ON THE HISTORICAL ROOTS OF SCIENTIFIC REASON

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The first of two lectures to be given in the “Experience and Truth” conference, Taiwan, November 2007, and also incorporated into the “Humanities Chair” of the Institute for Advanced Studies in Humanities and Social Sciences, National Taiwan University.

The second lecture will be “Where do mathematical objects come from?”

This talk and its successor are intended as contributions to very traditional issues: truth, reason, knowledge and the sciences. These are ancient topics, although my spiritual ancestor is more Leibniz, and hence by indirection Plato, than it is Aristotle, the honorary past president of all studies such as my own. What we add, in our historicist times, is that we are aware that nothing is eternally fixed, unless it is purely formal. Everything evolves; most things decay. Even the constants of nature, such as Planck’s constant and the speed of light, may not be quite so constant as they are taken to be in simple down-home physics.¹ Many thinkers who are not analytic philosophers take it to be obvious, that even truth has a history. I shall say the opposite, yet my colleagues, the analytic philosophers, may fear that I set my sails to much in that direction. Caution is called for.

My topics are entirely impersonal, but I shall take the liberty of making the occasional personal remark. People keep on asking me how I can identify as an analytic philosopher and yet be so comfortable in making use of the past in order to understand the present. An occasional aside will suggest answers to that question.

My approach is at once radical and simple. I shall keep on coming back to that. On the one hand I rely on truisms. On the other hand I make them yield conclusions about reasoning that are rather radical, so radical that sometimes I shall have to say, yes, I really do mean what I am saying.

Learning how to think

My fundamental observation is that reasoning, finding out, and techniques of discovery have a history. It is not just the history of facts discovered, theories proposed, and technologies invented. We had to learn how to find out about the world.

There are two sides to that. We had to bring to the surface various kinds of innate ability that human beings may have had forever, but whose exercise does not come naturally. And we had to evolve social organizations within which those abilities could be fostered. On the one side, then, I wish to draw in cognitive science, the study of mental capacities. On the other side, I turn to the history of civilizations and of their institutions.

We have many different cognitive abilities, and human history runs on many paths. Not surprisingly, there are many ways to conduct scientific research. Mathematicians construct deductive proofs (among other things). The laboratory sciences demand not just ‘experiment’, but the building of apparatus to elicit, and often to create, phenomena. They also need theoretical modelling both to guess what to do, and to make sense of what they have done. Taxonomists classify living things according to

evolving principles. These are some of the many ways of finding out, practiced in what we call the sciences. They have histories, which can be told in innumerable ways.

I like to tell the story of each style of thinking as having a sharp beginning, a fixing of how to go on, usually after centuries, perhaps millennia, of inchoate precursors. I acquired this habit early, in The Emergence of Probability, first published in 1975. It was an archaeology of knowledge, after Michel Foucault, the first one to be written in English, and the first in any language to deploy an analytic idiom. I explain the connection to intellectual archaeology in the new Introduction to the 2006 edition of the book. ²

I argued in 1975 that anything we can readily recognize as probable reasoning began around 1650. New kinds of statements began to be made by people all over Europe and we began to enter a new world, a world of chance, the world we inhabit today, of which Ulrich Beck’s ‘risk society’ is only one among many prominences. Yes, you can find anticipations of things that were said after 1650 in many places, times, and climates. In the original book I mentioned a striking mastery of probability-type reasoning in a Sanskrit classic over two millennia ago. There must be parallel examples in China to which no one has yet drawn attention. For thirty years scholars have produced examples of anticipations of probability in order to refute my claim. Nevertheless, as I explain in my new Introduction to Emergence, I stick to my claim. Many writers have found rich elements of other systems of thought, in which we can recognize hesitant precursors of our own. Only after 1650 did human beings begin to put the possible pieces together, and see that confidence in diverse opinions and the frequencies with which things happen have the same underlying structure.

I shall not dwell on probability for more than another moment – I have devoted too much of my life to it. In a parody “Philosophical Dictionary” composed by Dan Dennett a long time ago, the verb “to hack” is defined as obsessive attention to detail, with this example, “he spent years hacking his way through the statistical jungle.” But for just a moment let me hack one more time. Lots of truths about probability are not temporal at all: they are mathematical theorems of the probability calculus. Some other statements using probability may be true at all times and places, say facts about the half-life of radium. Many more are tensed, true or false at a time. But, to exaggerate in order to display the point, no such statements, of any of the 3 sorts, could be made before 1650. There was no way to assert those truths; no conditions for their truth or verification were in place. Methods of reasoning about them had not yet come into being. There was, as I shall put it later, no truthfulness about what we call probability, until my arbitrary date of 1650.

Crombie’s styles of scientific thinking

Those remarks exaggerate brusque change. To compensate, I put this enthusiasm for mutations in systems of thought, inherited from Gaston Bachelard, together with a great respect for continuity. I got the idea of a small manifold of distinct styles of scientific thinking from the historian of science A. C. Crombie. I encountered his ideas at a conference in 1979 in Pisa, and have never looked back. He thought in traditional terms of the more or less continual evolution and growth of methods of scientific reasoning, all the way back to ancient times. (One of his six methods is what we call probability and statistics.) I regard his as a different perspective, a different emphasis from mine. I feel no sense of incompatibility. He wanted to organize a global history of science on an encyclopaedic scale. He had

an ambition to produce a historical anthropology of European science. That is a valuable phrase that I suppose he took from Jean-Pierre Vernant. I do not really agree with his project, but it launched my own.

In 1994 he finally produced his life work, the 3-volume *Styles of Scientific Thinking in the European Tradition: The history of argument and explanation especially in the mathematical and biomedical sciences and arts*. These are three curious and obsessive tomes that no one else has made much use of, but which perhaps unwittingly conveyed to me a new vision of truth and reason. Crombie spoke of exactly six styles of scientific thinking in the European tradition, each developing according to its own trajectory and time scale. I have no commitment to his precise taxonomy of six, except as a plausible initial sketch. Here I shall use brief tags for each of his styles: mathematical, hypothetical modelling, experimental, statistical, taxonomic, and historical-genetic. None of these 6 styles defines a modern scientific discipline. Styles (in his sense) are not sciences.

Most of the sciences use most of the six styles of thinking. Take an extreme example. Taxonomic reasoning must seem wholly removed from mathematics – until you reflect that some of the most profound theorems are about classification, say the exhaustive classification of the finite groups. And such theorems – to placate those who fear my overly abrupt vision of the sciences – go back to the five regular solids that so impressed Plato and his heirs. Conversely, taxonomists construct phylogenetic trees based on fossil and now molecular-genetic evidence. A standard tool of analysis is the method of maximum likelihood, developed in applied statistics and using fairly elementary yet deep mathematical principles.

I am engaged in turning Crombie’s odd history into an even odder philosophy. There is a long sentence of Crombie’s that I treat as a sort of pivot for making the turn:

> We can establish in the classic scientific movement a taxonomy of six styles of scientific thinking, distinguished by their objects and by their methods of reasoning.

The words here, with which I do the turning, are objects and methods of reasoning. They are, in themselves, anodyne. The objects with which the mathematical style concerns itself are often called, by analytic philosophers, abstract objects, such as numbers and groups. The objects with which the taxonomic style concerns itself are, for example, the species and genera of systematic biology, not mere classifications of living things, which are found in all languages, but objects bearing a definite role of sub- and super-ordination to other objects of the same sort.

I much regret using the word ‘style’ at all, for it has acquired ever so many connotations since Heinrich Wölfflin introduced it into art history early in the 20th century. There was a great vogue for the word in Germany, including its compound *Denkstil*. There we think of an enormously important book, published in 1935, by Ludwik Fleck, translated as *Genesis and Development of a Scientific Fact*, a title that omits the German subtitle, *Introduction to the theory of the thought-style and the thought-collective*. Fleck’s idea of a Denkstil differs profoundly from Crombie’s notion of a style of thinking. Fleck is concerned with a way of thinking, a way of finding out that is current in a specific community, a thought-collective, at a definite time, and which, evolves, mutates, or dies in a brief span of time. The thought-collective is a good name for what Kuhn less aptly named a “disciplinary matrix,” a body of

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core workers and their appendages in some field of inquiry. Crombie’s styles of scientific thinking are very much a matter of the longue durée, evolving and mutating yes, but persisting over a long period of time and being used to varying degrees in all of the scientific disciplines and more.

One reason, for disquiet about the word “style,” is, then, that it already has an entrenched usage in wok that I respect enormously. A second is that the word is used widely in many contexts, and even in the sciences it is altogether natural to contrast the style of one famous researcher to that of another. A whole series of papers to that effect were published after I first wrote about styles of reasoning, but they were not talking about what I meant by styles. Arnold Davidson appropriated my “styles of reasoning” in his important study of perversion, but meant something wholly different from what I meant. Recently Geoffrey Lloyd has been using “style of inquiry” as a fundamental analytical tool in his persuasive comparative studies of ancient Greek and Chinese science. “Style” seems as worn out and almost dead duck. Nevertheless, after much word-searching and soul-searching, I feel stuck with Crombie’s expression, and will speak of long-standing styles of scientific thinking in the European tradition.

**Self-authenticating styles**

So far, so simple. Now I forewarn of the radical. I pass from the historical to the metaphysical, from historical anthropology to philosophical anthropology, and from bland description to an attempt to undo ontological debates.

I contend that every style of scientific thinking introduces new objects, and new criteria for the truth of falsehood of statements about those objects. A style, with its specific methods of reasoning, does not answer to any criteria except its own. It is not good because it helps find out the truth in some domain. It itself defines the criteria for truth-telling in its domain. Thus in a certain sense each style is autonomous and “self-authenticating”. That certainly sounds radical, and that is what I mean.

Few such surprising doctrines are all that new. In the heyday of logical positivism, Moritz Schlick coined the slogan, “the meaning of a statement is its method of verification.” That was quickly revised and then abandoned, but has more merits than are usually assigned to it. Schlick meant a method of verification appropriate to an individual statement, rather than very general kinds of method. Without embracing his strict verificationism, we assert that until there are methods of reasoning that bear on the truth or falsehood of a statement, the question of its truth or falsehood does not arise. Meaning, as simplistic forms of analytic philosophy have insisted, demands the possibility of applying truth conditions. Taking advantage of Bernard Williams’ “truthfulness” below, it can be said that statements come up for grabs, as true or false, only when there comes into play a method for reasoning about their truth.

Most ordinary statements are not like this. They have truth conditions, methods of verification and so forth, independently of styles of scientific thinking. The cat is on the mat. Likewise most objects are not introduced into discourse alongside styles of scientific thinking. Sticks and stones.

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Notice that I do not even want to suggest for example that statements about dinosaurs in the distant past were false in that distant past, just because we could not assert them. That would be absurd. The statement, “Dinosaurs roamed the earth during the Jurassic epoch” is true, but it makes no sense to say that it is true now, or was true a million years ago. It is a statement that English grammar – I shall speak just for my own first language – throws into the past tense, but it is not true or false at a time at all. Likewise I do not mean that the Pythagorean Theorem became true – or perhaps became necessarily true – only when geometry came into being as a body of knowledge. The Pythagorean theorem is untensed, it is not true or false at a time. It may be helpful to say here that the statement about dinosaurs was not up for grabs as true or false in the Jurassic. It was not a possible thing to say, in the Jurassic period, because nobody was saying anything, then.

**Society**

A style of scientific thinking answers to no criteria but its own. This does not mean that it needs no support, as if it were some sort of free-standing inhabitant of what Popper called the Third World, a reincarnation of the headier postulates of Plato and Frege. It is people who think. People reason. People find out. People need to eat, and even if they are rich, they need people to listen to them. Styles are enabled by institutions. Lloyd’s comparative study of ancient science, mentioned earlier, attends closely to the institutional settings, in Greece and China, that allowed various “styles of inquiry” to flourish in one or the other society.

Styles of scientific thinking in the European tradition are just as much social practices as Fleck’s *Denksstile*. They are less local and more enduring in part because they are built on fundamental cognitive capacities. If they become extinct it will not be by refutation, but by being abandoned.

Barry Allen has argued that the witchcraft trials and inquisition of the early modern period employ, are just as much a style of scientific thinking as any of Crombie’s canonical six. I do not know enough about the realities of witch-detection, and hence plead no contest. An ironist might propose a witch-detection module, to go along with the cheater-detection module much favoured in evolutionary psychology. We stopped using the tests for detecting witchcraft, not because they failed to detect witches – they seem to have been unduly good at it – but because the entire enterprise became socially insupportable.

The Paracelsan doctrine of similitudes may be a style of scientific thinking that met the same fate. Wisps of it remain in homeopathic medicine. Did Freud introduce a new style of scientific thinking? Psychoanalysis and Marxist historical determinism both deploy Crombie’s historico-genetic style. This, his sixth style, may be verging towards extinction. There is, however, a complex story of the historicising of natural history, and its taxonomy, via natural selection, and then that being transformed into molecular genetics.

Herman Hesse’s *The Glass Bead Game* is a parable for a society built around a style of scientific thinking, wholly viable, maintained by complex institutions. From our point of view, the system is just human enough to be fascinating, and yet is absurd. Nowadays we pay pure mathematicians and string theorists out of the public purse.

Here are a few sentences from a source that you would not expect, J. M. Coetzee’s Tanner Lecture on Human Values. Coetzee presents a lecture about animals by the Australian novelist, Elizabeth Costello. She in turn supplements a lecture imagined by Kafka in 1917. It is by Red Peter, an ape who has learned to talk. We read that “seen from outside, from a being who is alien to it, reason is simply a vast tautology. Of course reason will validate reason as the first principle of the universe –
what else should it do. Dethrone itself? Reasoning systems, as systems of totality, do not have that power."

From time to time there is, in Richard Bernstein’s phrase, a rage against reason. One such time was the late 1960s, when science was seen as a tool of capitalist colonial war. Young American rebels tried to sack places of research. That tactic was foolish but right. If you want to criticize a style of thinking, you cannot do it on its own terms (for then you are merely thinking according to that style, trying to do it better). You must destroy its institutional base. This was of course the underlying thesis of the Cultural Revolution in the People's Republic of China.

Objects

There are parallel observations about objects. Think of the abstract mathematical objects (‘Platonist’), of the unobservable theoretical entities of physics, or of systematic biology with its taxa. Each style is specific to it its own domain, but only because it introduces the objects peculiar to that domain. It does not create them – to say that would be foolish – but they have no place in thought outside the styles.

As a corollary, the new classes of objects make possible the interminable ontological debates in those domains, for example between Platonism and nominalism in mathematics, or scientific realism versus the various types of instrumentalism and empiricism. Ontological debates within the sciences result from (a) the introduction of objects by styles of thinking (b) the fact that we talk about these objects using sentences in which names for the objects serve as grammatical subjects, and (c), as already emphasized by Nietzsche, most languages demand an existential presupposition for terms in the subject position. This is certainly true of European languages, so it is not surprising that these ontological debates are above all European in nature.

Just as most common statements are independent of scientific styles of thinking, so the most deep-seated ontological debates have nothing to do with styles. Is there an external world? Are there other minds but mine? Or (a vexed problem in various guises for philosophers of logic) are there universals, classes, properties, or do only individuals exist? Ontological issues all, but they have nothing to do with styles of scientific thinking.

I hold these doctrines, about statements, objects, and styles of scientific thinking, to be profoundly rationalist in character, very much in the spirit of my mentor Leibniz. Far from implying some sort of relativism, the doctrine that styles of thinking are self-authenticating is part of an explanation of what we call objectivity. As Lorraine Daston has done so much to show, the epistemological concept of objectivity has a chequered past, protean and polysemic. When I speak of objectivity I mean chiefly to affirm that the truths discovered in the sciences are simply true, independent of what we think, or of how we discover them. That is wholly consistent with saying that their truth conditions are products of the styles of thinking in whose domain they fall.

Leibniz and Bourdieu

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Allow me a comparison here, another one that you may not expect. Pierre Bourdieu’s *Pascalian Meditations* of 1997 was, in effect, his philosophical testament.\(^\text{10}\) He too was much concerned with the objectivity of the sciences, including of course his own sociology. In my opinion he was unduly hostile to most social studies of science, whereas I learn much from the best practitioners of science studies, even when some of them think I am an old stick-in-the-mud who has learned nothing.

Bourdieu was deeply committed to a rationalist vision – it is too often forgotten that his early work is both about Leibniz, and that it is truly Leibnizian. Since he was a significant man of our times, he was historicist too. In the chapter called ‘The Historicity of Reason’ in his *Meditations*, he wrote that “We have to acknowledge that reason did not fall from heaven as a mysterious and forever inexplicable gift, and that is therefore historical through and through; but we are not forced to conclude, as is often supposed, that it is reducible to history.” He went on to insist that, “It is in history, and only in history, that we must seek the principle of the relative independence of reason with respect to history.” Moreover – and here I paraphrase an overly French sentence – he thought that “the singular history of reason takes place” in “completely specific,” “strictly historical,” but entirely exceptional conditions.\(^\text{11}\) I could present my use of styles of scientific thinking as a long gloss of that passage of Bourdieu’s. For it is in completely specific conditions that new styles of scientific thinking come into being and flourish.

This kind of historicism, which I cheerfully call Leibnizian, is very close to my own. It is the exact opposite of Husserl’s final project in the *Crisis*. I think his historical and scientific insights are enormously important, but my project is wholly different from his (insofar as I understand it). He thought that we had fundamentally lost touch with what we really were doing in the sciences, because we had buried the original intuitions under innumerable layers of sediment. That was the deep source of the crisis, of which the evident and superficial signs were the state of Germany, heartland of European culture and science, in 1936. The task of a transcendental phenomenology was to recover the grounding intuitions and rework up to the present from there. The noble aim was to rescue European civilization from disaster. Much as I respect what Husserl wanted to do, I believe that all ideas of recovering a first understanding is fundamentally mistaken, quite aside from the imagined political benefits.\(^\text{12}\) I shall nevertheless choose to name one style of scientific reasoning “Galilean,” and that name goes almost directly back to Husserl, who, it seems to me, had a remarkable grasp of the profound changes effected in scientific reason at the time of Galileo.

**Bernard Williams’ “Truthfulness”**

My kind of historical specificity, to repeat Bourdieu’s phrase, is directed not at the experience of grounding reason but at Leibnizian conceptions. I make a change that looks like casuistry, a play on words. I turn from truth and reason to truthfulness and reasoning. Now “truthfulness” is not a common English word. I take it from Bernard Williams’s last book, *Truth and Truthfulness: An Essay in Genealogy*.\(^\text{13}\) It is the most interesting book in living memory about truth and its value. It is about many things, and my use of it is selective. I want to extend its notion of a genealogy of truthfulness. Williams

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\(^\text{11}\) *Méditations Pascaliennes*, 130 ; *Pascalian Meditations*, 109.

\(^\text{12}\) I explain this in connection with mathematics and Husserl’s “Ursprung” in “Husserl on the origins of geometry,” in David Hyder, *Husserl’s Epistemology*, Stanford University Press, forthcoming.

metaphorical use of this idea genealogy is strictly taken from Nietzsche, with a somewhat different emphasis from that of Foucault, who of course was inspired by the same source.

Williams offered two sub-genealogies, one of telling the truth about the historical past, and the other of authenticity about oneself. The time frame of his chapters on these topics is quite different: the Athens of Thucydides and the Paris of Rousseau and Diderot. He chose them for several reasons. Their intrinsic interest. Their importance to his own philosophical work. But also because these were among the two most contested domains of truth-telling during the recent so-called culture wars. One of his aims was to show that the possibility of telling the truth about the past (a) has a history (contrary to dogmatic thinkers who conceive of history as the relating of facts, facts that could have been recounted any time, anywhere). And (b) this is a history of how objectivity about the past came into being (contrary to dogmatic thinkers who say that there is no objective historical truth). Likewise for the self and authenticity: he opposed (a), those who insist that there just are truths about individual human beings, independent of the context in which they are understood, and (b), those who say there is no objective truth about a person, only the stories that people tell about themselves.

I shall try to extend Williams's idea to the sciences, where, notoriously, a few years ago there was a similar (a)/(b) debate – timeless fact versus no objectivity. I do so by connecting Williams’s genealogical approach with Crombie’s idea of a handful of evolving styles of scientific thinking.

Williams most emphatically did not write a genealogy of truth.

One thing I shall not consider, however, is the history of the concept of truth, because I do not believe there is any such history. The concept of truth itself – that is to say, the quite basic role that truth plays in relation to language, meaning and belief – is not culturally various, but always and everywhere the same.” (p. 61).

Truth, then, has no history, beyond the fact that it is coeval with the emergence of linguistic structures to convey information. This conception is Aristotelian and Tarskian, The adjective ‘true’ has many uses, but truth is a formal concept, essential to semantics but with no semantics of its own. Thus it spans all informative discourse, and has no genealogy.

Aristotle: To say that that which is the case, is the case, and that which is not the case, is not the case, is to say the truth. That is a blank, formal, assertion, conveying in passing the fundamental fact that the adjective ‘true’ primarily applies to what is said, was said, or can be said. There is undoubtedly a history of when human beings began to talk, to say things informatively, to make what we can recognize as assertions. But there is no further history of truth than that.

I read Aristotle’s maxim as an early version of Tarski’s equally formal semantic theory of truth. Its scheme, “$s$ is true if and only if $p$”, makes as plain as Aristotle did that the adjective applies to sentences. The somewhat overblown theory of metalanguages derives from that trifling grammatical fact. Tarski himself wrote that his semantic theory appeared to be consistent with, and even to express the core motivation for, every substantive “theory” of truth, every theory which says what truth “is” – correspondence, coherence or whatever. That is one way of saying that his own theory is formal, and content-free.

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14 Aristotle *Metaphysics* Γ. 1011b25. Cristopher Kirwan, Oxford Clarendon Aristotle 23: To say that which is is, and that which is not, is not, is true. W. D. Ross: To say that what is, is, and that what is not, is not, is true.
I now prefer as a matter both of principle and convenience to say nothing substantive about truth.\textsuperscript{15} Why “convenience”? Because truth has recently been such a contested notion among analytic philosophers, with their competing theories of truth. I have views about those debates, as is shown by my doctrine that truth is a purely formal notion, but those views are not important to my investigation of styles of scientific thinking.

By truthfulness, Williams meant telling the truth about something. This has two aspects. The truthful person is both quite accurate and quite sincere. These are fairly independent virtues. Williams was concerned with how and when it became possible to be accurate and sincere about some subject matter. A genealogy of truthfulness about X, will have two branches, whose rhythms may be very different. Thus Williams’s history chapter focuses on accuracy and his chapter on self-knowledge focuses on sincerity. Since I am interested in the sciences, I focus on accuracy, although it is important also to see what happens to sincerity in the scientific context.

\textbf{Truthfulness about the past}

Williams spurns vague generalities. The transformation in the Western conception of telling the truth about the past happened at a definite moment, at the time of the work of one man. And that man was Thucydides. Thucydides has become emblematic, a sort of icon, standing for the beginning of the writing of history in the Mediterranean world. No man stands alone: it is Thucydides, his hearers, and his readers who count, in that highly specific social universe which was Athens. There is another specific social universe in which history writing came into being in much the same way as it did in Greece: ancient China. The invention of writing is a precondition for history in Williams’s sense, so China is the place to look for a decisive conceptual change like that which occurred in the Mediterranean at the time of Thucydides.

The standard candidate is Sima Qian (145-90 BCE) or perhaps his father, Sima Tan. One should not think that exactly the same change in conceptions of the past occurred in China and Greece. The Simas, father and son, had high official status at court for long periods, a status which had no counterpart in Greece at the time Thucydides. Sima Qian’s history of two millennia became the paradigm, quite literally, of Chinese historiography – the model of historiography that was followed for centuries. One could argue that it was a social and cultural history unknown in the West until the twentieth century.

It is not news that Thucydides is the first real Western historian. Hume said as much. So have generations of scholars, each with their own explanation of what makes this moment in historiography something new. Williams’s version is in terms of truthfulness: there was, “most basically, a shift in conceptions of what it is to tell the truth about the past.” This is the fundamental move in the historical parts of Williams’ genealogy of truthfulness. It is a logical operator whose form is this schema, call it (*)

\begin{equation}
(*) \text{ A shift in conceptions of what it is to tell the truth about } X.
\end{equation}

That sounds as if X is a given, a timeless given, X = the past, or, in the case of the emergence of authenticity, X = the self. No. New ways to tell the truth about X change our conceptions of X itself. Williams talks of a shift from “a ‘local’ to an ‘objective’ view of the past.” (p. 163). Here are some more snippets about the new idea of history:

\textsuperscript{15} Thus I re-express most of what I said about truth, in the two previous papers mentioned in footnote 6, in terms of truthfulness.
1) Historical time provides a rigid and determinate structure for the past. (p. 162).

2) … this significant change that took place in the fifth century B.C., the invention in the West of historical time.

3) Did the change bring with it an increase in explanatory power? Surely, yes; and this was so in terms of anyone’s conception of explanation. (p. 170)

4) Does that mean that those who operate in the new style, who have the “objective” conception of time, are more rational or again better informed that then others? No, if that means (as it is usually meant to imply) that those in the traditional practice were confused or believed something else. (p. 170)

5) The invention of historical time was an intellectual advance, but not every intellectual advance consists of refuting error or uncovering confusion. (p. 171)

I won’t go on, you will see the point.

There was a new conception of the past, and correspondingly new kinds of statements to make about it. There is the demand that every event should happen before, after, or overlap with, every other event. Williams claims that earlier writers, including Herodotus, were not constrained by such an idea. But obviously he does not suggest the silly idea that events themselves got into a dateable ordering only in the time of Thucydides!

The schema extracted from Williams is altogether formal and can be applied across the board. To repeat,

(*) a shift in conceptions of what it is to tell the truth about X.

Snippet (2) above suggests a 2nd schema:

(**) This significant change took place in the Y century, and its emblem is Z.

When X= the past, Y = the fifth century BCE, and Z = Thucydides.

**Mathematics**

Williams’s schema can be applied to the sciences. He might not have welcomed this. His *Ethics and the Limits of Philosophy* kept the sciences distinct from humane questions about values. Let us see, however, how it can be done. Start with geometrical relations. Who shall stand in for Thucydides? Here is the legend: “A new light flashed upon the mind of the first man (be he Thales or some other) who demonstrated the properties of the isosceles triangle” – thus Kant. In that paragraph Kant waxes poetical about entering “upon the sure path of science,” “that royal road.” Kant calls this “the intellectual revolution” which was the coming into being of mathematical demonstration. It was the discovery of our capacity for mathematical proof, or rather proof in geometry.

The emblem of the debut of geometry is Thales, which is not to say that there was for sure any such historical figure. The legend is that when X = geometrical relations, Y = early in the sixth century BCE, and Z = Thales.

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17 *Critique of Pure Reason* B xi.
We need two things to understand styles of scientific thinking: on the one hand, the study of mental capacities, and on the other, the history of civilizations and of their institutions. What cultural elements were needed to sustain a discovery about our cognitive capacities? In answer, ask why proof loomed so large in ancient Greece. There was lots of mathematics in Babylonia, and certainly in China, as shown most recently by the work of Karine Chemla and Guo Shuchun. Chinese had splendid computational devices, but no proofs such as matured in Euclid. Why? The answer favoured by Reviel Netz starts with the familiar fact that Athenians were the most argumentative bunch of people ever known. They countenanced no higher authority than themselves, when it came to settling an argument. In China (to parody) an issue could be settled by edict, so compelling proof – Wittgenstein’s “hardness of the logical must” – had no special interest. But in Athens, proofs seemed to have the strange power of establishing truths, of themselves, for those who could study them. That mattered, because of the practices of settling arguments current in Greece. It did not carry the same weight in China.

There is a lot of evidence that spatial, geometrical, thinking involves cognitive capacities different from arithmetical, combinatorial and algorithmic reasoning. Let us introduce another legend for the algorithmic or combinatorial style of thinking, the legend of Al-Khwarizmi (about 780-850) working in the House of Wisdom at Baghdad, after whom the very word “algorithm” is named. The title of one of his books gave us the word “algebra.” So now we have a new X, Y, and Z: a shift in conceptions of what it is to tell the truth about numbers and other quantities; this significant change took place in the ninth century and its emblem was Al-Khwarizmi.

Notice that in both cases, geometrical and combinatorial thinking, the new methods of reasoning, including proofs, provide a wholly new level of “explanatory power.” See (3) above. And to repeat (4), this does not mean that those who operated in the new style, who attained the “objective” conception of space or of computation, were more rational or again better informed that their predecessors.

Kant may have got it right, in the Transcendental Aesthetic, when he located arithmetical and geometry in separate compartments of the mind. He linked one to the experience of the succession of discrete unities in time, the other to the experience of spatial configurations. However we account for them, it is quite plausible to invoke the idea of “mental modules” here – fundamentally distinct capacities inherent in the human brain. Of course there is a big debate about modularity among cognitive scientists, ranging from Dan Sperber’s “modularity all the way down,” to that of Jerry Fodor, who pioneered the transfer of modularity from Chomsky on grammar to a wider range of applications. “Modularity gone mad,” writes Fodor of Sperber. It may nevertheless appear that module-talk in moderation is a good parable, while we are still finding out about the brain. As Bernard Williams says about modules somewhere in Truth and Truthfulness, “Why not?” Why not, especially if there has been a long-standing awareness of what appear to be distinct faculties, long preceding Kant.

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No single module corresponds to each style of reasoning. Each demands many, and modules of different types. What will be called the laboratory style requires a combination of hand and eye coordination, of deductive skills, and a lot else.

Recall the paired virtues associated with truthfulness. There is a wholly new standard of accuracy about geometrical relations, to the extent that “accuracy” no longer seems quite the right word. On the other hand sincerity seems to drop out because proof becomes the sole criterion of correctness in this new domain. Or from a social point of view, what matters is not that the geometer is sincere, but that initiates see that the argument of the geometer is indeed a proof. We reject the Kantian genius-in-history lore that “a new light flashed upon the mind of the first man who demonstrated …” As if one man did the trick! It takes a community for there to be a practice of proving. But there is no other standard, of what is a correct proof, than proof itself. Proofs are self-authenticating. Many a proposed proof has proven to be fallacious, but the standard of validity or fallacy is proof itself.

In the case of geometry, we have a new way of telling the truth, and of ensuring that it is the truth – namely proof. In quotation (4), Williams spoke of “operating in a new style.” It is probably fortuitous, but it is handy that Crombie and Williams used that same mischievous word, “style”.

**The Galilean and the Laboratory styles**

In the case of the introduction of each style of finding out there is a “radical shift in conceptions of what it is to tell the truth about X.” Husserl can be read as making exactly that claim about the mathematization of the world that he attributed to Galileo. Later writers, whose expertise is as seemingly different as the cosmologist Steven Weinberg and the grammarian Noam Chomsky, have attributed talk of the “Galilean style” to Husserl. They are not so different, for cosmology and grammar are sciences where experiment (in any strict understanding of the word) is not possible: one must make theoretical models and test them against observation. The Galilean style is what Crombie called the style of hypothetical modelling. Crombie too gave Galileo pride of place in the history of that style, though not as originator.

Galileo then serves as emblem. Notice how easy it is to pass the buck. Neither Husserl nor Kant is used as an authority. The rhetorical trick is rather: look here – we have been saying this all along forever and anon, only we have not noticed that we were saying it.

Then there is talk of an experimental style. This probably has no strict beginning: human beings have been curious, looking, tinkering, exploring, even measuring, for ever. A new kind of truth-telling did begin when a community, call it that of Robert Boyle, not merely studied hypothetical models, controlled by observation and measurement, but also created instruments and apparatus for interfering with the course of nature in order to extract its deepest secrets. Here X = (let us say) the minute unobservable parts of material nature – what in quite recent times philosophers came to call theoretical entities. Laboratory science involves a distinctive practice of truthfulness that begins with creating devices that work: one can even begin to speak of truthful apparatus. In mechanical matters we more commonly, and rightly, call the instruments reliable, an oddly mechanical mixture of Williams’s virtues of sincerity and accuracy.

The classic account of this event is to be found in Steven Shapin and Simon Schaffer’s *Leviathan and the Air Pump: Hobbes, Boyle and the Experimental Life*. One of the many wonderful features of that book is that the main protagonist is not a person but an instrument, the air pump. The other players,
Boyle and Hobbes, are perfectly cast. The book includes a translation of a disquisition by Hobbes, published in 1662. Hobbes was aware that Boyle was shifting the conception of what it is to tell the truth about matter (and the void), by using instruments to create new phenomena, and making our statements answer to the phenomena produced by apparatus.

There are quite enough public phenomena in the world already, Hobbes says huffily, in this tract. We do not need more, that are produced in a closet, and checked only by the members of a secret society (the Royal Society of London). Hobbes knew that a new style of thinking was in the making, and hated it. His primary objection was, perhaps, that whereas geometry was open to all who worked at it, and had a distinctive certainty, the new physics was a matter for initiates working in private, and produced merely probable knowledge, *opinio*, not *scientia*. But he also says very clearly that God has given us enough phenomena, and we do not need you to give us more dubious ones, Mr Boyle.

One need not choose Boyle as the emblem of the laboratory style. The same new criteria of truth checked by artificial phenomena were emerging all over Europe. Boyle serves well because a great and articulate old man, Hobbes, saw the writing on the wall, and spat at it.

Schaffer and Shapin, and Shapin in his book, *A Social History of Truth*, urge that what made the new laboratory science possible was the custom of trust among the gentry who formed the Royal Society. A gentleman could be trusted to report truly the phenomena that had been produced and witnessed. Whether Shapin is historically correct or not, he nicely draws attention to the (defeasible) assumption that whoever writes up an experimental inquiry is telling the truth.

Our practice of peer review has absorbed much of the assumption of sincerity right across the board, in all the sciences, including mathematics. Now we ask peers to review, whereas once we relied on peers, that is, English aristocrats and landed gentlemen.

As you find other “shifts in the conception in what it is to tell the truth about X,” at rather sharp points in time, you quickly hit upon a figure of almost legendary proportions – Thucydides. Rousseau/Diderot, Thales, Galileo, Boyle. For X = the species and the higher taxa of systematics, legend congeals around Linnaeus. For X = probabilities it is Pascal.

The doctrine of the hero in history has been totally debunked by every historian of science in living memory. But there is still truth in folklore. The fact that we have a legend and a handy emblem for each style of scientific thinking shows how tradition already recognizes that each style is a radical innovation which can, for ease of story telling, be associated with a giant. Each giant, to repeat, is only a figurehead. For example, where previous histories of probability had given an exemplary role to Pascal, part of the argument of *The Emergence of Probability* was precisely that a new conception of probability seem to spring up, almost spontaneously, after 1650, in most regions of Europe.

**Logic**

Logic has come to mean deductive logic, perhaps as formalized by first-order logic. There is an older tradition, in which logic names the canons of reasoning. Peirce divided logic into three parts, deduction, induction and abduction, or what in the nineteenth century was called the method of hypothesis. These are not styles of scientific thinking, in the sense of the present paper. They are surely founded on human cognitive capacities, although their expression varies from language to language, and from culture to culture. Peirce could well have added classification as a fourth branch of logic.

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equally founded on some cognitive capacities. All four are human universals, not simply because they are accessible to all, but because they are the background within which human discourse can take place. They have no beginning. They are part of our animal nature. Popper liked to say that amoeba make inductions: so be it, if you will.

None of the four branches of logic is among the “methods of reasoning” which characterize styles of scientific thinking. They are rather presupposed, in varying degrees, by every style. There is no “emblem,” no legendary beginning for logic as understood by Peirce, although Aristotle certainly set the analysis of deductive reasoning on its way.

Evidently the geometrical and algorithmic styles are more closely linked to deductive logic than are other styles. The Galilean style, Crombie’s style of hypothetical modelling, must be an outgrowth of abduction. The probabilistic style of thinking builds upon our inductive capacities. The taxonomic style builds on naïve classification. The ways of finding out discussed in the paper are part of what Bourdieu called “the singular history of reason,” namely cultural discoveries that take place in highly specific and local situations, and which take for granted a background of what Peirce called logic.

**Conclusion: three radical propositions**

This is not a philosophy to expound briefly. But certain aspects can be highlighted. Here are some of the philosophical implications of this approach to styles of scientific thinking.

1 **Styles are self-authenticating**

   Each way of finding out introduces its own criteria of evidence, proof and demonstration. Each determines the criteria of truth-telling that applies in its own domain. That leads to a somewhat radical thesis about truthfulness and objectivity.

   Scientific styles are in a certain sense self-authenticating. For each style there is a class of sentences that are candidates for truth or for falsehood only in the context of the style. The only way to find out whether they are true or false is by using the relevant style. The criteria of truthfulness are determined by the style. All individual propositions are fallible. In reasoning according to a style, one can always make mistakes. But it is in the framework of the given style that one establishes that an error has been made.

   A style of scientific thinking could wither, fail, or become extinct. In that case we are disinclined to call it scientific any more. The disappearance of a style is always caused by external forces. On the present theory, a style of thinking begins with the discovery of how innate capacities can be used in new ways to find out about something. That is what has traditionally been called part of the “internal” history of science. But what maintains some way of finding out must be its use in a cultural context. That is part of an “external” history. The self-authentication is internal, the perseverance, external.

**Ontological debates**

Each new style in the sciences introduces a new class of objects to study, in effect, a new X in Williams’s schema (*). But matters do not rest there. Each new class of objects invites an ontological debate, often described as realism versus some kind of anti-realism. These debates are mere by-products of the styles of thinking.

Take the controversies about mathematics between so-called Platonists and constructivists. Or take the confrontation between those who say that unobservable theoretical entities are real, and those,
from Auguste Comte to Bas van Fraassen, who deny it. In systematics, some contend that the species are real, but not the higher taxa. Others insist that genera, classes, orders are real, are part of the natural order. And so on. Each ontological debate takes place within its own scientific style. That is because every style of finding out creates its own objects. We are on verge of a genealogical theory about the nature of the classic ontological debates in the sciences.

**Cognitive foundations and cultural history**

Third, each of these ways of finding out is grounded in typically human capacities, including cognitive ones and physiological ones. Undoubtedly these capacities are the product of evolution by natural selection. They are universal.

But scientific styles are themselves the product of cultural innovation and evolution, chiefly in the Mediterranean regions – North Africa, West Asia and Greece – and later in Europe. Each has a beginning in history, which sometimes exists chiefly in the form of legend, and each has its own trajectory of development.

Cognitive science and neuroscience are proper attempts to understand the capacities underlying this history, and help explain how what evolved at certain specific moments in unimportant parts of the world, has spread to become part of the universal human heritage.

From a different perspective, we should see a study of styles of scientific thinking as part of “the natural history of human beings” (Wittgenstein *Philosophical Investigations*, §415), or as part of a philosophical anthropology.

**Endnote: looking beyond Europe**

This lecture has been extraordinarily Eurocentric. This is because the sciences, as we now know them, took off from what is called, with excessive simplification, the “scientific revolution” of early modern Europe. Now they are everywhere in the world. Within the lifetime of almost everyone attending this lecture, a majority of the Nobel Prizes in the sciences, and of the Field’s prizes in mathematics, will be won by individuals from South and South-East Asia.

You will, nevertheless, very rightly have the sense that a lecture called “The Historical Roots of Scientific Reason,” ought to pay more attention to ancient Chinese mathematics, which is the peer, in so many ways, of ancient Mediterranean mathematics. I shall do so. It is partly for this reason that I have made mathematical reasoning the topic of my second lecture. My title is, “Where do mathematical objects come from?” This is a European question, in the sense that some and only some Western philosophers have been profoundly moved by it. Yet it is, we might say, a merely local question. I shall begin my second lecture by discussing exactly this fact. Thus I shall engage in a little comparative philosophical anthropology. I eagerly look forward to your response to that.