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SCIENCE AND MYTH

JOHN MAYNARD SMITH

> At twenty, I thought 'we should do without myths and confine ourselves to science... but it really won't do'.
>
> Maynard Smith

Recently, after giving a radio talk on Charles Darwin, I received through the post a pamphlet by Don Smith entitled 'Why Are There Gays At All? Why Hasn't Evolution Eliminated Gayness Millions of Years Ago?' The pamphlet points to a genuine concern: the prevalence of homosexual behaviour in our species is not understood, and is certainly not something that would be predicted from Darwinian theory. Smith wrote the pamphlet because he believes the persecution of gays has been strengthened and justified by the existence of a theory of evolution that asserts gays are unfit because they do not reproduce. He also believes that gays can be protected from future persecution only if it can be shown that they have played an essential and creative role in evolution. His argument is that, in evolution, novelty arises when individuals adopt mating habits different from those typical of their species: it is summed up on a button that reads: 'Sexual deviation is the mainspring of evolution'.

I do not find this argument particularly persuasive, but that is not the point I want to make. I think Smith would have been better advised to have written: 'If people despise gays because gayness does not contribute to biological fitness, they are wrong to do so. It would be as sensible to persecute mathematicians because an ability to solve differential equations does not contribute to fitness. A scientific theory—Darwinism or any other—has nothing to say about the value of a human being.'

The point I am making is that Smith is demanding of evolutionary biology that it be a myth: that is, a story with a moral message. He is not alone in this. Elaine Morgan's book *The Descent of Woman* is an account of the origin of *Homo sapiens* that is intended to give mythical support to

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the women's movement by emphasizing the role of the female sex and, in particular, the mother–child bond. She claims, with reason, that many other accounts of human evolution have, perhaps unconsciously, placed undue emphasis on the role of males. Earlier, George Bernard Shaw wrote *Back to Methuselah* (1922) avowedly as an evolutionary myth, because he found in Darwinism a justification of selfishness and brutality, and because he wished instead to support the Lamarckian theory of the inheritance of acquired characters, which he saw as justifying free will and individual endeavour.

We should not be surprised by Don Smith, Elaine Morgan, and Bernard Shaw. In all societies, people have constructed myths about the origins of the universe and of humans. The function of these myths is to define our place in nature, and thus to give us a sense of purpose and value. Since Darwinism is, among other things, an account of human origins, is it any wonder that it is expected to carry a moral message?

The people and the objects that figure in a myth stand not only for themselves, but also as symbols of other things. To some extent, myths and their symbolic components develop simply because human beings find it difficult to accept any input as meaningless. Shown an ink blot, we see witches, bats, and dragons. This refusal to accept input as mere noise lies at the root of divination by tarot cards, tea leaves, the livers or shoulders of animals, or the sticks of the *I Ching*. It may also account for the strangely late development of a mathematical theory of probability or of any scientific theory with a chance element. As anthropologist Dan Sperber has written: 'Symbolic thought is capable, precisely, of transforming noise into information.'

Another—and in the present context, more important—function of myths is to give moral and evaluative guidance. Some myth making is quite conscious. In *Back to Methuselah*, for example, Shaw deliberately invented a story that would have the moral effect he desired. More usually, however, I suspect that a myth-maker conceives a story that moves him or her in a particular way—at its lowest, it reinforces prejudices, and at its highest, to borrow Aristotle's words, it evokes feelings of pity and fear. People repeat myths because they hope to persuade others to behave in certain ways.

This raises the question of why we use myths rather than simple statements of instruction. Why do we talk of King Alfred and the cakes, for example, instead of saying that people in important positions should be modest? Perhaps a story whose meaning has to be puzzled out or guessed carries more conviction than a mere instruction. What we imagine is more important than what we are told.

Sometimes, I find it hard to discover how far people distinguish stories intended to give moral guidance from those meant simply to supply technical help. Confusion seems particularly likely to crop up when rituals are involved. For example, if, before going into battle, a man sharpens his spear and undergoes ritual purification (or, for that matter, cleans his rifle and goes to mass), he may regard the two procedures as equally efficacious. Indeed, they may well be so, one in preparing the spear and the other himself. If we regard the former as more practical, we do so only because we understand metallurgy better than psychology.

Despite the difficulty, most people do try to distinguish procedures and technical instructions that alter the external world from procedures and stories intended to alter our own state of consciousness or persuade us that certain things are right. Indeed, we take some trouble when educating our children to give hints about which category of information is being transmitted. For example, a surprisingly large proportion of the stories read aloud to children, particularly those with a moral message, are about talking animals or even talking steam-engines. It is as if we wanted to be sure that the stories are not taken literally.

While such efforts may be successful in many spheres of human endeavour, the examples of Don Smith and Bernard Shaw show how hard it is for many people to separate science, and especially evolution, from myth. One reaction to this difficulty is to assert that there is no difference, that evolution theory has no more claim to objective truth than Genesis. Many scientists would be enraged by such an assertion, but rage is no substitute for argument. In the last century, it was widely held that the scientific method, conceived of as establishing theories by induction from observation, led to certain knowledge. Darwin and Einstein have robbed us of that certainty—or have liberated us from that prison. If, as Darwin showed, there is not a fixed and finite number of things in the universe, each with a knowable essence, then induction is logically impossible. Einstein, in turn, showed that what scientists had been most confident of—classical mechanics—was at worst false and at best a special case of a more general theory. After that twin blow, certain knowledge is something we can expect only at our funerals.

But it is one thing to admit that scientific knowledge cannot be certain, and another to claim that there is no difference between science and myth. Karl Popper, perhaps the most influential contemporary philosopher of science, has told us that it was the impact of Einstein, and in particular the wish to distinguish Einstein's theory from those of Freud, Adler, and Marx, that led him to propose falsifiability as the criterion for separating science from pseudo-science. If a theory is scientific, he suggested,

observations can be conceived of which, if they were accepted, would show the theory to be false. In contrast, he suggests that no conceivable pattern of human behaviour could falsify Freudian theory.

Popper's views have been attacked, primarily on the grounds that there are no such things as theory-free observations. Every observation is subject to interpretation, conscious and unconscious. Consequently, there can never be certain grounds for rejecting a scientific theory, and hence the distinction between science and pseudo-science disappears.

This criticism seems to me largely to miss the point. If Popper were claiming that scientific knowledge were certain, then the impossibility of certain falsification would indeed be damaging. But he makes no such claim. He insists on two things. First, a scientific theory must assert that certain kinds of events cannot happen, so that the theory is falsified if these events are subsequently observed, and second, there is inevitably a logical asymmetry in any attempt to test a theory, so that a theory can be falsified but cannot be proved true by the acceptance of observation.

There is, however, a tide of ideas that would deny the distinction. The emotional force behind this tide derives, in part, from an entirely proper disgust at some of the consequences of technology in the modern world and, in part, from an equally proper wish to treat the ideas of other peoples as of equal value to our own. What is common to these two reactions is the conviction, which I share, that scientific theories are not the only kind of ideas that we need. A frequently drawn corollary of this conviction, which I do not share, is that scientific ideas are not distinguishable from other ideas.

One source of the belief that science and myth can be lumped together lay, surprisingly, in Marxism, a philosophy that has led to two very different interpretations of science. One interpretation was pioneered by the British physicist J. D. Bernal. For Bernal, the crucial thing about science was that it made socialism possible by providing the techniques needed to satisfy people's wants. He saw science as being distorted under capitalism—for example, by being pressed into the service of military research; nevertheless, he does not appear to have thought that capitalism would prevent science from making progress toward an understanding of nature. In this, his views coincided with those of Marx himself, who largely excluded science from the set of ideas—for example, about religion, philosophy, and law—that he saw as reflecting the class interests of those who held them.

Thus Bernal regarded science as the greatest hope for the future, and would have rejected any suggestion that science is indistinguishable from myth. However, another thread within Marxism has led to a different end.

In 1931, the Russian B. Hessen argued that not only had Newton been influenced by the technical problems of his day (for example, gunnery and navigation), but also that the form his theory took reflected contemporary society. Such a view is perhaps more easily understood and more obviously true when applied to Darwin, whose theory did recognize in the natural world the processes of competition predominant in the society of his day. Indeed, both Darwin and Wallace stated that they borrowed their essential concept from the economist Malthus. If, then, this second thread of Marxist thinking argues, major scientific theories merely project on to nature features of contemporary society, they have more in common with myths than most scientists would readily accept.

Here, it seems to me, a crucial distinction must be made between the psychological sources of a theory and the testing of it. If Darwin's ideas, or Newton's, were accepted because they were socially appealing, then indeed science and myth would be indistinguishable. But I do not think that they were. They were accepted because of their explanatory power and ability to withstand experimental test. Of course, new ideas in science sometimes come from analogies with society, just as, in one scientific discipline, such as biology, they arise by analogy with others, such as physics and engineering. But what matters for the progress of science is not where the ideas come from, but how they are treated.

Society influences the development of science through both the problems that seem worth solving and the resources available for their solution. I have little doubt that society also influences scientists, both as individuals and groups, by making some ideas seem worth pursuing and others implausible or unpromising. For example, my own caution about applying to humans ideas drawn from a study of animal societies—a caution that contrasts with the enthusiasm of such scientists as E. O. Wilson and Richard Alexander—probably arose because I grew up under the shadow of Hitler and the Nazi theories of racial superiority and biological determinism, and not because of anything internal to biology or sociology.

There is, however, a caricature inherent in the externalist view of science that I reject emphatically. This is the idea that we can evaluate a scientific theory by reference to the society in which it was born, or to the moral and political conclusions that might be drawn from it. Once accept that view and science is dead, as genetics died in Russia in 1948, when Stalin supported Lysenko's Lamarckian views against the Mendelians. Stalin took his position partly in the hope of quick returns in agricultural productivity, and partly because Lysenko's belief in the inheritance of acquired characters seemed to accord better with Marxism than did the orthodox—and, as

it happens, more nearly correct—Mendelian doctrine that hereditary characteristics are transmitted from parent to offspring by genes, and that the genetic message is independent of changes induced in the body of the parent during its lifetime.

Today, the belief that there are no objective criteria whereby one can choose between rival theories (and hence, by implication, that one can allow one's prejudices full rein) derives largely, I think, from the work of Thomas Kuhn, although the conclusion is far from the one that he himself would wish to draw. Kuhn sees science as divided into 'normal' and 'revolutionary' periods. In a period of normal science, members of a scientific community agree about what assumptions can be made, what problems are worth solving, what will count as a solution, and what experimental methods should be used. Most important, they share a 'paradigm', or set of exemplary solutions to problems, that can be used as a standard. Revolutions occur when, usually as a result of long-continued failure to solve certain problems within the accepted frame, a fundamentally new set of assumptions and procedures replaces the old.

All this seems to bear some resemblance to reality; it also bears some resemblance to Popper's remark that 'there is much less accumulation of knowledge in science than there is revolutionary changing of scientific theories'. The main difficulty lies in Kuhn's account of how one paradigm replaces another. He speaks of a 'paradigm debate' in which the proponents 'fail to make complete contact with each other's viewpoints', and in which they 'see the world differently'. Again, there is much in what he says. During my lifetime in science, I have engaged in too many arguments, in which I and my opponent have talked right past one another, not to recognize this.

The fallacy is to suppose that because two scientists are unable to understand each other fully, there is no rational way, given time, of settling the issue between them. With the passage of time, choosing between two theories or two methods of approach becomes easier. Eventually, one or the other approach is more successful in overcoming its difficulties, or, as in the case of the particle and wave interpretations of light, a third theory is developed and subsumes them both. The trouble is that scientists must often commit themselves before the evidence is in. In Darwin's words, one must have 'a theory by which to work'. This is what gives an air of irrationality to the procedure, and has led some people to suppose that the choice between scientific theories is arbitrary.

Consider an example. After the rediscovery of Mendel's laws in 1900, a debate broke out between the Mendelians and the earlier school of biometricians, headed by Karl Pearson. Pearson refused to accept the new

theory, at least in part because it was incompatible with his previously held philosophical view that the business of science is merely to describe the world and not to imagine hypothetical entities such as genes. As the historian Bernard Norton has recently pointed out, Pearson understood that Mendelian inheritance could account for the phenomena of continuous variability, which he had been studying, but still rejected it on philosophical grounds—an interesting illustration of how philosophical preconceptions can be a poor guide to scientific practice. Yet, despite all this, no one today would doubt the utility either of Pearson's statistical methods or of Mendelian genetics.

If theories were genuinely incommensurable, and rational choice between them impossible, progress in science would not be expected. Kuhn himself accepts the reality of scientific progress, but only in the sense that the explanatory power of scientific theories has increased: he doubts whether it is sensible to say that science draws closer to the truth about what is 'really there'. I will return to this point in a moment.

Before leaving Kuhn, I want to suggest that, despite his insights, his insistence on a distinction between normal and revolutionary science, and on the incommensurability of paradigms, has been exaggerated. The major scientific revolution during my working life has been the rise of molecular biology, which has all the characteristics of a new 'disciplinary matrix' in Kuhn's sense—new scientists, new problems, new experimental methods, new journals, new textbooks, and new culture heroes. But where was the incommensurability? I myself was raised in the older discipline of classical genetics, and have never mastered the experimental methods of the new. Yet my almost immediate reaction to the Watson–Crick paper was that a mystery within my field had been cleared up. Those of us trained in classical genetics sometimes had difficulty in learning the new techniques, but there were few conceptual difficulties and no paradigm debate.

Perhaps the birth of molecular genetics was not a Kuhnian revolution. As it happens, I suspect that before we make much progress in developmental biology, a bigger conceptual revolution may be needed than the transition from classical to molecular genetics. All the same, if molecular biology could be born without the full panoply of a paradigm debate, where does that leave the concepts of normal and revolutionary science?

The history of genetics also forces us to look again at Kuhn's suggestion that progress in science is progress in explaining, but not progress in knowing what the world is really like. To me, the change from the concept of the gene as a Mendelian factor to the gene as a piece of a chromosome, and thence to the gene as a molecule of DNA, does look like progress in knowing what the world is like. But perhaps that is a question I should leave to the philosophers.

I would not have spent so much time discussing the difference between scientific theories and myths if the difference between them were obvious. Indeed, they have much in common. Both are constructs of the human mind, and both are intended to have a significance wider than the direct assertions they contain. Popper suggested falsifiability as the criterion distinguishing them, and I think he was right. However, we can often also distinguish them by their function: the function of a scientific theory is to account for experience—often, it is true, the rather esoteric experience emerging from deliberate experiment; the function of a myth is to provide a source and justification for values. What, then, should be the relation between them?

Three views are tenable. The first, sometimes expressed as a demand for 'normative science', is that the same mental constructs should serve both as myths and as scientific theories. If I am right, this widely held view underlies the criticism of Darwinism from gays, from the women's movement, from socialists, and so on. It explains the preference expressed by some churchmen for 'Big Bang' as opposed to 'steady state' theories of cosmology. Although well-intentioned, it seems to me pernicious in its effects. Applied to evolution theory, it means either that we must embrace Darwinism and draw from it the conclusion that gays are unnatural and social services wicked, or that we must embrace Lamarckism whether or not the genetic evidence supports it. Normative science will be bad morality or bad science, and most probably both.

The second view is that we should do without myths and confine ourselves to science. This is the view I held at the age of 20, but it really won't do. If, as I now believe, scientific theories say nothing about what is right, but only about what is possible, we need some other source of values, and that source has to be myth, in the broadest sense of the term.

The third view, and I think the only sensible one, is that we need both myths and scientific theories, but that we must be as clear as we can be about which is which. In essence, this was the view urged by the French molecular biologist Jacques Monod in *Chance and Necessity* (1972). Oddly, Monod was almost universally derided by his critics for arguing that one can derive values from science, when in fact he argued the precise opposite. His case was that there is no place in science for teleological, or value-laden, hypotheses. Yet, to do science, one must first be committed to some values—not least, to the value of seeking the truth. Since this value cannot be derived from science, it must be seen as a prior moral

commitment, needed before science is possible. So far from values being derived from science, Monod saw science as depending on values.

Although I disagree with some aspects of his book, I agree with Monod on two basic points. First, values do not derive from science, but are necessary for the practice of science. Second, we should distinguish as clearly as we can between science and myth. We should make this distinction, not because we could then discard the myths and retain only science, but because the roles they play are different. Scientific theories tell us what is possible; myths tell us what is desirable. Both are needed to guide proper action.