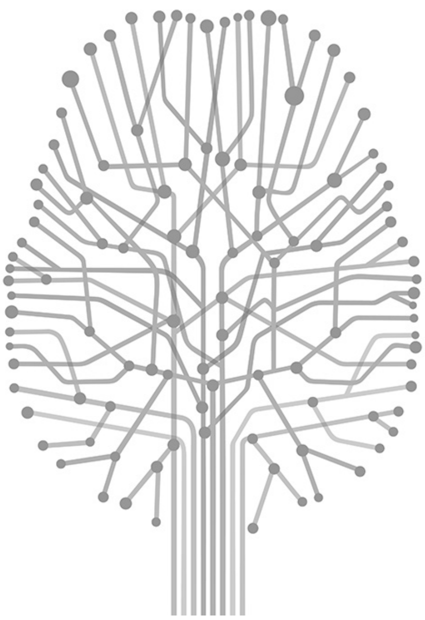
THE MEANING OF SCIENCE

An Introduction to the Philosophy of Science

Tim Lewens



BASIC BOOKS

A Member of the Perseus Books Group

New York

Published in Great Britain by Allen Lane, The Penguin Press

Published in the United States by Basic Books,

A Member of the Perseus Books Group

Designed by Jeff Williams

Library of Congress Cataloging-in-Publication Data
Names: Lewens, Tim.
Title: The meaning of science : an introduction to the philosophy of science / Tim Lewens.
Description: New York : Basic Books, [2015] | Includes bibliographical references and index.
Identifiers: LCCN 2015039234 | ISBN 9780465097487 (hardcover) | ISBN 9780465097494 (e-book)
Subjects: LCSH: Science—Philosophy.
Classification: LCC Q175 .L477 2015 | DDC 501--dc23 LC record available at http://lccn.loc.gov/2015039234

10 9 8 7 6 5 4 3 2 1

Contents

Chapter Five

Value and Veracity

The Division of Advisory Labor

In 2012, the Royal Society—one of the most prestigious scientific academies in the world—together with the Royal Academy of Engineering produced a scientific review of "hydraulic fracturing," a technique for the extraction of shale gas that is more normally known under the notorious name of "fracking." The review had been requested by the United Kingdom Government's Chief Scientific Advisor, Sir John Beddington (himself a fellow of the Royal Society). In the opening sections of the review, the report's authors made their responsibilities clear:

> This report has not attempted to determine whether shale gas extraction should go ahead. This remains the responsibility of the Government. This report has analysed the technical aspects of the environmental, health and safety risks associated with shale gas extraction to inform decision making.[1]

There is an implied division of labor here, typical of reports of this sort, between stating the evidence and offering policy recommendations. In February 2011 the UK Secretary of State for Health asked the Human Fertilisation and Embryology Authority (HFEA) to carry out a similar "scientific review"—this time, to determine "expert views on the effectiveness and safety of mitochondrial transfer."[2] Mitochondria are structures inside animal cells, located outside the nucleus, which contain a very small number of genes that are essential for healthy development and functioning. Disorders of the mitochondria can be systemic and progressive, and they are often passed from mothers to their children. The HFEA was being asked to provide a strictly technical evaluation of a set of novel techniques that hold the promise of allowing people with diseases of the mitochondria to have children who are genetically related to them, and who are also free from these serious diseases. These technical issues were again thought to be distinct from more value-laden concerns about whether it would be right for people to be born—as they would under the proposed techniques—with genetic material from three different contributors, and whether it would be right for fertility clinics to intervene in the human germ-line.

This common division of labor might simply reflect a difference in democratic responsibility: scientists have not been elected, hence it is not their job to say how policy should be formed, even if they have strong views on the matter, and even if the verdict of the best scientific work points clearly in a particular direction. But the division may also suggest to many that there is a strict contrast between the wholly neutral presentation of evidence that derives from science and the evaluative responses various interested parties may have to the evidence. Science, so the story goes, is entirely value-free (or at least, it is value-free when it has not been hijacked by tendentious interest groups). Policy, on the other hand, is what emerges when elected representatives bring their divergent values into contact with objective scientific evidence.

This image of science as value-free might seem to be intimately tied to the scientific realism that was defended in the previous chapter. I defined scientific realism as the view that science provides increasingly accurate representations of the portions of the world it deals with. If science can acquaint us with the facts, it might seem that science must be free of values. For surely there is a distinction between matters of fact and matters of value. The first concern how things are, the second concern how they should be. On this sensible-sounding view, while science tells us how things are, we need to use other forms of reflection, coupled with emotional appraisal, to tell us how they ought to change or whether they should stay the same.

In this chapter we will see that although these linkages between scientific realism and the conception of science as value-free are seductive, they are misleading. Science is permeated with evaluative concerns, but this does not undermine the ability of scientists to reveal the workings of the world to us, nor does it undermine the ability of scientists to advise policymakers on wise courses of action. If science were not informed by values, then the ability of scientists to give prudent advice would be severely limited.

Stalinist Biology

In some notorious cases it seems clear that values have influenced scientific theorizing in ways that are profound and detrimental. The fate of genetics in Stalin's Soviet Union perhaps constitutes the best-known case of all. On July 31, 1948, the

Soviet biologist Trofim Lysenko gave a speech to the All-Union Lenin Academy of Agricultural Sciences in Moscow, during which he reported on the state of biological research. Lysenko's report had been commissioned by Stalin, and Stalin later gave the speech his official approval. Lysenko claimed that the theory of genetics and evolution favored by most American and European scientists was a corruption of Charles Darwin's important work. This genetic theory, which he sometimes referred to as "Neo-Darwinism" and sometimes as "Mendelism-Morganism," was not genuine science at all. Instead, said Lysenko, it was a piece of idealism, or metaphysics.[3]

Lysenko argued that a faulty piece of bourgeois economic theory—namely, the idea that humans, animals, and plants are all locked in a competitive struggle for existence with their fellow species members—had had an unfortunate influence on Darwin, and that its pernicious effects had been magnified by the work of twentieth-century Darwinian thinkers. He went on to claim that the notion of the gene as the persisting, unchanging unit of inheritance—an idea that Lysenko associated with the Austro-Hungarian naturalist and abbot Gregor Mendel, and with the American pioneer of fruit-fly genetics Thomas Hunt Morgan—was an absurdity. It was a manifest fiction that flew in the face of what Lysenko took to be obvious truths about the ways in which the environment could influence organic inheritance, and the ways in which traits acquired during the lives of parents could be passed on to their offspring.

The supposedly idealistic theory of Mendelism-Morganism could not compete with the "creative Soviet Darwinism" that Lysenko championed. This was a "materialist and dialectical approach"—in other words, a properly Marxist approach—which paid due attention to the biological facts, which was oriented toward the practical goal of increasing agricultural productivity, and which accepted that an organism's environmental conditions could be skilfully manipulated so that valuable new capacities would appear in plants and animals. Lysenko called this theory "Michurinism," which he named after the Russian plant breeder Ivan V. Michurin.

Lysenko himself was the son of a peasant. What technical training he had was free of the taint of the prerevolutionary bourgeoisie; indeed, he had very little formal education at all. This made him a suitable emblem for Stalin's own image of the engines of progress. Lysenko's reputation was built on a series of breathtaking claims for his abilities to promote agricultural yields, backed by dubious experiments that he ensured were rarely challenged. Once his brand of anti-Mendelian biology took hold as official Soviet science, the views of Mendelians were denounced as bourgeois, or fascist. This did long-lasting damage to science in the Soviet Union. As the historian Robert Young recalled:

> When I was in the Soviet Union in 1971, I met a number of refugees from biology who had found a haven in the history of science. They described the worst effects of shambolic curricula and of censorship in scientific publishing. There were no genetics textbooks published between 1938 and the early 1960s, and no genetics at all was taught to generations of medical students. Imagine trying to practice modern medicine with that gap in one's knowledge. One form of "stupidity" in the period was the inability to memorize and regurgitate Lysenkoist nonsense. I remember one vivid account of a biologist who failed his exams on this topic. On the other hand, there were holes in the net. The original Watson-Crick article on DNA did get published in an obscure work on nucleotide chemistry—which immediately sold out.[4]

Young's comments understate the harm Lysenko did to scientific life in the Soviet Union: scientists lost their jobs, and some died, for their opposition to Lysenko's views. The geneticist Nikolai Vavilov, for example, who had studied in 1913–1914 with William Bateson—one of the earliest pioneers of Mendelian genetics—repeatedly criticized Lysenko's scientific claims.[5] He was arrested in 1940 and died in prison of malnutrition in 1943.[6]

The Lysenko affair shows some of the dangers of mixing science and values. It would be tempting to extend this trite observation in two more general ways. First, one might suggest that good science must be purged of all that is political, ideological, or evaluative. The evidence must simply be allowed to speak for itself. Second, one might conjecture that the Lysenko affair is surely a rare blemish in the history of science—a tyrant such as Stalin was required to sustain such an episode of institutionalized wishful thinking. These days, the thought might go, our scientists are unencumbered by bias. Both thoughts are misplaced, as the rest of this chapter shows.

Women's Orgasms

Hearts are clearly for pumping blood, lungs are for drawing air into the body. But sometimes scientists are unsure of the biological functions of anatomical structures, especially when those structures belong to species that are long extinct. Many species of hadrosaur, also known as duck-billed dinosaurs, had large hollow crests on the tops of their heads. What were these for? Suggestions have included a form of snorkel, an air-tank to enable underwater exploration, and a resonating chamber to amplify calls.[7] We should not suppose, though, that every biological structure must have its own function, as though organisms were composed of neatly designed interlocking elements.

What are the nipples of male humans for? The most tempting answer is that they have no function at all. Male nipples play no role in the survival and reproduction of men. Female nipples, on the other hand, play an obvious biological role in lactation. Although some genes are specific to men, and others are specific to women, the great majority of the genes that are involved in development from egg to adult are common to both sexes. Males have nipples because males and females develop through broadly similar processes, and females need nipples to nourish their young. Male nipples are an evolutionary side effect of female lactation.

What about women's orgasms? What are they for? In a wonderful case study, the philosopher of science Elisabeth Lloyd argues that various forms of bias have affected scientists' work in this domain.[8] Lloyd is happy to acknowledge that the pleasure women get from sex has the biological function of encouraging sexual activity, and thereby reproduction. Her target is instead the specific functionality claimed for orgasm, rather than that claimed for sexual pleasure in general. Lloyd argues that the most plausible hypothesis for female orgasms is that they, like male nipples, have no function with respect to survival and reproduction. Instead, they are best thought of as evolutionary side effects—this time, of the physiological structures underpinning male orgasms. Lloyd is open to the idea that data might eventually be produced demonstrating that women's orgasms do have a biological function. Her claim is merely that as things stand (or rather, as things stood back in 2005 when her book was published), evidence favors the "side-effect hypothesis."

In endorsing what I am here calling the side-effect hypothesis, Lloyd is not asserting that women's orgasms are unimportant, or imaginary, or only mildly enjoyable. Some commentators have attacked Lloyd on the grounds that her skepticism about the biological function of female orgasms devalues them.[9] These attacks are unfair. The abilities to play the piano, to solve complex equations, and to write prose are also unlikely to have functions with respect to survival and reproduction, but there is nothing unreal or frivolous about them. Someone who suggests that sprinting ability, but not footballing skills, assisted the survival and reproduction of our ancestors does not thereby imply that Usain Bolt is a more significant sportsman than Lionel Messi. In order to draw attention to the fact that she regards orgasms as real and valuable, Lloyd has largely dropped her original language of female orgasms as "by-products." That evoked unfortunate images of industrial waste or jars of Marmite. Instead, she now tends to refer to the female orgasm as a "fantastic bonus."

It is not possible to summarize all of Lloyd's evidence in favor of the side-effect hypothesis here, but we can get a flavor of it. Her basic case draws on the facts that, for women, sexual intercourse is often not accompanied by orgasm (even though the women in question are entirely capable of having orgasms) and that orgasms are instead most readily produced by masturbation. This means that female orgasm has no obvious direct link with reproduction. She quotes with approval the American biologist and sexologist Alfred Kinsey's remarks on how intercourse often fails to elicit orgasm: "It is true that the average female responds more slowly than the average male in coitus, but this seems due to the ineffectiveness of the usual coital techniques."[10]

Lloyd goes on to argue that there is little or no credible evidence supporting the various suggestions that have been put forward for biological functions for female orgasms. The zoologist Desmond Morris, for example, suggested back in 1967 that female orgasm helped to solve the potentially fatal problems posed to our bipedal species by gravity. As he put it: "There is . . . a great advantage in any reaction that tends to keep the female horizontal when the male ejaculates and stops copulation. The violent response of female orgasm, leaving the female sexually satiated and exhausted, has precisely this effect."[11] Orgasms tire women out, and cause them to stay lying down. Thanks to this, fertilization is not threatened. A similar hypothesis was put forward in the 1980s, when Gordon Gallup and Susan Suarez suggested that "the average individual requires about five minutes of repose before returning to a normal state after orgasm, and some people even lose consciousness at the point of orgasm."[12]

Lloyd responds by pointing out that the "average individual" Gallup and Suarez specify here turns out not to be a woman at all; instead it is the average *man* who needs five minutes of rest after orgasm, as determined by Kinsey and colleagues in 1948. She also provides evidence indicating that men and women do not respond to orgasms in the same ways: while men might typically need a lie-down, women often continue in a state of arousal after orgasm. Responding to Morris's image of female orgasm keeping the woman prone, Lloyd points out that this presupposes that the orgasmic woman is lying down. She then draws our attention to further research (available when Morris wrote his own piece) indicating that the most effective position for clitoral stimulation, and hence orgasm, during intercourse is when the woman is on top of the man. Under those circumstances, orgasm would seem to encourage, rather than prevent, the draining effects of gravity.[13]

The views of Morris, Gallup, and Suarez are fairly old, and one might think of them as easy targets. Lloyd considers many other theories of the female orgasm, including the far more recent "upsuck" theory, a hypothesis that remains influential today. The basic idea of the upsuck theory is that female orgasm increases the chances of fertilization, because orgasm results in ejaculated sperm being sucked by the uterus into the reproductive tract.

Lloyd recognizes that there is a study, done on just one woman, suggesting that pressure in the uterus drops after orgasm, which might indicate potential for a sort of vacuum suction effect. But she questions the idea that this results in any sperm being sucked into the cervix, or the body of the uterus. For example, she cites a study by Masters and Johnson—pioneers in the 1950s and '60s of the laboratory-based study of intercourse—that reported "[no] evidence of the slightest sucking effect," and she notes that the contractions of the uterus that accompany orgasm may push sperm out rather than sucking it in.[14] She concludes her review with the comment that "three studies suggest no upsuck related to orgasm, and the one study that does consists of a total of two experiments done on the same woman, which document not upsuck itself but a change in uterine pressure."[15]

Although Lloyd claimed there was no good evidence back in 2005 in support of biological functions for female orgasms, she was not foolish enough to suggest that such evidence could never appear. Ten years have passed since her skeptical assessment. Even so, the very best verdict we can come to for proponents of biological functions for female orgasms is that the question remains unsettled.[16] For example, a 2012 review goes against Lloyd's skeptical view, informing readers that "a variety of evidence suggests that female orgasm increases the odds of conception."[17] The authors of that review lean quite heavily on a particular version of the upsuck theory: they claim that orgasm promotes the release of the hormone oxytocin. They also report that, in general, oxytocin promotes the "transport" of sperm through the cervix.

Back in 2005, Lloyd raised an important challenge for this idea: orgasm is not the only way to cause the release of oxytocin, and the amount of oxytocin that orgasm releases is small.[18] Oxytocin levels also increase through sexual stimulation alone, even when orgasm does not occur. The question, then, is whether the boost to oxytocin levels that seems to arise from orgasm is enough to make a significant difference to sperm transport, given that nonorgasmic sexual stimulation appears to raise oxytocin levels all by itself.

Recent work by the sexual physiologist Roy Levin has ended up reinforcing Lloyd's critical treatment of the "upsuck" hypothesis in forceful terms. Levin calls the upsuck theory a "zombie hypothesis"—an idea that simply refuses to lie down even when (from the perspective of the evidence) it is well and truly dead. He notes that the experiments used to show a link between oxytocin release and sperm transport involved injecting women with around four hundred times as much oxytocin as would normally be released in orgasm. So Lloyd's question of whether orgasm releases enough oxytocin to make a difference to sperm transport is a good one.[19] Alongside many other criticisms, Levin also argues that sexual arousal results in the cervix moving into a position well away from the location of ejaculated semen, with the result that even if orgasm produced a suction effect, the cervix would not be close enough to the semen for any of it to be sucked up. His conclusion is blunt: "There is no uncontroversial empirical evidence for the human female's orgasm having any significant role in facilitating sperm uptake by enhancing either its rate or the amount transported or both in natural coitus."[20]

Lloyd concludes, then, that there is no good evidence supporting any story of female orgasm's functionality, and Levin concurs. Why, though, have researchers been so enthusiastic in embracing hypotheses of function, in spite of the poverty of evidence? Lloyd makes two suggestions. First, she suggests there is a bias in favor of *adaptationism.* Very roughly speaking, the adaptationist is one who assumes that the organism can be atomized into distinct traits, each with its own function with respect to survival and reproduction—rather in the manner that an exploded diagram of a washing machine reveals a variety of parts, each of which has a job to do. As we have seen, there is no guarantee that every trait must be explained in this way—it is certainly implausible to think male nipples have biological functions—but researchers on female orgasm seem to have shown a particular enthusiasm for hypotheses framed in terms of biological function, which has led them to overstate evidence in favor of their views, and to overlook evidence against them.

Second, and more interesting, Lloyd suggests that researchers have tended to assume that female sexuality must be like male sexuality: male orgasm has an obvious reproductive function, it is reliably elicited in sexual intercourse, it often results in a period of tiredness. These sorts of assumptions have been projected onto female orgasm in a way that obscures abundant evidence showing how female orgasm and intercourse are only loosely connected. For women, intercourse results in orgasm comparatively rarely, masturbation results in orgasm far more reliably. Indeed, some of Lloyd's earlier work on sex research in primates demonstrates how the presumption that female sexuality *must* be linked closely to reproduction has closed off important areas of research.

Female bonobos (the species formerly known as "pygmy chimpanzees") often engage in something called "genito-genital rubbing": two females hold each other and "swing their hips laterally while keeping the front tips of their vulvae, where the clitorises protrude, in touch with each other." [21] The question of whether this is same-sex *sexual* behavior, or whether instead it is social behavior of a nonsexual kind, seems like a sensible one to ask. But Lloyd points out that this question was closed off from serious inquiry when some researchers stipulated that behavior in nonhuman primates is sexual only when it occurs in oestrus—that is, only when the animal is in a fertile phase of its menstrual cycle and certain hormone measures are high. Since genito-genital rubbing occurs during nonfertile periods, it follows that genito-genital rubbing cannot be sexual. Evidently this is not an important experimental result. It is a trivial consequence of stipulating that behavior can be sexual only if it occurs during a period of fertility.

Darwin's Capitalism

The moral one might draw from Lloyd's work is that various forms of bias distort a true picture of the world. Morris went astray because he assumed, unreflectively, that when women have sex they are like men. Research on bonobos went astray because investigators assumed, without any inquiry, that sexual behavior must be linked to reproduction. These researchers should have set their biases aside and allowed the evidence to speak for itself. On this view, science informed by values is bad science. Good science—science that reveals how things are, as opposed to how we would like them to be, or how we naively expect them to be—is purged of the distorting effects of values.

This conclusion is challenged by the case of Charles Darwin. Darwin is, of course, known today as a natural historian. But Darwin was not a career scientist of the sort who work in laboratories all over the world today. He never held a salaried university position, he did not lecture to undergraduates, he did not chase grant funding. How, then, was Darwin able to fund a lifetime of scientific inquiry? The answer is that he was an exceptionally wealthy man.

Initially Darwin inherited a sizable sum from his father Robert, who (although a medical doctor) made most of his own fortune from investments in canals, roads, and agricultural land. Charles continued this entrepreneurial tradition. His books enjoyed lucrative sales, but the income he received from various forms of speculation, including loans and further investments in land, railways, and the like, far outstripped his earnings from publishing. In short, Darwin was steeped in the industrial capitalist milieu that surrounded the wealthy Victorian entrepreneur.[22]

This capitalist outlook not only funded Darwin's work, it informed it. Darwin's theorizing is saturated with the language of the marketplace, and it is saturated with the vision of agricultural improvement that had helped to make him rich. These aspects of Darwin's writings were noted only a few years after the *Origin of Species* was published. Karl Marx, a great admirer of Darwin, wrote to Friedrich Engels on June 18, 1862: "It is remarkable how Darwin recognises among beasts and plants his English society with its division of labour, competition, opening up of new markets, 'inventions,' and the Malthusian 'struggle for existence.'"

Marx was right about all of this. Darwin frequently used economic forms of argument to suggest that a given biological environment would, over time, contain species that were increasingly specialized and increasingly diverse. Just as economic competition drives traders into new niches, so new ecological niches are opened up by competition in the struggle for life. And just as competition promotes division of labor, so an initially modest stock of biological species can, over time, become diversified into a wonderful array of specialists. For Darwin, nature is a marketplace.

In November 1875, several years after receiving his letter from Marx, Engels wrote his own letter about Darwin to the philosopher Pyotr Lavrov:[23]

> The whole Darwinist teaching of the struggle for existence is simply a transference from society to living nature of Hobbes' doctrine of "bellum omnium contra omnes" [the war of all against all] and of the bourgeois economic doctrine of competition together with Malthus' theory of population. When this conjuror's trick has been performed . . . the same theories are transferred back again from organic nature into history and it is now claimed that their validity as eternal laws of human society has been proved.

Engels' comments differ from Marx's in their tone. Engels seems to suggest that because Darwin's theorizing is a reflection of his Victorian bourgeois economic outlook, this must mean that Darwin's theorizing is unreliable. This was just the line of reasoning that Trofim Lysenko would later endorse, when he claimed that Malthus had led Darwin astray. But why should we accept Engels's inference?

Darwin did indeed see the natural world through capitalist spectacles, but spectacles often help us to see things more clearly. Darwin's theorizing can be shown to be dubious only if we also think that the natural world is nothing like a marketplace. That will take argument; more specifically, it will require that we try to undermine the analogies Darwin draws between competition among members of a species for the resources required for survival and reproduction, and competition among manufacturers for customers.

There are similarities in both domains. In both domains, for example, Darwin suggests that, under suitable circumstances, specialization and increased efficiency can be promoted as though by a "hidden hand": "The more diversified the descendants from any one species become in structure, constitution, and habits, by so much will they be better enabled to seize on many and widely diversified places in the polity of nature, and so be enabled to increase in numbers."[24]

One might try to argue, in a manner reminiscent of Karl Popper, that while the outlook of Victorian capitalism played a role in inspiring Darwin's thoughts, it had no role in the detailed scientific case he made in favor of his vision of evolution by natural selection. This effort to insulate scientific justification from questions of value seems implausible, at least in Darwin's case. We have just seen that Darwin gives us a market-based rationale for how natural selection can promote diversity from initially uniform beginnings, hence why it is reasonable to think that natural selection is the primary agent of nature's spectacular diversity. What is more, the effort to insulate values from the project of scientific justification is unnecessary in any project that aims to vindicate the scientific image of the world: what matters is not, in this case, whether Darwin's views are influenced by his bourgeois ideology but whether that ideology acts to distort, or to reveal, the workings of the natural world.

Sometimes it is capitalism that informs respected theories, but sometimes it is Marxism. Over the past thirty years or so, an important group of evolutionary theorists have begun to stress the ways in which organisms of all types actively construct the environments in which they live. Beavers build dams, which in turn create ponds where beavers are safer from predators and where they have better access to food. Earthworms secrete mucous that coat their tunnel walls, ensuring a damp environment that suits their semi-aquatic physiology. These anecdotes illustrate the foolishness of an image of evolutionary change as a process whereby organisms are the passive victims of active environmental forces. This perspective of "niche construction" has been of considerable value in highlighting the active roles of organisms in determining evolutionary history.[25] And it has its roots in the work of the Harvard biologist Richard Lewontin, a self-confessed Marxist, and a man who explicitly conceived of evolution in Marxist terms as a dialectical interaction between organism and environment.[26]

We must be careful, then, not to generalize from the cases of Darwin and Lysenko to argue that a capitalist approach illuminates nature whereas a Marxist approach distorts it. And we do not have to endorse all—or even many—of the commitments of a capitalist worldview to agree that Darwin's entrepreneurial outlook helped him to see aspects of the natural world that others had missed.

Climate Change and Communication

We have just seen that values play a role as an input to the generation of scientific knowledge. They are also involved on the output side, when scientific knowledge is put to work in the process of policy formation. As usual, this is best illustrated by stepping away from science at first.[27]

Suppose that a friend has come to tea. You serve her a large slice of cake, which you bought from the shops that morning. Before taking a bite she asks, "Are there nuts in this cake?" If her reason for asking is simply that she isn't especially keen on nuts, then you may well reply with a "no," based simply on what the cake tastes like to you. If her reason for asking is that nuts will make her ill, then you might have a fairly close look through the ingredients list before replying "no." And if her reason for asking is that she is likely to suffer a fatal allergic reaction if exposed even to trace amounts of nuts, then you may well take time to study the ingredients list to check if there is a guarantee that the cake is nut-free before replying "no."

In the case of the cake, the amount of evidence you require before responding to your friend with a "no" increases with the costs of error. If you say there are no nuts and the cost of getting it wrong is simply that your friend won't like the cake much, then no great harm has been done and it is reasonable to expend only a little energy gathering evidence for your verdict. If you say there are no nuts and the cost of getting it wrong is your friend's life, then evidently you need to put considerable effort into checking that you are right.

What do cakes and nuts have to do with scientific advice? Suppose a government health official commissions a report on the health risks associated with cell phone use.[28] And suppose a scientist who is compiling the report comes across a poorly designed study indicating that excessive use of cell phones might cause brain damage. Perhaps the study in question has examined a very small number of people who suffered brain damage after using their phones, and it has ignored the need to check these results against the incidence of brain damage among people who never use cell phones.

Should the scientist simply dismiss that study altogether on the grounds that it is methodologically flawed? This would be too quick. The evidence from the study is very weak, but weak evidence should be taken into account under circumstances when the costs of error—in this case, the costs of dismissing a study that might turn out to have uncovered genuine harm—are potentially very high. That is exactly why, if your friend will die from ingesting nuts, you should warn her about nuts in her cake even when you have only a whiff of a reason to think there are any.

Why can't our imaginary scientist, compiling her report for the health official, simply record *all* the available evidence in a way that is uninformed by values? The answer is that a report cannot be infinitely long, and she needs to exercise judgment when deciding what evidence is relevant. Faced with the question of whether to include a poorly designed study, she needs to ask herself about the seriousness of the consequences—that is, she must take a stand on the moral gravity of the consequences—if she dismisses work that is later revealed to have been onto something. It turns out that questions of value are inescapable for responsible scientific activity.

These worries are not merely philosophers' abstractions, cooked up through reflection on an imaginary inquiry into cell phone use. Precisely the same worries have arisen in the context of the reports issued by the Intergovernmental Panel on Climate Change (IPCC), as my colleague Stephen John has recently shown.[29]

Every five years or so, the IPCC produces documents called "Assessment Reports." As the IPCC puts it, the function of these reports is to give policy-makers a summary of "the state of scientific, technical and socio-economic knowledge on climate change, its causes, potential impacts and response strategies." But what sources should be consulted when the sum total of knowledge on these matters is compiled? The IPCC's own answer is that "priority is given to peer-reviewed scientific, technical and socio-economic literature."

Peer-review is a rigorous process of quality control. By requiring that the sources of information for its reports normally be subject to peer-review, the IPCC increases the chances that the work it draws on will be free from falsehoods. That might seem like an unequivocally good thing. But while non-peer-reviewed studies may well contain many falsehoods, they might also contain important truths that, when overlooked, could be disastrous. John illustrates the practical impact of these concerns vividly, by examining the IPCC's changing assessment about the integrity of the West Antarctic Ice Sheet (WAIS). His analysis makes use of important sociological work by Jessica O'Reilly and colleagues, including their interviews with climate scientists.[30]

In its Third Assessment Report, published in 2001, the IPCC raised the possibility that the WAIS might collapse, leading to rising sea levels. But in spite of its acknowledgment that there was "high uncertainty" about the risk of collapse in the long term, the report noted that there was no risk of the ice sheet collapsing before 2100. This consensus had changed dramatically by the time the IPCC's Fourth Assessment Report appeared in 2007. Far from suggesting that the WAIS would remain intact for another century, the Fourth Report suggested that the WAIS might already be in the process of collapsing. In spite of this important acknowledgment, there was no effort to quantify the likely rate of ice loss from the WAIS in either the short or long term, and so the Fourth Report's estimate of future increases in sea levels did not include contributions from the collapsing WAIS.

Why didn't the Fourth Report include a quantified estimate for ice loss from the WAIS? Data and models had been produced well before the Fourth Report's publication that could have produced such estimates, but they had not been published in peer-reviewed form. One scientist complained to O'Reilly and colleagues that "it seemed to us we just couldn't do it [i.e., provide a quantified estimate for the effect of the WAIS collapse] because the IPCC depends on using peer-reviewed results." Of course, if the IPCC's reports began to include results that had not been subjected to peer-review, then the chances the reports will include errors would increase. But the costs of admitting error need to be traded off against the benefits of incorporating valuable work more quickly. The IPCC's reports cannot, and should not, be wholly free of values, because the IPCC must make an evaluative decision about how this balancing act is to be achieved. This statement is not meant to suggest that the IPCC's reports are improper, or unfairly biased: rather, it is simply a statement of the practical necessity of making a value-based judgement about whether to admit evidence that is shaky, but potentially significant.

Taking Sensible Precautions

These reflections on the costs of error and the benefits of timeliness help to give a firm grounding to the "Precautionary Principle," a principle that has been exceptionally important in environmental policy and health policy in the European Union and beyond.[31] There is no single agreed-upon formulation of the Precautionary Principle, but it is often understood, informally, as the notion that when dealing with potentially serious risks to health or to the environment, it is better to be safe than sorry.

Some commentators have taken the view that the Precautionary Principle is objectionably opposed to technical progress, and that it encourages hysterical regulatory responses to "phantom risks." These hostile reactions are easy to understand if we think the Precautionary Principle tells us that whenever some proposed course of action carries the potential for serious harm—even if there is no strong evidence that it will do so—then that course of action should be prohibited. Formulating the Precautionary Principle in this way would result in the banning of genetically modified (GM) crops even if there is only the shadow of a suspicion that "super weeds" might overrun the world. It would halt medical progress, for scientists can never demonstrate with certainty that new drugs, or new fertility treatments, are safe.

This version of the Precautionary Principle is not, in fact, opposed to technology. Instead, as the American academic lawyer Cass Sunstein (who served as President Obama's regulation tsar between 2009 and 2012) has argued, the real problem with this version of the principle is that it is incoherent.[32] It recommends nothing, either pro- or anti-technology. For suppose we suspect cell phones may cause brain damage, even though we admit there is no good evidence supporting this conjecture. And suppose we also suspect cell phones may prevent deaths from abduction and exposure, by allowing people to call home, even though we admit there is no good evidence supporting this conjecture, either. Precaution tells us we must ban mobiles, and that we must not ban mobiles. Precaution tells us nothing.

Fortunately, we do not need to throw precaution to the wind. One of the most important efforts to state the Precautionary Principle came at the "Earth Summit," held in Rio de Janeiro in 1992. Principle 15 of the Rio Declaration stated: "Where there are threats of serious or irreversible damage, lack of full scientific certainty shall not be used as a reason for postponing cost-effective measures to prevent environmental degradation."[33] This principle does *not* tell us that the mere possibility of disaster is enough to veto a proposed course of action. That is just as well, for possibilities of disaster are easy to come by, and they typically accompany all of our possible choices. Permitting the cultivation of GM crops brings the possibility of takeover by super weeds; halting the cultivation of GM crops brings the possibility of prolonging the harmful effects of drought, which new drought-tolerant varieties might allow us to evade.

To clarify what the Rio Declaration *does* say, imagine once again that I am about to distribute cake, but this time at a party for small children. I vaguely recall that the cake has nuts in it, but I'm not sure, because I've thrown away the box it came in. Suppose I am considering warning the parents present that the cake contains nuts. It would obviously be absurd to insist that I cannot issue this warning until I have established with certainty that there are nuts in the cake. My warning costs very little to issue, it is unlikely to do any harm (except to one or two unlucky children who may needlessly forego a slice of cake that is, in fact, nut-free), and it may avert very serious consequences. The Rio Declaration merely codifies this piece of common sense by saying that lack of scientific certainty should not stand in the way of acting to reduce harm, so long as the actions in question are cost-effective.

Under some circumstances this precautionary position will be pro-technology, not anti-technology. If early indications from a clinical trial seem to indicate massive health benefits, and many lives saved, in the event that a brand-new drug were to replace the standard treatment, then mere lack of certainty about its efficacy should not stand in the way of the new drug being more widely adopted, albeit in a carefully monitored fashion.

It is perhaps better not to think in terms of a "Precautionary Principle"—which might give us a recipe for how to act under circumstances of ignorance—but instead to think in terms of a "precautionary stance"—a posture that acknowledges scientific fallibility, and which is mindful of the costs of making mistakes. The precautionary stance reminds us that our actions should be, so far as is possible, reversible, so that if we learn that we've made a mistake we can undo, or at least limit, the damage arising from our chosen path. In March 2006, at Northwick Park Hospital in the United Kingdom, six healthy men's lives were put at risk through the severe adverse reactions they suffered in tests on the anti-inflammatory drug TGN1412.[34] Evidently, it would have been better for the men if there had been longer intervals between each one's dose. That way, the trial could have been halted before all the participants were exposed.

The influential sociologist Ulrich Beck has argued, in a dramatic fashion, that an ethos of scientific purity can have disastrous consequences if carried over to the practical domain of policy:[35]

> Scientists insist on the "quality" of their work and keep their theoretical and methodological standards high in order to assure their careers and material success. . . . The insistence that connections are not established may look good for a scientist and be praiseworthy in general. When dealing with risks, the contrary is the case for the victims; *they multiply the risks*. . . . To put it bluntly, insisting on the *purity* of the scientific analysis leads to the pollution and contamination of air, foodstuffs, water, soil, plants, animals and people.

Beck tells us that scientists are reluctant to assert causal linkages between chemicals and health risks unless they are proven to a high degree of certainty. He also suggests that this reluctance derives, in part, from those scientists' concerns for their personal wealth and advancement. That is unnecessarily inflammatory. There are good reasons for scientists to insist on solidity in their results. If scientific work is to have a cumulative character—if, that is, later generations are to build on the work of their predecessors—then it is important that its foundations are secure. In other words, it is important that the body of accepted scientific wisdom is—as far as is feasible—free from error.

This requirement explains the significant burden of proof required before research is deemed reliable enough to enter the expanding corpus of scientific knowledge. We have seen enough in this chapter to understand that these legitimate scientific concerns over evidential reliability must give way when scientific research is put to work in policy. Governments, and the scientific policy committees that advise them, are not primarily concerned with curating a slowly expanding body of reliable information. Instead, their own immediate concerns lie with the health and safety of their citizens. Here, the requirements of timely action demand that policy-makers sometimes act on the basis of poorly designed studies and flawed pieces of research. Slipshod methods do not inevitably produce misleading results. The precautionary stance asks us to remember this.[36]

Further Reading

For broad overviews of debates about science and value, see the following:

Hugh Lacey, *Is Science Value Free?* (London: Routledge, 1999).

Harold Kincaid, John Dupré and Alison Wylie, eds., *Value-Free Science: Ideals and Illusions* (Oxford: Oxford University Press, 2007).

Many of the arguments of this chapter are inspired by the work of Heather Douglas:

Heather Douglas, *Science, Policy and the Value-Free Ideal* (Pittsburgh: Pittsburgh University Press, 2009).

For further details regarding work on female orgasms, see:

Elisabeth Lloyd, *The Case of the Female Orgasm: Bias in the Study of Evolution* (Cambridge, MA: Harvard University Press, 2005).

23. Larry Laudan is often credited, probably erroneously, with being an early proponent of the Pessimistic Induction; see L. Laudan, "A Confutation of Convergent Realism," *Philosophy of Science* 48 (1981): 19–49.

24. P. Lipton, "Tracking Track Records," *Aristotelian Society, Supplementary Volume* 74 (2000): 179–205.

25. For significant challenges to this response to the Pessimistic Induction, see K. Stanford, "No Refuge for Realism," *Philosophy of Science* 70 (2003): 913–925, and H. Chang, "Preservative Realism and Its Discontents: Revisiting Caloric," *Philosophy of Science* 70 (2003): 902–912.

26. Stanford, *Exceeding Our Grasp.*

27. My thinking on these matters has been greatly influenced by the unpublished work of my former PhD student Sam Nicholson: S. Nicholson, "Pessimistic Inductions and the Tracking Condition," PhD dissertation, University of Cambridge, 2011.

Chapter 5: Value and Veracity

1. *Shale Gas Extraction in the UK: A Review of Hydraulic Fracturing,* Royal Society/Royal Academy of Engineering (2012): 5, http://www.raeng.org.uk/publications/reports/shale-gas-extraction-in-the-uk.

2. *Scientific Review of the Safety and Efficacy of Methods to Avoid Mitochondrial Disease Through Assisted Conception,* HFEA (2011), http://www.hfea.gov.uk/docs/2011–04–18_Mitochondria_review_-_final_report.PDF.

3. My account of Lysenko's speech is based on W. deJong Lambert, *The Cold War Politics of Genetic Research: An Introduction to the Lysenko Affair* (Dordrecht, Holland: Springer, 2012).

4. R. M. Young, "Getting Started on Lysenkoism," *Radical Science Journal* 6–7 (1978): 81–105.

5. S. C. Harland, "Nicolai Ivanovitch Vavilov, 1885–1942," *Obituary Notices of the Royal Society* 9 (1954): 259–264.

6. L. Graham, *Science in Russia and the Soviet Union* (Cambridge: Cambridge University Press, 1993), p. 130.

7. For details, see D. Turner, "The Functions of Fossils: Inference and Explanation in Functional Morphology," *Studies in History and Philosophy of Biological and Biomedical Sciences* 31 (2000): 193–212.

8. E. A. Lloyd, *The Case of the Female Orgasm: Bias in the Study of Evolution* (Cambridge, MA: Harvard University Press, 2005).

9. For examples, see Lloyd's website: http://mypage.iu.edu/~ealloyd/.

10. A. Kinsey et al., *Sexual Behavior in the Human Female* (Philadelphia: W. B. Saunders, 1953), p. 164.

11. D. Morris, *The Naked Ape: A Zoologist's Study of the Human Animal* (New York: McGraw-Hill, 1967), p. 79.

12. G. Gallup and S. Suarez, "Optimal Reproductive Strategies for Bipedalism," *Journal of Human Evolution* 12 (1983), p. 195.

13. Lloyd, *The Case of the Female Orgasm*, p. 58.

14. W. H. Masters and V. E. Johnson, *Human Sexual Response* (Boston: Little, Brown 1966), p. 123; Lloyd, *The Case of the Female Orgasm,* p. 182.

15. Lloyd, *The Case of the Female Orgasm,* p. 190.

16. For Lloyd's own resolutely skeptical update, see E. Lloyd, "The Evolution of Female Orgasm: New Evidence and Feminist Critiques," in F. de Sousa and G. Munevar, eds., *Sex, Reproduction and Darwinism* (London: Pickering and Chatto, 2012).

17. D. Puts, K. Dawood, and L. Welling, "Why Women Have Orgasms: An Evolutionary Analysis," *Archives of Sexual Behavior* 41 (2012): 1127–1143.

18. Lloyd, *The Case of the Female Orgasm,* p. 188.

19. R. Levin, "Can the Controversy About the Putative Role of the Human Female Orgasm in Sperm Transport Be Settled with Our Current Physiological Knowledge of Coitus?," *Journal of Sexual Medicine* 8 (2011): 1566–1578.

20. R. Levin, "The Human Female Orgasm: A Critical Evaluation of Its Proposed Reproductive Functions," *Sexual and Relationship Therapy* 26 (2011): 301–314.

21. E. Lloyd, "Pre-Theoretical Assumptions in Evolutionary Explanations of Female Sexuality," *Philosophical Studies* 69 (1993): 139–153.

22. For details on Darwin's early life, see Janet Browne's peerless biography: J. Browne, *Charles Darwin: Voyaging* (London: Pimlico, 2003).

23. The letters from Marx and Engels are both taken from A. Schmidt, *The Concept of Nature in Marx,* trans. B. Fowkes, from the German edition of 1962 (London: New Left Books, 1971).

24. C. Darwin, *On the Origin of Species* (London: John Murray, 1859), p. 108.

25. For details, see J. Odling-Smee, K. Laland, and M. Feldman, *Niche Construction: The Neglected Process in Evolution* (Princeton, NJ: Princeton University Press, 2003).

26. See, for example, R. Levins and R. Lewontin, *The Dialectical Biologist* (Cambridge, MA: Harvard University Press, 1985).

27. The arguments of this section are heavily influenced by the important work of Heather Douglas: H. Douglas, *Science, Policy and the Value-Free Ideal* (Pittsburgh: Pittsburgh University Press, 2009).

28. For a detailed study of responses to allegations of cell phone risk, see A. Burgess, *Cellular Phones, Public Fears and a Culture of Precaution* (Cambridge: Cambridge University Press, 2003).

29. S. John, "From Social Values to *p*-Values: The Social Epistemology of the International Panel on Climate Change," *Journal of Applied Philosophy* (forthcoming).

30. J. O'Reilly, N. Oreskes, and M. Oppenheimer, "The Rapid Disintegration of Projections: The West Antarctic Ice Sheet and the Intergovernmental Panel on Climate Change," *Social Studies of Science* 42 (2012): 709–731.

31. The argument of this section draws on my article "Taking Sensible Precautions," *Lancet* 371 (2008): 1992–1993.

32. C. Sunstein, *Laws of Fear: Beyond the Precautionary Principle* (Cambridge: Cambridge University Press, 2005).

33. *Rio Declaration on Environment and Development,* http://www.unesco.org/education/nfsunesco/pdf/RIO_E.PDF.

34. G. Suntharalingam et al., "Cytokine Storm in a Phase 1 Trial of the Anti-CD28 Monoclonal Antibody TGN1412," *New England Journal of Medicine* 355 (2006): 1018–1028.

35. U. Beck, *Risk Society* (London: Sage, 1992), p. 62; italics in original.

36. S. John, "In Defence of Bad Science and Irrational Policies," *Ethical Theory and Moral Practice* 13 (2010): 3–18.

Chapter 6: Human Kindness

1. C. Darwin, *The Descent of Man* (London: John Murray, 1871), p. 106.

2. M. Ghiselin, "Darwin and Evolutionary Psychology," *Science* 179 (1973): 967.

3. M. Ghiselin, *The Economy of Nature and the Evolution of Sex* (Berkeley: University of California Press, 1974).

4. Darwin, *The Descent of Man,* p. 87.

5. C. Darwin, *On the Origin of Species* (London: John Murray, 1859).

6. R. Alexander, "Evolutionary Selection and the Nature of Humanity," in V. Hösle and C. Illies, eds., *Darwinism and Philosophy* (Notre Dame, IN: University of Notre Dame Press, 2005), p. 309.

7. D. Zitterbart et al., "Coordinated Movements Prevent Jamming in an Emperor Penguin Huddle," *PLoS One* (2011), DOI: 10.1371/journal.pone.0020260.

8. J. Birch, "Gene Mobility and the Concept of Relatedness," *Biology and Philosophy* 29 (2014): 445–476.

9. For further details about social behavior in bacteria, see ibid.; for an elegant discussion of the differences between biological and psychological altruism, see E. Sober and D. Wilson, *Unto Others* (Cambridge, MA: Harvard University Press, 1999).

10. R. Trivers, "The Evolution of Reciprocal Altruism," *Quarterly Review of Biology* 46 (1971): 35–57.

11. S. West, A. Griffin, and A. Gardner, "Social Semantics: Altruism, Cooperation, Mutualism, Strong Reciprocity and Group Selection," *Journal of Evolutionary Biology* 20 (2007): 415–432.

12. R. Dawkins, *The Selfish Gene,* 30th Anniversary Edition (Oxford: Oxford University Press, 2006), p. 4.