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Is psychiatric research scientific?

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Lots of people want to know whether psychiatric research is scientific (eg Cawley 1993; Reznek 1991; Faust and Ziskin 1988; Chur-Hansen and Parker 2005). Opinions vary and tempers flare but most participants in the debates have taken two points for granted: First, all assume that asking whether psychiatric research can be scientific is a reasonable question to ask. Second, all take it that whether or not psychiatric research is scientific matters. In this chapter I contest both these claims.

I start by reviewing the work of those philosophers of science who sought a 'demarcation criterion' that would distinguish science from non-science. Philosophers of science now generally agree that the search for a demarcation criterion has failed. However, in other disciplines the search for a means of distinguishing science from pseudoscience continues. I review the current debate in psychology and psychiatry. Then, returning to philosophical work, I discuss and support accounts according to which 'science' is best considered a family resemblance term. This suggests that whether psychiatric research is scientific may be a fuzzy matter, and that psychiatric science might be like more paradigmatic sciences in some respects but unlike them in others. Instead of asking whether psychiatric research is scientific, we would do better to ask more specific questions, such as: What are the forms of explanation used in psychiatry? Are they like those used in other sciences? Are mental disorders natural kinds? Can psychiatric theory be reduced to more fundamental theories?

Furthermore, as 'psychiatric research' is diverse, these questions may themselves be too broad - different areas of psychiatric research plausibly use different forms of explanation, some types of mental disorder may be natural kinds while others are not, and so on. In general the best questions to ask will be very particular. Instead of asking whether psychiatric research is scientific, we should ask questions like: Given what we know about schizophrenia does it seem to be a natural kind? Might cognitive theories of autism be reduced to theories that posit neurological problems?

Finally, I shall briefly consider why it is that the question of whether psychiatry is a science is so popular. If the question is malformed, what accounts for its enduring appeal?

The remainder of the chapter falls into the following sections:

1. Work on demarcation in philosophy.
2. The debate in psychology and psychiatry.
3. Returning to philosophy - 'Science' as a family resemblance term
4. Asking better questions
5. Demanding higher epistemic standards across the board.
6. Why have people wanted to know whether psychiatry is a science?

1. Work on demarcation within philosophy

When considering philosophical work on the demarcation problem we should start with the work of Karl Popper (1963). Karl Popper was one of the greatest philosophers of the twentieth century and wrote extensively on demarcation. Popper was greatly impressed by the physics of his time, and sceptical of other popular contemporary theories, in particular Freudian psychoanalysis. Popper sought a criterion that would demarcate the theories of physics from those of psychoanalysis, and more generally distinguish science from pseudoscience. In his theory of falsification Popper thought he had found what he was looking for. According to Popper, scientific theories are those that can be falsified – that is if they are false it is possible for this to be revealed by empirical evidence. Pseudoscientific theories, on the other hand, are not falsifiable – whatever happens the theory will be able to come up with some explanation.

Using his criterion of falsifiability, Popper classified the theories of relativity theory as science and those of psychoanalysis as pseudoscience. Let's take relativity theory first: Relativity theory can be used to make highly specific predictions, for example it predicts that the light from the stars should

be bent by the mass of the sun. During a solar eclipse it is possible to measure this bending, and, as it happens the observed effect fits with that predicted. For Popper, the noticeable thing about relativity theory is that it commits itself to making specific empirical claims. In doing this it runs the risk of being shown wrong by events. The theory is falsifiable and so counts as science according to Popper. On the other hand, Popper claims psychoanalysts have an answer for every eventuality – if an analysand accepts an interpretation then it's proved right, but if they deny it the analyst can simply excuse this by saying that they are in denial. As Popper thinks that psychoanalysis cannot be proved wrong, he considers it to be unfalsifiable and so pseudoscience.

Popper's demarcation criterion is still widely discussed in medical and lay debates about the scientific status of psychiatry and psychoanalysis (for example, Grant and Harari 2005), but has now largely been abandoned by philosophers. Philosophers have rejected Popper's demarcation criterion because it runs into various well-known problems, perhaps the greatest of which is the Quine-Duhem problem (a similar if not identical problem is also sometimes known as 'the experimenters' regress') (Duhem 1954, Quine 1961, Collins and Pinch 1993). The problem is that theories only make concrete predictions in concert with various auxiliary hypotheses. Popper says that scientists should come up with theories and then see if experiments falsify them. However when an unexpected observation is made, although this might be because the theory is false, it might be because something has gone wrong elsewhere - maybe some part of the experimental apparatus has broken, or maybe the statistics have been analysed incorrectly. Even when all imaginable sources of error have been checked out the theory may still actually be true in spite of the results not being as expected. This is because in some cases factors that are as yet unknown might be interfering with the results.

A consequence of the Quine-Duhem problem is that a theory can never be conclusively falsified. When an experiment gives an unexpected result the problem may always lie somewhere other than the theory. Popper knew about the Quine-Duhem problem and modified his account to deal with it. He says that good scientists should agree before an experiment which auxiliary hypotheses they will check if they face an unexpected result (so, they agree to check the wires haven't fallen off and to repeat any statistical analyses, but they don't resort to positing that unknown factors are interfering with their results) (Popper 1959 §20). However, Popper's modified account is much less attractive than the simple criterion of falsification. With the modifications, distinguishing between science and pseudoscience is no longer a simple matter. It no longer depends simply on the question of whether a theory makes empirical predictions that might turn out to be false, but also on whether the

practitioners have the correct ‘scientific’ outlook – whether they only check auxiliary hypotheses to an extent that is ‘reasonable’.

Of course, other philosophers of science have proposed demarcation criteria apart from Popper. Perhaps most influentially Thomas Kuhn suggested that a field becomes a science when it finds its first ‘paradigm’ (1970). A Kuhnian paradigm is a commonly agreed framework for doing research in some area. Scientists who share a paradigm will agree on the questions that should be addressed, on the methods that should be used, and on what counts as good research in some area. Once a field has found its first paradigm scientists in that area can begin to do ‘normal science’ – that is puzzle solving within the taken for granted framework of the paradigm. So, physicists working within the paradigm of Newtonian mechanics, for example, can spend time worrying about how to apply the laws of mechanics to collisions involving multiple bodies or the flow of liquids, but they never question the fundamental correctness of the Newtonian framework. The puzzle solving that is characteristic of normal science practiced within a paradigm is of central importance for Kuhn because it is what enables a science to make progress.

Kuhn’s demarcation criterion is also inadequate, however. As Paul Feyerabend points out, puzzle solving within some commonly accepted framework is not solely characteristic of science (1970). For example organised crime shares this feature – once a criminal mastermind has come up with something that works, other criminals content themselves with developing variations on the theme. Similarly, theologians, and philosophers working within particular schools, work on intellectual puzzles set out within a particular intellectual tradition.

2. The debate in psychology and psychiatry

Philosophers have now fairly much given up on demarcating science from pseudoscience, but the demarcation problem remains a live issue in other areas of the academy. In psychology in particular, a large body of work addresses the issue of what it is that makes work scientific or pseudoscientific. In recent years a number of writers within psychology have become concerned that psychology is being brought into disrepute by practitioners who engage in pseudoscience. Some worry about the range of dubious therapies that are now offered to the public (Lilienfeld, Lynn and Lohr 2003).Others think that the debates over Multiple Personality Disorder, the detection of child abuse, and the possibility of recovered memories, are suspect and have led the field into crisis (Tavris 2004).

These concerns have led to an attempt within psychology to identify science and pseudoscience and to cleanse psychology of its more dubious theories and practitioners: A number of books on this theme have been published, most notably Lilienfeld, Lynn and Lohr's (2003) *Science and Pseudoscience in Clinical Psychology*; a new journal, *The Scientific Review of Mental Health Practice* (SRMHP), has been set up with the explicit aim of distinguishing the scientific wheat from the pseudoscientific chaff in clinical psychology, psychiatry and allied disciplines; there have been calls for psychology undergraduates to be taught courses that will help them to distinguish science from pseudoscience (Lilienfeld, Lohr and Morier 2001).

In contrast to the earlier philosophical attempts at demarcation, in the psychological literature most writers have not sought to define science or pseudoscience in terms of necessary and sufficient conditions. Rather writers have tended to provide lists of criteria that make a field more or less scientific. These lists are highly reminiscent of the lists of symptoms that are used to identify psychopathology. In the same sort of way that a patient with a number of depressive symptoms is considered more likely to suffer from depression, a field that shows a number of specified signs is considered more likely to be a pseudoscience.

Different writers provide various different lists of the symptoms of pseudoscience and science. Tavris (2003) notes that in contrast with pseudoscience, sciences tend to characterised by a willingness to question received wisdom, and rely on gathering empirical evidence to determine whether a prediction or belief is valid. In addition she notes that scientific claims tend to be falsifiable in Popper's sense.

Lilienfeld, Lohr and Morier (2001, p.182) provide a longer list of criteria. They think that the difference between science and pseudoscience in a difference of degree rather than kind, but think that pseudoscientific claims often have a number of markers.

> Among these characteristics are (a) unfalsifiability (Popper, 1959), (b) absence of self-correction (Herbert et al., in press), (c) overuse of ad hoc immunizing tactics designed to protect theories from refutation (Lakatos,1978), (d) absence of 'connectivity' (Stanovich, 1998, p.116) with other domains of knowledge (i.e., failure to build on extant scientific constructs; Bunge, 1967), (e) the placing of the burden of proof on critics rather than on the proponents of claims (Shermer, 1997), (f) the use of obscurantist language (i.e., language that seems to have as its primary function to confuse rather

than clarify; Hockenbury & Hockenbury, 1999; van Rillaer, 1991), and (g) overreliance on anecdotes and testimonials at the expense of systematic evidence (Herbert et al., 2000).[1]

The point of such lists is to enable pseudoscience to be identified and done away with, and indeed a number of papers have used such lists to condemn specific practices as pseudoscientific. For example, Herbert, Lilienfeld, Lohr et al (2000) examine the technique of 'Eye Movement Desensitization and Reprocessing (EMDR) a supposed treatment for anxiety disorders. They spot the symptoms of pseudoscience and thus dismiss the technique. Similarly, papers in Lilienfeld, Lynn and Lohr's (ed) (2003) *Science and Pseudoscience in Clinical Psychology* use lists of criteria that make a field pseudoscientific in their attempt to root out dubious therapies.

Distinguishing scholarly research from rubbish is important. However, I shall argue that it is a mistake to try and diagnose pseudoscience in way analogous to the way that a clinician diagnoses psychopathology. Looking at Lilienfeld, Lohr and Morier's criteria I have several worries. Firstly, it is unclear how the criteria are to be weighted: Is obscure language as big a sin as unfalsifiability? How is a reliance on anecdotes to be weighted against an absence of 'connectivity'?

My more serious worry is that considered individually each of the criteria is insufficient to indicate that a discipline has problems. Depending on the context, unfalsifiability, or obscure language, or meeting any of the other criteria may not be indicative of pseudoscience. I will show this by considering each criterion in turn.

a. Unfalsifiability – as mentioned earlier the Quine-Duhem problem means that conclusive falsification is always very difficult (if not impossible). In addition, as pointed out by Larry Laudan, certain statements that are surely scientific are not candidates for falsification. In

[1] In a later book *Science and Pseudoscience in Clinical Pyschology* (2003) Lilienfled, Lynn and Lohr present the same list of criteria but also add:

h. evasion of peer review (2003, p. 6)
i. emphasis on confirmation rather than refutation (p.7)
j. absence of boundary conditions – they claims that 'most well-supported scientific theories possess boundary conditions, that is, well-articulated limits under which predicted phenomena do an do not apply (p.9)
k. the mantra of holism 'proponents of pseudoscientific claims…typically maintain that scientific claims can be evaluated only within the context of broader claims and therefore cannot be judged in isolation' (p.9)

particular many existential statements are unfalsifiable. For example, 'There was a missing link between apes and humans', 'There are black holes'. (Laudan 1996 p.218).

b. **Absence of self-correction –** sometimes a field will not self-correct because it is right – thus geometrical optics has not progressed for centuries.

c. Overuse of ad hoc revisions – this criterion is of little use for spotting pseudoscience. Revisions will only be considered to be 'over-used' and 'ad hoc' if they are independently considered problematic.

d. Absence of 'connectivity' – many scientifically accepted phenomena cannot be connected into a satisfactory world theory because there is as yet no understanding of how they occur. For example, there as yet there is no commonly accepted way of making sense of EPR correlations in quantum mechanics.

e. **Placing the burden of proof on critics –** oddly this criterion conflicts with (a) unfalsifiability. The falsificationist thinks that a scientist should be free to propose any hypothesis so long as it has empirically testable consequences, and that the task of science is then to seek to demonstrate the falsity of the theory. In lay terms, Popper's theory of falsificationism places the burden of proof on critics!

f. Use of obscurantist language – again this criterion is of little use – whether language is considered 'obscurantist' or of 'necessary theoretical complexity' depends on whether one considers the theory plausible or not.

g. **Over reliance on anecdotes and testimonials –** Again, only someone who considers a theory problematic will think that it displays an 'over-reliance' on single case studies. Depending on the question, the use of single case studies may be appropriate. Although a single case study is seldom sufficient to show that an effect will always, or even normally, occur, it often will be adequate for showing that something is possible. Take for example, the much reported single case of D.F. – the woman who can guide her hand through a vertical or horizontal slot without having conscious awareness of the slot's orientation (Goodale and Milner 1996). This single case is sufficient to make it conceivable that vision for action and for recognition can come apart.

I conclude that each of the seven criteria fails when considered individually. At this Lilienfeld and colleagues may not be too distressed. Their criteria are not intended to offer a fool-proof way of spotting pseudoscience when considered individually, but are instead taken to be diagnostic of a problem when they co-occur in a particular field. Similarly, sadness when considered alone may not

be indicative of depression but if it lasts for a considerable time and co-occurs with psychomotor retardation and sleep-problems then it suggests that there is a problem.

However, claiming that pseudoscience is only to be suspected when a certain number, say four, of the seven criteria are met will not do. I will argue that in some cases a claim may not meet any criteria and yet still plausibly be 'pseudoscientific'. Conversely, in other cases a claim may meet multiple criteria and yet still be scientifically respectable. Here I will discuss each possibility in turn.

Lilienfeld et al's criteria give rise to false negatives.

In some cases we may want to consider a claim pseudoscience even though it fails to meet any of the seven criteria. Consider the field of ufology – the study of UFOs. Some practitioners in the field are keen to meet standards of scientific acceptability. They publish in peer-reviewed journals, avoid obscure language, and pride themselves in seeking to test their claims empirically and applying standard statistical tests of significance (Cross 2004). Let's suppose that an ufologist is investigating the trauma that can be suffered by 'alien abductees'. The researcher suspects that different types of alien have different types of transport and are differentially likely to harm abductees. Such a researcher may come up with the following hypothesis:

> People who have been abducted by UFOs with flashing lights are more likely to suffer post-abduction trauma than those who are abducted by UFOs with constant lights.

The ufologist's hypothesis is not obscure. It is falsifiable. The ufologist may seek to assess the claim empirically, for example by studying a sample of abductees and examining whether there is a statistically significant correlation between reports of flashing lights and trauma. Keen to adhere to the standards of scientific acceptability the ufologist may send the resulting paper off to an appropriate peer-review journal – *The Journal of UFO Studies*, say.

The ufologist fails to meet Lilienfeld et al's criteria for pseudoscience. And, yet, I suggest, the odds are that they are a crank. The fundamental problem isn't methodological, however. The basic problem is that a fundamental assumption on which their research is based – that aliens frequently come to earth – is highly unlikely to be true. As the methods employed by ufologists are similar to those used by reputable scientists and yet ufology is a paradigmatic example of pseudoscience, I

conclude that displaying Lilienfeld et al's hallmarks of science is not sufficient to render a field scientific.

Lilienfeld et al's criteria give rise to false positives.

Conversely, some claims that we'd normally consider scientifically respectable meet several of Lilienfeld et al's criteria for pseudoscience. Consider the following claim:

There are brocken spectres.

Brocken spectres are rare phenomena. They sometimes occur when a mountaineer gets above the clouds and their shadow is cast on the clouds below creating the appearance of a spectre. The claim 'There are brocken spectres' is unfalsifiable (if I go looking for a broken spectre and don't see any this doesn't mean that they don't occur). The language is somewhat obscure. The evidence for the existence of brocken spectres is anecdotal and testimonial. There is an 'absence of self-correction'. At least until it was discovered what caused brocken spectres there was an 'absence of connectivity' with other scientific claims. And yet brocken spectres do occur, and the claim is scientifically respectable despite the fact that it displays many of Lilienfeld et al's signs of pseudoscience. Applying Lilienfeld et al's criteria gives rise to false positives.

We have already seen that Lilienfeld et al's criteria cannot distinguish science from pseudoscience when applied singly. Now we have seen that they cannot distinguish science from pseudoscience when applied in concert either.

3. Returning to philosophy: Science as a family resemblance term

Recent philosophical work presents a picture of the distinction between science and pseudoscience that is somewhat similar to that presented in the psychological literature. In philosophy too, attempts to define 'science' and 'pseudoscience' in terms of necessary and sufficient conditions have been largely abandoned. Here, though, the similarity ends. Philosophers and psychologists tend to draw quite different consequences from the idea that 'science' cannot be precisely defined. Psychologists have sought to use lists of possible symptoms to diagnose pseudoscience. Philosophers have tended to conclude that seeking to demarcate science from pseudoscience is a

mistake. Here I examine in greater detail recent literature from the philosophy and history of science and then ask why it is that the projects of psychologists and philosophers have diverged.

Recently a number of philosophers of science have suggested that seeking a demarcation criterion is misguided. Rather than it being possible to define 'science' in terms of necessary and sufficient conditions, these philosophers suggest that 'science' is best conceived of as a family resemblance term (Dupré 1993, Pickstone 2000) in the sense introduced by Ludwig Wittgenstein. In the *Philosophical Investigations* Wittgenstein asks us to consider what games have in common. He suggests that there is no one feature that they all share. Many games are competitive, but some are not (ring-a-ring-a-roses). Some games are fun, but others are not (I Spy is quite dull). Many have rules, but some don't (playing cops and robbers). Most involve other people, but some can be played alone (patience). Wittgenstein concludes that:

> if you look at them you will not see something that is common to all, but similarities, relationships and a whole series of them at that…I can think of no better way to characterise these similarities than 'family resemblances'; for the various resemblances of a family: build, features, colour of eyes, gait, temperament, etc etc. overlap and criss-cross in the same way – And I shall say: games form a family (1953: §66)

Similarly, the various sciences can be seen as forming a family. There is no one feature that they all share, but rather a network of resemblances unites them. Many sciences make predictions, but others do not. Some posit unobservable entities, some do not. Some, but not all, involve experiments. The claim that there is no one feature that all sciences have in common is made all the more plausible then we consider the wide range of activities that are classed as 'science' The activities of theoretical physicists, evolutionary biologists, natural historians, game theorists, epidemiologists, neuropsychologists, and materials scientists all get classed as scientific. However plausibly there is no one feature that all these areas share.

John Pickstone's (2000) *Ways of Knowing* develops the thesis that there are many different types of scientific activity. Different sciences in different times seek to understand the world in different ways. Some sciences have depended on natural history – the describing and classification of things. Others have focussed an analysis – looking within phenomena and breaking them down to fundamental elements. Others have learnt through experiment – they have sort to control phenomena and to create novelties. Pickstone thinks that methods come to be used in different areas

at different times, and that the importance of the different methods varies with subject and period. Looking to the future he expects new methods to become available – he cites simulation as a method that may hold future promise.

Pickstone's picture of science stresses the diversity of the activities that we consider 'scientific'. Arguing along similar lines, Larry Laudan concludes that,

> The evident epistemic heterogeneity of the activities and beliefs customarily regarded as scientific should alert us to the probable futility of seeking an epistemic version of a demarcation criterion (1996 p.221)

What's more, such a view also brings out that the same methods that are used within the sciences can also be used in fields that we would not normally class as 'sciences'. Consider the skills used to analyse statistical data – the same skills of sampling and extrapolation are needed by the market researcher, or the ecologist, or in social science. Similarly the skills of the natural historian – collecting and classifying – can be employed by those who order collections of beetles, as well as by those who order collections of wine.

One moral that I think we can draw from this picture of science is that asking whether a field is scientific will seldom be the right question to ask. As Pickstone shows us, the methods that are used in the sciences are often also employed elsewhere, and whether a field gets counted as 'science' or not often depends on contingent historical factors. Thus, instead of asking 'Is psychiatric research scientific?', we would do better to ask questions like 'What methods have been used in addressing the research question?' and 'Are these methods generally reliable and appropriate in the case at hand?'.

Writing in the psychological literature, McNally has drawn much the same conclusion. As he puts it,

> Rather than asking, Is this pseudoscience or genuine science? we should ask, What arguments and evidence support this clinical claim? We should be concerned with belief-worthiness, epistemic warrant, evidential basis, empirical support (pick your favourite locution), rather than attempting to determine whether the theory or practice falls on the

proper side of a demarcation criterion that separates science from pseudoscience' McNally 2003 p.4

4. Asking better questions

I conclude that science is a family resemblance term and that asking whether psychiatric research is scientific is thus not a very useful question. Rather than asking whether psychiatry is a science we would do better to ask finer grained questions: What methods does psychiatry use to explain and predict phenomena? Are these methods generally reliable? Are they appropriately applied in psychiatry? What's more, although these questions are better questions than asking 'Is psychiatric research scientific?' we would do yet better to get yet finer-grained. Psychiatry is a diverse field – the sorts of questions that research psychiatrists ask and the methods that they employ vary wildly across different areas. Consider, the sorts of research that are published in psychiatry journals

- Randomised Controlled Trials to test drugs and psychological treatment efficacy
- Papers that compare rates of disorder with other factors and examine, for example, whether place of residence, or drug abuse, increases the risk of disorder
- Papers that assess patient satisfaction with facilities
- Papers that look at the physiological, or neuropsychological, or cognitive correlates of disorder.
- Brain imaging studies
- Cluster-analytic studies
- Studies that assess whether diagnostic criteria can be reliably applied.

And, of course, this list is not exhaustive. It merely acts as a reminder of the range of research conducted by psychiatrists. The methods used by research psychiatrists are diverse. Some of the methods used are similar to those used in other disciplines. So, for example, the statistical methods

employed in RCTS were originally developed for comparing seed varieties and fertilisers in agriculture. The methods used in cluster analysis were first used by biologists – especially bacteriologists. Other methods may be unique to psychiatry.

Depending on the methods employed, different questions may be asked about the research at issue. Some questions will be largely empirical or mathematical – for example determining how large a sample needs to be before statistical methods will yield robust results. Others questions about methods may be philosophical. For example, the methodology used in RCTs commits one to certain views about the foundations of probability theory, and whether these assumptions are acceptable is a question that is explored by philosophers (amongst others) (Ashcroft 2004). As another example, elsewhere I have argued that certain interpretations of cluster analytic studies commit one to certain views about the nature of properties (Cooper 2005). The sociology and history of science also have work to do if we want to know which methods are likely to produce reliable results under which circumstances. For example, the history of psychiatry shows us that new treatments have often been introduced by over-enthusiastic proponents, and that the reports of the initial users of a treatment cannot be relied on for assessing treatment efficacy.

Not only are the methods that psychiatrists employ diverse, so too are the conditions that they research: Some psychiatric research examines genetically caused disorders like Down's Syndrome. Other work looks at conditions that appear clearly pathological but where no biological cause is known, for example schizophrenia. Some disorders appear to vary radically with social environment, for example Multiple Personality Disorder. Some psychiatric research examines conditions that are not clearly pathological at all, but that might be normal under certain conditions, for example, suicidal thoughts, or hearing voices.

Depending on the type of condition under consideration very different conceptual issues are raised. In the case of suicidal thoughts one might worry about the relevance of Donald Davidson's claims that rational thoughts are necessarily holistically connected (Davidson 1970). Maybe this means that rational thoughts of suicide will resist reduction, for example. Here we will not get drawn into considering the acceptability and implications of Davidson's claims, but will just note that any such concerns could only affect rational thoughts, and would leave work on non-rational mental states, such as feelings of panic or nightmares untouched. Other worries specifically concern disorders that appear subject to 'looping effects', in the sense introduced by Ian Hacking (1995a, 1995b). The symptoms of certain disorders appear to be affected by our descriptions of them – so the symptoms

of Multiple Personality Disorder have plausibly altered as the media presentations of the condition have changed. Disorders that display such effects pose specific problems. Hacking argues that in such cases epidemiological studies will make little sense, for example, as the disorder is necessarily a moving target that shifts as it is studied. Once again, such worries are specific. Certain conditions and certain conditions only can be expected to be affected by such concerns.

Thinking about the diversity of research methods that psychiatrists employ and the range of phenomena that they examine makes it plausible that there will not be very much that one can say about the standing of psychiatric research in general. If there are conceptual problems with the use of certain methods, or with certain types of investigation into certain conditions, then there is no need to think that this implies there will be problems with other types of psychiatric research.

5. Demanding high epistemic standards across the board

If we cease to consider the distinction between science and non-science as being of any importance we can concentrate instead on more significant questions – such as how we can best find out truths. In all areas where people purport to be seeking truths they should be expected to conduct their research in epistemically responsible ways. Most obviously this will lead us to demand more from research in areas that have been traditionally regarded as non-scientific. For example, at present when a philosopher or historian presents figures and discusses trends they can just give tables of raw figures (as I do in Cooper 2005, p116, for example). However, we may well decide that statistical techniques that are good in fields traditionally regarded scientific are also good elsewhere, and demand that all of those who discuss 'trends' supply robust evidence that the trends under discussion are actually statistically significant. Similarly, the methods of systematic review might usefully be employed in disciplines outside the traditional sciences.

However, it's not only 'non-scientific' work from which we should expect more. In the non-sciences we accept that the epistemic status of many claims depends on the personal integrity of the expert in question. So, if we want a builder's opinion on a job we will typically worry a lot about whether the builder we ask is honest. We are more likely to trust someone who we know and with whom we are likely to have continued contact, we may ask for multiple quotes and compare them, we may seek advice from someone who has nothing to gain personally from the answer that we are given.

In science, on the other hand, I suggest that the thought that there is a 'scientific method' which turns raw data into reliable facts has led to a neglect of the question of personal trustworthiness. We have a picture whereby the 'scientific method' works well regardless of who the researchers are or what their interests might be. However the idea that 'the scientific method' can operate regardless of the personal integrity of researchers should be rejected. Work by historians and sociologists of science has shown that it is an essential part of scientific practice that scientists make judgements about the trustworthiness of different practitioners. Harry Collins, for example, has provided numerous cases where judgements as to whether an experiment has been performed correctly ultimately come down to judgements of the personal integrity of the researchers involved (Collins 1985). Science cannot work unless the scientific community can decide which results to believe and so judgements of trustworthiness play an essential role in the enterprise. For those who are unconvinced by Collins' work it is also possible to argue that the allegiances of researchers matters more directly: Take the data that is produced by Randomised Controlled Trials in psychiatry. A RCT is 'scientific', if anything is. However, numerous studies have shown that the results that are found by an RCT depend on who is paying the bill. Researchers who are paid by drugs companies find results that favour drugs companies (Perlis et al 2005, Baker et al 2003). Here we not need to consider the mechanisms via which this occurs, we just need to note that it happens. The fact that findings vary with the researchers' allegiance is sufficient to show that the interests of researchers matter. As such, in the same way that we seek out independent advice in non-scientific fields we should also expect scientific researchers to be financially disinterested. In non-scientific fields we expect that those who review products, for example the people who test kitchen implements for consumer magazines, should have no financial incentive to deliver one judgement above another. We should extend the same demands to those who test drugs.

6. Why have people wanted to know whether psychiatry is a science?

In this chapter I have argued that the question 'Is psychiatric research scientific?' is the wrong question to ask. What then accounts for its enduring appeal? I suggest that the attraction of asking whether psychiatric research is scientific has two explanations.

First, many people genuinely want to know what to make of psychiatric research. They want to know whether psychiatrists should be treated as experts or charlatans. Asking whether such research is scientific has been seen as a way of addressing this question. Roughly, if psychiatric research is scientific, then, it has been assumed, researchers in psychiatry should have the same expert status as

other scientists – their findings will be trustworthy. If psychiatry is only a pseudoscience on the other hand, then what research psychiatrists say can be ignored. However, once one considers the diversity of psychiatric research and the diversity of the conditions that psychiatrists examine the idea that one might reach a conclusion as to whether the whole lot is science (and so presumably decent stuff) or pseudoscience (and so presumably not worth listening to) seems laughable. Working out whether psychiatric research is empirically and conceptually sound will of necessity be a piece-meal and difficult job.

The second reason why asking whether psychiatry is a science has had broad appeal is that it has played a key role in the rhetoric associated with boundary drawing (as noted by Still and Dryden 2004). When two parties wish to dismiss each others claims, for one side to denounce the other as 'pseudoscience' operates as a way of rubbishing particular positions. Thus the whole of psychiatry or psychology is denounced as pseudoscience by parties opposed to these disciplines, such as scientology. Or, at a finer grain, particular theories within psychology are denounced as pseudoscience by theorists who wish to exclude certain researchers. Calling an area pseudoscience has acted as a battle-cry. In this chapter, however, I have argued that 'science' cannot be defined and is not necessarily epistemically superior to 'non-science'. As such the temptation to praise a field as 'scientific' or denounce it as 'pseudoscience' is not helpful and should be resisted.

7. Conclusion

In this chapter I have argued that 'science' is a family resemblance term. Science has no defining features and asking whether a field is scientific is a red herring. Instead we should ask more specific questions. We need to ask whether the methods used to address specific questions are appropriate. The research methods used by psychiatrists are diverse; so too are the conditions that they investigate. As such, there is likely to be little that can be said about the epistemology of psychiatry in general. The best questions to ask will be very particular. Instead of asking whether psychiatry is a science, we should ask questions like 'Given what we know about schizophrenia does it seem to be a natural kind?' or 'Might cognitive theories of autism be reduced to theories that posit neurological problems?'. If we want to know which psychiatric claims to believe and how to interpret them, there are no short-cuts that can be made. Psychiatric research tackles diverse problems in diverse ways and assessing whether it is empirically and conceptually sound will be a piece-meal and difficult task.

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References:

Ashcroft, R. (2004) Current epistemological problems in evidence based medicine. *Journal of Medical Ethics.* **30**, 131-5.

Baker, C., Johnsrud, M., Crismon M. et al (2003) Quantitative analysis of sponsorship bias in economic studies of antidepressants. *British Journal of Psychiatry.* **183**,498-506.

Cawley, R.H. (1993) Psychiatry is more than a science. *British Journal of Psychiatry*. **162**, 154-160

Chur-Hansen, A. and Parker, D. (2005) Is psychiatry an art or a science? The views of psychiatrists and trainees. *Australasian Psychiatry*. **13**, 415-418

Collins, H. M., (1985) *Changing Order: Replication and Induction in Scientific Practice*, Beverley Hills & London: Sage.

Collins, H. and Pinch, T. (1993) *The Golem – What Everyone Should Know About Science*. Cambridge: Cambridge University Press.

Cooper, R. (2005) *Classifying Madness.* Dordrecht: Springer.

Cross, A. (2004) The flexibility of scientific rhetoric: A case study of UFO researchers. *Qualitative Sociology*. **27**,3-34.

Davidson, D. (1970) Mental Events, reprinted in Davidson (1980) *Essays on Actions and Events*. pp207-227 (Oxford: Oxford University Press)

Duhem, P. (1954) *The Aim and Structure of Physical Theory*. Princeton, NJ: Princeton University.

Dupré, J. (1993) *The Disorder of Things.* Cambridge, MA: Harvard University Press.

Faust, D. and Ziskin, J. (1988) The expert witness in psychology and psychiatry. *Science*. 241. No 4861. pp31-35

Feyerabend, P. (1970) Consolations for the Specialist, in. I. Lakatos and A Musgrove (eds.) *Criticism and the Growth of Knowledge* pp197-230. (Cambridge: Cambridge University Press).

Fox, R. (1996) Charlatanism, Scientism, and Psychology's Social Contract. *American Psychologist*. **51**, 777-784

Goodale, M. and Milner, A. (1996) Separate Visual Pathways for Perception and Action. In *Human Cognitive Neuropsychology: A Textbook with Readings*.

Grant, D. and E. Harari (2005) Psychoanalysis, science and the seductive theory of Karl Popper. *Australian and New Zealand Journal of Psychiatry*. **39**, 446-452.

Hacking, I. (1995a.) *Rewriting the Soul.* Princeton: Princeton University Press.

Hacking, I. (1995b.) The looping effects of human kinds. In D. Sperber, D. Premack and A. Premack (eds.) *Causal Cognition.* pp.351-394. (Oxford: Clarendon Press)

Herbert, J., Lilienfeld, S., Lohr J. et al (2000) Science and pseudoscience in the development of eye movement desensitization and reprocessing: Implications for Clinical Psychology. *Clinical Psychology Review*. **20**, 945-971

Kuhn, T (1970) *The Structure of Scientific Revolutions*. Chicago: University of Chicago Press.

Laudan, L. (1996) *Beyond Positivism and Relativism*. Boulder, Colorado: Westview Press.

Lilienfeld, S., Lohr, J. and Morier, D. (2001) The teaching of courses in the science and pseudoscience of psychology: useful resources. *Teaching of Psychology*. **28**, 182-191

Lilienfeld, S., Lynn,S. and Lohr, J. (2003) *Science and Pseudoscience in Clinical Psychology*. New York: The Guilford Press.

McNally, R. (2003) Is the pseudoscience concept useful for clinical psychology? *The Scientific Review of Mental Health Practice. 2* available at http://www.srmhp.org/0202/pseudoscience.html (accessed 2 Jan 2008)

Perlis, R., Perlis, C., Wu, Y. et al (2005) Industry sponsorship and financial conflict of interest in the reporting of clinical trials in psychiatry. *American Journal of Psychiatry*. **162**, 1957-60.

Pickstone. J. (2000) *Ways of Knowing.* Manchester: Manchester University Press.

Popper, K. (1959) *Logic of Scientific Discovery*. London: Hutchinson.

Popper, K. (1963) *Conjectures and Refutations*. London: Routledge and Kegan Paul

Quine, W.V.O. (1961) Two dogmas of empiricism. In his *From a Logical Point of View*. Pp. 20-46. New York: Harper and Row.

Reznek, L. (1991) *The Philosophical Defence of Psychiatry*. London: Routledge.

Still, A. and W. Dryden (2004) The social psychology of pseudoscience: A brief history. *Journal for the Theory of Social Behaviour*. **34**, 265-290

Tavris, C. (2/28/2003) Mind games: Psychological warfare between therapist and scientists. *Chronicle of Higher Education*. 49 available at http://www.psych.utoronto.ca/users/pgsa/Course%20Readings/Clinical%20Issues/Mind%20Games.pdf (accessed 2 Jan 2008)

Wittgenstein, L. (1953) *Philosophical Investigations.* Oxford: Blackwell.