436 at < http://www.bmj.com/cgi/content/full/335/7617/436?ck=nck >
[114] Andy Kirk (Chair of The Society of Homeopaths), Letter sent to *The Guardian*, 22 October 2007.
[115] 'Medicines Act 1968', *UK Statute Law Database* at <http://www.statutelaw.gov.uk/content.aspx?LegType=All+Primary&PageNumber=61&NavFrom=2&parentActiveTextDocId=1662209&ActiveTextDocId=1662226&filesize=3007>, accessed 20 February 2009.
[116] Harris L Coulter, *Divided Legacy: The Conflict between Homoeopathy and the American Medical Association,* vol. III (Berkeley: North Atlantic Books, 1982), p. 124.
[117] Harris L Coulter, Divided Legacy: *The Conflict between Homoeopathy and the American Medical Association*, vol. III (Berkeley: North Atlantic Books, 1982), pp. 182-7.
[118] David Leonhardt, 'Why Doctors So Often Get it Wrong', *New York Times*, 22 February 2006 at <http://www.nytimes.com/2006/02/22/business/22leonhardt.html>, accessed 29 December 2007.

[119] See, for example, Thomas Lindsley Bradford, *The Logic of Figures or Comparative Results of Homeopathic and Other Treatments* (Philadelphia: Boericke and Tafel, 1900), repr. edn (Kessinger Publishing: [United States], [n.d.]), pp. 115-146.
[120] NHS Information Centre for Health and Social Care, *GP Earnings and Expenses Enquiry 2006/07: Initial Report*, October 2008, p. 2 at <http://www.ic.nhs.uk/webfiles/publications/gpearnex0607a/GP%20Earnings%20and%20Expenses%20Enquiry%202006-07%20Initial%20Report.pdf>, accessed 19 February 2009.
[121] NHS Information Centre for Health and Social Care, *GP Earnings and Expenses Enquiry 2006/07: Initial Report*, October 2008, p. 2 at <http://www.ic.nhs.uk/webfiles/publications/gpearnex0607a/GP%20Earnings%20and%20Expenses%20Enquiry%202006-07%20Initial%20Report.pdf>, accessed 19 February 2009.
[122] 'Homeopath: Salary and conditions', *Prospects.ac.uk* website at <http://www.prospects.ac.uk/p/types_of_job/homeopath_salary.jsp>, accessed 28 February 2009.
[123] *Spending On Health: A Global Overview*, World Health Organization, 2007, <http://www.who.int/mediacentre/factsheets/fs319/en/index.html>, accessed 21 February 2008.
[124] *The World Medicines Situation*, World Health Organization, 2004, <http://www.who.int/medicinedocs/en/d/Js6160e/7.html>, accessed 21 February 2008.
[125] 'Global Drug Sales Rise 7% in 2006', *Pharma & Bioingredients* website at <http://www.pharmabioingredients.com/articles/2007/04/global-drug-sales-rise-7>, accessed 4 December 2008.
[126] 'Global Listing for Pharmaceutical Industry', *Fortune 500*, 21 July 2008, <http://money.cnn.com/magazines/fortune/global500/2008/industries/21/index.html>, accessed 21 February 2008.
[127] Anthony Storr, 'Introduction' to Henrik R Wulff, Stig Andur Pedersen and Raben Rosenberg, *Philosophy of medicine: an introduction* (Oxford: Blackwell Scientific Publications, 1986), pp. ix and x.
[128] Lee Jong-wook (Director-General, World Health Organization), 'Introduction' to *Preventing Chronic Diseases: a vital investment: WHO global report* (Geneva: World Health Organization, 2005), p. vii, also at <http://www.who.int/chp/chronic_disease_report/full_report.pdf>, accessed 17 February 2009.
[129] Michael E Chernew, Richard A Hirth, and David M Cutler, 'Increased Spending On Health Care: How Much Can The United States Afford?', *Health Affairs*, 22 (2003), 15-25, p. 15, also at <http://content.healthaffairs.org/cgi/reprint/22/4/15.pdf>, accessed 21 February 2009.
[130] Paolo Bellavite and Andrea Signorini, *The Emerging Science of Homeopathy: Complexity, biodynamics, and nanopharmacology* (Berkley: North Atlantic Books, 2002).
[131] 'Call to curb rising NHS drug bill', *BBC News* website, 3 April 2008 at <http://news.bbc.co.uk/1/hi/health/7190267.stm>, accessed 27 November 2008.
[132] Sarah Boseley,'Adverse drug reactions cost NHS £2bn', *The Guardian*, 3 April 2008, <http://www.guardian.co.uk/society/2008/apr/03/nhs.drugsandalcohol>, accessed 14 November 2008.