What Does Climategate Say About Science?

by
Terence Kealey, Vice Chancellor, University of Buckingham,
Buckingham MK 18 1EG, UK terence.kealey@buckingham.ac.uk

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> "The hard core of a programme is rendered unfalsifiable by the methodological decision of its protagonists."
> Imre Lakatos *Criticism and the Growth of Knowledge* 1974

> "The scientist is restricted by his instruments, money, the attitudes of his colleagues, his playmates, and by innumerable physiological, sociological, historical constraints."
> Paul Feyerabend *Against Method* 1975

The emails sent by members of the climatic research centre at the University of East Anglia have provoked international outrage, as have the many flawed global warming papers that have appeared in recent years such as those describing the hockey stick graph[1], to say nothing of the flawed predictions of the Intergovernmental Panel on Climate Change (IPCC) over such issues as the rate of disappearance of the glaciers in the Himalayas. But such outrage has been naive because it has been premised on the assumption that scientists are - and should be - dispassionate seekers after truth. Yet in fact scientists are and should be advocates. Science has always been rooted in advocacy, as was illustrated by an episode from its very beginnings during the 5th century BC.

Pythagoras (of the Theorum) was a good scientist but he was of a mystical bent and he revered 'rational' numbers (whole numbers or whole fractions). He believed they explained the Harmony of the Spheres. Pythagoras, indeed, believed that whole numbers underpinned the universe from music to the movement of the planets. But Pythagoras had a student called Hippasus, and Hippasus discovered that the square root of 2, $\sqrt{2}$, is not a rational number. It is in fact an 'irrational'

number, and its exact quantity will never be precisely calculated because, as Hippasus showed two and a half thousand years ago, irrational numbers can never be definitively calculated. This proof upset Pythagoras and he asked Hippasus to retract it. But Hippasus refused, so Pythagoras had him drowned.

That's what scientists are like in their natural state. Now – call me soft – but I think Pythagoras went too far; I think that scientists should desist from killing each other or even from telling outright falsehoods. But, like advocates in court, scientists can nonetheless be expected to put forward only one very partial case – and that as strongly as possible – and no-one should expect a scientist to be anything other than a biased advocate.

Consider the early controversy over the age of the earth. The 19th century geologist Sir Charles Lyell had, by his study of the rate of erosion of cliffs, proposed the earth not to have been created at 9.00 am on the 23rd of October 4004 BC but, rather, some hundreds of millions of years earlier. But, as we know from volcanoes, the core of the earth is red hot. And when contemporary geologists measured the temperature of the molten core, and when they calculated its rate of heat loss, they concluded that the earth could be only a few millions of years old. Had it been any older its core would have completely cooled. Lyell had apparently been falsified.

In the face of this apparent falsification, did Lyell's followers ditch their ideas? No. Like advocates presented with contradictory data that cannot be challenged, they simply ignored it. They knew how old the sedimentary rocks had to be, and they didn't believe the falsifiers. So, not knowing how to falsify the falsifiers, they simply pressed on with their own pre-existing programme of research, assuming that something helpful would turn up eventually. Which it did. Somebody in some other discipline discovered radioactivity, somebody discovered the core of the earth to be radioactive, somebody discovered that radioactive reactions emitted heat and *hey presto* the problem was resolved: the core of the earth generates heat, which is why it is still hot; and the earth is indeed very old.

In his 1605 book *The Advancement of Knowledge*, which helped launch the modern discipline we call the philosophy of science, Francis Bacon proposed a four-step process by which science advanced, namely by (i) observation, (ii) induction, (iii) deduction and (iv) experimentation. Bacon saw this as an almost mechanical or determinist activity based on logic, which he supposed precluded individualistic human whims. But because the number of potential observations is so large (does the

colour of an astronomer's socks correlate with his or her recordings of the movement of a planet?) scientists must inevitably select the observations they believe to be relevant, from which they then deduce and induce the theories they seek to test. Scientists therefore select particular theories out of a range of possibilities. And they then (being human) design experiments to prove their own theories right.

Consequently, contrary to what many people believe that Karol Popper wrote, science is in practice not about falsification.[2] In practice great scientists ignore embarrassing data, and they refuse to feel falsified when they don't want to be. Scientists know they are working at the limits of knowledge, which means that that knowledge must necessarily be imperfect, so (like Charles Lyell) scientists will refuse to draw definitive negative conclusions from unhelpful new findings because they know that those new findings might themselves need re-evaluation in the light of further subsequent data (such as radioactivity) that has yet to be revealed.

Indeed, as Thomas Kuhn explained in his classic 1962 book *The Structure of Scientific Revolutions*, scientists' personal attachment to their own theories in the face of conflicting data means that the research community's dispassionate collective verdict over what is 'truth' can be delivered only after all the competing data has come in and only after all the arguments have been made (or, as was said humorously by Max Planck:- "A new scientific truth does not triumph by convincing its opponents and making them see the light but rather because its opponents eventually die and a new generation grows up that is familiar with it"). These arguments have been summarised by Alan Chalmers of Finders University in his excellent introduction to the philosophy of science *What Is This Thing Called Science?* (3rd ed 1999, Open University).

Consequently, we can see how the climate change scientists of the IPCC and of the conventional global warming paradigm saw no conflict between their partiality in the arguments they put forward and their responsibilities to 'truth', just as advocates in court under the common law see no conflict between their partiality in the arguments they put forward and their responsibilities to 'justice'. In both cases, the scientists and advocates see their prime responsibility as being the putting forward of the best arguments to support their case/client, and they delegate the adjudication over impartial 'truth' to the jury of peers.

Such partiality cannot excuse misrepresentation, of course, nor the persistent non-disclosure of inconvenient facts, and those will always be ethical crimes, but it

would be naïve of the general public to expect scientists always to present their work and theories dispassionately. It would also be naïve of the general public to expect scientists to disclose all their data promptly. In his otherwise excellent 2010 book *The Hockey Stick Illusion* (Independent Minds) where he dismissed the claims of many climate change scientists, AW Montford nonetheless professed astonishment that researchers might feel that they can legitimately withhold original data. But as Tim Birkhead recently reported in the *Times Higher Education*, such withholding is a conventional aspect of many disciplines in science. Indeed, it is endorsed by the British Government's research councils. Thus the Natural Environment Research Council states that "individual scientists, principal-investigator teams and programmes will be permitted a reasonable period of exclusive access to data sets they have collected" while the Biotechnology and Biological Sciences Research Council states that "researchers have a legitimate interest in benefiting from their own time and effort in producing the data, but not in prolonged exclusive use."[3]

But why should scientists publish anything at all? In his 1942 essay *The Normative Structure of Science* Robert Merton, the great sociologist of science, described science with the acronym CUDOS (note how it is pronounced). The letters stand for Communism, Universalism, Disinterestedness and Organised Scepticism, by which Merton meant that scientists share knowledge (communism), that knowledge is judged objectively (universalism), that scientists act in ways that appear selfless, and that ideas are tested collectively.

But actually Merton was being ahistorical. *Pace* his acronym, scientists indeed seek either kudos or money or both (ie, they are not communistic, they are self-seeking, which is legitimate but not particularly noble) but their publishing has always been dictated by self-interest. Indeed, in its natural state science was originally characterised by the paradox of secret publishing: researchers did not want others to benefit from their advances. So some scientists, having dated the report of a discovery, would seal and deposit it with a college or lawyer, to open it only to dispute priority with a later competitive publication. Others would publish in code or in anagrams: Galileo published his discovery of the rings of Saturn in 1610 as *smaismrmilmepoetaleumibunenugttauiras* for *Altissimum planetam tergeminum observavi* (I have observed the most distant planet to have a triple form) while Robert Hooke published his law of elasticity in 1660 as *ceiiinosssttuu* for *ut tensio sic vis* (stress is proportional to strain.)

Secrecy was originally normal: when around 1600 a young London obstetrician called Peter Chamberlen invented the obstetric forceps, for over a century he, his younger brother, his younger brother's son and that son's son (all obstetricians) kept the invention a secret. Rich women, knowing that the Chamberlens were the best obstetricians in Europe, engaged them to deliver their babies, but the price those women paid (apart from handsome fees) was to be blindfolded and trapped alone with the Chamberlens in a locked room during labour so that no one could discover the secret of the forceps. That emerged only during the 1720s when the last Chamberlen, having retired rich but childless, finally divulged it.

It was Robert Boyle who, by his leadership of the Royal Society of London, which was created exactly 350 years ago this year, negotiated (i) the convention whereby priority - and therefore esteem - goes to the scientist who *publishes* first, not to the scientist who might have made the discovery earlier but who has kept the findings secret, and (ii) the convention that papers are accepted for publication only if they contain a methods section as well as a results section, to allow reproducibility.

We see here, therefore, that science is not *innately* a public good: it is innately a discreet one where, in a state of nature, scientists would publish not their methods but only their findings – and where they would sometimes delay or obscure the publication even of those. But it was Boyle who realised, in classic game theory mode, that if the Fellows (aka members) of the infant Royal Society collaborated with each other in publishing their findings (i) openly, and (ii) including their methods sections, then the scientists within the Society would do better, by virtue of their access to the whole of the Society's membership's collective discoveries, than would those isolated researchers who worked outside the circle of mutual disclosure. And it was because the Royal Society's original experiments were conducted collectively but in the presence only of its Fellows, and because its publications were preferentially circulated to its Fellows, that the Fellows enjoyed an advantage over non-Fellows.

Science, therefore, only *appears* to be public because, over the centuries, most scientists globally have gradually modelled themselves on the Royal Society's 'new' conventions, the better to take advantage of the mutuality of knowledge. But not all scientists have done so completely, and as Birkhead showed in his *THE* article many disciplines have elaborated the convention of publishing their findings a year or two before they publish their data, thus keeping a lead on the further study of their data. Everyone in those disciplines agrees that, since the exploitation of other people's data

is so much easier than discovering it for oneself, a discoverer's year or more of monopoly is only fair.

To conclude, therefore, scientists are not disinterested, they are interested, and as a consequence science is not dispassionate or fully transparent, rather it is human and partially arcane. As I argue elsewhere, science is not the public good of modern myth, it is a collegiate and quasi-private or invisible college good.[4] That means, by the way, that it requires no public subsidies. More relevantly, it means that individual scientists' pronouncements should be seen more as advertisements than as definitive. Peer review, too, is merely a mechanism by which scientists keep a collective control over access to their quasi-private enterprise.

One the e-mails leaked from the University of East Anglia included this from Professor Phil Jones, referring to two papers that apparently falsified his work:- "I can't see either of these papers being in the next IPCC report. Kevin and I will keep them out somehow - even if we have to redefine what the peer-review literature is!" So what? Climategate tells us no more than the philosophers of science have long told us about research, and the public should be less naïve.

Notes and References

1. Mann ME, Bradley RS, Hughes MK, 1999, Northern Hemisphere Temperatures During the Past Millennium *Geophysical Research Letters* 26: 759–762

2. It should be noted that falsification and falsifiability are different. As Popper proposed, a statement cannot be seen as scientific unless it is falsifiable and can thus be tested by the scientific method. So the statement that the moon is made of green cheese is a scientific one, because it can be tested and falsified. But the fact that none of the moon missions to date has found green cheese does not falsify the hypothesis because not every part of the moon has yet been explored.

3. Birkhead T, 2009, Whose Data is it Anyway? *Times Higher Education* 1,901, 27.

4. Kealey T, 2008, *Sex, Science and Profits* William Heinemann.