Knowledge, Constructivism and African Scholarship[1]

David Spurrett

School of Philosophy and Ethics

spurrett@ukzn.ac.za

Introduction

In the lecture from which this piece is extracted I took on three tasks: First to sketch some arguments in favour of a *naturalist* conception of philosophy, defending the conclusions that philosophy is not an autonomous enterprise, so that it had better be continuous with scientific enquiry if it is to get anywhere. (A supplementary claim here was that the natural and social sciences should be viewed as more integrated than they usually are.) Second, to offer some reasons for rejecting all identifiable forms of social constructivism about knowledge. Finally, to say something about what 'African Scholarship' might mean, given the preceding considerations. There I briefly defended the claim that there is no epistemically interesting sense in which there *is* such a thing as African knowledge.

In this version I drop the first task, since most of the other arguments don't specifically depend on naturalism about philosophy. That said, it may help some to know that the position I defend is broadly Quinean, and that I mostly endorse Quine's slogan that "philosophy of science is philosophy enough", that is that philosophy of science is all the philosophy we need (Quine 1953[1966]: 151). Mostly what I'll say here is concerned with knowledge.

It's surely worth having some definite idea what knowledge is, especially given all the enthusiastic rhetoric we so often hear about universities as 'centres of knowledge production', imperatives to do justice to 'indigenous knowledge' and so forth.[2] Too often discussion seems to proceed either in the absence of *any* conception of knowledge, or along with at least tacit endorsement of some kind of social constructivism about knowledge. Social constructivists think that what's true, or what beliefs (if any) are justified, depends in important and unavoidable ways on social contingencies, and in so doing deny the traditional conception.[3]

Let me say a little more about the traditional conception of knowledge, to clarify what it involves, to introduce some unavoidable terminology, and to identify the start of some threads that I hope will run through the rest of this lecture. What does it mean to say that knowledge is justified true belief? Each of the components sounds simple enough, but philosophers have found it difficult to give satisfactory accounts of all of them.

---

[1] This piece is based on, and extracted from, my inaugural lecture 'Philosophy Enough' delivered in the Howard College Theatre on 2 April 2008.

[2] The official mission of UKZN is to be 'The Premier University of African Scholarship'.

[3] Some of reality is of course socially constructed – culture, relationships, etc. But it isn't philosophically *interesting* to say that those are socially constructed, and it's mistaken to infer that the fact that they are demands revision of the traditional conception of knowledge.

Belief is a mental state, in particular a representational one.[4] When we believe something we are of the view that something *is* the way our belief represents it to be. What we believe is often some putative fact about the 'external world'[5] – from banalities like that it is raining to more specialist claims like that type 1 diabetes arises when pancreatic beta cells are damaged by the immune system, and fail to produce enough insulin. But beliefs have wider range – we can, among other things, have beliefs about formal matters such as that zero is the only number that is both real and imaginary,[6] or constructed worlds, for example that Hamlet's father is dead. It's close to true by definition that we *think* our beliefs are true – saying 'I believe X' and 'I think that X is true' are mostly interchangeable. To believe something is not, furthermore, to index it to our self or some group of people as we might with a declaration about taste. For the traditionalist about knowledge we *can* coherently say "margaritas taste better than wine *to me*" but not "it is true *to me* that the Earth's glaciers are melting".

Our beliefs are in fact true, whether we know it or not, when what they represent is indeed the case.[7] What promises to stop it being a matter of luck which of our beliefs are true is the third part of the traditional view of knowledge: justification. For some belief to count as knowledge we need more than a true belief, we need to have formed the belief as a result of encountering and responding to considerations that *genuinely* give reason for holding the belief.

The traditionalist about knowledge, as Paul Boghossian (2006) notes, endorses three kinds of objectivism. The word objectivism here signals the opposite of subjectivism – what's subjective depends in some interesting way on the contribution of the individual person, what's objective is independent of that. I'll list two of kinds of objectivism here, and then defend them. I'll get to the third closer to the end.

First is objectivism about facts. The traditionalist thinks that there are some facts, some 'ways the world is', independent of what anyone thinks or whether anyone thinks about them. There are interesting and powerful arguments for fact objectivism, but I won't have time to go into them tonight, instead I'll defend objectivism against leading criticisms of it.[8] (It's worth noting that the realism I take them to justify is one significant respect in which I'm not fully Quinean.)

Second is objectivism about justification. The traditionalist thinks that some reasons, typically *evidence*,[9] really do justify the holding of some beliefs. This need not be complicated – that a qualified observer under suitable conditions perceives moving air indoors justifies her believing that a door or window is open. Many justifications are *very* complicated. For example the claim

---

[4] I don't mean to endorse any particular theory of representation here, and although it doesn't matter for present purposes support a basically attributionist account of belief – see Dennett (e.g. 1991).

[5] It's non-obvious what to count as external. For traditional dualists it's the whole material world. For simple early forms of physicalism (I have in mind early mind-brain identity theories) it's everything outside the skull. Phenomenological considerations as well as a growing tendency in recent cognitive science suggest that thinking and mental life more generally are less straightforwardly demarcated. For a survey see Clark (1997).

[6] This claim assumes the definition of an imaginary number as a number whose square is a real number not greater than zero.

[7] Some philosophical theories propose subtle and interesting accounts of the intermediate steps in the relationship between belief and fact. Some of the most fruitful work here is being done in the philosophy of science (See, e.g., Ruttkamp 2002).

[8] For a recent robustly empiricist and naturalist account of the case for realism see Chapters 2 and 2 of Ladyman and Ross (2007).

[9] Other kinds of reason include rules of inference (for various kinds of inference) and regulative principles.

that functional magnetic resonance imaging of the brain can tell us something about what parts of the brain are selectively recruited for specific kinds of tasks depends on a complicated chain of inference relating, among other things, the energy metabolism of neurons, which store minimal energy so that active cells need oxygen transfers from the blood, differences in magnetic susceptibility of oxygenated and deoxygenated blood, and the physics, including detailed microphysics, that explains how pulses of magnetic field to be alternated with bursts of imaging tuned to detect local variations in blood oxygen levels, as well as a host of formal techniques to manage and interpret the resulting multi-dimensional data. That justifications can get so complicated is part of why we rely so much on specialists who also rely on each other. Few justifications if any are infallible – so we can be justified and mistaken.

Universities are naturally thought of as, at least, that is to say they're also more than that, centres where specialists in the justification of various kinds of knowledge congregate and work on extending knowledge, and training newcomers in knowledge and techniques of justification. Universities, that is, make a straightforward kind of sense to the traditionalist about knowledge.

Social constructivism rejected

There are three main kinds of constructivism about knowledge. They correspond to the three kinds of objectivism. As noted earlier I'll focus initially on two of them. To recap, the two kinds of objectivism that the traditionalist about knowledge endorses are, first objectivism about facts, which is the view that there are some facts, some ways the world is, independent of what anyone thinks. The second is objectivism about justification, the view that some reasons (typically evidence) really justify the holding of some beliefs.

To say that something is socially constructed is just to say that it is produced by a process that is interestingly, irreducibly, social – that it could not have been produced without social involvement of some sort. So, to say that *knowledge* is socially constructed must mean either that facts or justifications are socially constructed. This is an interesting this to suggest partly because different social groups have their own contingent (parochial, temporary) needs and interests. So if knowledge is socially constructed then for different social groups with different needs and interests, different beliefs would be justified, or different facts be the case.

The traditional picture can and does allow for a social dimension to knowledge. It's *obvious* that what we try to find out about depends partly on our contingent, social needs and interests. It's also pretty obvious that what we think already can and sometimes does influence or distort what we observe and how we interpret it. There are plenty of shameful examples of nationalist, racist, misogynist, homophobic and other ideologically crazed episodes in the history of science. The traditional picture can account for these without abandoning objectivism about facts or justifications. The traditionalist thinks, for example, that the putative facts about the inheritance of acquired characteristics trumpeted by Lysenko and his followers were *never* there to be found, and that their justifications for beliefs about them in preference to genetic theories of inheritance were *never* any good. The constructivist thinks that there are no facts *at all* independent of contingent social needs and interests, or that all justifications are relative to sets of contingent social needs and interests. I'll start with fact constructivism.

Although social constructivism about facts seems to be widely endorsed, there are no remotely impressive arguments in favour of it. Sometimes what gets offered is a 'bait and switch' where an

argument for the *Social Relativity of Descriptions* is offered, followed by a conclusion stating the *Description Dependence of Facts*.[10] But the social relativity of descriptions is a banality that anyone should endorse: what we describe, and how we describe it, is clearly not independent of our contingent needs and interests.[11] That's just not the same thing as the world which is described being *itself* dependent on them. It is partly up to us, as we come collectively to worry more and more about the effects of rising global temperatures, to find out more and more, and have more and more to say, about the rate at which glaciers are melting. So we now, partly because of changing needs and interests, have more detailed description of that process than ever before. It doesn't follow that we have any control over what we find, and most of what we're finding is very unwelcome.

Constructivism about facts also doesn't make sense – it does violence to the content of many of the facts that it proposes to tell us something about. This is important: Many of our beliefs concern things that came before people, or that work independent of people and so are beyond human control, for example that diamonds are brought close to the surface of the earth by igneous rock formations called kimberlites, formed hundreds of kilometers within the Earth's mantle, and forced upward by violent geological processes, or that differently charged components of atoms are held together by interactions between quarks and gluons producing what is called the strong force. Saying that *those* facts are constructed seems to confuse the order of causation, or be conceptually incoherent. And constructivism about facts seems to allow that different groups could construct incompatible facts, so that for some of them the planets might move in circular orbits, while for others they move in ellipses, which seems intolerably to debase the notion of a fact, or require a proliferation of incompatible facts.

Relativistic constructivism, sometimes championed by Richard Rorty (e.g. 1981: 328-9),[12] avoids some of these problems by relativising the notion of a fact to perspectives or theories, so that no claims of the form 'X is true' are allowed, but only ones of the form 'relative to theory T, X is true'. Along with claims to the effect that there are multiple different theories, that there are no facts in virtue of which some are better than others, and that what theory someone endorses depends on her contingent needs and interests, we have a somewhat more promising kind of fact constructivism (Boghossian 2006: Chapter 5).

It's *also* hopeless. Its own central claims cannot be stated without serious problems of consistency, since the relativist wants to say *absolutely* that there are no un-relativised facts, not merely that according to relativism there aren't any. And, leaving that aside, the relativist needs there to be facts about what theories we accept for the putative normal facts to be indexed to. If there are such facts, then *again* the relativist position is self-defeating, and committed to the intrinsically bizarre thesis that the only facts we are sure of are those about what theories we accept, even as we drain facts out of the world and the evidence for theories about it. A recent cartoon in the New Yorker shows a disappointed looking person in a doctor's consulting room, asking the doctor "Can't you diagnose me with something you have free samples for?"[13] One way of reading this, and a way that

[10] I'm following Boghossian (2006, Chapter 3) here.
[11] Part of the reason Gregor Mendel's research on plant breeding took place when and where it did, to give one of very many possible examples, was because of the interests of nineteenth century Austrian agriculture (Orel 1996, Chapter 2).
[12] Rorty's views are more complex than this. For a recent engagement with some of the complications relating to relativism see Clare (2008).
[13] Cartoon by Barbara Smaller, Published in *The New Yorker* March 24, 2008.

is funny, in confusing diagnosis with causation, against a backdrop of fact objectivism, is that the patient is saying "can't you make it true that I've got something it costs less to treat?"

So fact-constructivism needn't bother us at all – there's no reason to take it seriously, and we run into disastrous problems if we try to. It's also worth noting, as many have, that for all its apparent radicalism, fact constructivism is politically suspect as well. If there aren't any objective facts about who suffers injustice and at whose hands, or facts about what is unjust in the first place, then either there's no point trying to fix the world or we should simply blame the poor, the enslaved, and others who suffer for not either constructing more enjoyable facts for themselves, or constructing facts such that it was good to be poor, etc. (e.g. Norris 1992). We should not mourn the failure of fact constructivism.

A second kind of constructivism focuses on justifications. This 'epistemic constructivism' is compatible with fact constructivism, but can be treated separately. According to epistemic constructivists even if there are facts of the matter about how the world is, there aren't facts about what beliefs are justified, so that incompatible beliefs can simultaneously be justified, as long as each is individually justified relative to some theory or tradition. This claim about justification *relative* to a theory is part of why this kind of constructivism is also sometimes called epistemic relativism. As an autobiographical aside, I used to be an enthusiastic epistemic relativist. It's an affliction popular among undergraduates, and in my case, somewhat perversely, it was Rorty's *defence* of it that put me off the doctrine for good, during my final year of undergraduate study.

The best argument for epistemic relativism considers real or imagined confrontations between supposed users of radically different theories of justification (Boghossian 2006, Chapter 5). One famous example, discussed by various philosophers, is the disagreement between Galileo and Cardinal Bellarmine over the motion of the Earth. Bellarmine, as in this respect a good Aristotelean, believed that every heavenly body rotated around the Earth, which did not itself move. A key piece of evidence for Galileo was the moons of Jupiter, which he had first observed with a telescope of his own construction. If bodies could manifestly orbit something other than the Earth, it was less crazy to suppose that the Earth orbited the sun. Galileo famously arranged for Bellarmine and others to take a look for themselves, with inconclusive results. Telescopic observation isn't a simple matter, especially not with early telescopes, and so it was difficult for others to see what Galileo said was there. More to the point some refused to look, saying that they already had a source of authoritative knowledge, and that was the Bible. Following a description of this incident, Rorty asks what determines that 'Scripture is not an excellent source of evidence for the way the heavens are set up?' (Rorty 1981: p329).

On Rorty's view it anachronistically begs the question to condemn Bellarmine for being unscientific – he operated with a different approach to justification, according to which a different belief was justified. Sure enough, *relative to Galileo's approach* there were moons orbiting Jupiter. We know that the two approaches give different conclusions, what we need isn't someone banging the table on behalf of science, we need an argument for favouring one whole approach over another. There, he and others argue, we run into a wall. Both sides can produce justifications of particular beliefs according to their rules, but no approach seems to be able to justify itself to the other. It's popular to quote Wittgenstein at this point, who wrote in *On Certainty* that:

> When two principles really do meet which cannot be reconciled with one another, then each man declares the other a fool and a heretic (1979, para. 611).

In fact, the problem is worse: No system can justify itself by its own lights *unless* it has access to some principles that are somehow self-justifying. This is the problem some call the norm-circularity of justification. Some traditional epistemology did in fact attempt to find self-justifying epistemic principles. (An epistemic principle here is just a rule for forming beliefs given some evidence.) There's no time to go into the details here, but these approaches (Descartes is one example) are instances of the kind of pure philosophy that good naturalists cannot accept, because they're persuaded that the resources they claim to draw on are useless.

If this gloomy picture is roughly correct, then the situation is as follows. No belief is truly justified, but only justified relative to some specific system of justification. There are many such systems, none of them capable of giving someone using another system reason to switch, and none of them capable of justifying their own adherents' endorsement. This is an epistemological tower of Babel – where perhaps the best we can expect is to exist in a state of liberal tolerance along with baffled incomprehension. It's popular to express the situation by reference to talk of 'incommensurability', inspired by the work of Thomas Kuhn (1962), or of 'language games', here inspired by Wittgenstein (1953).

Are things really this bad?

Let's get clearer on what's at stake. A key claim made by the epistemic relativist is that assertions of the unqualified form 'evidence E justifies belief B' are never true, but that relativised ones of the form 'evidence E justifies belief B according to a system of justification S, that I accept' can be. And a key part of this argument depends on the claim earlier made about justifications – that we *can't* arrive at justified beliefs about epistemic facts because we can't justify our own epistemic practices.

Notice also that attempting to endorse the epistemic relativist position gets us into serious trouble. It's hard to see how we could endorse it, and continue to accept, in the required sense, any epistemic principles at all. How could we coherently think that no beliefs are justified, and at the same time think that ours were, relative to *anything* (Boghossian 2006, 84f)? Also, the relativist commitment to pluralism seems to cause trouble – the relativist should agree selectively with systems according to which this or that belief is *not* justified given some evidence, so siding with Bellarmine against the telescope, but the relativist isn't supposed to have preferences of that sort at all (Boghossian 2006, 89f).

Fortunately we don't have to go there. The supposed problem of the norm-circularity of justification has been seriously overblown. There are several reasons for this. One is that people have much more in common with each other *as far as epistemic principles go* than typical statements of the problem seem to allow. On a wide range of matters, for example, Galileo and Bellarmine agreed – not merely about *what* to believe, but about *why* to believe it. I'm not only referring to what for them were everyday matters like justifying believing that some text was in Italian rather than Latin. They also agreed about many things connected to their dispute – for example the approximate distances between the Earth and the heavenly bodies they did agree existed, and how to measure those distances. They agreed about the apparent motion of most of those bodies, and that *some* pattern of real motions would be a good explanation for the apparent motions. So there's more hope for proper engagement that relativists allow.

A second reason is that users of any given approach can, even if they don't, attend to clues that there's something wrong with their own ways of justifying beliefs. These clues are thrown up either

when things don't go as they expect, or when they observe others, using different principles, doing better at something. To put it bluntly, whether or not you *try* to interact with people who think differently, it's difficult to hold onto your conviction that powered flight in vehicles carrying humans is impossible when someone flies past in an aeroplane.

Finally, in this short list, Wittgenstein is wrong and there is plenty that we can do when two apparently irreconcilable principles meet besides directing abuse at each another. We can back up and try to get clearer about what we *do* agree on. We can suspend conflict while we try to get better data, especially where conflicting principles lead us to different expectations. We can allow ourselves to be perplexed by inconsistencies and difficulties with our own theories, which are never as self-enclosed or consistent as some relativists seem to think. We can try to figure out the outlines of a more general theory of which our conflicting two turn out to be interesting special cases. Not only *can* we do those things, scientists routinely *do* them. The unifications in physics that I said a little about earlier illustrate this. Galileo's claim that a thrown projectile will trace a parabolic path turns out approximately as a special case of Newton's laws which say that they travel in sections of ellipses, if you suppose (falsely) that gravitational force operates in parallel lines. String theory, or superstring theory, is supposed to be a new theory at once constrained by the lessons and successes of general relativity and the standard model of particle physics. (For a popular exposition see Greene 1999.) The key idea can be simply stated – fundamental particles are not point-like, but one dimensional tense strings which vibrate in various ways, including being able to vibrate in additional spatial dimensions, and can split and combine, as well as be connected to themselves. For reasons I don't pretend to understand the basic repertoire of states and interactions of these objects correspond well to the set of basic particles in the Standard Model, and also (unlike the Standard Model) gives a graviton (a closed, massless string of spin 2) to explain gravitational force. There are various other tantalising payoffs to superstring theory, including unifying the four fundamental forces. There's more than one string theory, and strictly speaking no string theory has even been properly stated yet – some very interesting and promising ideas are taking shape though. I don't need any string theory to be correct – the existence of the attempt to make one in the ways it is being done makes the point I need about what we can do when theories in general appear both to conflict and be worth keeping, and that's try to work together on making something new that keeps what we agree is valuable while lacking the problems that set theories against one another.

It's also worth noting that Rorty sometimes suggests something similarly, cautiously, optimistic although without getting there via history and philosophy of science. In the face of apparently intractable disagreement he recommends a kind of non-dogmatic and non-coercive exploratory conversation where all participants accept that their own views are vulnerable to revision (see Clare 2008). I add that no set of institutions has ever done better than modern science at providing fora in which such debate can take place.

The upshot of this is not that anyone knows for sure that her epistemic principles *are* correct. But it does mean that we can reject the relativism that says we must accept that none *could be* correct, and it warrants optimism about the prospects for ongoing improvement through diligent critical and empirical effort. We can safely be (cautious) epistemic objectivists.

This is, I suggest, good news for those in the university business. If there are no facts of the matter about what evidence justifies what beliefs, then investing in communities of putative experts at just that would be a clear waste, especially because some of them need such expensive machines. If it's up to anyone to construct what knowledge they will, perhaps universities are at best cultural

ornaments, but then they are, despite the presence of some artists, musicians, writers, pretty inefficient ornaments.

I said earlier that there were three kinds of constructivism. I need to say a little about the third before my final section. The third kind of objectivism, to which this constructivism is opposed, is constructivism about the 'rational explanation of belief'. If this objectivism is correct, which I think it is, then we sometimes have the beliefs we do purely for epistemic reasons. What this means is that we can sometimes be moved to believe something *only* because of evidence which actually bears on the truth of the belief. Clearly we need this kind of objectivism as well as the preceding two if we are ever to have knowledge: it's not good enough if there are facts about the world, and facts about what beliefs are justified, but we can never respond properly to that evidence. This is just what the constructivist about the rational explanation of belief denies, claiming instead that there is always a pragmatic element in our belief formation, that what we believe *always* has *something* to do with our contingent needs and interests.

The main reason[14] for thinking this is supposedly that the evidence always *underdetermines* what beliefs are correct, which is to say that it is always consistent with a range (according to some an infinite one) of possible interpretations, and so it's never the evidence itself that decides what we believe. While it is clear that *when* the evidence is consistent with a range of possibilities, it won't be the evidence alone that settles what we believe, the evidence just *isn't* always such as to leave a wide range of significantly different options open. Often it takes large numbers of very smart people to come up with *one* hypothesis or theory that satisfies a body of evidence, especially if attempting to satisfy constraints of a number of different sorts. And the early arguments for underdetermination make the situation seem more difficult than it is – Pierre Duhem (1954) famously argued that anomalous motion of planets could equally well be explained by hypothesising other planets (the procedure that led to Herschel's discovery of Uranus) as it could by hypothesising unexpected behaviour of telescopes. But the behaviour of measuring instruments, or parts of theories, can be and is independently assessed – it's not true that everything is *equally* up for grabs. So the third kind of constructivism is also not worth taking seriously as a challenge to the possibility of knowledge – we should be careful to guard against our prejudices and preferences interfering with our search for knowledge, but we should not give up on the search.

African scholarship and conclusion

With the constructivist challenge to naturalism headed off, I want now to turn to my final topic, 'African scholarship'. As I said earlier, I'm going to argue that there's no 'epistemically interesting' sense in which there is such a thing as 'African knowledge'. This is apt to sound like a very radical or reactionary thing to say, so let me first set out some of the things I *don't* mean by it. After that I'll add some remarks on what I do mean, and then close by trying to draw things together.

I don't mean to endorse the ridiculous claim that there are no facts about Africa. Nor make the absurd claim that some of them might have been historically neglected as a result of the interests of powerful groups, including neglect by those working at African universities sometimes worrying more than they should about views of what was worth working on and how that were dominant

[14] I'm leaving *strong* constructivism about the rational explanation of belief aside here – the view for example defended by Bloor (1976). See also Lint (1973).

elsewhere. These include facts about African culture (including languages) politics and history, but also, for example, facts about the non-human aspects of Africa, often primarily examined with a view to exploitation by outsiders.

I certainly don't mean to endorse the also ridiculous claim that there aren't some facts that it is disproportionately important for some Africans to know about, or that many of those have not been subject to the same relative neglect. It's also clear to me both that there are many facts contingently known primarily or only to Africans, and that it is an open possibility that there may be distinctive ways of justifying true beliefs – that is valid epistemic principles – contingently known, at least so far, only to current or former Africans.

So what *do* I mean?

Very briefly I mean that none of the ways in which we might know about Africa, or as Africans, requires revising the traditional philosophical conception of knowledge, or accepting any kind of constructivism.

Let me put some flesh on this notion, by referring to the opening quotation in the Inaugural address of the Vice-Chancellor of the University of KwaZulu-Natal, Professor Makgoba, in 2005. It's not clear from the remainder of the inaugural address that the intention of our Vice-Chancellor was to *endorse* the view that he quoted, so much as to provoke, and I'll use the quotation in the latter spirit. He reported that in 1980, in the John Radcliffe Hospital, Oxford a Nigerian colleague said:

> If a Nigerian had written William Wordsworth's famous poem, The Daffodils,[15] in Nigeria, this Nigerian would have been found guilty by a military tribunal. He would have been sentenced to death or life imprisonment or more kindly he would have been certified mentally ill and sent to a mental asylum for long-term treatment and rehabilitation.

The un-named Nigerian colleague is further reported to have explained in justification of this very startling assertion that:

> Daffodils do not grow in Nigeria and this flower has no meaning—culturally or existentially in the life of a Nigerian.

I find a lot that is superficially wrong with both the sentiment and the justification. It proposes an astonishingly brutal limitation on freedom of expression, and the suggestion that the army should be the arbiter of cultural acceptability is worse than alarming. It's manifestly self contradictory, insofar as if a Nigerian *had* written the poem, there would have been at least one Nigerian for whom the plant *did* have an existential meaning. Furthermore it is worth noting, even though the point is in some sense incidental, that a key claim in the justification is biologically mistaken. The botanical name of the Daffodil is the Narcissus, part of the family Amaryllis, and that plant is native to Europe, North Africa, and Asia. A number of species of the family, including some Narcissae, are widespread in Northern Nigeria, and some of them are currently under study by ethnobotanists because of a variety of medically interesting properties including analgesic and antiviral ones (Rønsted et al, 2008; López et al, 2002). Some work in the area of medicinal extracts from Amaryllis family plants has been done by colleagues at the Pietermaritzburg campus of the

[15] Sic. Wordsworth's 1804 poem is, of course, called merely 'Daffodils'.

University of KwaZulu-Natal (e.g. Fennell and van Staden 2001). It would be a shame to have them slaughtered or incarcerated.

There are more important things that are profoundly wrong here. The quotation suggests an ideal of partitioned, more specifically national bodies of ideas, perhaps also of knowledge, with clear boundaries. I earlier approvingly quoted Wilson saying that '… no intellectual vice is more crippling than defiantly self-indulgent anthropocentrism.'[16] Perhaps Wilson was being optimistic – or just not thinking that there could be narrower parochialisms to worry about.

Some, indeed most, of the things we can know about transcend all political boundaries. The laws of physics, chemistry and evolutionary biology as well as all mathematical knowledge among others apply at levels of generality vastly wider than the transient, local and contingent boundaries over which people often obsess. Scientists, as professional knowers, are properly 'cosmopolitan' in the sense that Kwame Anthony Appiah recently reminded us – whatever else they are, they are in some sense citizens of the universe, pulled away from all parochialisms (Appiah 2006). The exceptional cases where these boundaries *are* relevant to general sciences, for example where isolation, sometimes socially or politically enforced, has led to some sub-set of the human population having a distinctive susceptibility or immunity to a disease, such as sickle cell anaemia, or other medically interesting property such as primary lactose intolerance don't map neatly onto political boundaries. Some of the most interesting examples of distinctions between people turned up by science have nothing to do with politics, or even with species – for example a gene responsible for red hair also leads to enhanced response to some kinds of analgesics, but only in female humans and, as far as we know so far, female mice although it makes their hair yellowish (Mogil *et al*, 2003).[17]

Our national boundaries may well shape our interests, and tell us what is most urgent for us to work on – the Dutch are rational to worry more in the first instance about global warming as it affects sea levels, the Swiss as it affects melting glaciers. But they don't define *kinds* of knowledge, or tell us *what it is* to know about something, and they don't tell us where we might find valuable insights from elsewhere. Also, crucially, the project of determining standards of knowledge and evidence is supra-national, owned at once by nobody and everybody. Recent remarks by the South African Minister of Health, Manto Tshabalala-Msimang suggest, therefore, a view that those in the knowledge business ought strenuously to reject. In a report of February 24th of this year she is quoted as saying that "We cannot use Western models of protocols for research and development," and that medicines that had been in use for thousands of years should not get "bogged down in clinical trials" (BBC, 2008).

The sense in which these models and protocols are 'Western' is trivial and contingent – they're significantly, but not exclusively, the *products* of 'Western' scientists, including the pioneering work in statistics of Pascal, Fermat and Gauss. But the history of mathematics is deeply international, and involves significant contributions from Africa, including ancient Egyptian mathematics, and nearly a thousand years of Islamic mathematics, much of it done in Africa. And the processes by which the protocols for trials are developed and refined are institutionally competitive, in ways which make demonstrable improvement – in statistical analysis, in

[16] *On Human Nature*, pp5-6.

[17] The class of analgesics in question appear to be effective only in females. Work on sex differences in pain modulation is relatively recent, partly because the phenomenon was only relatively recently discovered. I've somewhat simplified the description of the main finding of the cited paper in the text, mostly to avoid introducing genetic terminology.

administration procedures, in subject selection and so forth – matter much more than political allegiance. These protocols are a terrific example of science working to reduce the effects of preference, including political allegiance, in the formation of belief in favour of increasingly precise identification of evidence bearing on the actual efficacy of a treatment.[18]

Minister Tshabalala-Msimang's remarks suggest, in the face of this, that there is already or could be such a thing as 'African' science, which found different incompatible facts, perhaps because of following different epistemic principles. We've seen that neither kind of constructivism is supportable. This *doesn't* mean that some, perhaps many, traditional remedies don't work. Some of them will – we know this already, although we wouldn't know as much if we'd slaughtered the African scientists studying daffodils. Some of them probably *won't* work – there are entire practices, such as homeopathy, which don't. Apparently confirmatory anecdotes don't by themselves establish effectiveness. Some traditional therapies may well be harmful – earlier Western traditional healers happily prescribed Mercury for the treatment of syphilis despite there being no non-anecdotal evidence that it did no good, and apparently without noticing that it was seriously toxic, being especially harmful to the central nervous system.

We might *want* it to be true that most local traditional remedies work wonderfully, just as we might want a soccer team that could qualify for the World Cup for reasons other than our being the host nation. But our wishing wouldn't make it so. And we know of many other cases where the evidence is clear that what people fervently wish for isn't so. Enthusiasm for homeopathy remains bizarrely widespread, even though no rigorous trial has ever found it to have an effect – part of the reason for the enthusiasm, it seems clear, is that people want it to be effective more than they want to follow reliable procedures for acquiring true beliefs.

None of this says that experienced collected and preserved by groups other than professional scientists has no value. Many effective remedies have been discovered that way, as well as much that does no good. Uncontrolled observation without rigorous statistical analysis, though, cannot discover differences between apparent effects and placebo, or detect risks of longer run toxicity. It cannot, without assistance, be guided by molecular and genetic models in the development of new therapies or isolation of effective compounds from effective preparations. In the case of herbal remedies, the state of our knowledge, internationally speaking, is very limited – of over 1300 published studies of herbal remedies only three, as of October 2007, meet the standards of a rigorous clinical trial (e.g. Guo *et al* 2007). There's clearly a lot of work to be done – and I hope some of it is done here at UKZN.

What I've said so far tonight, and I'm almost done now, might seem more intellectually macho than I've intended. I've argued that we, including the philosophers among us, should be naturalists and that we need not abandon the traditional conception of knowledge because of the challenge of constructivism. I've *not* argued that finding knowledge is easy, or that anybody gets to be rude in the process. We're all apt to get impatient with disagreement sometimes. As Appiah notes in his *Cosmopolitanism*, our impatience at people who are incredulous at stories about disease-causing

[18] This isn't to say that it's perfect *yet*, and a significant source of distortion arises from the fact of sponsorship or production of many trials by people with too much interest in the outcome – the companies that develop the drugs. There's significant evidence of corruption and distortion here, including cases of 'ghost authorship', where someone – sometimes a drug company – writes a paper or conducts a study *and* writes a paper, and then gets an academic to put her or his name to it (e.g. Gøtzsche et al, 2007; Healy and Cattell 2003). There's also indication of 'disease-mongering' where drug companies and others fabricate ailments in order to create demand for treatments (PLoS Medicine, 2006).

entities called viruses too tiny to be seen should be tempered by remembering how long it took scientists to become convinced, and by respect for the fact that, in the absence of painstaking work and demonstration, alternative beliefs, for example that people who are hated by witches get diseases, are not *so* obviously false.

If we really are going to discharge Sellar's proposal that we, not merely philosophers, make headway showing how things hang together, we need to engage when we disagree, and in a more optimistic fashion. This seems to me especially in the humanities where it often seems as though a kind of 'fruitcake' pedagogy is tolerated – where one can learn Freud with no biology or neuroscience (despite Freud's own fascination with both), learn cognitive psychology with no Freud, and so forth. There's too much incentive for students to Balkanise their knowledge that way. Not only that, politely ignoring the differences between how we within the academy do things, which members of the academic staff certainly do sometimes, is a weak and defective kind of tolerance. Engagement would be more respectful, if also confusing. I'll close with another remark of Appiah, who wrote that '[d]epending on the circumstances conversations across boundaries can be delightful, or just vexing: what they mainly are, though, is inevitable' (Appiah 2006: xix).

References

Appiah, K.A. (2006) *Cosmopolitanism*, London: Penguin.

BBC (2008) 'No clinical trials for SA healers' http://news.bbc.co.uk/2/hi/africa/7261215.stm, accessed 30 March 2008.

Bloor (1976) *Knowledge and Social Imagery.* London: Routledge & Kegan Paul.

Boghossian, P. 2006. *Fear of Knowledge*, Oxford: Oxford University Press.

Clare, J. (2008) Sketch of a conversational society, *South African Journal of Philosophy*, 27(2): 80-91.

Dilthey, W. (1989) *Introduction to the Human Sciences*, Volume 1 of R.A. Makkreel and F. Rodi (eds.) *Selected Works*, Princeton, NJ: Princeton University Press.

Duhem, P. (1954) *The Aim and Structure of Physical Theory*, translated by Philip P. Wiener, Princeton, NJ: Princeton University Press.

Fennel, C.W. and van Staden, J. (2001) *Crinum* species in traditional and modern medicine, *Journal of Ethnopharmacology,* 78 (1), pp. 15-26.

Friedman, M. (1974) Explanation and scientific understanding. *Journal of Philosophy.* 71: 5-19.

Gøtzsche, P.C., Hróbjartsson, A., Johansen, H.K., Haahr, M.T., Altman, D.G., and Chan, A-W. (2007) Ghost Authorship in Industry-Initiated Randomised Trials, *PLoS Medicine*, 4(1), 0047-0052.

Guo, R., Canter, P. H., and Ernst, E. 2007.A systematic review of randomised clinical trials of individualised herbal medicine in any indication, *Postgraduate Medical Journal*, J 83: 633-637.

Healy, D. and Cattell, D. (2003) Interface between authorship, industry and science in the domain of therapeutics, *British Journal of Psychiatry*, 183, 22-27.

Kitcher, P. (1981) Explanatory unification. *Philosophy of Science.* 48: 507-531.

Kuhn, T.S. (1962) *The Structure of Scientific Revolutions*, Chicago: University of Chicago Press .

Ladyman, J. & Ross, D. with Spurrett, D. & Collier, J. (2007) *Everything Must Go: Metaphysics Naturalized*, Oxford: Oxford University Press

Lint, J. (1973) *Rigor Mortis*, New York: Fairleigh University Press.

López, S., Bastida, J., Viladomat, F. and Codina, C. (2002) Acetylcholinesterase inhibitory activity of some Amaryllidaceae alkaloids and *Narcissus* extracts, *Life Sciences*, 71(21), 2521-2529.

Mogil, J.S., Wilson, S.G., Chesler, E.J, Rankin, A.L., Nemmani, K.V.S., Lariviere, W.R., Groces, M.K., Wallace, M.R., Kaplan, L., Staud, R., Ness, T.J., Glover, T.L., Stankova, M., Mayorov, A., Hruby, V.J., Grisel, J.E. and Fillingim, R. B. 2003. The melanocortin-1 receptor gene mediates female-specific mechanisms of analgesia in mice and humans, *Proceedings of the National Academy of Sciences*, 100(8), pp 4867-4872.

Montague, P.R., & Berns, G. 2002. Neural economics and the biological substrates of valuation. *Neuron* 36: 265-284.

Norris, C. (1992) *Uncritical Theory: Postmodernism, Intellectuals & the Gulf War*. Amherst: University of Massachusetts Press.

Orel, V. (1996). *Gregor Mendel: The First Geneticist*, Oxford: Oxford University Press.

PLoS Medicine (2006) A collection of articles on disease-mongering in PLoS Medicine, URL: http://collections.plos.org/plosmedicine/diseasemongering-2006.php, accessed 30 March 2008.

Quine, W.V. 1953[1966]. Mr. Strawson on logical theory. *Mind*, 62(248): 433-451. Reprinted in *The Ways of Paradox and Other Essays*, 1966, New York: Random House.

Quine, W.V.O. 1951. Two Dogmas of Empiricism. *The Philosophical Review*, 60(1), 20-43.

Rønsted, N., Savolainen, V., Mølgaard, P. and Jäger, A.K. (2008) Phylogenetic selection of *Narcissus* species for drug discovery, *Biochemical Systematics and Ecology*, 36(5-6), 417-422.

Rorty 1981. *Philosophy and the Mirror of Nature*, Oxford: Blackwell.

Ross, D., Ladyman, J. & Spurrett, D. (2007) In Defence of Scientism, in Ladyman, J. & Ross, D. with Spurrett, D. & Collier, J. *Everything Must Go: Metaphysics Naturalized*, Oxford: Oxford University Press, pp 1-65.

Ruttkamp, E. 2002. *A model-theoretic realist interpretation of science*. Dordrecht: Kluwer Academic Publishers.

Wilson, E.O. (1979) *On Human Nature*, Harvard University Press.

Winch, P. (1958) *The Idea of a Social Science*, London: Routledge and Kegan Paul.

Wittgenstein, L. (1953) *Philosophical Investigations*, G.E.M. Anscombe and R. Rhees (eds.), G.E.M. Anscombe (trans.), Oxford: Blackwell.

Wittgenstein, L. (1979) *On Certainty*, ed. G.E.M.Anscombe and G.H.von Wright Translated by Denis Paul and G.E.M.Anscombe. Oxford: Blackwell.