The Demarcation Problem and Alternative Medicine

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Contents

1. Introduction

In this paper I intend to examine Popper's views on a philosophical problem with which he was very preoccupied throughout his life – namely the problem of demarcation. My aim is partly to expound Popper's ideas against their historical background, but also to try to assess the merit of his ideas and their relevance today. I will begin in this introduction by giving a general sketch of my attitude to Popper's philosophy. This can best be done by listing a number of points, some of which will be in favour of Popper and some critical of him. Point 1 is in favour of Popper. It is that he always worked on important philosophical problems. Indeed the problem of demarcation is central to philosophy of science and indeed to epistemology in general. Regrettably many philosophers go wrong by devoting a great deal of ingenuity to rather trivial questions. The result of such investigations can never be very interesting, and the ability to see what questions are important is part of what makes a great philosopher. Point 2 is also in favour of Popper. It is that his contributions to the problems he considered were always a considerable advance on what had gone before. Thus his views on demarcation are very much superior to those of the Vienna Circle as I will argue in more detail later in the paper. Point 3 is critical of Popper. It is that his proposed solutions to philosophical problems, even though they are superior to what went before, almost always contain quite severe flaws.

These flaws generally arise because of Popper's tendency to be too dismissive of his opponents and also to over-simplify the situation. Popper's weaknesses in these respects have led, in my opinion, to some wrong attitudes regarding the assessment of his work. Indeed one finds two diametrically opposed attitudes, both of which are wrong in my view. The first wrong attitude is an over-reverential attitude which treats Popper as a guru whose views are always correct. This is wrong because, as I have just claimed, Popper is nearly always both brilliant and flawed. The second wrong attitude (which is much more common than the first) is to dismiss Popper's ideas completely. Someone who takes this line will characteristically begin by pointing out quite correctly some flaw in a theory of Popper's. He or she will announce in triumph that Popper is wrong on this point, and then conclude, incorrectly, that Popper is really quite incompetent and it is not worth considering his ideas at all. This attitude is behind the very great neglect of Popper in the English speaking world, but the mistake here is to throw out the baby with the bath water. Although there are nearly always some flaws in Popper's views, there are nearly always also some very valuable ideas and insights into the problem he is tackling. Thus the correct attitude to Popper's philosophy is that of trying to develop his ideas critically so that the good points in them can be retained and the flaws rectified. Point 4 is again a point in favour of Popper. It is that his theoretical ideas in philosophy always have an important bearing on practical problems. Moreover they often have a bearing on practical problems which have arisen recently and which Popper himself never considered. I have tried to illustrate this point in the present paper by relating the theoretical considerations of the demarcation problem to the practical problem of what attitude we should adopt towards alternative medicine. This is a problem which Popper himself never considered, but which has become of great importance in recent years. In this way I want to show that Popper's ideas are not just an episode in the history of philosophy, but have considerable relevance today. So much then by way of general introduction, let us now start looking at the demarcation problem in more detail.

2. The Demarcation Problem and the Vienna Circle

The demarcation problem can be defined as that of demarcating *scientific* theories from other sorts of theories. The problem seems to have emerged in the eighteenth century partly as a reaction to the great successes of the scientific revolution and of Newtonian physics. It seemed to most eighteenth century thinkers that Newton's theory was a new type of theory, a

scientific theory, superior in kind to previous theories. But what then constituted the difference between a scientific theory and these other types of theory? This is the demarcation problem and we find discussions of it in such leading eighteenth century thinkers as Hume and Kant.

The demarcation problem is often formulated as that of distinguishing between science and metaphysics. In this formulation, however, metaphysics must be taken in a broad sense to include both religious doctrines, such as the doctrine of the Trinity, and pseudo-sciences such as astrology. But metaphysics in this broad sense should also include metaphysics in the narrow sense – that is to say, the general theories produced by philosophers, such as Plato's theory of Ideas, Leibniz's Monadology, and Hegel's account of the Absolute. Philosophers in the past have certainly produced such theories, but should they have done so? Do such theories have any value at all? Or would it be better if intellectuals concentrated on producing theories which were scientific? These are some of the issues connected with the demarcation problem.

Although discussions of the demarcation problem go back to the eighteenth century, I will begin the treatment of it in this lecture by examining the views on this question of Popper's immediate predecessors: *the Vienna Circle*. The Vienna Circle were in turn strongly influenced by Wittgenstein's *Tractatus*. Indeed the Vienna Circle devoted a whole academic year (1926-27) of their weekly seminar to reading the *Tractatus* sentence by sentence.

In the *Tractatus* Wittgenstein had argued that metaphysics is entirely meaningless, and this was accepted by the Vienna Circle. The classic expression of their point of view is to be found in Carnap's 1932 article: 'The Elimination of Metaphysics through Logical Analysis of Language'. Here Carnap writes emphatically that "In the domain of *metaphysics*, including all philosophy of value and normative theory, logical analysis yields the negative result *that the alleged statements in this domain are entirely meaningless*. Therewith a radical elimination of metaphysics is attained." (1932, pp. 60-1).

The Vienna Circle argued for the view that metaphysics is meaningless by adopting what was called the verifiability criterion of meaning. This is how Carnap formulates this criterion in his 1932 paper (p. 76): "The meaning of a statement lies in the method of its verification. A statement asserts only so much as is verifiable with respect to it. Therefore a sentence can be used only to assert an empirical proposition, if indeed it is used to assert anything at all."

To illustrate his thesis, Carnap considers the following passages from Heidegger's *Was Ist Metaphysik?* (1929): "Where do we seek the Nothing? How do we find the Nothing? … We know the Nothing. … *Anxiety reveals*

the Nothing. … That for which and because of which we were anxious, was 'really' – nothing. Indeed: the Nothing itself – as such – was present. … *What about this Nothing? – The Nothing itself nothings.*" (cf. Carnap, 1932, p. 69; as Carnap points out, the emphasis is in the original).

With a certain ponderousness which is not lacking in humour, Carnap tries to translate Heidegger's propositions into a precise logical language, and concludes that sentences like '*The Nothing itself nothings*' cannot even be expressed in such a language. Still less can we specify the finite set of simple observations statements which would verify the claim that *the Nothing itself nothings*, if, indeed, it were true. It follows, according to Carnap, that '*The Nothing itself nothings'* is meaningless – a pseudo-proposition. The same holds, according to Carnap and the Vienna Circle, for other sentences purporting to express metaphysical truths.

3. Popper on the Demarcation Problem

Popper's views on the demarcation problem differed from those of the Vienna Circle on two fundamental points. First of all Popper denied that metaphysics was always meaningless. I am sure that he would have regarded some metaphysical statements as meaningless, but he also thought that there were metaphysical statements which were perfectly meaningful and were even positively helpful to science. Popper still thought that there was a demarcation between science and metaphysics, but for him it was not a demarcation between sense and nonsense. Secondly Popper proposed that *verifiability* should be replaced by *falsifiability* as the criterion of demarcation between science and metaphysics. I will now deal with these points in turn.

Carnap argues for the meaninglessness of metaphysics by taking '*The Nothing itself nothings*' as his example of a metaphysical statement. Popper argues for his thesis that metaphysics can be meaningful and indeed positively helpful to science by taking as his example the history of *atomism*.

Atomism was first introduced in the West by the pre-Socratic thinkers Leucippus and Democritus. It continued as a powerful trend in the ancient world with Epicurus in Greece and Lucretius in Rome. This ancient atomism must, I think, be classed as metaphysical rather than scientific.

Ancient atomism was revived in Western Europe in the seventeenth century, and discussed by the leading scientists of the day. It should still, at that time, be considered as a metaphysical rather than a scientific hypothesis. At the beginning of the nineteenth century, Dalton reintroduced atomism to solve some problems in chemistry; while half-way through the nineteenth century, Maxwell brought atomism into mathematical physics in connection

with the kinetic theory of gases. By the end of the nineteenth century, atomism can definitely be considered as a scientific hypothesis; but this scientific development would scarcely have been possible without the earlier history of atomism as metaphysics.

Anyone who maintains that metaphysics is always meaningless is faced with some difficult choices in dealing with this example. One option would be to say that atomism was always scientific, and so meaningful; but then it has to be maintained that the atomism of the ancient Greeks was scientific, and this is hardly plausible. The other option would be to say that atomism was meaningless from ancient Greek times until some point in the nineteenth century, when it became scientific and so meaningful; but then it must be held that a perfectly meaningless theory was somehow used by Dalton, Maxwell, and others in order to formulate their scientific and meaningful theories. This again is an absurd and untenable position. Thus there is no escape from the conclusion that metaphysics can be both meaningful and useful for scientific progress. Popper's views on this point are a definite advance on those of the Vienna Circle.

Let us now turn to Popper's second main point, namely his advocacy of *falsifiability* rather than *verifiability*. This is how he himself puts the point (1963, p. 281): "My criticism of the verifiability criterion has always been this: against the intention of its defenders, … *it did exclude the most important and interesting of all scientific statements*, that is to say, the scientific theories, *the universal laws* of nature." We can illustrate this by taking the usual simple example of a universal law of nature, namely: 'all ravens are black'. This is not verifiable by any finite conjunction of observation statements about ravens; but it is falsifiable by observing a white raven. Indeed the similar generalisation 'All swans are white' was falsified in just this way. This simple argument does indeed show that falsifiability is superior to verifiability as a demarcation criterion, and once again justifies the claim I made in the introduction that Popper's contributions to the problems he considered were always a considerable advance on what had gone before.

In the introduction I also claimed that Popper's proposed solutions to philosophical problems, even though they are superior to what went before, almost always contain quite severe flaws. This point applies to the falsifiability criterion. Popper advocates the falsifiability criterion in his *Logic of Scientific Discovery* published in 1934 (see p. 40), and he continued to maintain its correctness right up till is death in 1994, 60 years later. This was despite the fact that some fairly decisive criticisms of falsifiability as a demarcation criterion had been published in the interim. We shall consider these in the next section.

4. Falsifiability and the Duhem-Quine Thesis

The serious objections to falsifiability as a demarcation criterion are based on what is known as the Duhem-Quine thesis. The first objection of this type (based on the work of Duhem rather than Quine) appeared in 1935 in Neurath's paper: 'Pseudorationalismus der Falsifikation'. Further objections along similar lines appeared later, and, in particular, Lakatos subjected falsifiability to an extensive criticism in his 1970 paper: 'Falsificationism and the Methodology of Scientific Research Programmes'. Significantly an appendix to that paper (pp. 93-101) is entitled: 'Popper, Falsificationism and the 'Duhem-Quine Thesis''.

To illustrate this type of criticism let us take as an example Newton's first law. Few would deny that this is an example of a scientific law, and yet it turns out not to be falsifiable by observation.

Newton's first law states that all bodies continue in a state of rest or uniform motion in a straight line unless acted on by a force. Suppose however that we observe a body which is neither at rest nor in uniform motion in a straight line and which is apparently not acted on by any force. Such an observation would seem to refute Newton's first law, but the problem is that it need not be taken as doing so. In such circumstances we can always postulate an invisible force acting on the body. This is exactly what Newton did in the case of the planets, which move in ellipses rather than straight lines. He postulated that these planets were acted on by the invisible force of universal gravitation. A similar device could be applied to any apparent exception to Newton's first law which thus appears to be both scientific and immune to falsification.

The problem here is posed by the Duhem-Quine thesis which Duhem formulated as follows (1904-5, p. 187):

" … the physicist can never subject an isolated hypothesis to experimental test, but only a whole group of hypotheses; when the experiment is in disagreement with his predictions, what he learns is that at least one of the hypotheses constituting this group is unacceptable and ought to be modified; but the experiment does not designate which one should be changed."

Newton's first law is a perfect example of this since it cannot be tested on its own, as an isolated hypothesis, but only as part of a theoretical group. Let us call Newton's first law T. To obtain observable consequences from Newton's theory, we have to add to T:

(1) further theoretical assumptions: namely, the second and third laws of motion and the law of gravity – call the conjunction of these T', and

(2) auxiliary assumptions: for example, that non-gravitational forces have no appreciable effect on the motion of the Sun and the planets, that the mass of the Sun is very much greater than the mass of any planet, and so on – call the conjunction of these A.

Now, from the conjunction T & T' & A, we can indeed deduce observable consequences regarding the motions of the planets. Call one such consequence O. Suppose now that we observe not-O. We cannot conclude 'not-T', but only 'either not-T or not-T' or not-A'. To put the point another way, we know that one of the assumptions used in the deduction is false, but we do not know which one. So none of the assumptions has been falsified. Moreover, as the Duhem-Quine thesis holds for any application of Newton's first law to explain observable phenomena, it follows that this law is unfalsifiable.

Popper did attempt to deal with this difficulty. See, for example, his 1983, p. 187. But his attempts were not very convincing.[1] Here, however, a danger arises which I mentioned in the introduction. It is the danger of throwing out the baby with the bath water. It is perfectly true that there are some laws such as Newton's first law which are indeed scientific but which are not falsifiable because of the Duhem-Quine thesis. This has led some critics of Popper to conclude that no scientific laws are falsifiable for the same reason, and that therefore the concept of falsifiablity is useless and irrelevant for science. Such a conclusion is too extreme in my view. In every branch of science there is a generally acknowledged class of observation statements, that is, of singular statements which can come to be accepted or rejected by the scientific community on the basis of observation and experiment. There are moreover laws and generalisations which can be falsified by a finite set of observation statements. 'All swans are white' was indeed falsified by the observation of black swans in Australia. Kepler's first law that planets move in ellipses with the Sun at one focus could be falsified by a finite set of astronomical observations of a planet which did not lie on an ellipse. In a sense this did happen historically since accurate observations of the planets did show small deviations from an ellipse owing to the gravitational attractions of other planets. However Kepler's first law continues to hold to a high degree of approximation although it could easily be falsified by observing some curious planetary behaviour.

So falsifiablility is part of the story but not the whole story. This suggests that we might divide scientific hypotheses into two levels. Level 1

hypotheses are falsifiable, while level 2 hypotheses, such as Newton's first law, are not falsifiable but still confirmable. Metaphysical statements, however, would be too far removed from the empirical basis to be even confirmable. This amounts to taking confirmability rather than falsifiability as the demarcation criterion separating science from metaphysics.

Popper had, in effect , a 3 level model. The 3 levels being observation statements, scientific hypotheses, and metaphysical statements. My suggestion is to replace this by a 4 level model in which scientific hypotheses are divided into those which are falsifiable and those which are not falsifiable but which are still confirmable. This 4 level model is shown in Figure 1.

Figure 1

Level	*Type of Statement*	*Criterion*	*Example*
3	Metaphysical	Not confirmable	Greek atomism
2	Scientific	Confirmable but not falsifiable	Newton's first law
1	Scientific	Falsifiable and confirmable	Kepler's first law
0	Observation	Truth-value determinable by observation	Statement recording position of Mars at a a particular time

Of course in order to make this approach precise it would be necessary to develop a theory of confirmation, and this is no easy task.[2] I will not undertake it in the present lecture, but will rather discuss two features of the 4 level model which will be useful in our discussion of alternative medicine in the next section.

First of all the 4 level model opens up the possibility of reconciling some of the ideas of Kuhn with those of Popper. Newton's theory is on level 2 which means that while it can be confirmed or disconfirmed by observation, it cannot be directly falsified by observation. Now according to Kuhn, Newton's theory constituted the core of the Newtonian paradigm. Like other paradigms, this was not overthrown simply as the result of

observations but only as the result of the complicated process of a scientific revolution. This is what one would expect of level 2 theories which cannot be knocked out by a single observational blow, but only undermined by a series of disconfirming instances. However there still exist level 1 theories to which Popper's concept of falsifiability and so his methodology of conjectures and refutations can apply.

Secondly let us consider one of these level 1 hypotheses, namely Kepler's first law that all planets move in ellipses with the Sun at one focus. This law can be tested by observing a sequence of positions of a planet and checking whether these lie on an ellipse of the required type. If they do not, the law is falsified. If they do, the law is confirmed. I propose to call this type of confirmation: *direct confirmation*. Now Kepler's first law, in an approximate form, can be deduced from Newton's theory together of course with some auxiliary assumptions. This opens up a channel for some *indirect confirmation* of Kepler's first law. Newton's theory is confirmed not only by observations on the planets, but by observations on the tides, on the motion of pendula, on the motion of projectiles etc. Since these observations confirm Newton's theory and since Kepler's first law in an approximate form is derivable from Newton's theory, it follows that Kepler's first law is indirectly confirmed by these observations on the tides, on pendula, etc. which, prior to the introduction of Newton's theory might well have seemed completely irrelevant to Kepler's laws.

Popper himself makes what is essentially this point but using a different terminology and with a different though related example. He writes (1972, p. 20):

"Thus I assert that with the corroboration of Newton's theory, and the description of the earth as a rotating planet, the degree of corroboration of the statement *s* 'The sun rises in Rome once in every twenty-four hours' has greatly increased. For, on its own, *s* is not very well testable; but Newton's theory, and the theory of the rotation of the earth are well testable. And if these are true, *s* will be true also."

To put the point again, this time in Kuhnian terms that Popper would not of course accept., a law which is derivable from the core assumptions of an accepted paradigm gains indirect confirmation in addition to any direct confirmation which it obtains from being tested out by observation and experiment. Suppose, however, that a level 1 law is tested out and confirmed by observation and experiment, but that, far from agreeing with the core, level 2, theories of the dominant paradigm, it contradicts them. Then the direct confirmation of the law is reduced by the indirect disconfirmation arising out of its disagreement with the dominant paradigm.

This point will be important in our discussion of alternative medicine in the next section.

5. Alternative Medicine

Mainstream medicine is based on a number of sciences such as anatomy, physiology, pathology, bacteriology, virology, etc. Alternative medicine is by no means a unified field. It contains many different approaches, such as aromatherapy, homeopathy, traditional Chinese medicine, traditional Indian medicine (Ayurveda), and so on. However these various approaches have in common that the treatments which they prescribe are not based on any scientific results, but rather on metaphysical theories. Thus, for example, acupuncture is based on the traditional Chinese theory of *qi* (force or energy). Qi theory is clearly metaphysical. Moreover the qi lines on which acupuncture is based, do not agree at all closely with the paths of nerves as accepted by standard anatomy and physiology. This example and other similar ones show that the difference between mainstream and alternative medicine is very closely connected with the demarcation between science and metaphysics.

Some practitioners of alternative medicine do see it as an alternative which should be used instead of mainstream medicine. However there is a more modest position, according to which alternative medicine can add some benefits which are not provided by mainstream medicine, and so should be used in addition to mainstream medicine. This more modest position suggests that the name 'complementary medicine' might be more suitable than 'alternative medicine'. In fact it is common to use the expression 'complementary and alternative medicine' or 'CAM' for short, and I will do so in what follows.

CAM is becoming increasingly popular in western societies. As Parusniková says (2002, p. 169):

"In western societies, complementary and alternative medicine (CAM) has become increasingly popular over the past two decades. The demand for alternative forms of treatment has been rising steadily, with estimates of CAM usage ranging from twenty to forty percent of the populations in the UK and USA. These numbers vary from source to source, but even the conservative estimates are formidably high …"

Some of the detailed estimates of CAM usage are also interesting. As regards the USA, Parusniková quotes the following statistics (2002, p. 184): " … it appears that visits to alternative practitioners increased … by 47%

between 1991-1997 (from 427 to 629 million), 'thereby exceeding total visits to all US primary care physicians'." Moreover (2002, p. 184): "In the major industrial countries, another study found that the highest incidence of CAM users is in Germany and Canada (around 60%), followed by France and Australia (close to 50%)." Similar data are to be found in Borgerson (2005).

This enthusiasm for CAM is somewhat surprising. After all, since scientific medicine got off the ground in the middle of the 19th century, its successes have been truly remarkable. The first breakthrough was the discovery that many of the most serious diseases affecting mankind (cholera, plague, tuberculosis, etc.) were caused by specific bacteria. This opened up the possibility of prevention though hygienic and antiseptic precautions, and, from the 1930s onwards, cure through the use of increasing numbers of antibiotics. The Black Death wiped out between a third and a half of Europe's population in the Middle Ages. Now someone who is infected with the plague bacterium can be cured by a single course of antibiotics.

Surgery has also made quite remarkable progress. The discovery of anaesthetics enabled operations to be performed painlessly – a *sine qua non* for any delicate work. The advances in bacteriology just mentioned led to the introduction of antispetic surgery making surgery much safer than before. As a result a large number of conditions can now be treated routinely which before would have resulted in death or a life of pain and disablement. Moreover medical advances have by no means ended, and recent advances in the study of the human genome are opening up the possibility of curing hitherto intractable diseases. Of course it need hardly be said that scientific medicine has not achieved a 100% success. There are still unpleasant and incurable diseases which lead to death or disablement – though even in these cases some alleviation is often now possible through surgery or the use of drugs. Given, however, the remarkable achievements of scientific medicine, it seems surprising that large sections of the general public are growing disillusioned with it and seeking alternative approaches to dealing with their illnesses. Perhaps there are some sociological factors behind this, but my aim in this lecture is not to consider these, but rather to ask whether the philosophical discussions of the demarcation problem shed any light on what attitude we should take to alternative medicine.

Let us begin then with the Vienna Circle's views. They held that only scientific propositions were meaningful while metaphysics was meaningless. Now we noted that it is characteristic of CAM that its treatments are justified by an appeal to metaphysical rather than scientific theories, so that, for example, acupuncture is justified by the theory of qi. Such justifications would be entirely meaningless according to the Vienna Circle. So it is likely

that these hard-headed science lovers would have dismissed alternative medicine as mumbo-jumbo.

Popper, however, had a different attitude. He held that metaphysics can be not only meaningful but even useful for science. Indeed a metaphysical theory might suggest a scientific conjecture. Popper's views could lead to a much more sympathetic appraisal of scientific medicine. For example the theory of qi, while not scientific, might still be regarded as meaningful and as suggestive of conjectures regarding what kinds of acupuncture treatment might prove effective. However, a Popperian would need to make an important proviso in any such defence of CAM. Any conjecture to the effect that a particular type of treatment might be effective would have to be severely tested to see whether it was corroborated or refuted. Now randomised control trials (or RCTs) are usually considered the best way of testing whether a treatment works. Thus a Popperian would probably require the testing of alternative therapies using RCTs.

In fact in the last decade or so there has been a movement called Evidence Based Medicine or EBM. EBM arranges evidence in a hierarchy with RCTs at the top. Kirstin Borgerson in her 2005 article: 'Evidence-Based Alternative Medicine?' considers how CAM should react to EBM. She argues that there are 3 options (p. 504):

"Alternative medical researchers … can follow accepted EBM standards and design studies according to the demands of the evidence hierarchy. Alternately, they can eschew these constraints in favour of studies that are ranked much lower on the evidence hierarchy such as qualitative studies, cohort studies, and case studies. Finally, they can critically engage with the EBM standards and devise new research designs that more closely reflect the needs and goals of alternative medical practitioners."

Option 1 has indeed been adopted by some researchers. Ted Kaptchuk gives some results as regards acupuncture in his 2002 article (pp. 378-9). It seems that RCTs definitely show that acupuncture is effective for adult postoperative and chemotherapy nausea, and for postoperative dental pain. Some studies indicate that acupuncture gives relief of pain on other diverse conditions, but the evidence here is a bit inconclusive.

This option 1 seems reasonable and is supported by Popper's philosophy. Yet it is challenged by some people. They claim that EBM's stress on RCTs is biased in favour of the pharmaceutical companies. It is true that RCTs are very suited to drug therapy. It is easy to make an inactive placebo pill which looks and tastes exactly like the genuine pill under test. Subjects can be assigned placebo or genuine pill at random and in a double

blind fashion which means that neither the patient nor the doctor knows whether the placebo pill or the genuine pill is being administered. With CAM treatments such a procedure is not so easy. For example, with acupuncture it is not possible to perform a double blind trial.

Another more theoretical objection is that most CAM treatments are much more focussed on the individual and so preclude the group/statistical approach of RCTs. Christine Barry gives homeopathy as an example of this in her 2006 (pp. 2647-8):

"The nature of Homeopathy precludes the straightforward administration of clinical trials to measure it. … Each remedy picture includes multiple physical symptoms in multiple body locations, diverse psychological and emotional states, and aspects of behaviours that are not part of biomedical diagnoses. … Different individuals with the same biomedical diagnosis will be prescribed different remedies, as their symptom and personality picture will likely be different in each case."

There are other objections to RCTs. For example one study shows (quoted by Borgerson on p. 506, Ftnte 1) that "a large percentage (roughly estimated around 80%) of medical treatments currently offered in hospitals and clinics across North America have never been tested by an RCT." Thus higher standards, it is claimed, are unfairly being imposed on CAM treatments. There is also the point that RCTs are expensive to carry out. Pharmaceutical companies have the money to finance such trials, but many CAM enthusiasts are not so rich.

What then about Borgerson's other two options. The second accepts the EBM hierarchy of evidence, but claims that CAM has to use only forms of evidence lower down the hierarchy. However, this suggests that CAM must always be inferior. Borgerson suggests that it is probably a better strategy for CAM to adopt option 3, i.e. to challenge EBM's evidence hierarchy. This line is taken by Barry in her 2006 article. She claims that CAM should be supported by the kind of evidence used by anthropologists. Her idea is that to test for example homeopathy a researcher should join the homeopathy community in the way that anthropological researchers join primitive tribes. Like such anthropologists, homeopathic observers should report what they find. Barry's findings sound favourable to homeopathy. She says (p. 2654):

"Users of homeopathy did not see a need for scientific testing and were happy with their judgement of whether the treatment was working for them."

And again:

"Evidence for them was the evidence of their own eyes and embodied experiences."

The problem with this approach is that users can easily be deceived as to whether a treatment they receive is effective. Suppose they take the treatment and after a period of time, they feel a lot better. They may be convinced that the treatment is very good. However, the body has natural recuperative powers and the improvement may have been entirely owing to these. Indeed the treatment might even have slowed the natural healing process which would perhaps have occurred more quickly without any treatment.

I'd like now to come back to the general discussion of the demarcation problem, and consider the issues to do with the Duhem thesis and the 4 level model. Let us consider a case in which an alternative therapy, perhaps suggested by some metaphysical theory, is actually shown to be effective in randomised trials. Can we say that it is just as acceptable as any science-based therapy? Here there are a number of cases. First of all the illness may be one for which there is no effective science-based therapy. In such a case it obviously sensible to use the alternative therapy, and, indeed, it would be wrong not to do so. Secondly, however, there might be a science-based therapy which performs statistically at about the same level as the alternative therapy. Is there here any reason for preferring the science-based therapy? It is at this point that some considerations involving the 4 level model and Kuhn's idea of a paradigm enter the picture.

A science-based therapy is one whose workings are explained in terms of the dominant paradigms of the medical sciences, or, using another terminology, in terms of the accepted level 2 hypotheses – together perhaps with some auxiliary assumptions. Thus, for example, the effectiveness of penicillin in treating osteomyelitis is explained by the theory that osteomyelitis is caused by the bacterium staphylococcus aureus, and the law that penicillin destroys staphylococcus aureus both in vitro and in vivo. Now, as we pointed out using the example of the derivation of Kepler's first law from Newton's theory, such a derivation of a level 1 law from accepted level 2 laws provides an indirect confirmation of the level 1 law. In the medical case, the level 1 law is that a specific treatment will prove effective in curing a particular disease. If this law is derivable from well-confirmed level 2 laws, this provides some indirect confirmation for it, which is additional to the confirmation it may obtain from success in randomised trials. Thus a science-based treatment can be confirmed both directly and indirectly. An alternative therapy, however, cannot be confirmed indirectly.

Such therapies are justified by metaphysical theories, and such theories cannot be confirmed empirically and so cannot pass on confirmation to their consequences. Indeed such metaphysical theories may lose plausibility by contradicting the well-confirmed core theories of basic science. Thus the qi theory is perhaps rendered less plausible by its failure to agree with the known pathways of the nervous system. So, while a science-based therapy can be confirmed both directly and indirectly, an alternative therapy can be confirmed only directly. Thus if the direct confirmation is the same in both cases, there is still good reason to prefer the science-based therapy.

Having said this, however, there is a further Popperian point which can be made in favour of alternative medicine, and which is indeed stressed in Parusniková 2002. This point regards alternative medicine in relation to medical research and the development of medicine. Parusniková draws attention to a general feature of Popper's philosophy of science, namely that Popper always emphasises the importance of criticism in science. For Popper criticism is constantly needed in science to counter dogmatic tendencies and thus to permit the progress of science through the development of new ideas. Now suppose some alternative therapy is definitely shown to be effective on randomised trials. This may not be a reason for using it. There might be an equally effective science-based therapy. However, the success of an alternative therapy based on principles, which are quite different from and which indeed may contradict those of scientific medicine, should surely be an occasion for reflection. Why should such a therapy work? Is its success explicable in terms of the usual theories of basic science? Or might its success show that some at least of these theories need to be modified? Why, for example, should acupuncture be effective in dealing with postoperative dental pain? There are still many mysteries connected with the nature of pain which are far from being resolved. Thus any successes of alternative medicine could provide a useful stimulus for the development of medical research. As Parusniková puts it (2002, p. 183):

" … the critical attitude would imply that the medical establishment should be open-minded to alternatives and investigate them eagerly.

And if some inspiration can be found in CAM, all the better."

Actually this has happened to some extent. There have been some studies of possible mechanisms of acupuncture in terms not of qi theory but of modern neuroscience. Kaptchuk has this to say (p. 379):

"Extensive research has shown that acupuncture analgesia may be initiated by stimulation, in the muscles, of high-threshold, small-diameter nerves.

These nerves are able to send messages to the spinal cord and then activate the spinal cord, brain stem (periaqueductal gray area), and hypothalamic (arcuate) neurons, which, in turn, trigger endogenous opioid mechanisms. These responses include changes in plasma or cortico-spinal fluid levels of endogenous opioids (for example endorphins and enkephalins) …"

So on this account the subtle pain caused by the needles raises the level of endorphins and enkephalins which in turn overcomes the original pain. Doctors who take this approach may use acupuncture to relieve pain without paying much attention to the traditional acupuncture points based on the supposed meridians and flow of qi.

One of the curious things about evidence based medicine is that it pays little or not attention to such basic-science evidence – that is to what we have called indirect confirmation from level 2 theories. Indeed Borgerson says (p. 504):

"EBM … is meant to replace the "authority-based medicine" of the past, whereby physicians tended to base clinical decisions on basic science (pathophysiology) … rather than on research evidence."

This seems to me a false dichotomy since of course basic science is based on research evidence and in many cases provides indirect confirmation for the effectiveness of treatments. However, again according to Borgerson (p. 511):

" … basic scientific research is currently ranked as lowest on the evidence hierarchy."

This is surely absurd and contrary to the assessments of most doctors. For example, Kaptchuk says (p. 379):

"Numerous surveys show that, of all the complementary medical systems, acupuncture enjoys the most credibility in the medical community … . The RCT research is probably not the main basis for this positive opinion. A more likely reason is the existence of a substantial body of data showing that acupuncture in the laboratory has measurable and replicable physiologic effects that can begin to offer plausible mechanisms for the presumed actions."

The effectiveness of penicillin, as we have pointed out, was very well confirmed by basic scientific research, even though no RCTs were carried

out, and the same may be true of many of the 80% of treatments which have been adopted in North American hospitals without RCTs.

Why does EBM make this remarkable mistake about the nature of evidence? It is tempting to detect the influence of the pharmaceutical companies here. There are many drugs whose effectiveness is confirmed by RCTs but which have no indirect confirmation from level 2 scientific theories. Most anti-depressants fall into this category. It is in the interest of the pharmaceutical companies to say that there is very strong evidence in favour of using such drugs, whereas it would probably be fairer to say that the evidence in favour of using such drugs is rather weak. Indeed there have been many problems with such drugs. If the mechanism of a drug is not understood in terms of basic science, it may well be producing short term benefits at the expense of longer term damage and deterioration.

Notes

[1] For a discussion, see Gillies, 1993, pp. 212-214.

[2] For some preliminary suggestions, see Gillies, 1993, pp. 214-221, but these stand in need of further development.

[3] As well as the published paper 2002, I have benefited by reading some of Parusnikova's unpublished works on this subject and from several discussions with her about CAM.

[4] This was pointed out to me by Zuzana Parusnikova in a comment on an earlier draft of this paper.

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