Scepticism about objectivity

Scepticism about objectivity and the concepts associated with it (truth, reality, correspondence, and so on) is a pervasive characteristic of methodological writing on qualitative research methods. You don't have to be a phenomenologist to regard the idea as questionable, or even distasteful. Whatever abstract nouns they adopt label their positions – interpretivism, constructivism, constructionism, hermeneutics, phenomenology – a majority of qualitative writers agree that objectivity is, at best, a dubious idea and, at worst, a discredited one; and, during the last 30 years, a Cambrian explosion of qualitative research studies, garnished with snippets of postmodern sensibility, has fostered the idea that there is no 'single, objective reality', a creed which, if not yet orthodox, is so well established that many authors no longer acknowledge its *prima facie* implausibility. According to Lincoln & Guba (2003, p. 279), 'Objectivity is a chimera: a mythological creature that never existed, save in the imaginations of those who believe that knowing can be separated from the knower'. Burr (2003, p. 152) says that 'social constructionism would regard objectivity as an impossibility'. Davies & Dodd (2002, p. 282) argue that 'the elimination of subjectivity... is impossible', and that trying to eliminate it is 'detrimental to research and ethical research practice'. Years earlier, Filstead (1979, pp. 35-6), had suggested: 'The qualitative paradigm does not conceive of the world as an external force, objectively identifiable and independent of man. Rather, there are multiple realities.'

These pronouncements from the mainstream methodological literature are all echoed in nursing. Some classic statements by phenomenological writers include: 'It does not make sense to speak of a division between a "subjective" mind and the "objective" world' (Omery & Mack, 1995, p. 141). 'There is no such thing as an . . . objectively "true" account of "things in themselves" . . . [and] there is no technical procedure for "validating" that an account corresponds to this timeless, objective "truth" ' (Leonard, 1999, p. 60). 'There exist multiple, socially constructed realities' (Koch, 1999, p. 25). Views of the same kind have been expressed regularly ever since. For example, a methodological paper by Lowes & Prowse (2001), which refers to the 'illusion of objectivity in phenomenological data generation', has been extensively cited in the nursing literature (and recently by Anderson & Kiger 2008, Stayt 2009, Reid *et al.* 2010, Hughes *et al.* 2010, Shorter & Stayt 2010, Vouzavali *et al.* 2011). In arguing that 'it is neither possible nor desirable to exclude researcher preconceptions to achieve researcher objectivity in the pursuit of rigour', and that 'in a fundamental shift away from traditional notions of objectivity, Heideggerian phenomenologists propose that all knowledge originates from people who are already in the world and seeking to understand other people who are already in the world' (pp. 472/474), Lowes & Prowse situate themselves clearly in the tradition of those who think objectivity is a chimera because 'knowing cannot be separated from the knower' (or because, in another version, the knower cannot be separated from the known: Lincoln & Guba 1985, p. 37) .[1]

The collapse of Lincoln & Guba's conception of a paradigm makes one type of attack on objectivity unavailable. Objectivity can no longer be associated with a defunct 'paradigm', or swiftly dismissed as 'positivist'. The idea that there is a 'dominant paradigm' which 'privileges' just one 'way of knowing' – and privileges objectivity along with it – must be quietly binned. So must claims that objectivity is the result of the sinister influence of Descartes, or an Enlightenment prejudice, or a modernist notion that became obsolete with the arrival of postmodernism, or a corporate ideology, or the expression of hard-line logical positivism in the health services, or an instrument of medical dominance over nurses.

All these theories have a slight whiff of paranoia about them, and the concept of a paradigm, Lincoln & Guba style, has been rather kind to paranoia. The 'dominant paradigm', like 'dominant discourses', has conjured up the image of oppression-by-means-of-epistemological-hegemony, and this is sufficient for some writers to condemn objectivity without further argument, and without any reference to the recent philosophy of science.[2]

This is only to say that quick and dirty rejections of objectivity are no longer possible. It is still possible to offer specific arguments against objectivity, and to give reasons why it is impracticable, undesirable, unnecessary, impossible, or illusory. In practice, however, most of the arguments tend to be variations on a single theme. Here, by way of illustration, are some remarks by authors from different disciplines.

Sociology 'Findings are inevitably influenced by the researcher's perspective and values, thus making it impossible to conduct objective, value-free research, although the researcher can declare and be transparent about his or her assumptions.' (Snape & Spencer 2003, p. 17)

Psychology
'Within this paradigm, the neutrality of the researchers is emphasized as they must confine themselves to examining *what is* – what is objective – and dislodging themselves from all bias, emotion, values, or anything *subjective* that would compromise the objectivity of the research' (Sciarra 1999, p. 39)

Education
'The fall of empirical science from its supremacy among the paradigms of knowing had for its cause the fatal flaw of an arrogant belief in human objectivity... It now appears that it is as impossible to view social behaviour with a detached eye as it is to see with a detached retina. (Campbell 1988, p. 60-1)

Ethnography
'By draping their scientific activities in claims of neutrality, detachment, and objectivity, scientists... leave their (unconscious) minds wide open to political and cultural assumptions. Such hidden influences and biases are particularly insidious... While scientists firmly believe that as long as they are not *conscious* of any bias or political agenda, they are neutral and objective, when in fact they are only unconscious.' (Namenwirth 1986, p. 29)

Library science
Total objectivity is impossible for researchers who are, after all, human beings. The difference between the two research traditions is... that naturalistic researchers systematically acknowledge and document their biases rather than striving to rise above them. (Mellon 1990, p. 26)

Information systems
'Action research substitutes engagement for the objective detachment of positivist science' (Baskerville 2001 , p. 206)

Nursing
'One of the traits of this type of research is the quest for objectivity and distance between researcher and those studied so that biases can be avoided.' (Holloway & Wheeler 2010, p. 5) 'Scientists, like all men and women, are opinionated, dogmatic and ideological. Therefore, the idea of objectivity loses its meaning.' (Streubert Speziale & Carpenter 2007, p. 12)

And of course...
'We have allowed ourselves to become too preoccupied with objectivity... But there is a solution to that problem, namely, to require the inquirer to "come clean" about predispositions and feelings.' (Guba & Lincoln 1989, p. 112-3)

It's not hard to see the common thread here. The basic idea is that all researchers have certain attributes that make objectivity impossible. Because they are human beings, they cannot help but have opinions, emotions, values, biases, predispositions and feelings. They make assumptions, including political and cultural assumptions; they are dogmatic and ideological. In short, they (like everybody else) are replete with a human subjectivity from which they cannot escape and which they cannot just decide to cancel. Deleting this subjectivity, or detaching oneself from it, is impossible. And that means that objectivity is impossible, too.

For this argument to go through, objectivity has to be conceived as a putative psychological state. It has to be assumed that being 'objective' requires the researcher to somehow disengage from one or more of the attributes just mentioned. If there is such a thing as objectivity – if the idea is even to make sense – it must be possible to disable the dogma, ban the biases, eject the emotions, vanquish the values. There must be a sort of psychic scouring, an evacuation of the psychological fixtures and fittings that make us human. And because that can't be done, objectivity is simply not an option. Objectivity, the argument says, requires the *absence* or *suppression* of some mental X. But such an absence cannot be achieved by anyone. So objectivity is impossible, or 'loses all meaning'. To imagine that one is being objective is to practise a form of self-deception.

So objectivity, if such a thing exists, is a psychological state, one identified with the absence of certain mental attributes. Anyone who believes that scientists can attain this state, when in fact it is impossible, has an 'arrogant belief in human objectivity'; and it is the non-existence of this objectivity which has caused 'the fall of empirical science from its supremacy among the paradigms of knowing'. But is this 'absence', this suppression, really what objectivity means? Is the capacity for mental self-purification – the ability to strip oneself of emotions, feelings, opinions, assumptions, and values – really what being objective requires? Can the concept really be identified with a form of psychological self-purging?

According to Karl Popper, the answer is 'no'. It is a point which he emphasises on several occasions. Here is an example:

> 'The naïve view that scientific objectivity rests on the mental or psychological attitude of the individual scientist, on his training, care, and scientific detachment, generates as a reaction the sceptical view that scientists can never be objective. On this view their lack of objectivity may be negligible in the natural sciences where their passions are not excited, but for the social sciences where social prejudices, class bias, and personal interests are involved, it may be fatal. This doctrine... is based on the naïve view that objectivity depends on the psychology of the individual scientist... [However,] if we had to depend on his detachment, science, even natural science, would be quite impossible. (Popper 1961, p. 155)

Interestingly, Popper and the objectivity-deniers do agree about one thing. *If* objectivity is a matter of psychological self-purification, *then* there is no such thing as objectivity. But they disagree about what comes next. The deniers accept the premise: that's precisely what objectivity is, or would be; so there's no such thing. Popper doesn't accept it. That's precisely what objectivity isn't, and the deniers' view is 'naïve'; so the argument fails.

The obvious question is: if objectivity *isn't* self-suppression or psychic self-scouring, what is it? I am going to approach this question a bit obliquely, deferring a full answer till later (when I will also sketch Popper's view). Why I adopt the oblique approach is something that will become evident, I hope, we proceed. At any rate, I am now going to apparently change the subject, and talk about visual illusions.

Visual illusions

The Müller-Lyer illusion (Diagram 1) is probably the best known, and certainly the most thoroughly studied, of all visual illusions (Gregory 1998). For those jaded by its familiarity, I have included an alternative version (Diagram 2), which I will comment on later. The point of the illusion is, of course, that vertical line A *looks* comfortably shorter than vertical line B; but in fact the two lines are of equal length. Interestingly, despite a great deal of psychological research, there is no consensus about how to account for this discrepancy, although the most consistent trend is to assume that it has something to do with perspective, and the way in which the perceptual system compensates for cues suggesting distance (Robinson 1972; Gregory 1998). I will return to the question of explanation in a moment. Here, I will merely make some uncontroversial observations about the Müller-Lyer, and other similar illusions.

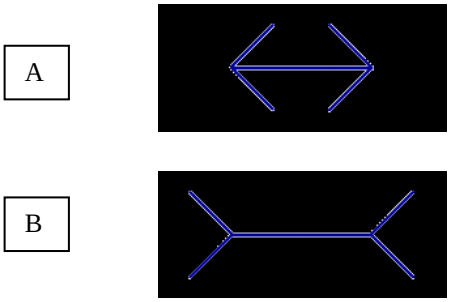

Diagram 1
The Müller-Lyer illusion

Four features in particular are worth noting. First, there is a clear distinction between 'how it seems to be' (B>A) and 'how it really is' (A=B). Second, we determine *how it really is* by adopting a particular procedure, measurement, which involves the use of a particular instrument, a ruler. Third, everybody accepts that this is an appropriate method of settling the question of whether the two lines are of equal length or not. Fourth, assuming that they carry out the procedure correctly, anyone using this method will obtain the same result.

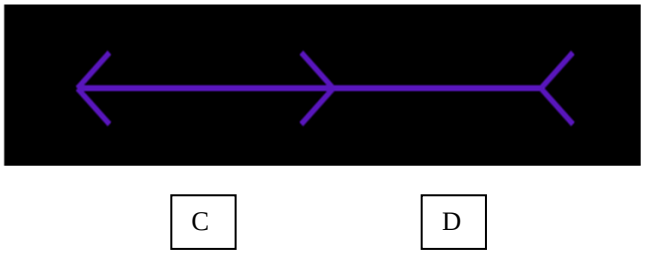

Diagram 2
The Müller-Lyer illusion: variant

Implicit in these remarks is a sort of pared-down epistemological position, which reflects the simplicity of the illusion itself. The first feature suggests that we can't trust our initial judgment, and that there is always a possibility that this judgment is wrong. The second indicates that the difference between *how it is* and *how it seems*, and our ability to discriminate between the two, depends on the use of a method. The third suggests that the connection between the method and the *is*/*seems* distinction is conditional on an agreement between interested parties. And the fourth adds a primitive notion of reliability, which is again an essential component of this minimalist epistemology.

I shall call these four features 'epistemological conditions', and note that they are interdependent. For example, unless the fourth condition holds, the third condition will not do so; and if the third does not, neither can the first two. In what follows, I will use this stripped-down epistemology as a benchmark. What we can say about illusions such as the Müller-Lyer is not just analogical, but rather a philosophy of science in its simplest form.

One additional comment, which is not epistemological but psychological. It is that the visual illusion is not dispelled once we have measured the lines. 'The knowledge that the two lines are of equal size does little to affect the perception that they are not' (Sloman 1996, p. 11). Sloman takes this as one piece of evidence (among many others) that there are two 'systems of reasoning'. The basic idea is that, if all neurologically based systems were integrated, then *knowing*-as-a-result-of-measuring that the two lines were of equal length would presumably result in *seeing* them as equal. But it doesn't, and that suggests that there are at least two different systems, or modules, in play: a visual one, and a cognitive-rational one, with the former being unaffected by developments in the latter. This is an idea that I will return to.

Meanwhile, a note on the Müller-Lyer variant (Diagram 2) might help to reinforce the point. Segment C, remarkably, is 20% longer than segment D. Visually, this is rather difficult to believe even after you have checked the measurements.

There are many other optical illusions, of course, in addition to the Müller-Lyer, and some of them are less familiar. Here are two:

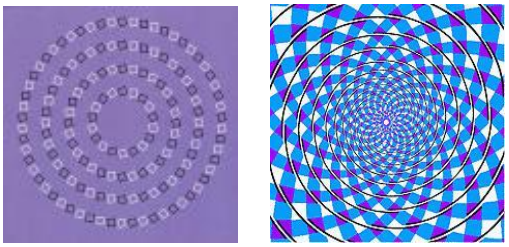

Diagram 3
Spirals

Both graphics look as if they depict spirals, but closer inspection shows that, in each case, it is a set of concentric circles. This can be quite difficult to see, but it helps to place something over each graphic so that half of it is covered. This normally dispels the impression of spirals, and makes the concentric circles easier to discern.

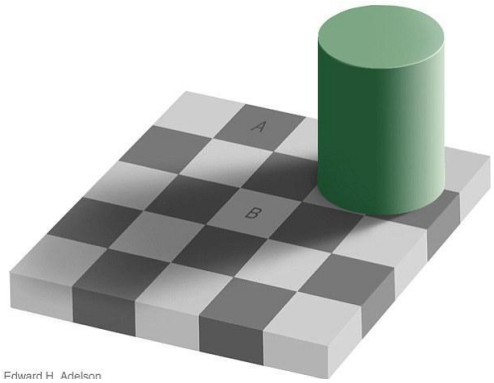

Diagram 4
Chessboard: Edward H Adelson

This is one of the least well known, but most astonishing, of visual illusions, devised by psychologist Edward Adelson. Remarkably, the squares labelled A and B are exactly the same colour and shade. It is difficult, if not impossible, to believe this without checking. The best way to do this is to photocopy the diagram, and then use a heavy-duty marker pen to efface all but the two squares concerned. It will then be clear that they are indeed identical. The effect is created by the way in which our expectations that alternate squares on a chessboard are dark and light interacts with the way we visually compensate for what appears to be a shadow cast over the board. It is a compelling illustration of the idea that 'we see what we expect to see'.

Once again, we cannot trust our initial judgment, and it turns out that there is a discrepancy between *how it is* and *how it seems*. In this case, we have to devise a procedure to determine whether A and B are the same shade or not; but it is a method to which everyone readily agrees, and which produces the same result, irrespective of who carries it out. Moreover, knowing that squares A and B are identical does nothing to dispel the illusion: they still look completely different. The use of a checking method has no impact on the visual conviction that they cannot possibly be the same. To put it another way: the intuition that they are *not* the same is highly resistant to evidence that they are. But intuitions, as I shall suggest later, are like that.

Cognitive illusions

Visual illusions can be regarded as belonging to a much wider class of cognitive illusions, in which the visual system does not play as significant a role, but other cognitive systems do (Myers 2002, Wilson 2002). As Piatelli-Palmarini (1996, p. 17) puts it: 'Phenomena identical to optical illusions exist in the world of thought'. It would seem that every form of data processing, whether in the perceptual system or in systems dedicated to other types of judgment, can succumb to illusions. These illusions are not simply 'mistakes' of judgment and reasoning – any more than our perception of Adelson's chessboard is a 'mistake'. There are three main reasons for this. First, illusions are systematic and predictable. The vast majority of people fall for them. Ordinary mistakes just happen; there is no particular pattern to them. Second, like visual illusions, cognitive illusions are highly resistant to the evidence that they *are* illusions. They persist, even when it has been shown that *how it seems* is not *how it is*; and this, too, is not typical of everyday mistakes. 'Some parts of our minds are unable to use knowledge available to other parts of our minds. As with the eye, our mental "modules" remain impervious to the corrections offered by logic, arithmetic, and rational judgment' (Piatelli-Palmarini 1996, pp. 17-8). Third, we are more likely to make ordinary mistakes in certain circumstances: when we're tired, preoccupied, drunk, or just feeling lazy. Cognitive illusions, like optical illusions, can occur when we're awake, motivated, focused, and sober.

There is an wide variety of such illusions, ranging from simple, everyday experiences to less familiar forms of reasoning. We can begin with some relatively homely examples, two of them akin to optical illusions. Consider the following four statements. Which one of them is false?

- Cows are more closely related to horses than to whales
- Cardiff is west of Edinburgh
- Rome is south of New York
- There are more words beginning with 'r' than words that have 'r' as their third letter

As it happens, they are all false; but the vast majority of people think they are all true. The reasons why they seem to be correct are interesting, but slightly different in each case. Cows *look* more like horses than they look like whales. They are about the same size, and they have four legs and graze in fields. Genetically, however, they are more closely related to whales, however counterintuitive this may seem (Buntjer *et al.* 1997). But that, of course, is the point. *How it seems* turns out to be different from *how it is*, and intuition proves not to be a very reliable guide. Similarly, a standard map of the UK – a weather map, for example – certainly makes it *look* as if Edinburgh is east of Cardiff. It is on the eastern side of the country, after all, while Cardiff is on the western side. However, the stretch of land that runs from the northern parts of England up through Scotland does not lie on an exactly north-south line. Instead, it tilts over slightly – and to such an extent that Edinburgh is, in fact, marginally west of Cardiff (as its longitude in an atlas will confirm).[3] Think of Rome, and you think of very hot summers. Think of New York and it is more likely to be cold winters. Surely, then, Rome must be south of New York. But no. The climate is not a reliable substitute for latitude, because (obviously) the two cities are on different continents; Rome is in the sheltered Mediterranean basin, while New York is on the exposed North Atlantic coast. Finally, it is much easier to think of words beginning with 'r' than words which have 'r' as their third letter, mainly because we are more accustomed to classifying words by their first letter – in dictionaries, for instance – than by their third. However, that does not alter the fact that there are vastly more words whose third letter is 'r' than words whose first letter is 'r'.

The resistance of illusions to disconfirming evidence applies to all these examples, but most obviously to the first. Even so, I still find it difficult-to-impossible to *think of* Cardiff as east of Edinburgh; and New York still *feels* as if it's north of Rome. Similarly, although I could retrieve lots of words which have 'r' as their third letter, I was able to produce a list of words beginning with 'r' far more quickly.

Here is a bit of simple book keeping. A farmer bought a horse for $60 and sold it for $70. A little later he bought it back for $80, and subsequently sold it again for $90. How much money did he make on the these deals overall? Most people answer $10, because he obviously makes $10 on the first sale, loses that on the second purchase, but makes it again on the second sale. Apparently, most German banking executives say $10 as well, at least according to a German colleague of Myers (2002, p. 108). Trying this out on students, I have found that about 75% of them say $10; but I've heard everything from zero to $30, and a couple of people even suggested that he made a loss. The correct answer is $20, which is somewhat easier to see if you first work out the total expenditure ($60 + $80 = $140) and then the total income ($70 + $90 = $160). What accounts for the illusion, though, is the time sequence and the fact that, as they read through it, the majority of people automatically deduct $10 for the difference between the first sale ($70) and the second purchase ($80).

Another puzzle. If you tore a sheet of ordinary paper in half, put the two halves together, tore again, put the four sheets together… and so on; and if you did that 25 times – the sheet of paper would have to be a large one – roughly how thick would the resulting stack of paper be? Guesstimate. I have tried this on scores of people, and not one of them has estimated more than 18 inches. The correct answer is about 2 *miles*. Don't try it at home. The illusion here arises from the fact that it is almost impossible for a non-mathematician to imagine exponential increases, which this is. We vaguely picture the tear-and-stack procedure up to perhaps the first three or four iterations, and then lose track; at which point we imagine the resulting thickness, and multiply it by about ten. I suspect most people would be surprised to learn that the final stack was, say, ten yards (it isn't); but they could accommodate it. But almost everyone is astonished by the 2 miles, and some at first refuse to believe it. The persistence of the illusion is partly confirmed by the fact that, when asked how thick the stack would be if the tearing-and-stacking routine was repeated 40 times, the usual tendency is to guess at something like 100 miles. The right answer is about 65,000 miles. Most people's intuition does not adjust to getting the first version of the puzzle wrong, and their estimates for the second are no better than their estimates for the first. The illusion is so powerful that they do not learn from getting it wrong earlier.

In both the last two examples, the procedure for determining whether *how it seems* is also *how it is* (or not) is simple mathematics; indeed, simple arithmetic in the first case. Nobody doubts that this is the right way to go about testing their intuitions – even if they find the result difficult to believe at first – just as no-one doubts that consulting the atlas is the right way to test intuitions about geography, or that checking the DNA of cows, whales and horses is the best way to test their intuitions about evolutionary relationships. So this type of illusion conforms to the template suggested by the Müller-Lyer.

One type of cognitive illusion, more significant than the relatively trivial examples we have considered so far, has been studied systematically during the last forty years. These are illusions arising from the widespread, indeed almost universal, tendency to get-it-wrong with logical and probabilistic reasoning (Kahneman *et al.* 1982, Gilovich *et al.* 2002). The assessment of risk, including medical risk, is one important field in which probability judgments are made, and it has been shown that doctors, nurses, and other health professionals routinely succumb to illusions of this kind, especially in evaluating the results of tests (Eddy 1988).

An entertaining example of a probability illusion is the so-called Monty Hall Problem (Myers 2002). In this problem, a game-show contestant is invited to choose one of three doors, behind one (but only one) of which is an attractive prize. Once a door has been chosen, the host (Monty Hall) opens one of the two remaining doors, and shows that there is no prize behind it. He then tells the contestant she can either stick with her original choice or switch to the third door. Most people are convinced that this is a 50/50 decision. But, in fact, the chance of winning if you *switch* doors is 2/3. It is only 1/3 if you stick to the door you chose initially (Granberg & Brown 1995).

To see this, imagine that the three doors are A, B and C, and that the big prize is behind door C. As the contestant, you have three initial options. We can go through what happens with each of these choices, assuming that you adopt a 'switching' strategy:

- Choose door A. Monty opens door B, and invites you to stick or switch. You switch to door C. You win.
- Choose door B. Monty opens door A, and invites you to stick or switch. You switch to door C. You win.
- Choose door C. Monty opens door B, and invites you to stick or switch. You switch to door A. You win.

Three possibilities, and with the 'switching' strategy you will win two of them. Now we can try it with a 'sticking' strategy.

- Choose door A. Monty opens door B, and invites you to stick or switch. You stick. You lose.
- Choose door B. Monty opens door A, and invites you to stick or switch. You stick. You lose.
- Choose door C. Monty opens door B, and invites you to stick or switch. You stick. You win.

Again, three possibilities, but this time you win only one. So switching wins 2/3 and sticking wins 1/3. The same will happen, obviously, if the prize is behind one of the other two doors. The illusion – and it is a very powerful one – that the stick-or-switch choice is 50/50 is based on the assumption that, when you get down to two doors, the prize is equally likely to be behind both of them. But it isn't. When you made your initial choice, your chance of success was 1/3. That doesn't change just because Monty has

now opened one of the two remaining doors (and he is clearly *not* going to open the door with the big prize behind it). After the initial choice, the probability of the prize being behind one of the *other* two doors was 2/3. But one of them has now been eliminated. That's really helpful, because it means that the 2/3 probability is now attached to just one door rather than two. In opening one of the doors, Monty has actually done you a favour.

In this example, then, a readily understandable type of judgment is clearly related to probability theory. Not only do the vast majority of people get this judgment wrong, but the incorrect answer often strikes them as 'obvious'; and it can take several runs through the procedure adopted above to convince them otherwise. Once again, an initial judgment proves unreliable, and we need a method to check whether it is correct or not.

Probability illusions are not, of course, confined to television show programmes. They turn up in health care, and particularly diagnostic tests. This is an example. A urine sample from a patient shows raised sugar levels. But only 1 in a 100 such tests are indicative of diabetes. So you do a fasting blood sugar test. This test correctly identifies 80% of diabetics (this is called its degree of sensitivity) and 90% of non-diabetics (the degree of specificity). The fasting blood sugar also shows raised sugar levels. What is now the probability that the patient has diabetes?

Estimates of the probability, even among health care professionals is usually high, well above 50%, and frequently 80%, or close to it. The right answer is about 7.5%. The illusion depends on a confusion between two distinct probabilities: (a) the probability that the test will be positive for someone who has diabetes, and (b) the probability that someone has diabetes if the test is positive. An alternative way of putting this is to distinguish between (a) the likelihood that the test will be positive, given the someone has diabetes, and (b) the likelihood that someone has diabetes, if the test is positive. Sensitivity, the test characteristic which in this case was 80%, measures (a). However, the original question – what is now the probability that the person has diabetes, given a positive fasting blood sugar test – asks about (b).

To see why the (b) probability is about 7%, consider 1,000 fasting blood sugar tests, all administered to people whose original urine samples showed elevated sugar levels. Of these, we know from the overall base rate that about 1% will have diabetes. That's 10 people. The remaining 990 won't have diabetes.

Consider the 10 who do have diabetes. The sensitivity of the fasting blood sugar test is 80%, so the test will be positive in 8 of those 10 cases. The other two cases will show negative, because the test is not perfect. So that's 8 positives and 2 negatives so far.

Now consider the 990 who don't have diabetes. The specificity of the test is 90%, so the it will show as a negative in 90% of those case, which is 891. That leaves 99 (10%) which will be positive, because no test (as before) is perfect. So that is 99 positives and 891 negatives.

Put the two groups of tests together. Overall, we have 107 positives (8+99) and 893 negatives (2+891). But how many of the 107 positives actually have diabetes? Well, we have already said that a total of 10 people have diabetes (out of the population of 1,000), but two of the them have negative tests. So of the 107 positives, only 8 will actually have diabetes. It follows that the probability of any of these people having diabetes, given a positive test, is 8/107 or, other words, 7.48%. The table below summarises the position.

	Test positive	*Test negative*	*Total*
Have diabetes	8	2	10
Don't have diabetes	99	891	990
Total	107	893	1000

Of course, if your fasting blood sugar shows negative, the probability that you have diabetes is 2/893, or about 0.22%. A negative test cannot demonstrate beyond any shadow of a doubt that you don't have diabetes – again, it is not perfect – but it *can* show that the probability is vanishingly small. This is why we talk about 'false negatives' (a negative test which fails to detect a condition which is in fact present) as well as 'false positives' (a positive test which says that the patient has the condition when in fact she doesn't).

I will offer one further example of probability illusion, this time linked to a significant health care issue of recent times. Many people believe the claim that the MMR vaccine causes autism. Perhaps the main reason for this is personal experience. For example, consider some comments from the *Daily Telegraph* website, 9th July 2007.[4]

> I think MMR causes autism. My son is autistic. He was diagnose shortly after him having his MMR jab. I noticed a change in him after him having the jab, he started to arrange his toys and would not talk. Sometimes he wouldn't talk for up to 5 hours. Before his injection he was a bright loving normal boy.
>
> Whether there is a proven link between MMR and autism or not – all you cynical, scientifically inclined individuals need to just shut your "politically correct" ranting and listen to parent's concerns. My son too, was developing normally and had started talking but soon after having his MMR jab, stopped talking completely. Now, almost 2 and half, he still does not speak. Unless you have a child that you have to watch painfully trying to communicate, don't talk about how "safe" the MMR jab is as you are none the wiser! It is only right that parents would want to question the suitability of subjecting their child to 3 vaccines in one go.

In these cases, as in many others, the conviction that there is a causal link between MMR and autism is a result of the diagnosis being made, or changed behaviour being observed, soon after the vaccination. This is an extraordinarily powerful illusion – if Y follows X, then obviously X must be the cause of Y – particularly if Y is an emotionally loaded life event. The conviction arises from the sense that one has witnessed the causal connection actually happening, and the fact that there seems to have been no other precipitating factor. The autism must have had a causal trigger, the trigger must have occurred shortly before the autism became apparent... and the only candidate for the trigger is the MMR. Therefore, the MMR *must* have caused the autism. There can be no other explanation. This kind of certainty is usually impervious to any argument based on statistics. I *know* that X caused Y; therefore, either the statistics are wrong, or else there is a conspiracy to persuade me (and other parents) that the MMR is safe. We will not understand the reduced MMR uptake if we do not appreciate just how convincing this illusion is.

But it *is* an illusion, just as much as the Müller-Lyer. Demonstrating this, conclusively, requires some fairly sophisticated statistical techniques. However, the key idea is straightforward. The conviction is usually based on a single case (the parent's own child), or a small number of cases (other children who developed autism shortly after being vaccinated with MMR). These are all X-then-Y cases. Once the suspicion that X-caused-Y has been formulated, there tends to be a search for further X-then-Y cases in

order to substantiate (this is something called confirmation bias, and we'll come back to it later). The trouble is, this approach ignores all the cases in which there was X but no Y, and all the cases in which there was Y but no X. In other words, it ignores all the cases in which the MMR vaccination was not followed by autism, and all the cases in which autism was diagnosed despite the fact that the child was not MMR-immunised. If it can be shown that children who have had the MMR vaccine are no more likely to be diagnosed with autism than children who have not had MMR; and if it can be shown that children with autism are no more likely to have had the MMR vaccine than children who do not have autism... then the claim that there is a causal connection is undermined, however convincing personal experience may be. This would be the equivalent of measuring the horizontal lines in the Müller-Lyer, and discovering that they are the same length, no matter how obvious it seems that they aren't.

As an illustration, I will take just one example from the considerable literature on this topic (Smeeth *et al.* 2004). In this study, a group of children who were diagnosed with autism, or some other pervasive development disorder (PDD), were compared with a group of children with no diagnosis. The PDD group consisted of 1,294 children, each of whom was matched (by age, sex and general practice) with up to five controls. An index date was set for both groups. For the PDD group, it was the age at first diagnosis. For each control case, it was the date at which they were the same age (to the nearest month) as their matched case was at the time the latter was first diagnosed. So, for example, a child diagnosed with PDD at the age of, say, 3 years 8 months was matched with up to five children who, at exactly the same age, did not have a PDD diagnosis.

The logic of this procedure is fairly obvious. If MMR causes autism, then it is more likely that children in the PPD group would have had the vaccination *before* the index/diagnosis date than the children with whom they were matched. But this proved not to be the case. The following table, a simpler version of one presented in Smeeth *et al.* (2004), gives the overall position.[5]

Cases compared with controls

	Children with PDD	Children with no diagnosis
Total	1294	4469
MMR vaccination before index date	1010 *78.1%*	3671 *82.1%*

So the greater likelihood of having had the MMR before the diagnosis/index date doesn't show up. If anything, in fact, it is the controls who are more likely to have had the vaccination beforehand. In other words, the evidence is inconsistent with the hypothesis that MMR causes autism.

Here again, then, we have an illusion related to our inability to assess probabilities. For some parents whose children were diagnosed with autism, or another PDD, shortly after the MMR vaccination, it is starkly obvious that the MMR caused the condition. What other explanation can there be? Surely, it is not a coincidence. Indeed, the theme of 'it can't be a coincidence' turns up frequently in the stories told by such parents, as illustrated by two more posts from the *Daily Telegraph* website. 'After the MMR he had a fever then stopped talking. Is this coincidence?' 'I am absolutely sure that the MMR jab is the trigger for my son's autism. He was developing normally until the jab. Within ten days of receiving it he became brain damaged; the coincidence is too great.' So the conviction ('I am absolutely sure') is based on a single case, personal experience, and the emotional pain associated with it. The single case is entirely persuasive, and like other illusions, is resistant to the evidence that it *is* an illusion. Here, the

evidence consists of 5,763 cases, not just one; and it shows that the there is no association between the MMR vaccine and autism. The situation is not materially different from being deceived by the Müller-Lyer, or Adelson's chess board illusion, both of which are highly resistant to the evidence provided by procedures designed to test them (measuring the lines, or effacing the other squares).

In this example, as in so many others, *how it seems* can be completely convincing... and yet still at odds with *how it is*. In a large population, in this instance 5,763 cases, there are bound to be some in which autistic behaviour began, or was diagnosed, very shortly after ('within ten days of') getting the MMR. This *is* just a coincidence; but, for the person to whom it happens, it seems as if it cannot possibly be.

Heuristics and biases

Although, in presenting these examples, I have moved from charming 'Gosh, who'd have thought it?' illusions like Adelson's chess board, to illusions which are more obviously relevant to health care and health-related research, it might still seem that cognitive illusions are relatively rare, and that the ones I have discussed stand out precisely because they are untypical (for instance, there aren't many illusions of the Cardiff/Edinburgh or New York/Rome type). In this section, however, I want to suggest that they are rife, and that we are constantly at risk of succumbing to them. The subtitle to Piattelli-Palmarini's book on this topic ('How mistakes of reason rule our minds') may be slightly over-egging it, but there is no doubt about the extent of our vulnerability to these illusory experiences. This is something 'found almost anywhere, in almost anyone, and just about at any moment' (Piatelli-Palmarini (1996, p. 2).

The study of cognitive illusions has been going on for at least fifty years, but took off during the 1970s (Gilovich & Griffin 2002), under the heading of 'heuristics and biases'. I should briefly explain the two terms. 'Heuristic' refers to a type of judgment-making 'short-cut', several examples of which we have already seen. These short-cuts may be adopted consciously or unconsciously. For instance, in assessing whether Edinburgh is east or west of Cardiff, we adopt the short-cut of asking ourselves which side of the country the two cities are on. Edinburgh is on the east coast, Cardiff on the west coast. That seems to settle the matter... but of course it doesn't. It turns out that being on the eastern side of the UK, while Cardiff is on the western side, does not entail that Edinburgh is east of Cardiff. 'Bias' is perhaps a more unfortunate term, since it is easily confused with prejudice or lack of impartiality. However, *cognitive biases*, the sense used in the expression 'heuristics and biases', are not prejudices, and the term does not signal a deliberate lack of fairness or open-mindedness. Instead, it refers to unconscious cognitive mechanisms which are triggered in situations of various sorts, and which more often than not lead to bad or mistaken judgments. For example, the claim that MMR causes autism – the evidence being that, in a particular case known to the speaker, the MMR vaccine was followed by the appearance of autistic behaviour – is rooted in a cognitive bias which mistakes 'X-occurred-then-Y-occurred' for 'X-caused-Y'. This bias has a number of names, but is often called the *post hoc fallacy*.[6]

The difference between 'heuristic' and 'bias' is largely contextual. Heuristics are cognitive short-cuts. It is reasonable, indeed rational, to use them because they are accessible, rapid, economic... and often right. They are 'fast and frugal' (Gigerenzer *et al.* 1999, Gigerenzer *et al.* 2002), where 'frugal' means that they do not require much in the way of resources: they impose no great cognitive load, demand no difficult calculations, require no expert knowledge, and take up little time. To determine the genetic relation between cows, horse and whales requires either specialist knowledge of evolutionary biology and genetics, or the ability to invest time in doing the necessary reading. Substituting resemblance as a proxy criterion is obviously much quicker, and requires no particular expertise. In this instance, it gives you the wrong answer, but attribute substitution remains a sane and sensible strategy. The term 'bias' is reserved for circumstances in which the substitution of an attribute is very likely to go wrong, or when other cognitive short-cuts regularly and predictably deliver incorrect answers. It is over-simplifying to

say that a bias is a heuristic which hasn't worked – or which routinely doesn't work in certain types of situation – but, for our purposes, it's close enough. 'Cognitive illusion' is just a term for the outcome of cognitive bias, what happens when the vast majority of people fall predictably into the same cognitive trap.

The number of cognitive illusions is so great, and they are so frequent, that they can be classified into types. There is no generally recognised standard list, and many of them overlap; but the same biases do tend to crop up in most typologies.[7] We have already seen examples of some of them. The belief that cows must be more closely related to horses than to whales is an illustration of the *representativeness heuristic*. This is the temptation to use resemblance as a proxy for something else – in the cow/whale case, the genetic connection between two species. Of course, resemblance frequently *is* an indicator of family relationships; but not always. And when it isn't, it can lead us astray. Obviously, cows resemble horses far more than they do whales; but the fact remains that they are more closely related to whales genetically. Though there is no name for the geographical illusions, they are like the representativeness heuristic in one respect: they both involve the substitution of one attribute for another in the making of the judgment. Just as we substitute resemblance for genetic relationship in the cow/whale example, so we substitute climate for latitude in the New York/Rome example, and 'side of the country' for 'east or west' in the Edinburgh/Cardiff example. This is termed *attribute substitution* (Kahneman & Frederick 2002), and it underlies many forms of cognitive illusion. The idea is that, in making a judgment, we find it difficult to evaluate the 'target' attribute (genetic relationship, latitude) directly, so we substitute a different attribute (resemblance, climate) that is much more readily assessable. The substitution may be conscious or unconscious, and the substituted attribute will usually have *some* connection with the target attribute; but the connection is not a reliable one, and certainly does not apply in all cases.

Another familiar cognitive bias, which appears on all the lists, is *neglect of base rate* (or the base-rate fallacy). Again, we have seen an example of this in the fasting blood sugar test for diabetes. When we learn that the sensitivity of the test is 80% – it correctly identifies 80% of diabetics – we assign greater significance to that fact than we do to the fact that the proportion of diabetics in the general population is only 1%. A positive fasting blood sugar test does increase the probability that the person concerned has diabetes, but not by much because the 'background likelihood' of having diabetes is so low. This 'background likelihood' is called the 'base rate'; so when we forget about that, and attach undue weight to the test's sensitivity, we are *neglecting the base rate*.

I should also say something more about *confirmation bias*, mentioned in passing during the discussion of the MMR vaccine. Suppose we develop a hypothesis, for example that X-causes-Y, and our evidence is that there are several cases in which X is immediately followed by Y. What additional evidence would suggest that the hypothesis was correct? The overwhelming temptation is to reply: further cases of X being followed by Y. A bit more thought might also suggest: cases in which neither X nor Y happened. So we go hunting for more cases like this: X-followed-by-Y, and no-X-no-Y. We find them, and infer that the hypothesis is confirmed.

This is classic confirmation bias. Let's consider the possible cases. There are four:

(a) X happens, then Y happens
(b) X happens, but Y does not happen
(c) X does not happen, but Y happens
(d) X does not happen, and Y does not happen

In table form:

	Y happens	Y doesn't happen
X happens	a	b
X doesn't happen	c	d

The search for additional evidence suggested above targets (a) cases and (d) cases. But in searching for cases of that kind, we screen out, ignore, dismiss, or simply do not look for (b) and (c) cases. We are, in other words, searching only for cases that are *consistent* with the hypothesis. We are not bothering to look for cases that are *inconsistent* with it. Yet there may be a large number of (b) and (c) cases... We won't know if we don't look. Suppose, for example, that the number of cases in each cell is as follows:

	Y happens	Y doesn't happen
X happens	25 (a)	30 (b)
X doesn't happen	30 (c)	15 (d)

The confirmation bias strategy, searching for (a) and (d) cases, will find (potentially) a total of 40. But it will overlook the 60 (b) and (d) cases. More particularly, it will not recognise that Y happens more frequently when X didn't happen (30 cases) than when it did (25 cases); and that there are more cases in which Y does *not* follow X (30) than cases in which Y does follow X (25). Looking at the table as a whole, it becomes obvious that the evidence is inconsistent with the hypothesis. But the search for only confirming (a) and (d) cases – confirmation bias – would simply not spot this. Effectively, this is what happens in the MMR case. There are plenty of MMR-followed-by-autism cases, and these are used as evidence to support the hypothesis that MMR causes autism. But the search for confirming cases will miss the huge number of *dis*confirming cases, those in which MMR was not followed by autism, and in which autism was not preceded by MMR. For an excellent review of the 'ubiquitous phenomenon' of confirmation bias, see Nickerson (1998).

To close this section, I will briefly describe some other well known sources of cognitive illusions. I will return to a few of them in the next section, when I will indicate the ways in which they can compromise research. Here, I will do no more than indicate the type of bias or heuristic that accounts for a particular kind of illusion. For accessible introductions to cognitive illusions, and the heuristics and biases which account for them, see Gilovich (1991), Piatelli-Palmarini (1996), Dawes (2001), Myers (2002), and Chabris & Simons (2011). For more technical material, see Kunda (1999) and Gilovich *et al.* (2002).

Anchoring bias. Making a judgment based heavily on a single factor, which may not even be relevant to the topic. For example, if an arbitrary number is prominently displayed, and people are invited to estimate how many African states are members of the United Nations, the estimates will be 'anchored' by the number. The higher the number, the higher the estimates (Chapman & Johnson 2002). No matter how irrelevant the anchoring factor, one's judgments can still be unconsciously affected by it (Tversky & Kahneman 1974, Reidpath & Diamond 1995). This shades into...

Primacy effect. Making a judgment based heavily on initial events rather than subsequent events, or the first piece of evidence one comes across. This is less likely than anchoring bias to be unconscious, but it is clearly related. Something which attracts the attention at the outset, whether it is an irrelevant factor (like the arbitrary number) or the first thing one notices (like the piece of evidence), influences

the final judgment to an incommensurate extent. The upshot is that an initial judgment, made on the basis of something one notices at an early stage, tends to survive the appearance of new (and possibly countervailing) evidence, and becomes the final judgment as well (Baron 2000). It is worth adding that primacy effects are known to occur in clinical judgment making (Curley *et al.* 1988).

Availability bias. This is another form of attribute substitution, in which the attribute of typicality is substituted by what is readily available in memory, and by what is more dramatic, vivid, emotionally charged. For example, one might imagine that accidents involving scissors, hammers and chainsaws are far more frequent than accidents with toilets; and accidents with chainsaws are certainly more likely to be dramatic, and presumably have more traumatic consequences. But, according to the US Consumer Product Safety Commission, accidents involving toilets are more common (or were more common in 1997, at least): they were responsible for 44,335 estimated injuries, while chainsaws were responsible for 29,684 (cited in Myers 2002, p. 205). Similarly, beds, mattresses and pillows were implicated in 456,559 estimated injuries during 1998, compared to 384,967 estimated injuries due to home workshop power saws, playground equipment, and cooking ranges/ovens *combined* (Myers 2002).

Inattentional blindness. Before reading further, it might be a good idea to visit the following URL, http://www.simonslab.com/videos.html, and take the selective attention test, which is the first video on the page. If you read the rest now, you'll spoil the effect.

Attention blindness is the failure to recognise, or even notice, unexpected features of a situation. About 50% of people who take the 'test' fail to notice the unusual event. I did. I played the video again, and at first refused to believe that it was the same video as the one I had just watched. The point is that we often do not notice things that we don't expect to be there, even when they are as intrusively obvious – in retrospect – as a gorilla among a group of basketball players. Many similar effects can be demonstrated (see Chabris & Simons 2011 and http://www.dansimons.com/videos.html). While the best known effects are visual, Chabris & Simons note that 'it's not just limited to visual attention, but applies equally well to all our senses and even to broader patterns in the world around us' (p. 39).

Framing effect. Judgments and decisions can often be affected by the 'frame' in which information is presented. The same information presented in two different ways will elicit two different responses. In psychological experiments, the best known examples of framing concern decisions about risk, which depend to a considerable extent on how the relevant data is presented. For example, participants were asked to choose between two programmes designed to combat a disease likely to affect 600 people.

Program A outcome	200 people will be saved.
Program B outcome	A one-third probability that 600 people will be saved, and a two-thirds probability that no-one will be saved.

70% chose option A. Meanwhile, another group of participants were given the following options:

Program C outcome	400 people will die.
Program D outcome	A one-third probability that no-one will die, and a two-third probability that 600 people will die.

This time, 78% chose option D. The point is, however, that programmes A and C are exactly the same. Equally, programmes B and D are the same. Yet, in the first frame, option A is preferred, while in the second frame option D is preferred. Although most framing studies have examined risk perception and preference construction (Lichenstein & Slovic 2006), the basic idea – the way in which information is presented affects people's judgments about it – is a general one, applying to many different contexts.

Illusory correlation. Pretty much what it sounds like: seeing a correlation where there is none. This is related to confirmation bias, in that it is usually the result of focusing on cases in which two events co-occur and forgetting that there might be cases in which they don't. Illusory correlations are at the root of various myths such as: more babies are born at full moon; infertile couples who adopt become more likely to conceive; sugar makes children hyperactive; cell phone cause brain cancer; weather changes trigger arthritis pain; getting cold and wet causes colds; a positive attitude helps people to fight cancer, famous people tend to have embarrassing brothers (Myers 2002).

Congruence bias. Suppose you are given a sequence of numbers: 2, 4, 6, 8. You are informed that this sequence conforms to a certain rule, and that your task is to find the rule. The way you have to do this is to present other number sequences, and ask whether they conform to the rule or not. So you try: 3, 5, 7, 9. You are told that this sequence does indeed conform to the rule. Then you try 110, 112, 114, 116. Again, you are informed that the sequence conforms. The third sequence you try is 1.5, 3.5, 5.5, 7.5. This sequence, it turns out, conforms to the rule as well. So you are now ready to formulate the rule. 'The underlying rule,' you say, 'is that each number in the sequence is two greater than the one which precedes it'. You are told that you're wrong. Eh? How did that happen. Well, the actual rule is: 'each number in the sequence is bigger than the one preceding it'. So not 'numbers ascending by two', but just 'numbers ascending' (Wason 1960). This is an example of congruence bias: the tendency to test just one hypothesis – sometimes, as in this case, because it is so 'obvious', conforming to a familiar pattern – rather than testing several alternatives. It is related to both confirmation bias and the primacy effect, given that the first hypothesis is the only one to be tested.

Fundamental attribution error. Sometimes known as correspondence bias. The tendency to attribute someone's behaviour to their disposition rather than to the situation they find themselves in, and to do this to a greater extent than is warranted. A great deal of evidence suggests that negligible – even trivial – factors in a situation can influence behaviour to an unexpected degree, with individual character traits having a relatively minor role. The classic experiments focus on helping behaviour. In one study, for example, people emerging from a telephone box were confronted by a passer-by whose folder full of papers had been scattered across the pavement. One might imagine that whether the person stopped to help the passer-by would depend on whether they were kind, helpful, altruistic people. However, some of them had found a dime in the coin return slot (planted by the researchers), while others hadn't. Most of those who had found a dime in the telephone box stopped to help. All but one of those who had not found a dime failed to do so (Isen & Levin 1972). Findings of this sort have been replicated repeatedly (Doris 2002), and it seems clear that behaviour is far more context-dependent than we usually imagine. Nevertheless, most of us still attribute the behaviour of others to character traits and dispositions, while discounting (or at least assigning far less weight to) situational factors. In doing so, we are committing the fundamental attribution error.

Observer expectancy effect. The idea that an observer's expectations can influence outcomes has been tested in a number of different contexts. One of the most well known is Rosenthal & Jacobson (1992), which demonstrates that, if teachers expect pupils to improve their performance, that is what happens. In a slightly different guise, observer expectancy effects also occur in research contexts, particularly in clinical trials, where there is a tendency for non-blinded researchers to generate results consistent with their preferences (Schulz *et al.* 2002). Specifically, when an outcome assessor knows which subjects have been randomized into which arm of the trial, the treatment effect of the intervention being tested is regularly and significantly exaggerated (Wood *et al.* 2008). Interestingly, the bias associated with this lack of blinding is almost exclusively confined to trials with subjective outcome measures, such as participant-rated pain scores. Apparently, there is less scope for bias with 'harder' measures. It is, of course, for this reason – the fact that the researcher's awareness of who is, and who is not, receiving the experimental treatment affects the processing of data – that blinding is adopted as a protocol in the first place.

Belief bias. A form of attribute substitution, in which 'Do I believe this conclusion?' is substituted for 'Is this conclusion valid?' It is usually illustrated by a syllogism like the following (Stanovich 2003):

All living things need water.
Roses need water.
Therefore, roses are living things.

This is an invalid inference, though most people are initially inclined to think it valid, including 70% of university students (Sá *et al.* 1999). What happens is that they recognise the syllogism's conclusion as something they already believe to be true, but confuse believability with validity, and therefore assume that the inference is valid. If this syllogism is compared with another, the point becomes obvious:

All insects need oxygen.
Mice need oxygen.
Therefore, mice are insects.

The form of inference is exactly the same as in the first example, but now the fallacy is easier to spot, because the believability of the conclusion is not a distraction. As Stanovich (2003, p. 292) observes, 'prior knowledge does not get in the way'. The possibility of belief bias generalises to other kinds of inference. For example, the methodological appraisal of scientific studies can be compromised by prior beliefs about the findings (Koehler 1993). This has been demonstrated in the context of clinical trials (Resch *et al.* 2000, Kaptchuk 2003), with well designed studies being dismissed because they indicate that an unconventional treatment is effective (or that an established treatment is ineffective), and poorly designed studies being accepted because they appear to confirm widely held opinions.

Self-serving bias. Any account, judgment or interpretation which presents the person concerned in a more positive light may be an example of self-serving bias. 'Researchers have repeatedly demonstrated that people on average tend to think they are more charitable, cooperative, considerate, fair, kind, loyal, and sincere than the typical person but less belligerent, deceitful, gullible, lazy, impolite, mean, and unethical—just to name a few' (Epley & Dunning 2000). Most of us think that we are more intelligent than our peers, better looking, more ethical, less biased, more likely to go to heaven. We are also better than average drivers, have a better than average sense of humour, and a better than average ability to get along with others (Myers 2002). Nearly everybody believes that they contribute more to joint, team or departmental projects than the majority of their colleagues. The same is true of marriage. Wives and husbands are usually convinced that they do more of the housework than their spouses think they do (Kruger & Gilovich 1999); and divorced people routinely blame the breakup on a partner who failed to pull their weight (Gray & Silver 1990). Despite all this – what a wonderful irony – most of us think that we're less self-serving than other people, and that we're less susceptible to bias in general (Pronin *et al.* 2004).

This selection should give some indication of the pervasiveness of the cognitive mechanisms which routinely produce the illusion that something has obviously been got right when in fact it has been got wrong. The titles and subtitles of some of the more popular books signal the sheer ordinariness of these illusions: 'The fallibility of human reason in everyday life' (Gilovich 1991), 'How mistakes of reason rule our minds' (Piatelli-Palmarini 1996), 'Everyday irrationality' (Dawes 2001). The conflict between an initial judgment and the evidence that it is mistaken is not, Gendler (2010) insists, 'an anomalous or idiosyncratic feature of arcane or unusual cases', but a 'central feature of our mental lives' (p. 124). I began my review with the familiar (the Müller-Lyer), the charming but trivial (Edinburgh and Cardiff), and the entertaining (Monty Hall). But we have now reached a point at which we appear to be awash in cognitive illusions, pitfalls at every turn. Soon, I'll return to the theme of objectivity. But first I want to look briefly at the difference that cognitive illusions can make, not just to everyday life, but to research.

Cognitive illusions and research

Any sort of illusion represents the possibility that we can, regularly and predictably, get it wrong when processing and evaluating data. Whether we're talking about incoming sensory data, as with optical illusions,[8] data on geographical locations, data on helping behaviour, data on our own character traits, data pertaining to probabilities, or data about cause and effect, we sometimes find ourselves led astray by familiar cognitive routines which for the most part work well, but which routinely break down in certain circumstances.

Data analysis in empirical enquiry is merely a special case of data processing in general, so cognitive illusions are going happen in this domain, just as they do in every other. This applies to quantitative as well as qualitative studies. But, as Trout (1998) notes, qualitative methods have a special vulnerability, and they are especially prone, in his view, to 'the base-rate fallacy, the availability bias, fallacies of representativeness, and other problems' (p. 226). Like Trout, I will pay particular attention to illusions which are likely to affect qualitative research, but I will refer to experimental designs as well.

I have already suggested that belief bias and observer expectancy effect crop up in research. In belief bias, believability is taken as a criterion of methodological soundness. In any study, it is likely that the method employed will be assumed to be valid if the findings are consistent with an accepted theory, but invalid if the findings are inconsistent with it (Fugelsang *et al.* 2004). In observer expectancy effect, the processing of data may be influenced by what the researcher hopes for or anticipates, unless steps (such as blinding) are taken to minimise the risk of this happening.

In both cases, the examples I referred to were clinical trials, but there is no reason to think that either bias is limited to experimental designs. Consider the 'phenomenological nod', a reaction which van Manen proposes as a criterion for what counts as a good phenomenological description. It looks suspiciously like belief bias, except that van Manen takes a positive view of it, arguing that it is proof of a well conducted study. It would appear, then, that instead of regarding believability as a bias-risking *substitute* for soundness, van Manen regards it as an *indicator* of methodological propriety. This position has found favour with some nurse researchers, who assume that if a finding commands assent, as confirmed by the phenomenological 'nod', the study gains 'rigour' credits. For example:

> Rigour or trustworthiness was established in several ways... The 'phenomenological nod' of agreement (Munhall 1994) was obtained from three non-participant nurses, who indicated that the written interpretation of the phenomenon resonated with them, offering additional evidence of credibility. (Burhans & Alligood 2010, p. 1693)

Resonance as rigour, concurrence as credibility. In phenomenological writing, belief bias is reinvented as a cognitive virtue. Instead of *distinguishing* between believability and methodological rigour, writers following van Manen imagine that believability *contributes* to rigour. This is rather like thinking that a phenomenological nod to the conclusion that mice are insects enhances the rigour and credibility of the logical inference examined earlier.

Observer expectancy effect – the likelihood of obtaining findings corresponding to what the researcher anticipates, as the result of unconscious manipulation of the data – is also not confined to experimental method. Any qualitative study runs the risk that, during data collection and analysis, the researcher's expectations will subconsciously affect the outcome, at least to some extent. In fact, because observer expectancy effect is far more likely with self-reported 'subjective' data than with 'harder' independent measures, the probability that the 'codes' and 'themes' of qualitative analysis reflect the researcher's own assumptions and preconceptions is rather high, unless the researcher takes steps to reduce the risk of it happening.

Phenomenologists, of course are aware of this as a potential problem, and they have what they regard as solutions to it: reflexivity and/or 'bracketing'. George & Thomas (2010) provide an example. They refer to phenomenology as 'a process which began with bracketing the researcher's biases' (p. 1092), and then go to say (p. 1094):

> Bracketing presumes that researchers are capable of separating their knowledge from personal experience, at least temporarily, while collecting and analysing data. The procedure... requires a bracketing interview of the principal investigator. This is designed to heighten awareness of any preconceptions or biases arising from personal experiences and minimizes the potential for distortion of during data collection and analysis.

Unfortunately, bracketing misses the point, in common with variations on the theme such as 'bridling' (Nilsson *et al.* 2010); or reflexivity (the 'continual process of critical self-reflection on one's personal biases, preconceived notions, assumptions, theoretical predispositions and ideological commitments': Powers & Knapp 2006, p. 148); or 'personal notes', which record the researcher's 'feelings, beliefs, biases and thoughts', and which thereby mitigate her biases (Burhans & Alligood 2010, p. 1693). The problem with all these devices is that they presuppose the effectiveness of good intentions. The whole point is that the cognitive mechanisms responsible for bias and illusion are usually unavailable for conscious scrutiny. In particular, observer expectancy effect is subconscious. It is not accessible to self-reflection and bracketing interviews. If the conscious determination to mitigate bias were sufficient, we would not need to blind in clinical trials. An assurance from the researcher that she firmly intended not to let her expectations affect the processing of her outcome-measure data would be enough.

The 'bracketing' idea fits the template of objectivity-as-psychic-self-scouring, discussed earlier in this chapter. In order to prevent one's expectations from influencing the study's findings, it is necessary to identify them and 'put them out of play' (Drew 2001). However, this idea clearly presupposes that the preconceptions to be identified and decommissioned are transparent enough to be identified, even if a bit of prompting and nudging – the bracketing interview – is required. The procedure puts much more weight on introspection than the evidence suggests it can bear. Indeed, most people are vulnerable to what Pronin *et al.* (2004) describe as the 'introspection illusion', the 'tendency to treat introspections about the basis of their own judgments and decisions as highly probative or "sovereign" but not to give similar weight to the introspections of others' (p. 782). Admittedly, most of us are prepared to concede that, generally speaking, we are not completely immune to bias. However, on any particular occasion our 'introspection reassures us that the judgments in question were correct and supported by sound reasons... On the basis of such introspective evidence, we are apt to conclude that although we may well have been guilty of particular biases on some past occasions, we are innocent of bias in the specific assessment about which we have introspected' (Pronin *et al.* 2004, p. 783). It would appear, then, that 'bracketing' (along with its variants) represents the intersection of the introspection illusion, self-serving bias, and overconfidence. The blasé assumption that introspection is a reliable guide to cognitive processes is effectively a form of naive realism with respect to the researcher's own mind.[9]

There are, of course, other ways in which cognitive bias might influence the process of data analysis, in addition to observer expectancy effect. They include the primacy effect, congruence bias, inattentional blindness, and conceivably anchoring bias. Both primacy and anchoring suggest the possibility that the researcher may lock on to an initial analytical idea – whether something that catches her eye in one of the interview transcripts she reads first, or perhaps an interesting thought/insight she has while reading a particularly dramatic or thought-provoking account – and that this idea eventually emerges as the key concept in her report. The initial idea is so attractive, or becomes so entrenched, that it serves as a filter for the other data, and the remaining transcripts are interpreted in such a way as to make a fit. I suspect, reviewing some of my own research, that I have fallen for this one myself, with an initial bright idea

derived from a single interview (primacy), or prompted by the reading of a book unrelated to the topic (anchoring), serving as the analytical hub of the study. The sense is that, if I had read a different book, or if I had begun the analysis with a different transcript, I might have had a different bright idea, and might ultimately have drawn different conclusions. Naturally, I have no way of confirming or testing this suspicion; and it is conceivable that I am now reinterpreting my past experience in order to make my account of it conform to what I am currently writing about (this could be construed as an example of hindsight bias). But it seems highly likely that primacy and anchoring are implicated in qualitative data analysis, and it is unfortunate that (as far as I am aware) there are no studies of these mechanisms in research practice.

Much the same can be said of congruence bias, in which an interesting pattern in the data is discerned, and the ensuing analysis is devoted to confirming that pattern. As a consequence, other patterns, which might suggest a more accurate and convincing explanation, go unrecognised. In this case, however, the bias is acknowledged as a methodological problem in the social research literature. Authors such as Seale (1999) and Atkinson *et al.* (2003) understand that it is not enough to demonstrate a consistency between the data and a favoured interpretation, because the data may be equally consistent with one or more alternative accounts (just as the sequence 2, 4, 6, 8 is consistent with several different 'rules'). So it is essential, in their view, to construct, examine and, wherever possible, test alternative explanations.

As for inattentional blindness, a failure to notice the unexpected – however obvious it might seem to be in retrospect – applies to data analysis just as much as it does to invisible gorillas. In fact, this bias is arguably the one which bracketing phenomenologists have in mind when they talk about putting their preconceptions 'out of play', reflecting the worry that 'taken for granted assumptions' will prevent the researcher from noticing anything which does not conform to their prior understandings of the subject matter. But this is not the only possibility. In an earlier chapter, I commented on the assumption that the population from which a sample of cases is drawn will exhibit less variance and more homogeneity than it actually possesses; and the search for 'common themes' is obviously a strategy implied by this assumption. Inattentional blindness suggests that, given this search for what all cases have in common, significant variations, deviations, and contraventions will either not be noticed, or be discounted. The mainstream social research literature recognises this as a methodological issue, and recommends the 'search for negative cases' as a result (Seale 1999, Atkinson *et al.* 2003).

In passing, it is worth noting that, although the different biases are theoretically distinct, they are likely to overlap in practice. In any particular instance of data analysis, for example, it may not be possible to discriminate between observer expectancy, belief bias, the primacy effect, congruence bias, anchoring bias, confirmation bias, and inattentional blindness, since they all involve the subconscious privileging of a favoured interpretation, and the screening out of plausible alternatives. To this extent, then, they complement and reinforce each other.

Most of the other biases described in the previous section also lurk in the research undergrowth, but their impact is somewhat different. For example, there is a group, including base rate neglect, illusory correlation, and the representativeness heuristic, which are relevant to sampling and generalisability. Illusory correlation and base rate neglect both refer to the risk of making unwarranted inferences about association or cause on the basis of a sample which does not include all the relevant types of case from a population. The MMR/autism question is a case in point. As we saw earlier, a sample of autism cases, all of whom had MMR vaccine shortly before developing autistic behaviour, does not demonstrate an association between the two, let alone a causal connection. Whether there is an association or not can only be determined by evaluating a sample which *also* includes non-autistic children and children who have not had the MMR. This is an example of illusory correlation, given that the apparent link between MMR and autism is an illusion produced by an inadequate sample. Simultaneously, it is an example of base rate neglect because the background rate of autism in the child population as a whole is ignored.

A less critical example is Ranheim *et al.* (2010), who invited a sample of seven nurses to participate in a project involving the administration of rhythmical embrocation (RE) to residents in older people's homes. The seven nurses all had experience of providing RE two or three times a week for four months and, when interviewed, they were asked about their experiences of doing so. 'The findings show that through the caring act of RE, the participant nurses were able to reach an understanding of deeper concerns in relation to another human being... The participating nurses' sensitivity and abilities in observation grew wider and more receptive by performing RE through the increase in self-awareness' (p. 245). Let's ignore one obvious problem here, that causal claims – the seven nurses *acquired* certain understandings, sensitivities and abilities *as a result of* administering RE – are made on the basis of self-reports, with all the pitfalls that implies.[10] Even so, the parallel between the Ranheim study and the MMR/autism example should be evident. Here again, we have a group of individuals with two characteristics: experience of providing X, and an enhanced ability to do Y. So, obviously, there must be a causal connection: providing the RE *led to* the enhanced ability. Just as the existence of a group of children who had MMR vaccine, and who now have autism, proves that the MMR *led to* the autism. No. The RE study is another example of illusory correlation. The fact that X happened and then Y happened, in seven cases, does not demonstrate that X caused Y. Before any causal claims can be made, we need to examine the whole population, including nurses who have not provided RE, and nurses who have not acquired enhanced abilities. We have a variation on what is by now a familiar table:

	Enhanced ability	No enhanced ability
RE experience	a	b
No RE experience	c	d

Studying (a) and (d) cases tells us nothing about cause, or even association. It is necessary to compare the numbers in cells (a) and (d) with the numbers in cells (b) and (c); or to carry out an experiment in which eligible nurses are randomly assigned either to a group providing RE or to a group not providing it, with 'ability enhancement' as the outcome measure.

The representativeness heuristic, in which similarity is taken as a proxy for some other attribute, such as group membership, causal connection or typicality, has a further bearing on the relationship between a sample and a population. The fact that cows and horses resemble each other, although they belong to entirely different genetic groups, is a Trivial Pursuit example; but the same 'similarity illusion' turns up in writing about social research.

The best example is the concept of 'transferability', originally introduced by Lincoln & Guba (1985). Transferability is a substitute for generalisability, a concept which Lincoln & Guba reject because, they argue, it depends on deterministic and reductionist assumptions (there are other reasons, too). But this creates a problem for them. If findings can never be generalised in the way that positivists encourage us to believe they can and should be, on what grounds should anybody other than the researcher take any notice of them? Lincoln & Guba pick up this line of questioning (p. 124, italics in original):

> How can one tell whether a working hypothesis developed in Context A might be applicable in Context B? We suggest that the answer to that question must be empirical: the degree of *transferability* is a direct function of the *similarity* between the two contexts, what we shall call "*fittingness*". Fittingness is defined as the degree of congruence between the sending and receiving context. If Context A and Context B are "sufficiently" congruent, then working hypotheses from the sending originating context *may* be applicable in the receiving context.

The implication of this suggestion is that the researcher should describe the people and circumstances of her sample in sufficient detail to permit the reader to assess whether the her own cases – the patients she is likely to see, for example – are similar enough to warrant a 'transfer' of the findings. The detail required is provided by 'thick description', a phrase borrowed from Geertz (1973). Lincoln & Guba (1985) suggest: 'It is the responsibility of the inquirer to provide a sufficient base to permit a person contemplating application in another receiving setting to make the needed comparisons of similarity' (pp. 359-360). Transferability is the most popular explanation of why phenomenological findings, however small the sample, can be of relevance to a wider population.

I think the idea of transferability is misconceived. Everything hangs on the idea that the reader can assess the degree of similarity between the researcher's cases and her own cases from a 'thick description'. However, 'similarity' is an uninformative notion. Some similarities are relevant, while others are not. Suppose the researcher says that the participants in her sample share characteristic X. Her thick descriptions suggest that her cases and my cases are similar in two respects, A and B. How can I be sure that A is a relevant similarity and B is not? Only by having independent, statistical evidence that there is an association between A and X, but not between Y and X.[11] If there is such evidence, then I can 'transfer' characteristic X to my own cases (I can, in other words, anticipate that my own cases will also have characteristic X). Otherwise not. So the idea of transferability requires statistical generalisations to make it work. But this is precisely what Lincoln & Guba reject.

I can now add that transferability is an illusion supported by the representativeness heuristic. Just as the similarity between cows and horses does not entail that they are members of the same genetic group, so the similarity between the researcher's sample and my patients does not entail that they are members of the same population. Just as we cannot 'transfer' the genetic make-up of horses to cows on the basis of perceived similarities – size, four legs, hooves, grazing – so I cannot 'transfer' the characteristics of the researcher's sample to my patients on the basis of the similarities incorporated in the thick description. Transferability is a methodological trap since it assumes that similarity is an indicator of typicality or membership of a population. But there may be no connection at all between the resemblance concerned and group membership. Only if we have independent evidence that there is such a connection can we be confident that the 'transfer' is justified. We are back to statistical generalisations.

It would be possible to continue this catalogue of bias, and illustrate the ways in which various illusions compromise research. I have said nothing, for instance, about the framing effect and its relevance to the literature on response error in questionnaire design (Tourangeau & Rasinski 1988, Schuman & Presser 1996, Sirken *et al.* 1999, Tourangeau *et al.* 2000, Presser *et al.* 2004). This literature is equally relevant to qualitative research (Paley 2010), but is rarely discussed by qualitative methodologists, Hammersley (2008) being a notable exception. Nor have I talked about the ways in which it is possible to confuse random variability in the data with significant difference, a problem which a large number of statistical tests have been designed to counteract (Paley *et al.* 2007, Paley *et al.* 2008). But I am happy to leave it there and return, in a moment, to the theme of objectivity.

The briefest of brief reviews

We are perpetually at risk of succumbing to cognitive illusions, which are the result of processing data in ways that lead, regularly and predictably, to demonstrable error (just as optical illusions are the result processing visual data in ways that lead, regularly and predictably, to demonstrable error). This applies to research as much as it applies to other domains. In all the examples we have considered, there are procedures which permit us to check whether an initial judgment is illusory or not, even if the judgment often proves highly resistant to the evidence those procedures deliver. The procedures in question vary:

mathematics, logic, probability and statistics... but also established methods in lexicography, genetics, psychology, geography, international law, or some other discipline. If there is no recognised procedure already to hand, it is sometimes necessary to devise one. But, in most cases, the procedure is canonical.

A supervisory and corrective function

The headline claim, stated succinctly. *Objectivity* is a way of referring to the existence of procedures designed to check whether an initial judgment is illusory.

Objectivity is what we do when we use a ruler to measure the Müller-Lyer lines; use logic to check that an inference about mice and insects is valid; use an atlas to check the latitudes of Rome and New York; use simple arithmetic to reckon up profit and loss; use probability theory and a blood test to determine how likely it is that the patient has diabetes; use statistics to evaluate the conviction that MMR vaccine causes autism; use blinding to prevent a experimenter from subconsciously manipulating the data.

Objective is what we are being when we use experiments to gauge cause and effect, statistics to decide whether we can generalise from a sample, and hypothesis testing to evaluate competing theories.

Objectivity is about minimising the risk that we have been led astray by cognitive bias, and succumbed to cognitive illusion. To be objective is to exercise a checking, supervisory and corrective function with respect to our initial judgments.

This is a not the concept rejected by those who regard objectivity as 'an impossibility' or dismiss it as a 'chimera'. It does not represent a 'dominant discourse'; the arbitrary 'privileging' of an Enlightenment prejudice; an obsolete modernist notion; rampant logical positivism; a corporate ideology; the 'medical model', the hegemony of medicine over nursing. It has no ontological or epistemological pretensions. It is simply a method – actually, a suite of methods – for minimising the risk of succumbing to illusion. If you reject *this* concept, you are effectively welcoming and celebrating the possibility of error. You are embracing illusion. You are endorsing initial judgment. You are insisting that the horizontal lines of the Müller-Lyer or of unequal length. You prefer to be wrong.

Seriously, if you don't like this concept of objectivity, you are dismissing the evident fact that we get things wrong. Repeatedly, routinely, predictably. You are so wedded to an anti-science, anti-positivist, postmodernist, interpretivist, constructivist, or whatever-it-happens-to-be philosophy that you refuse to accept that there is any distinction between *how it seems* and *how it is*. 'How it is', you are apparently willing to assert, just *is* 'how it seems'. End of story.

In which case, you might just as well announce that the squares on Adelson's chess board really *are* of different colours, and that Cardiff really *is* west of Edinburgh. As far as empirical enquiry is concerned, you are obviously closed for business.

So objectivity is a set of procedures designed to minimise the risk of succumbing to illusion. That is the central message. In closing this chapter, I will offer some footnotes, link back to the opening pages, and make a few connections with the philosophical, historical and psychological literature.

Subjective/objective

This is implicit in what I have already said, but worth spelling out. Rather than objectivity being some sort of proactive ideology – the motivated imposition of a cold austere, white-coated, morally dubious belief system – it is a reactive set of techniques. Whenever we discover a new way of getting-it-wrong,

a new cognitive illusion, we devise a procedure for reducing the risk of succumbing to it. Objectivity perpetually chases subjectivity. 'There is no objectivity without subjectivity to suppress... Objectivity and subjectivity are as inseparable as concave and convex; one defines the other' (Daston & Galison 2007, pp. 33/197). Objectivity kicks off from the possibility of illusion, the possibility of error. It is not in the business of defining reality.

Similarly, it is not in the business of presenting a 'view from nowhere' (Nagel 1989). Adopting a procedure designed to minimise the risk of succumbing to error does not require the researcher to be anywhere other than 'in the world', as Lowes & Prowse (2001) put it. It does not require anyone to divest themselves of their autobiography, their commitments, beliefs, values, preferences... in short, their embodied point of view. In measuring the Müller-Lyer lines, we remain thoroughly worlded. The act of measurement is not an attempt to 'step outside' the world or oneself; it is rather an act of aligning oneself to the world more precisely – metaphorically, an act of making oneself even more fully at home in the world than before. Extending the metaphor, cognitive illusions threaten to de-world us (literally, perhaps, if getting-it-wrong puts us in potential danger, as refusing vaccination does). Introducing the corrective procedure, bringing about the realignment, restores the connection; restores, one might say, the being-at-home-in-the-world.[12] So objectivity, on this account, is not a view from nowhere. It is the view of someone more thoroughly immersed in somewhere.

Social science

The argument is impervious to the distinction between natural and social science. Cognitive illusions do not stop happening just because you switch your attention from planets in orbit to human beings in social groups. Checking procedures do not suddenly become unnecessary. I will concede that studying human activity is somewhat different from studying planetary behaviour, for all the usual reasons. But the cognitive apparatus available to us has certain characteristics, and these characteristics do not vary with the subject matter of particular disciplines. Heuristics and biases are generic, and apt to precipitate illusions whatever the topic of enquiry. The fact that the 'ontology' of human beings is different from that of tables, chairs, planets, ball bearings and gas molecules is irrelevant. The possibility of illusion is permanently and systematically present. So the need for a range of procedures capable of identifying it, and then attempting to minimise the risk of it happening, is ineradicable. No amount of philosophical or phenomenological bluster can make the pervasive possibility of cognitive error go away (Paley 2005).

Objectivity as an epistemic virtue

Two qualifications. First, objectivity is not the only epistemic virtue, as Daston & Galison (2007) note. Their landmark work deals with the history of what they call 'mechanical objectivity' – this is a rough equivalent to my procedural concept[13] – which did not emerge until the second half of the 19th century. The other epistemic virtues to which they refer are 'truth to nature', which preceded objectivity, and 'trained judgment', which became significant in the early part of the 20th century. However, one should not imagine that any of these virtues superseded each other. All three of them survive as requirements in the pursuit of scientific knowledge, even though they are not always mutually compatible. Just as there are different moral principles which are all arguably legitimate, but which sometimes conflict, so there is a range of epistemic virtues which occasionally have divergent methodological implications.

Second, 'mechanical objectivity' has several aspects which I have not discussed here. 'Objectivity', as it came to be used by scientists in the 19th century, incorporated a number of different elements, and the criteria for 'being objective' varied accordingly. Aside from methods for reducing the risk of inferential error, there were also automatic procedures for registering data, the use of quantification, the belief in a

bedrock reality, and emotional detachment (Daston & Galison 2007, p. 29). Many of the authors who dismiss the idea of objectivity target just one of these, usually emotional detachment or quantification, and ignore the rest. This is clearly true of the writers I referred to early in this chapter, most of whom seem to identify 'objectivity' exclusively with emotional detachment. I shall say more about the latter in a moment; but I first want to emphasise that I regard the procedural concept – minimising the risk of succumbing to cognitive illusions – as an essential minimum. To be serious about empirical enquiry is to recognise that illusion is always possible, and that we need ways of reducing its impact. What, if anything, we require in addition to that is something we can argue about.

An error philosophy

'Not every philosophical diagnosis of error is an exercise in objectivity, because not all errors stem from subjectivity,' say Daston & Galison (2007, p. 32). There are other ways to go astray. However, if we extrapolate from avoiding subjectivity, as a specific form of error, to avoiding inferential error in general, then we arrive at an 'error philosophy of science'; or at least at a philosophy of science which has minimising the risk of error as its central theme. In Deborah Mayo's work, we find perhaps the best example. (Mayo 1996, Mayo & Spanos 2010).

The history of science, according to Mayo, is the history of the development of canonical methods for inquiring into error. As science grows, so does the range of recognizable sources of error, along with a repertoire of procedures for identifying and eliminating them. When a new source of error (a new sort of illusion) is identified, a standard procedure for recognizing it, and controlling for it, is devised. The range of errors into which canonical inquiries are routinely carried out are extensive, and are familiar to any researcher. They include errors about real effects, as opposed to accidental ones; errors which arise when an association is mistaken for a cause; and errors which occur when various design assumptions fail to hold. The repertoire of procedures for checking whether such errors have been made, in a range of research situations, is equally familiar. The point is that, when the evidence generated by a protocol is in accordance with what a hypothesis under examination predicts, *and* a series of canonical inquiries (tests) have failed to identify error, then the evidence can be taken as decent grounds for (provisionally) accepting the hypothesis.

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This position, in effect, generalizes the stripped down epistemology of simple illusions. The procedure appropriate to any particular illusion is generalized into the methodological canon, which is a series of reliable tests universally recognized by the scientific community. In one sense, however, Mayo's view is even more minimalist than the pared-down version. Instead of a distinction between *how it seems* and *how it really is*, she adopts a distinction, inherited from Popper and Neyman (Neyman & Pearson 1967), between 'how it seems (but isn't)' and 'what we can (for the time being) accept'. The error is identified, the illusion is dispelled, but the *how it really is* hypothesis is accepted only provisionally; it is not assumed to be certainly and inevitably true, and it may be rejected subsequently following further tests.

Mayo's book is about experimental research, and consequently the identification of error is predicated on the use of quantitative techniques: statistics are required to establish whether an error is being made in accepting or rejecting an experimental hypothesis. This presents a rather uncomfortable question to qualitative researchers. Since qualitative methods are not, in general, amenable to the application of statistical techniques, what (if anything) performs the corresponding function in qualitative studies? Without a convincing answer to this question, we have no reason to be confident that researchers are not succumbing to cognitive illusions of the kind discussed in this chapter. It is a question that cannot be waved away as a positivist obsession (Draper & Draper 2003), or dismissed as 'outmoded rhetoric' (Payne & Seymour 2003).

System 1 and System 2

On several occasions, I have referred to the fact that illusions, whether visual or cognitive, are resistant to evidence that they are wrong. This is a slightly odd, because one might expect the knowledge that the Adelson chessboard squares are the same colour to alter one's perception of them. The fact that it doesn't suggests that there are two independent systems at work, one which says 'different colours' and another that say 'same colour'. Similarly, in the Monty Hall problem, one system insists that the choice is 50/50, while the other accepts that switching gives a 2/3 probability of winning.

Psychologists have, in fact, proposed a number of 'dual processing' theories. These theories have in common the idea that cognition incorporates two distinct systems (Evans & Frankish 2009; Sloman 1996; Stanovich 2004). One of these, 'System 1' (Gilovich & Griffin 2002), is intuitive, holistic, fast and automatic; it is responsible for the majority of everyday judgments, which it arrives at without calculation or deliberation. It is the system which delivers the conviction that the Adelson chessboard squares are the same colour, that the Monty Hall choice is 50/50, that Cardiff is west of Edinburgh, that MMR causes autism. The other, 'System 2', is rule-based, conscious, analytical, and much slower. This system has to be deliberately invoked, because it rarely comes into play spontaneously. It is the system which delivers the realisation that the squares are different colours, that switching doors gives a 2/3 chance of winning, that Edinburgh is west of Cardiff, and that there is no association between autism and MMR. In arriving at these conclusions, it makes use of canonical procedures such as measurement, probability theory, and statistics.

It has been suggested that System 1 is the product of natural selection, while System 2 is dependent on culture and education. System 1 evolved to deal with prehistorical problems and circumstances. It is the animal part of our intelligence (Creswell & Plano Clark 2006), selected for by evolutionary processes (Stanovich & West 2003). In the modern world, it still performs well much of the time, but it does need relatively frequent correction by System 2. On this view, we tend to succumb to illusions when confronted with problems of a kind which System 1 is not well adapted for, at which point it becomes necessary to invoke System 2's rule-based procedures. In the Müller-Lyer illusion, System 1 perceives two lines of unequal length; System 2 looks for the ruler. Even then, System 1 finds it hard to accept the result (Sloman, 1996).

It would appear, then, that System 2 has a *supervisory and corrective function* with respect to System 1. It is frequently, though not exclusively, implemented in order to get right what System 1 gets wrong. It is used, in effect, 'to monitor and correct the intuitive judgments of S1' (Kahneman & Frederick 2002); 'to suppress S1's disposition to invalid inferences as a result of automatic contextualisation' (Stanovich & West 2003); 'to override or inhibit default responses emanating from S1' (Evans 2003); It follows that 'In the minority of cases where the outputs of the two systems conflict, people will often be better off if they can accomplish an analytic system override of the System 1-triggered output... override is statistically a good bet' (Stanovich 2009). Although a series of difficult questions about the System 1/ System 2 distinction have yet to be resolved,[14] dual process theory still provides a good candidate for the psychological mechanisms underlying error philosophy.

Canon and community

I suggested earlier that many of the writers who reject objectivity – and certainly the ones I mentioned at the beginning of the chapter – do so because they identify it exclusively with emotional detachment, or with the idea that it requires the researcher to purge or suppress all opinions, values, predispositions,

biases, assumptions, emotions and feelings. In this chapter, I have presented an alternative view, which depicts 'objectivity' as referring to a series of mainly canonical procedures designed to reduce the risk of succumbing to cognitive illusions.

However, these two views do have something in common. They both regard subjectivity – to accept this term as a codeword for assumptions, values, emotions, and so on – as antithetical to something. The *purging* view portrays it as opposed to the false ideal of objectivity. Indeed, it is the reason *why* this ideal is false. Objectivity requires an absence, a suppression, of subjectivity. But that is impossible. Therefore, objectivity is impossible too. The *procedural* view, meanwhile, portrays subjectivity as the source of cognitive illusions, cognitive bias. It stands in the way of seeing things clearly, undeceiving ourselves. The canonical procedures which 'objectivity' refers to are designed to minimise its impact.

Both views recognise that subjectivity is inescapable. The purveyors of purging take that as an knock-down argument against the impossible dream of objectivity. The proponents of procedure take it as a reason why the supervisory and corrective function is necessary. The key difference between them is that the former imagine that subjectivity must be purged, squashed, or banished (that this would be a condition of objectivity being possible), while the latter think it can be bypassed. Instead of having to expunge subjectivity, we accept that expunging isn't an option, and find an alternative route around it. The purveyors of purging think the only way to deal with the large boulder blocking the road is to get rid of it (but, sadly, this cannot be done). The proponents of procedure look for a detour. Subjectivity is unavoidable. Fine. So we devise a procedure for making it irrelevant, for taking it out of the loop. This, of course, is a crude and overly simple image. But it effectively conveys the basic idea.

Popper was less inclined to fanciful imagery, but he insisted that objectivity was not dependent on the psychological state of the individual scientist. Instead, the scientist goes to an independent tribunal, the court of science as an institution. She retains the partiality, passions and preconceptions that make her human, but submits her theories to 'the *friendly-hostile co-operation of many scientists*' (Popper 1966, p. 217). 'A scientist may offer his theory with the full conviction that it is unassailable. But this will not impress his fellow-scientists' (p. 218). Where I have emphasised the procedures which scientists adopt, Popper emphasises the social institution in which they are adopted. Procedures become canonical when the scientific community accepts them as such. Popper cites the community. I have cited the canon.

The scientific self

And yet there is something not wholly wrong about the idea that objectivity has to do with purging, or at least with suppression. I have suggested that the basic mistake objectivity deniers make is to focus on just one thread in the 'mechanical objectivity' tapestry, overlooking the fact there are others. However, according to Daston & Galison, emotional detachment *is* one aspect of mechanical objectivity. During the 19th century, various forms of scientific discipline were developed which became an aspect of what it meant to be 'objective', and this discipline was identified with what we would now call 'technologies of the self' (Foulcault 1988). The practices which implemented the 'scientific self' did not require the expunging of anything, nor did they involve the suppression of one's humanity. But they did involve a kind of self-restraint, one which accepted that (in Popper's terms) personal conviction does not impress other scientists. If Karl Pearson (1891) could claim that the scientist 'has above all things to aim at self-elimination in his judgments' (cited in Daston & Galison, p. 196), what he meant was that he had 'to provide an argument which is as true for each individual mind as for his own'. What I find convincing will not necessarily persuade others, so I must adopt a regime that can produce evidence others will be able to accept. The technology of the scientific self included training in observation, keeping journals of laboratory work, drawing specimens, channelling attention, and monitoring one's own beliefs and hypotheses – not in order to 'put them out of play' or pretend to forget them, but in order to exercise a

principled suspicion with respect to them. All of this is about providing public *evidence*, and providing evidence *publicly*. In trying to convince others, I must adopt a third-person view of myself, anticipating what the scientific community – Popper's 'many scientists' – will make of my arguments. Certainly, this is a form of detachment, even a form of detachment from the self (or at least a detachment from the first-person point of view). But it is an acquiring not a divesting, an addition not a subtraction. It does not require of me that I deny my own humanity.

Notes

1. Presumably – this must be Lowes & Prowse's view – non-Heideggerian researchers believe that some knowledge originates from people who are *not* already in the world. It's an odd idea. Personally, I have never come across any researchers who believe that they are 'not already in the world'. This isn't flippancy. I'm honestly not sure what Lowes & Prowse are getting at. I assume they have Heidegger's concept, being-in-the-world, in mind; and I know they think that objectivity implies 'stepping outside' something. But nobody imagines that objectivity means stepping outside the *world*, or that it entails not being in the world in the first place. So if Heideggerian phenomenologists are being contrasted with a group of writers who don't share their 'already-in-the-world' view of where knowledge comes from, I have no idea who the members of this group are supposed to be. I will return to this theme later in the chapter
2. By 'recent', I mean since 1990. It's worth pointing this out now – I will be saying a bit more later – because many methodologically inclined researchers in the health care disciplines appear to think that the philosophy of science stopped about 1962 with the publication of Kuhn's *The Structure of Scientific Revolutions*. Or, failing that, with Feyerabend, Lakatos, or Lauden. They are painfully unaware of what has happened during the fifty years since *Structure* or *Against Method* – things have moved on quite a bit – or what the current hot topics are. They are not familiar, in other words, with the debates about the role of models in scientific practice, Bayesian conceptions of science, the rehabilitation of positivism, constructive empiricism, realism and antirealism, essentialism and natural kinds, the significance of cognitive science, the new experimentalism, the return of reductionism, inference to best explanation, computer models of scientific inference, the developments in measurement theory, and the emphasis on the philosophy of particular sciences, particularly physics, chemistry, psychology, neuroscience and biology.
3. There is a similar, and equally unexpected, geographical oddity in the USA. Reno is *west* of Los Angeles, and not east of it, as one might assume, given that Reno is well inland, while Los Angeles is on the west coast. However, the Californian coastline runs roughly NW/SE, not N/S, so this is another case of geographical 'tilt'.
4. Check http://www.telegraph.co.uk/news/uknews/1556883/New-fear-over-MMR-link-with-rising-autism.html. This link was accessed on 23rd May 2016.
5. The statistical analysis presented in this paper is, of course, far more complex than this. In spite of the simplification, I don't think I have misrepresented the authors' conclusions. I would recommend that anybody interested in this particular topic read the whole paper, and indeed other contributions to the literature. That there is no evidence at all of an association between the MMR vaccine and autism, or any other PDD, can be seen from Honda *et al.* (2005), Madsen *et al.* (2002), Dales *et al.* (2001), Taylor *et al.* (1999), and an impressive number of comparable studies, all of which arrive at the same conclusion.

6. *Post hoc* is short for *post hoc, ergo propter hoc*. Literally, 'after this, therefore because of this'. The fallacy draws attention to the fact that, just because Y *came after* X, it does not follow that it *was caused by* X. My favourite football team experienced an unfortunate sequence of losses; however, the day after I bought a replica shirt of their centre forward, he scored two goals and the game was won. Clearly my purchase brought about the victory. Well, obviously not. But thinking like that is the *post hoc* fallacy. It quickly leads to superstition and magical thinking – if, for example, I continue to wear the replica shirt on future match days, imagining that my team will win again as a direct result.

7. The longest list I've seen is at Wikipedia: http://en.wikipedia.org/wiki/List_of_cognitive_biases. Like any Wikipedia item, this article should not been taken as gospel; nor should any of the Wikipedia articles linked to it. However, given the sheer number of biases identified, it does suggest that the risk of succumbing to cognitive illusion is, in general, rather high.

8. Or, for that matter, auditory illusions. See, for example, Deutsch (2011).

9. Eric Schwitzgebel (2011) is extremely interesting on introspection, which he agrees is 'unreliable as typically executed' (p. 120). However, he does think that it is possible for people to improve their introspective abilities through intensive training, although he is simultaneously aware of 'the daunting obstacles for any such program' (p. 74). To a considerable extent, he derives both the promise and the obstacles from a reading of Edward Titchener's introspection training manual, a volume of some 1,600 pages. Tichener was a psychologist at a time (the second half of the nineteenth century, and the early years of the twentieth) when the discipline used introspective methods almost exclusively. According to Titchener, naive, untrained people cannot introspect accurately and, like other psychologists of the period, he relied on 'observers with graduate training in psychology and thus presumably with at least several months, and often several or many years, of intensive experience of introspective methods' (Schwitzgebel 2011, p. 74). People who lack this training, Titchener argued, have preconceptions about the contents of their own minds, and confabulate freely (but unknowingly) in order to compensate for the numerous gaps and indeterminacies in their self-observation. This distrust of what people say about their own states of mind remains characteristic of modern social psychology. Hurlburt & Schwitzgebel (2007) and Wilson (2002) should be consulted for a discussion of how untrained subjects routinely but subconsciously fill in the introspective blanks, 'unaware of their unawareness' (Wilson & Bar-Anan 2008). It is, of course, deeply ironic that phenomenologists propose to avoid topic-related bias by using a method – introspection – which is itself prone to bias of a systematic and pervasive kind.

10. Actually, it's even worse than that. The participating nurses attended 'regular monthly meetings of the project team, where clinical experiences were discussed and compared with theoretical caring concepts'. In other words, they were coached by the researchers during the period during which they were administering RE. Seriously, how surprising is it that, when they were subsequently interviewed by one of the research team, they said exactly what the research team wanted to hear?

11. Edwall *et al.* (2008) makes a similar point. At least I think he does: some aspects of his argument are not entirely clear to me. He also suggests that the evidence need not be statistical, although there does need to be *some* empirical ground for thinking that X and A are likely to occur together. I can accept this amendment, while pointing out that, irrespective of whether the evidence is statistical in form, or whether it is based on a strongly supported causal model, it gives us independent reason for thinking that X and A are associated. So either the evidence *is* a generalisation, or it *warrants* a generalisation. Either way, without this independent evidence, transferability doesn't work.

12. Okay, the quasi-Heideggerian language here is slightly provocative, although I don't think I am saying anything that Heidegger is obliged to disagree with. Cognitive illusions are a bit like equipment failures. When the hammer falls apart, it needs to be repaired. Similarly, when my cognitive apparatus is such that my judgments about the world are compromised, it needs to be corrected. Repairing the hammer, correcting the judgment, makes it possible to have practical encounters with ready-to-hand objects again. However, the choice of language is partly satirical. I'm suggesting that you can use Heidegger's neologisms – you can even invent your own variations on them, complete with hyphens – to support a position which is the direct opposite of what most Heideggerian nurses want to argue.

13. A *very* rough equivalent, I should perhaps say. Daston & Galison's treatment is much more subtle and sophisticated than mine, partly as a consequence of their superlative historical scholarship. I think their book is a must-read for anyone seriously interested in unravelling the development of objectivity, and related concepts, over the last three centuries.

14. For example, it is not clear that there is a single system which can have all the features attributed to System 1; and there appears to be a growing consensus that a 'System 3' needs to be added, although different authors describe the third system in different ways. Some doubt also exists as to whether there are two *systems*, or two types of cognitive *process* (Evans 2009), or two types of *inference*, 'intuitive' and 'reflective' (Mercier & Sperber 2009); and there are other variations. Evans & Frankish (2009) is currently the most accessible source for debates about the future of dual process theories.

References

Anderson E.E. & Kiger A.M. (2008) 'I felt like a real nurse': student nurses out on their own. *Nurse Education Today* **28**, 443-449.

Atkinson P., Coffey A. & delamont S. (2003) *Key Themes in Qualitative Research: Continuities and Changes*. Altamira Press, Walnut Creek, CA.

Baron J. (2000) *Thinking and Deciding: 3rd Edition*. Cambridge University Press, Cambridge, UK.

Baskerville R. (2001) Conducting action research: high risk and high reward in theory and practice. In *Qualitative Research in IS: Issues and Trends* (E. M. Trauth, ed.). Ideas Publishing Group, Hershey, PA, pp. 192-217.

Buntjer J.B., Hoff I.A. & Lenstra J.A. (1997) Artiodactyl interspersed DNA repeats in cetacean genomes. *Journal of Molecular Evolution* **45**, 66-69.

Burhans L.M. & Alligood M.R. (2010) Quality nursing care in the words of nurses. *Journal of Advanced Nursing* **66**, 1689-1697.

Burr V. (2003) *Social Constructionism*. Routledge, London.

Campbell J.K. (1988) Inside lives: the quality of biography. In *Qualitative Research in education: Focus and Methods* (R. R. Sherman & R. B. Webb, eds), pp. 59-75.

Chabris C. & Simons D. (2011) *The Invisible Gorilla and Other Ways In Which Our Intuition Deceives Us*. Harper, London.

Chapman G.B. & Johnson E.J. (2002) Incorporating the ireelevant: anchors in judgments of belief and value. In *Heuristics and Biases: The Psychology of Intuitive Judgment* (T. Gilovich, D. Griffin & D. Kahneman, eds). Cambridge University Press, Cambridge, UK, pp. 120-138.

Creswell J.W. & Plano Clark V.L. (2006) *Designing and Conducting Mixed Methods Research*. Sage, London.

Curley S.P., Young M.J., Kingry M.J. & Yates J.F. (1988) promacy effects in clinical judgments of contingency. *Medical Decision Making* **8**, 216-222.

Dales L., Hammer S.J. & Smith N.J. (2001)) Time trends in autism and in MMR immunization coverage in California. *Journal of the American Medical Association* **285**, 1183-1185.

Daston L. & Galison P. (2007) *Objectivity*. Zone Books, New York.

Davies D. & Dodd J. (2002) Qualitative research and the question of rigour. *Qualitative Health Research* **12**, 279-289.

Dawes R.M. (2001) *Everyday Irrationality: How Pseudo-Scientists, Lunatics, and the Rest of Us Systematically Fail to Think Rationally*. Westview, Boulder, CO.

Deutsch D. (2011) *The Psychology of Music: Third Edition*. Academic Press, New York.

Doris J.M. (2002) *Lack of Character: Personality and Moral Behaviour*. Cambridge University Press, Cambridge, UK.

Draper J. & Draper P. (2003) Response to Watson's Guest Editorial 'Scientific methods are the only credible way forward for nursing research'. *Journal of Advanced Nursing* **44**, 546-7.

Drew N. (2001) Meaningfulness as an epistemologic concept for explicating the researcher's constitutive part in phenomenologic research. *Advances in Nursing Science* **23**, 16-31.

Eddy D.M. (1988) Variations in physician practice: the role of uncertainty. In *Professional Judgment: A Reader in Clinical Decision Making* (J. Dowie & A. S. Elstein, eds). Cambridge University Press, Cambridge, UK, pp. 000-000.

Edwall L.-L., Hellström A.-L., Öhrn I. & Danielson E. (2008) The lived experience of the diabetes nurse specialist regular check-ups, as narrated by patients with type 2 diabetes. *Journal of Clinical Nursing* **17**, 772-781.

Epley N. & Dunning D. (2000) 'Feeling holier than thou': are self-serving assessments produced by errors in self or social psychology. *Journal of Personality and Social Psychology* **79**, 861-875.

Evans J.S.B. (2009) How many dual process theories do we need? One, two, or many? In *In Two Minds: Dual Processes and Beyond* (J. S. B. Evans & K. Frankish, eds). Oxford University Press, Oxford, pp. 33-54.

Evans J.S.B. & Frankish K. (2009) In Two Minds: Dual Processes and Beyond. Oxford University Press, Oxford.

Filstead W. (1979) Qualitative methods: a needed perspective in evaluation research. In *Qualitative and Quantitative Methods in Evaluation Research* (T. Cook & C. Reichardt, eds). Sage, Beverly Hills.

Foulcault M. (1988) Technologies of the self. In *Technologies of the Self* (L. H. Martin, H. Gutman & P. H. Hutton, eds). University of Massachusetts Press, Amherst, pp. 16-49.

Fugelsang J.A., Stein C., Green A. & Dunbar K. (2004) Theory and data interactions of the scientific mind: evidence from the molecular and the cognitive laboratory. *Canadian Journal of Experimental Psychology* **58**, 86-95.
Geertz C. (1973) *The Interpretation of Cultures: Selected Essays by Clifford Geertz*. Basic, New York.
Gendler T.S. (2010) *Intuition, Imagination, and Philosophical Methodology*. Oxford University Press, Oxford.
George S.R. & Thomas S.P. (2010) Lived experience of diabetes among older, rural people. *Journal of Advanced Nursing* **66**, 1092-1100.
Gigerenzer G., Czerlinski J. & Martignon L. (2002) How good are fast and frugal heuristics? In *Heuristics and Biases: The Psychology of Intuitive Judgment* (T. Gilovich, D. Griffin & D. Kahneman, eds). Cambridge University Press, Cambridge, UK, pp. 559-581.
Gigerenzer G., Todd P.M. & ABC Research group (1999) *Simple Heuristics That Make Us Smart*. Oxford University Press, Oxford.
Gilovich T. (1991) *How We Know What Isn't So: The Fallibility of Human Reason in Everyday Life*. The Free Press, New York.
Gilovich T. & Griffin D. (2002) Introduction - heuristics and biases: then and now. In *Heuristics and Biases: The Psychology of Intuitive Judgment* (T. Gilovich, D. Griffin & D. Kahneman, eds). Cambridge University Press, Cambridge, UK, pp. 1-18.
Gilovich T., Griffin D. & Kahneman D. (2002) Heuristics and Biases: The Psychology of Intuitive Judgment. Cambridge University Press, Cambridge, UK.
Granberg D. & Brown T. (1995) The Monty Hall dilemma. *Personality and Social Psychology Bulletin* **21**, 711-723.
Gray J.D. & Silver R.C. (1990) Opposite sides of the same coin: former spouses' divergent perspectives in coping with their divorce. *Journal of Personality and Social Psychology* **59**, 1180-1191.
Gregory R.L. (1998) *Eye and Brain: The Psychology of Seeing*. Oxford University Press, Oxford.
Guba E.G. & Lincoln Y.S. (1989) *Fourth Generation Evaluation*. Sage, Newbury Park.
Hammersley M. (2008) *Questioning Qualitative Inquiry: Critical Essays*. Sage, Los Angeles.
Holloway I. & Wheeler S. (2010) *Qualitative Research in Nursing and Healthcare. Third Edition*. Wiley-Blackwell, Oxford.
Honda H., Shimizu Y. & Rutter M. (2005) No effect of MMR withdrawal onm the incidence of autism: a total population study. *Journal of Child Psychology and Psychiatry* **46**, 572-579.
Hughes C., Knibb W. & Allan H. (2010) Laparoscopic surgery for endometrial cancer: a phenomenological study. *Journal of Advanced Nursing* **66**, 2500-2509.
Hurlburt R.T. & Schwitzgebel E. (2007) *Describing Inner experience? Proponent Meets Sceptic*. Bradford Books, MIT Press, Cambridge, MA.
Isen A.M. & Levin P.F. (1972) Effect of feeling good on helping: cookies and kindness. *Journal of Personality and Social Psychology* **21**, 384-388.
Kahneman D. & Frederick S. (2002) Representativeness revisited: attribute substitution in intuitive judgment. In *Heuristics and Biases: The Psychology of Intuitive Judgment* (T. Gilovich, D. Griffin & D. Kahneman, eds). Cambridge University Press, Cambridge, UK, pp. 49-81.
Kahneman D., Slovic P. & Tversky A. (1982) Judgement Under Uncertainty: Heuristics and Biases. Cambridge University Press, Cambridge, UK.
Kaptchuk T.J. (2003) Effect of interpretive bias on research evidence. *BMJ* **326**, 1453-1455.
Koehler J.J. (1993) The influence of prior belief on scientific judgments of evidence quality. *Organizational Behavior and Human Decision Processes* **56**, 28-55.
Kruger J. & Gilovich T. (1999) 'Naive cynicism' in everyday theories of responsibility assessment: on biased assumptions of bias. *Journal of Personality and Social Psychology* **76**, 743-753.
Kunda Z. (1999) *Social Cognition: Making Sense of People*. MIT Press, Cambridge, MA.
Lichenstein S. & Slovic P. (2006) The Construction of Preference. Cambridge University Press, Cambridge, UK.
Lincoln Y.S. & Guba E.G. (1985) *Naturalistic Inquiry*. Sage, Beverly Hills, CA.
Lincoln Y.S. & Guba E.G. (2003) Paradigmatic controversies, contradictions, and emerging influences. In *The Landscape of Qualitative Research: Theories and Issues. Second Edition* (N. K. Denzin & Y. S. Lincoln, eds). Sage, Thousand Oaks, CA, pp. 253-291.
Lowes L. & Prowse M.A. (2001) Standing outside the interview process? The illusion of objectivity in phenomenological data generation. *International Journal of Nursing Studies* **38**, 471-480.
Madsen K.M., Hviid A., Verstergard M., Schendel D., Wohlfahrt J., Thorsen P., Olsen J. & Melbye M. (2002) A population-based study of Measles, Mumps, and Rubella Vaccination and Autism

New England Journal of Medicine
347, 1477-1482.
Mayo D.G. (1996) *Error and the Growth of Experimental Knowledge*. University of Chicago Press, Chicago.
Mayo D.G. & Spanos A. (2010) Error and Inference: Recent Exchanges on Experimental Reasoning, Reliability, and the Objectivity and Rationality of Science. Cambridge University Press, Cambridge, UK.
Mellon C.A. (1990) *Naturalistic Inquiry for Library Science: Methods and Applications for Research, Evaluation, and Teaching*. Greenwood, New York.
Mercier H. & Sperber D. (2009) Intuitive and reflective inferences. In *In Two Minds: Dual Processes and Beyond* (J. S. B. T. Evans & K. Frankish, eds). Oxford University Press, Oxford, pp. 149-170.
Myers D.G. (2002) *Intuition: Its Powers and Perils*. Yale University Press, New Haven.
Nagel T. (1989) *The View from Nowhere*. Oxford University Press, New York.
Namenwirth M. (1986) Science seen through a feminist prism. In *Feminist Approaches to Science* (R. Bleir, ed.). Pergamon, New York, pp. 18-41.
Neyman J. & Pearson E.S. (1967) *Joint Statistical Papers*. University of California Press, Berkeley, CA.
Nickerson R.S. (1998) Confirmation bias: a ubiquitous phenomenon in many guises. *Review of General Psychology* **2**, 175-220.
Nilsson C., Bondas T. & Lundgren I. (2010) Previous birth experience in women with intense fear of childbirth. *Journal of Obstetric, Gynecologic and Neonatal Nursing* **39**, 298-309.
Paley J. (2005) Error and objectivity: cognitive illusions and qualitative research. *Nursing Philosophy* **6**, 196-209.
Paley J. (2010) Qualitative interviewing as measurement. *Nursing Philosophy* **11**, 112-126.
Paley J., Cheyne H., Dalgleish L., Duncan E.A.S. & Niven C.A. (2007) Nursing's ways of knowing and dual process theories of cognition. *Journal of Advanced Nursing* **60**, 692-701.
Paley J., Cheyne H., Dalgleish L., Duncan E.A.S. & Niven C.A. (2008) The null hypothesis: a reply to Gary Rolfe. *Journal of Advanced Nursing* **64**, 209-210.
Payne S. & Seymour J. (2003) Response to Watson's guest editorial 'Scientific methods are the only credible way forward for nursing research'. *Journal of Advanced Nursing* **44**, 547-548.
Piatelli-Palmarini M. (1996) *Inevitable Illusions: How Mistakes of reason Rule Our Minds*. John Wiley & Sons, Chichester.
Popper K.R. (1961) *The Poverty of Historicism*. Routledge & Kegan Paul, London.
Popper K.R. (1966) *The Open Society and Its Enemies, Volume 2: Fifth Edition*. Routledge, London.
Powers B.A. & Knapp T.R. (2006) *Dictionary of Nursing Thery and Research*. Springer Publishing Company, New York.
Presser S., Rothgeb J.M., Couper M.P., Lessler J.T., Martin E., Martin J. & Singer E. (2004) Methods for Testing and Evaluating Survey Questionnaires. Wiley Science, Hoboken, NJ.
Pronin E., Gilovich T. & Ross L. (2004) Objectivity in the eye of the beholder:divergent perceptions of bias in self versus others. *Psychological Review* **111**, 781-799.
Ranheim A., Kärner A., Arman M., Rehnsfeldt A.W. & Berterö C. (2010) Embodied reflection in practice - 'touching the core of caring'. *International Journal of Nursing Practice* **16**, 241-247.
Reid J., McKenna H.P., Fitzsimons D. & McCance T.V. (2010) An exploration of the experience of cancer cachexia: what patients and their families wand from healthcare professionals. *European Journal of Cancer Care* **19**, 682-689.
Reidpath D.D. & Diamond M.R. (1995) A nonexperimental demonstration of anchoring bias. *Psychological Reports* **76**, 800-802.
Resch K.I., Ernst E. & Garrow J. (2000) A randomized controlled study of revierwer bias against an unconventional therapy. *Journal of the Royal Society of Medicine* **93**, 164-167.
Robinson J.O. (1972) *The Psychology of Visual Illusion*. Hutchinson Universirty Library, London.
Rosenthal R. & Jacobson L. (1992) *Pygmalion in the Classroom*. Irvington, New York.
Sá W., West R.F. & Stanovich K. (1999) The domain specificity and generality of belief bias: searching for a generalizable critical thinking skill. *Journal of Educational Psychology* **91**, 497-510.
Schulz K.F., Chalmers I. & Altman D.G. (2002) The landscape and lexicon of blinding in randomized trials. *Annals of Internal Medicine* **136**, 254-259.
Schuman H. & Presser S. (1996) *Questions and Answers in Attitude Surveys*. Sage, Thousand Oaks, CA.
Schwitzgebel E. (2011) *Perplexities of Consciousness*. MIT Press, Cambridge, MA.

Sciarra D. (1999) The role of the qualitative researcher. In *Using Qualitative Methods in Psychology* (M. Kopala & L. A. Suzuki, eds), Thousand Oaks, CA, pp. 37-48.
Seale C. (1999) *The Quality of Qualitative Research*. Sage, London.
Shorter M. & Stayt L.C. (2010) Critical care nurses' experiences of grief in an adult intensive care unit. *Journal of Advanced Nursing* **66**, 159-167.
Sirken M.G., Herrman D.J., Schechter S., Schwarz N., Tanur J.M. & Tourangeau R. (1999) Cognition and Survey Research. Wiley Blackwell, Oxford.
Sloman S.A. (1996) The empirical case for two systems of reasoning. *Psychological Bulletin* **119**, 3-22.
Smeeth L., Cook C., Fombonne E., Heavey L., Rodrigues L.C., Smith P.G. & Hall A.J. (2004) MMR vaccination and pervasive developmental disorders: a case-control study. *The Lancet* **364**, September 11.
Snape D. & Spencer L. (2003) The foundations of qualitative research. In *Qualitative Research Practice: A Guide for Social Science Students and Researchers* (J. Ritchie & J. Lewis, eds). Sage, London, pp. 1-23.
Stanovich K. (2003) The fundamental computational biases of human cognition: heuristics that (sometimes) impair decision making and problem solving. In *The Psychology of Problem Solving* (J. E. Davidson & R. J. Sternberg, eds). Cambridge University Press, New York, pp. 291-342.
Stanovich K.E. (2004) *The Robot's rebellion: Finding Meaning in the Age of Darwin*. University of Chicago Press, Chicago.
Stanovich K.E. (2009) Distinguishing the reflective, algorithmic, and autonomous minds: is it time for a tri-process theory? In *In Two Minds: Dual Processes and Beyond* (J. S. B. Evans & K. Frankish, eds). Oxford University Press, Oxford, pp. 55-88.
Stanovich K.E. & West R.F. (2003) Evolutionary versus instrumental goals: how evolutionary psychology misconceives human rationality. In *Evolution and the Psychology of Thinking* (D. Over, ed.). Psychology Press, Hove, UK, pp. 171-230.
Stayt L.C. (2009) Death, empathy and self-preservation: the emotional labour of caring for families of the critically ill in adult intensive care. *Journal of Clinical Nursing* **18**, 1267-1275.
Streubert Speziale H.J. & Carpenter D.R. (2007) *Qualitative Research in Nursing, 4th edition*. Lippincott Williams & Wilkins, Philadelphia, PA.
Taylor B., Miller E., Farrington C.P., Petropoulos M.-C., Favot-Mayaud I., Li J. & Waight P.A. (1999)) Autism and Measles, Mumps, and Rubella Vaccine: no epidemiological evidence for a causal association. *Lancet* **353**, 2026-2029.
Tourangeau R. & Rasinski K. (1988) Cognitive processes underlying context effects in attitude measurement. *Psychological Bulletin* **103**, 299-314.
Tourangeau R., Rips I.J. & Rasinski K. (2000) *The Psychology of Survey Response*. Cambridge University Press, Cambridge, UK.
Trout J.D. (1998) *Measuring the Intentional World: Realism, Naturalism, and Quantitative Methods in the Behavioral Sciences*. Oxford University Press, New York.
Tversky A. & Kahneman D. (1974) Judgement under uncertainty: heuristics and biases. *Science* **185**, 1124-1131.
Vouzavali F.J.D., Papathanassoglou E.D.E., Karanikola M.N.K., Koutroubas A., Patiraki E.I. & Papadatou D. (2011) 'The patient is my space': hermeneutic investigation of the nurse-patient relationship in critical care. *Nursing in Critical Care* **16**, 140-151.
Wason P.C. (1960) On the failure to eliminate hypotheses in a conceptual task. *Quarterly Journal of Experimental Psychology* **12**, 129-140.
Wilson T.D. (2002) *Strangers to Ourselves: Discovering the Adaptive Unconscious*. The Belknap Press of Harvard University Press, Cambridge, MA.
Wilson T.D. & Bar-Anan Y. (2008) The unseen mind. *Science* **321**, 1046-1047.
Wood L., Eggar M., Gluud L.L., Schulz K.F., Jüni P., Altman D.G., Gluud C., Martin R.M., Wood A.J. & Sterne J.A. (2008) Empirical evidence of bias in treatment effect estimates in controlled trials with different interventions and outcomes: meta-epidemiological study. *BMJ* **336**, 601-605.