Whom do we Trust? Authority, Authenticity and the History of Science

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This conference takes as its starting point the concept of crisis. It may be important to point out that not everybody is in crisis. The experience of crisis – whether personal, political, ecological or economic – is not necessarily universal. But it is common. And it is important. At different levels, I think it is fair to say that each one of us here today has faced crises of various types and of different levels of seriousness and extent, and we shall do so again.

Near the heart of many forms of crisis, especially those with a sociological dimension, is the question to which I am addressing my remarks today, the question of trust. In different ways, many problems can be understood as crises in trust and trustworthiness. Do you trust your political representatives to make the best decisions on behalf of your country? Do you trust your bank manager

to place your private interests as a customer over those of the bank? Do you trust your children to behave well when you are not monitoring them? Do you trust the internet, or Wikipedia? Do you trust yourself?

Questions of this type have been the subject of much valuable research and reflection, and have generated a large and sophisticated literature, only some of which I am familiar with.[1]

It became common in the second half of the twentieth century to use the metaphor of a "revolution" to describe the development of scientific knowledge since the seventeenth century. In the seventeenth century and afterwards, many thinkers did believe that they were bringing about a new view of the world, especially with the development of more powerful instruments of observation, critically disjunctive experimental techniques, and the decisive shift from the acceptance of authority as a source of definitive knowledge towards the idea of constructing knowledge on the basis of sense perception, of reading God's Book of Nature.[2] We may remember that the motto of the Royal Society of London (1662–) was, and is, "Nullius in verba," "Take no man's word." This was a fundamental reversal of the direction of trust and authority.

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1. Hawley (*Trust: A Very Short Introduction*) provides an accessible introduction to the wider literature, and Cook (*Trust*) gives a comprehensive recent bibliographical guide to the subject. Amongst major contributions to the topic are Cook (*Trust in Society*), Fukuyama (*Trust: Social Virtues and the Creation of Prosperity*), Gambetta (*Trust: Making and Breaking Cooperative Relations*), Luhmann (*Trust and Power: Two Works*), and Sztompka (*Trust: A Sociological Theory*) and, as always, Giddens (*The Consequences of Modernity*, esp. 26–36). A sign of the growing maturity of the discipline was the founding in 2011 of the Journal of Trust Research.

2. Shapin, *The Scientific Revolution*, p. 74; on the Book of Nature, introduced by Galileo as an alternative source of knowledge, see, e.g., Berkel and Vanderjagt (*The Book of Nature in Early Modern and Modern History*), Harrison ("'The Book of Nature' and Early Modern Science"), and Vanderjagt and Berkel (*The Book of Nature in Antiquity and the Middle Ages*).

Before the seventeenth century, "verbum dei" was the highest possible court of authority. Authority and truth flowed from God, and the world was a poor illusion. But from the great turning point in 1610 when Thomas Harriot and Galileo Galilei independently observed sunspots, imperfections in God's perfect solar symbol, the conflict between scriptural authority and observation was set.[3]

Nevertheless, conceptualising these changes with the expression "the scientific revolution" was not common before Alexandre Koyré began using the term in 1939.[4] And the historiographical idea that a cognitive revolution took place in the seventeenth century is a historical construction that owes less to any social or epistemological reality than to twentieth-century writers like Herbert Butterfield, the Marxist J. D. Bernal's 1954 volume *The Scientific and Industrial Revolutions*, and the title of Kuhn's famous book *The Structure of Scientific Revolutions*.[5] As I. Bernard Cohen pointed out in 1985, "in the last thirty years or so the literature of the history of science has become saturated with references to revolutions in science and to the Scientific Revolution".[6] And when Shapin came to write his brilliant short history of the scientific revolution almost twenty years ago, he could begin with the words "There was no such thing as the Scientific Revolution, and this is a book about it".[7]

So what language and concepts have replaced the rather crude idea of revolution in science? Well, a range of ideas from social and cultural history have been fruitfully brought to bear on the history of science in the last few decades. Two

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3. Drake, "Galilei, Galileo"; Lohne, "Harriot, Thomas".
4. Cohen, *Revolution in Science*, pp. 396–8, Shapin, *The Scientific Revolution*, p. 2.
5. Cohen, *Revolution in Science*, ch. 26.
6. Ibid., p. 396.
7. Shapin, *The Scientific Revolution*, p. 1.

concepts that I particularly like were articulated by Randall Collins in his *Social History of Philosophies*. The first is the concept of rapid-discovery science. Collins argued that it was not the emergence of new scientific discoveries that made the seventeenth and eighteenth centuries in Europe special. New discoveries had been made continuously since antiquity. Rather, what changed was that investigators *discovered how to discover*.[8] It was a second-order phenomenon. The founding of learned societies and the distribution of journals was central to this process. So was the spread from laboratory to laboratory of technical innovation, what Collins called "technologizing the research front." The second unique concept he developed is that European science evolved a higher degree of consensus. Of course controversies persisted, and became essential to the growth of knowledge. But to a greater degree than ever before there emerged a shared process by which, over a period of years, previously intractable problems in natural philosophy could actually be solved. In earlier periods and in other cultural areas, such as South Asia, natural philosophers had a high tolerance for inconsistency, preferring to develop schools of dogmatic opposition. This was inevitable as long scientific enquiry was dominated by metaphysical questions for which the very means of solution were not known. As scientific instrumentation evolved, focus shifted to questions for which the means of answering were knowable, if not always achievable.[9]

But from the seventeenth century in Europe, investigators began to believe that results – often practical, laboratory-based results – could be refined, that everyone everywhere could expect the same results, and that problems were susceptible of resolution. Natural philosophy was becoming disembedded, both geo-

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8. Collins, *The Sociology of Philosophies*, pp. 532–43.

9. I have in mind here Isaiah Berlin's famous "three baskets" (Berlin, "The Purpose of Philosophy").

graphically and epistemologically. Trans-national scientific consensus started to be a feature of the age.[10]

And with these widening circles of communication, and the increasing diversification of scientific activity, trust became increasingly essential for cooperation and progress. In the words of the philosopher of science, Michael Polanyi (1891–1976),

> The amount of knowledge which we can justify from evidence directly available to us can never be large. The overwhelming proportion of our factual beliefs continue therefore to be held at second hand through trusting others, and in the great majority of cases our trust is placed in the authority of comparatively few people of widely acknowledged standing.[11]

In Europe, in the seventeenth and eighteenth centuries, trust emerged as a key feature of the new academy of natural science.

Examples of distrust

Let us now think about some concrete case-histories that illustrate distrust.

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10. There is much more to Collins' argument than this thumbnail sketch suggests. In particular, he posits three distinct but overlapping "revolutions" in this period: the maths revolution, the science revolution, the philosophical revolution (Collins, *The Sociology of Philosophies*, 556 *et passim*).

11. Polanyi, *Personal Knowledge*, p. 221.

An Australian Platypus

In 1797, Captain John Hunter, the Governor of New South Wales in Australia, watched a man spear a strange animal in a lagoon. He drew sketches and sent a pelt of the strange creature to England in 1798.[12]

The following year, George Shaw, Keeper of the Department of Natural History at the British Museum, published a description in his multi-volume scientific journal, *Naturalist's Miscellany*. He noted that,

> …it seems the most extraordinary in its conformation; exhibiting the perfect resemblance of the beak of a Duck engrafted on the head of a quadruped. So accurate is the similitude that, at first view, it naturally excites the idea of some deceptive preparation by artificial means.[13]

"It was impossible," Shaw wrote a few years later in 1802,

> not to entertain some distant doubts as to the genuine nature of the animal, and to surmise, that, though in appearance perfectly natural, there might still have been practised some arts of deception in its structure. I therefore hesitated as to admitting it into the present History of Quadrupeds.[14]

Shaw was not alone in doubting that the specimen was genuine.

A scientific description of the platypus by Everard Home by was published in the *Philosophical Transactions* the same year.[15] Home was reassured that the

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12. David Collins described the discovery in the second volume of *An Account of the English Colony in New South Wales*, published in 1802, pp. 62 and 321–28, cited by Cowley and Hubber ("Distinct Creation. Early European Images of Australian Animals", pp. 17–21).
13. Shaw and Nodder, "The Duck-Billed Platypus".
14. Shaw, "Platypus", p. 21.
15. Home, "Description of the Anatomy of the 'Ornithorhynchus paradoxus'".

platypus was not a hoax because he had been told in person by John Hunter, that the latter had witnessed them himself. He described his encounter:

> Governor Hunter, who has lately returned from New South Wales, where he had opportunities of seeing them alive, has favoured me with the following particulars respecting them.[16]

Here we see that the establishment of scientific trust depended on reputation and personal encounter. It required two men to meet and face each other, for a scientific specimen to be accepted as genuine.

Twenty years later, the controversial but learned anatomist Robert Knox explained that,

> It is well known that the specimens of this extraordinary animal first brought to Europe were considered by many as impositions. They reached England by vessels which had navigated the Indian seas, a circumstance in itself sufficient to rouse the suspicions of the scientific naturalist, aware of the monstrous impostures which the artful Chinese had so frequently practised on European adventurers; in short, the scientific felt inclined to class this rare production of nature with eastern mermaids and other works of art; but these conjectures were immediately dispelled by an appeal to anatomy.[17]

Knox was himself the author of several pioneering anatomical studies on the anatomy of the platypus, so it is natural that he would assert that it was the

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16. Ibid., p. 2.

17. Knox, "Observations on the Anatomy of the Duck-billed Animal of New South Wales, the Ornithorynchus paradoxus of Naturalists. Memoir I", p. 27. For more on the history of the platypus, see Gould, "To Be a Platypus"; Moyal, *Platypus*; Ritvo, *The Platypus and the Mermaid and Other Figments of the Classifying Imagination*.

anatomy that convinced the world of the creature's reality. However, as we saw, it was in fact the meeting between Home and Hunter, published in the *Philosophical Transactions*, that had been the decisive turning-point.

Walking on Ice

Knox's remarks raise the spectre of European prejudices concerning "wily" orientals. However, distrust worked in both directions. A hundred years earlier, the philosopher John Locke (1632–1704) illustrated a case of scientific distrust directed from Asia to Europe. He said,

> As it happened to a Dutch ambassador, who entertaining the King of Siam with the particularities of Holland, which he was inquisitive after, amongst other things, told him that the water in his country would sometimes, in cold weather, be so hard, that men walked upon it, and that it would bear an elephant, if he were there. To which the king replied, "hitherto, I have believed the strange things you have told me, because I look upon you as a sober fair man; but now I am sure you lie."[18]

In this case, authoritative personal contact was present, but still did not suffice to carry conviction, and we must ask why it did not. The king of Siam said that he would believe wonders, but not beyond a certain point. Perhaps the King felt that the ambassador had something to gain? More likely, the King did not participate in networks of mutual scientific exchange.

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18. Locke, *An Essay Concerning Human Understanding*, p. 502 (first published in 1689) discussed by Shapin, *A Social History of Truth*, 229 *et passim*. The episode may be illustrative and apocryphal rather than historical.

Epistemic dependency

It is after this time of increasing networking, collaboration and specialization that it begins to be felt that the comprehension of the content of scientific knowledge is exceeding the capability of a single person. Scholars begin to depend upon each other in a new way, and to consult each other for specialist knowledge that they themselves do not have the resources to develop. They begin to trust each other.

This feature of epistemic co-dependency has been remarked on often by historians of science as well as scientists themselves. The mathematician Erik Christopher Zeeman pointed out nearly thirty years ago that,

> The scientist has to take 95 per cent of his subject on trust. He has to because he can't possibly do all the experiments, therefore he has to take on trust the experiments all his colleagues and predecessors have done.[19]

To quote Michael Polanyi once again,

> Nobody knows more than a tiny fragment of science well enough to judge its validity and value at first hand. For the rest he has to rely on views accepted at second hand on the authority of a community of people accredited as scientists. But this accrediting depends in its turn on a complex organization. For each member of the community can judge at first hand only a small number of his fellow members, and yet eventually each is accredited by all. What happens is that each recognizes as scientists a number of others by whom he is recognized as such in return, and these relations form chains which transmit these mutual recognitions

19. Zeeman, "Private Games", p. 61.

> at second hand through the whole community. This is how each member becomes directly or indirectly accredited by all. The system extends into the past. Its members recognize the same set of persons as their masters and derive from this allegiance a common tradition, of which each carries on a particular strand.[20]

As was wittily observed by Ludmilla Jordanova in her brilliant short study of historiography, *History in Practice*, a historian can be defined as someone whom other historians agree is one of them,[21] and the same can be said of scientists.

Belief and Acceptance

What can we learn about belief and acceptance?

Robert Boyle

In his brilliant 1994 book *A Social History of Truth*,[22] Steven Shapin introduced us to the world of the eighteenth-century Irish scientist Robert Boyle (1627–1691) and placed Boyle at the centre of a discussion about the social mechanisms of scientific belief.

Boyle's early work was conducted in the alchemical tradition, but today he is often thought of as the first truly modern chemist, because he pushed his researches beyond these alchemical beginnings and established several foundational insights into the nature of the physical world. He is particularly famous

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20. Polanyi, *Personal Knowledge*, p. 173.
21. Paraphrase of Jordanova, *History in Practice*, p. 2.
22. To which I owe much of the inspiration of the present essay.

for his work on the pressure of gases, and on basing his chemical thought on the principle that matter consisted of atoms and clusters of atoms in motion and that every phenomenon was the result of collisions of particles in motion.

This theory was laid out in his most famous book, *The Sceptical Chymist* (1661). The title of this book already announced Boyle's intellectual programme: the application of scepticism, or what we might today understand as rational empiricism.[23] Boyle's subtitle reveals more of his research programme and self-image: *Chymico-Physical Doubts & Paradoxes*. Boyle returns several times in his writing to the idea that he is not sure of himself, that he worked in an atmosphere of uncertainty and exploration through observation.

> … I blush not to acknowledge that I much lesse scruple to confess that I doubt, when I do so, then to profess that I know what I do not.[24]

He would much rather admit his doubts, than claim to know things that he does not know.

And yet, Boyle's work was enormously influential. Why did Boyle's contemporaries believe him?

Shapin develops the argument that Boyle and other early scientists were mapped into a social network that was defined by an explicit and shared code of honour. They were gentlemen, not in a vague sense, but in a clear and mutually understood definition. What this meant was that they participated – in their own eyes and in the eyes of their peers – in a set of values that guaranteed that they would not lie. At the heart of the concept of seventeenth-century gentlemanliness was a concept that there was a consistency between

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23. Boyle, *The Sceptical Chymist or Chymico-Physical Doubts & Paradoxes*.

24. Ibid., pp. 1–2.

word and deed, word and reality, that generated trust and made collective social order possible.[25] Boyle's experiments and arguments were persuasive and acceptable to his peers because to lie or cheat would have destroyed him socially, would have deprived him of a sense of self that was essential to his personal identity.

There is another feature central to the seventeenth-century idea of the gentleman. He was a person of independent means. The gentleman had no need to be employed, and was therefore subject to no ties of obligation. Being financially independent, the gentleman was not subject to instruction, not subject to bias through employment. As an independent agent, therefore, his scientific findings were not subject to distortion for financial reasons. Financial freedom was understood to be a guarantee of truth.[26]

When Richard Brathwait laid a blueprint of the English Gentleman in his seventeenth century book of that title he made clear that a gentleman need not necessarily be rich, but that in the absence of wealth, cultivated habits of moderation made him free from neediness.

> Though heire of no great fortunes, yet his extensive hand will not shew it. Hee shapes his coat to his cloth; and scornes as much to bee holden, as to be a Gally-slave. …Learning hee holds not onely an additament, but ornament to Gentry. No complement gives more accomplishment.[27]

It is an elementary observation that any person or organisation that is dedicated to two or more separate goals will be likely to encounter situations when those goals are in conflict. In particular, a scientist who is dedicated to discovering

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25. Shapin, *A Social History of Truth*, p. 68.
26. Shapin, *A Social History of Truth*, ch. 2.
27. Brathwait, *The English Gentleman and the English Gentlewoman*, p. 255.

knowledge, but who also needs money, may arrive at a point when a choice has to be made between profit and scientific integrity. In different ways, we have seen this tension played out repeatedly right up to the present day.

Some of the most striking examples of such conflict can be drawn from the many scandals that have embroiled the pharmaceutical industry since the wide-scale industrialization of medicine at the start of the twentieth century.

The drug scandal surrounding Ciba-Geigy's drug Clioquinol in the sixties and seventies, for example, has been called "one of history's most horrifying cases of corporate negligence".[28] It revealed processes at work within Modern Establishment Medical practice that run counter to the vision of MEM as a purely rational and science-based process. It took eight years from the clinical demonstration that Clioquinol caused subacute mylo-optic neuropathy for Ciba-Geigy (today part of Novartis) to withdraw the drug. Even then, the company was not acting on the scientific evidence, which it had already been aware of for a long time, but because of an international campaign against the drug by its victims and their doctors. The mechanisms for self-scrutiny and self regulation within the modern medical establishment are imperfect, with tragic consequences for many patients.[29]

Alan Monheit recently reopened the question of trust in the context of several new medical scandals. For example, in September 2010, health care giant Johnson & Johnson announced that it was recalling hip replacement implants produced by its orthopedics unit. In doing so, the company was responding to two

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28. http://www.multinationalmonitor.org/hyper/issues/1993/04/mm0493_11.html,consulted 02-03-2014

29. Wujastyk, "Medical Error and Medical Truth", p. 224, citing Shiva, "Reductionist Science as Epistemological Violence", pp. 251–3.

years of consumer complaints that had led to additional hip replacement surgeries in a number of cases. The recall followed a series of other product withdrawals by Johnson & Johnson as well as concerns raised by the Food and Drug Administration regarding the com- pany's marketing of unapproved medical devices.[30] Monheit noted that,

> In an era when product recalls seem to be the rule rather than the exception, one might simply consider Johnson & Johnson's transgressions as just another instance of poor quality control and lax management and oversight. … However, in early 2013, it was revealed that the hip replacement recall was hardly an unfortunate and innocent occurrence, but instead a more pernicious mishap: the release of a product that executives knew was very likely to fail. Press reports revealed that internal analyses by the company projected a 40% failure rate over a five-year period and, to make matters worse, company officials dismissed similar findings from analyses by a British implant registry. This disturbing incident has led to the filing of more than 100,000 lawsuits; as of this March [2013] writing, the first plaintiff was awarded $8.3 million, with subsequent judgments expected to cost the company billions of dollars. Unfortunately, Johnson & Johnson's transgression involving harm from a medical product was not an isolated case that could have been averted.[31]

Major abrogations of public trust like the above are, regrettably, not uncommon. Initially, it is tempting to respond with anger at the unethical behaviour of the company executives behind these scandals, and to view these cases as

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30. Monheit, "A Matter of Trust".

31. Ibid., p. 3.

singular failures. But I would argue that the very structure of modern pharmaceutical corporations makes such catastrophes inevitable. A pharmaceutical company is dedicated to two goals: human well-being and shareholder profit. While periods of time may elapse when conflict does not seem to arise, it is bound to do so eventually, it is inherent in the structure of the corporate entity.

Conclusions

By the middle of the eighteenth century, the strength of the chivalric idea was already under challenge. David Hume asserted, in 1748, while discussing miracles, that,

> … there is not to be found, in all History, any Miracle attested by a sufficient Number of Men, of such unquestion'd Good-sense, Education and Learning as to secure us against all Delusion in themselves; of such undoubted Integrity, as to place them beyond all Suspicion of any Design to deceive others;[32]

In other words, any man may lie, and human testimony is always open to suspicion. Today, this seems as self-evident to us as it was self-evident to Boyle that a gentleman was incapable of lying.

The character of modern trust is what Luhmann has called "system trust".[33] In Giddens' terminology, we rely on systems of trust that are disembedded, that are no longer linked to local networks of people we know personally. We

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32. Hume, *Philosophical Essays Concerning Human Understanding*, p. 183, discussed by Shapin, *A Social History of Truth*, p. 411.

33. Luhmann, *Trust and Power: Two Works*, pp. 1–22, cited by Shapin, *A Social History of Truth*, p. 412.

rely on systems of professional qualification, on professional scrutiny. To cite Shapin, it would appear that,

> Objective knowledge is not now thought to be underwritten by the participation of "gentlemen, free and unconfine'd," but by institutions which most vigilantly constrain the free action of their members.[34]

And yet, underneath the appearance of institutional rigour, Shapin shows compellingly, just as Polanyi did,[35] how in actual fact investigators in modern science are often dependent on trust-relationships, and often work in small core-groups made up of individuals who know and trust each other personally.

There is a major turn taking place amongst historians of science towards showing how trust is structurally implicated in every single human transaction, whether medical, scientific or national. In the words of David Turnbull,

> A vast preponderance of our knowledge derives not from personal experience but from books, newspapers, journals, teachers, and experts. In other words our knowledge comes, directly or indirectly, from the testimony of others, in particular from those we trust. Thus our individual lived rationality is based in a range of social practices, traditions and moralities that are suppressed and concealed in the portrayal of rationality as an ahistorical, universalistic form of reasoning exemplified by science.[36]

Exciting new research into placebos in medicine,[37] for example, and new insights into the philosophy of medicine and the inadequacy of outdated posit-

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34. Shapin, *A Social History of Truth*, p. 413.
35. Polanyi, *Personal Knowledge*.
36. Turnbull, "Rationality, Objectivity, and Method", p. 847.
37. Moerman, *Meaning, Medicine and the 'Placebo Effect'*.

ivist dogmas concerning the nature of science[38] are offering fresh ways to understand the scientific world in which we live today, ways of understanding that place trust at the centre of the human experience.

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38. Kriel, *Matter, Mind, and Medicine*.

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