‘Language, Truth and Reason’ 30 years later

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A R T I C L E   I N F O

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A B S T R A C T

This paper traces the origins of the styles project, originally presented as ‘styles of scientific reasoning’. ‘Styles of scientific thinking & doing’ is a better label; the styles can also be called genres, or, ways of finding out. A. C. Crombie’s template of six fundamentally distinct ones was turned into a philosophical tool, but with a tinge of Paul Feyerabend’s anarchism. Ways of finding out are not defined by necessary and sufficient conditions, but can be recognized as distinct within a sweeping, anthropological, vision of the European sciences. The approach is unabashedly whiggish. The emergence of these styles is part of what Reviel Netz calls cognitive history, and is to be understood in an ecological way. How did a species like ours, on an Earth like this, develop a few quite general strategies for finding out about, and altering, its world? At a more analytical level, the project invokes Bernard Williams’ notion of truthfulness to explicate the idea that these styles are ‘self-authenticating’ and without foundations. The paper concludes with open questions. What role (for example) have these few fundamentally distinct genres of inquiry played in the formation of the anomalous Western idea of Nature apart from Man?

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The phrase styles of scientific reasoning was introduced in Hacking (1982) and cemented in this journal, Hacking (1992; both reprinted in Hacking, 2002). I remain committed to the programme sketched in those papers—although not to the label, ‘styles of reasoning’. There has been a fair amount of kite-flying since. This has been deliberately ephemeral, published, if at all, in obscure places. Much of it—including a fair amount of stuff that will never appear in print—is listed in the diligent bibliography at the end of Martin Kusch’s thorough critical survey in this journal (2010, p.172). The present paper is an update on the ‘styles project’ occasioned by a workshop in Cape Town, March, 2011.

1. In the beginning

I have been thinking about ‘styles of scientific thinking in the European tradition’ off and on, ever since I encountered A. C. Crombie at a conference in Pisa in 1978 (Crombie, 1981). The late Martin Hollis asked for a contribution to a collection of new essays, Rationality and Relativism (Hollis & Lukes, 1982). This volume was in part a reaction to the new sociology of scientific knowledge emanating from Edinburgh, the ‘strong programme’ of Barry Barnes and David Bloor. Lukes and Hollis did not care for Edinburgh one bit, but thought it deserved serious confrontation. The resulting essays were arranged from left to right, with the staunchly ‘relativist’ Barnes and Bloor at the far left, with (to my surprise) my piece next to theirs, and the more ‘rationalist’ essays coming later in the book.

2. LT&R

The title, ‘Language, Truth and Reason’, was a riff on Ayer’s polemical, logical positivist, masterpiece of 1936, Language, Truth and Logic. The topics of LT&R were exactly those philosophical staples in reverse order: first reason, then truth, and then language. The novelty, for analytic philosophy, was the realization that reason has a history.

Aside from Crombie, the initial inspiration may have been Paul Feyerabend’s scintillating Against Method (1975). Already by 1970 I was falling under the spell of Michel Foucault’s early archaeologies, which make one rethink not only the past, but truth itself. Thus Feyerabend and Foucault were buzzing about in a mind disciplined by Cambridge analytic philosophy. Crombie’s programme chimed in. Since then I have, very intermittently, used his list of six distinct
styles of scientific thinking in the European tradition’ as a template for the ways in which different methods of inquiry used in the sciences have their own historical trajectories, and have moulded scientific reason and even what counts as true (see the use made of Bernard Williams in §15 below).

The styles project uses the past as a way to understand the present. Although it has suggested historical research to others, and draws on far more historical data than it cites, in itself it adds no new content to the history of science. The accounts of the past to which it refers are (disconcertingly for many readers) as often folklore as archive-based research. Anthropology, sociology, and cognitive science, especially of the more speculative sort, are also invoked. In short, the project is philosophy attentive to, but not awed by, many neighbouring bodies of knowledge and theorizing.

Looking back at the state of play in 1982, the target of the Lukes and Hollis anthology, namely the nascent sociology of scientific knowledge, has turned into the amazingly rich field of science studies for which we are all enormously grateful. The styles project would have been happy to be accepted under that umbrella, but it is still ‘logic’, in a traditional understanding of the word. The very same logic as appeared in Ayer’s title, although not his logic. It is about timeless reason except that: ‘We have to acknowledge that reason did not fall from heaven as a mysterious and forever inexplicable gift, and that it is therefore historical through and through; but we are not forced to conclude, as is often supposed, that it is reducible to history’ (Bourdieu 2000, p. 109).

3. Out of anarchy

QTQR described itself as driven by anarcho-rationalism. That was a tip of the hat towards Feyerabend, who at the time was proclaiming himself an anarchist, later retracted to Dadaist. As is so common in philosophy, retreat to safer ground is a loss. The movement that called itself Dada was brilliant but flippant. Anarchism is stupid but serious, so I prefer that label. A more anodyne name, fitting the talk of the day, would have been rationalism without foundations.

There is no deep reason for, or cause of, the appearance at different times of a few distinct genres of scientific inquiry, often detectable in ancient Greece, and still flourishing. They were bred in a seething culture of groping and grumbling, an agar-agar of talk and technologies (including writing and drawing diagrams). They began to stabilize but also continued to evolve in an endless cycle of contingencies. This anarchic story is not quite that of a random walk, but there was no foreordained right route. There is no ‘reason’ why these types of argument have become part of our canon of what we call right reason. But they are our canon. The larger grounds for the canon are not self-consciously rational, and might be better described as ecological.

Ecological? On the one hand, scientific thinking—and doing—exploit human capacities, both mental and physical, which in the course of human history we have learned to use and hone. Many cognitive scientists argue that such capacities are innate, and favour a modular approach. Hence each distinct way of finding out about the world might be grounded in its own cognitive modules.

On the other hand, the discovery of how to use those capacities has happened in highly specific local settings at individual moments in time. Yet, since they are grounded in human capacities that are presumed to be universal, they have become part of the heritage of our species. Many would prefer to speak of culture rather than history. Why is this ‘ecological’? Because a creature with our bodies, including our hands and brains, has discovered how to use its endowments to interact with the world in which it finds itself. These very capacities may have been adapted for quite other ‘purposes’. It is rather unlikely that all our ways of finding out were selected for their survival value.

The throw-away label, ‘anarcho-rationalism’, expressed, on the one hand, Leibnizian convictions about reason, and on the other hand, it acknowledged that the reasoning used in the sciences is sedimented anarchy. What once were shifting sands became what is experienced as rock-hard right reason.

The metaphor of sedimentation comes from Husserl’s Crisis (1970), but is used in a way opposite to his. In caricature, Husserl thought it was the philosophical task of his time of European crisis to clear away the sediment. That was a specific response to the 1930s. We should, he thought, try to recover the original experiences underlying fundamental events, such as the discovery of mathematical proof (the Ursprung of geometry) and Galileo’s mathematicalization of nature. He wanted to remove the sediment, to reach, perhaps, a pre-Galilean state. In our contrary perspective, the sediment, hardened over a long time by great pressures into rock, is a collection of achievements founded on human ingenuity, innate propensities, and interaction with everything. Like any sedimentary deposit, it may undergo radical change in the future, but it cannot be undone.

4. Not just thinking

QTQR insisted that Crombie’s styles are not just styles of thinking. It seemed at the time that changing the gerund to reasoning might do the trick, for reasoning is also practical as well as theoretical; it involves as much doing as thinking. It would have been better to have been explicit, speaking of styles of scientific thinking & doing in the European tradition. By the time he wrote the preface to his big book (Crombie, 1994, p. i-vii), Crombie was well aware that the phenomena he was describing are ‘a product of [human] nature as conscious embodied beings’. This embodied creature uses not just its mind but its body to think and to act in the world in which it finds itself. There is also a lot more to be said for Crombie’s ‘thinking’, for, as he wrote in the first paragraph, ‘scientific thinking combines imagination and reasoning’. Probably bits of his 1994 preface are a gentle reprimand to QTQR of 1982, and to its successor text of 1992 (with which Crombie was well acquainted before its publication).

Crombie offered a sensible template of modes of investigation in the sciences. ‘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.’ (Crombie, 1994, vol. I, p. 83) For short, these may be called mathematical, experimental exploration, hypothetical modelling, probability, taxonomy and historico-genetic explanation. ‘The first three of these methods concern essentially the science of individual regularities, and the second three the science of the regularities of populations ordered in space and time.’ (Crombie, 1981, p. 284)

This list of six was wholly unoriginal, and that is one of its merits. It is a conventional organization of popular history of science. The novelty lay in the thought that the list was a stab at an exhaustive classification of complementary types of inquiry, a thought that Crombie went on to amplify with a painstaking recital of documents, some familiar, but many arcane. Interspersed in this mixture are many wise observations, but no one will ever read the entire three volumes of (1994) in their entirety.

Crombie said his styles have distinct objects and methods of reasoning. These words seem anodyne. The objects with which mathematics concerns itself are often called, by analytic philosophers, abstract objects, such as numbers, shapes, and groups. The objects with which taxonomy 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. Hypothetical modelling introduces non-observable theoretical entities.

Crombie sometimes referred to his six as methods (1981, p. 284) rather than styles (1994, vol. I, p. 83). Another available moniker is: genre of scientific inquiry or investigation. Often something more casual may be preferable: ways of finding out in the sciences. Not only finding out ‘that’ so and so, but also, finding ‘how to’. Finding out what’s true, and finding out how to change things. The styles project no longer has any commitment to the word ‘style’.

A difficulty with the word ‘style’ is that it has had such a varied history, most particularly in its adopted homeland, the history of art. As Simon Schaffer (2010) reminds us, a typically rich essay by Carlo Ginzburg (2001) shows that in the arts, the word has often been invoked in the subtexts of conservative ideologies that many of us find suspicious and sometimes repugnant. User beware! The longest chronicling of the use of the word in a special science is the survey of ‘mathematical style’ by Paolo Mancosu (2009). Some of the usages in his catalogue are very odd, but most seem pretty innocent, and a few of the old usages contain real insights for today.

The 1992 title, ‘Style for historians and philosophers’, was a mistake, for the paper addressed philosophy, not history. Historians do, nevertheless, use the word. Thus in 2008, the focus of the annual meeting of the European Society for the History Science was Styles of Thinking in Science and Technology (Hunger, Seebacher, & Holzer, 2009). Its prospectus begins with a bow to Crombie and states that the time has come for a full development of his lead. It points to diverse fields of important research: ‘it is time to reconsider this fascinating topical subject [styles of scientific thinking] in order to broaden earlier, Eurocentric approaches. Which philosophical, cultural, religious, political, economic influences can be identified that led to certain styles of thinking in science and technology all over the world and that influenced their further development? Cross-cultural influences and interrelations are of special interest. How are such traditions of thinking transmitted to later generations, to other cultures? How are they modified in the course of history?’

5. A rose by any other name...

Some would say that a skunk by any other name would smell as foul. LT&R and subsequent work tried to patent the phrase, ‘styles of scientific reasoning’, restricting it to items on Crombie’s list or similar. It had not been used previously in any specific context. It seemed available to choose as a name, so why not lay claim to it? That was a foolish mistake. The phrase at once took off and was used in all sorts of contexts that had nothing to do with LT&R or Crombie’s template. Quite rightly, too.

The phrase has now been applied to all sorts of things, starting most influentially with Arnold Davidson’s ‘Emergence of the Psychiatric Style of Reasoning’ (a subtitle to his early paper, ‘Closing the Corpses’, and his general analysis ‘Styles of Reasoning: From the History of Art to the Epistemology of Science’. (Both reprinted in Davidson, 2001.) Google directs one to many sites that speak of Davidson and Hacking on styles of reasoning as if they were a unit. That is a misapprehension. Much as I admire Davidson’s work, he took my phrase in a direction totally different from what I intended. He and many others were right and I was wrong. Of course the phrase can be applied to a host of, well, styles of reasoning, and it was stupid to try to patent it, and restrict it to members of a list.

Geoffrey Lloyd sensibly preferred ‘styles of inquiry’ in order to keep the nomenclature different; that was for his breath-taking series of cross-cultural studies of the sciences, for which references may be found in Lloyd (2010). For one of many excellent uses of the original phrase, we need go no further than the title of a book by a contributor to this issue, James Elwick (2007). His Styles of Reasoning in the British Life Sciences: Shared Assumptions, 1820-1858 is about a specific group of sciences, an insular band of practitioners, and a time-frame of 38 years. Of course it is about styles of reasoning used by those workers in those years. Elwick’s thesis, much condensed, is that despite vociferous disagreements among British naturalists, biologists, and medical men, there were certain underlying ways in which they approached their subject-matter: shared styles of reasoning.

6. NO necessary and sufficient conditions

It was hubris to try to patent ‘styles of reasoning’, but there was also an inexcusable error. Naming the class of items on Crombie’s list tended to reify it. It suggested that the styles he listed had some distinctive logical property that the philosopher might elucidate. Should there not be rough and ready necessary and sufficient conditions for being on Crombie’s list? No. His styles of scientific thinking & doing have no essence. That is not to say that the list is arbitrary, or that the idea behind it is vague. It is not even to imply that its members are bound together by family resemblances.

Crombie used the word in a purely descriptive way, like an early taxonomist noticing the birds and the bees. He said Look! These are several distinct genres of inquiry used in modern science, and they pretty well cover the waterfront. If we ask why they persist the answer is more likely to be ecological than logical or pragmatic. We do not use them because we have good reason to use them. They are what we use. They become our standards for good reason. (Anarcho-rationalism!)

Styles, genres, inquiry, and ways of finding out: all these words can be used to describe a great many more kinds of practices than Crombie’s six. Occasionally, when I wish to be clear I am talking just about the items in his template, I shall use the ugly name, hereby patented: styles of scientific thinking & doing in the European tradition. Happily context is usually sufficient, and resort to a dreadful convention is seldom needed.

7. Logic

Why isn’t logic on the list? Surely that’s reasoning if anything is! The short answer is that logic is topic-neutral in a way that styles of thinking & doing are not. It has three branches, as set out by C.S. Peirce, namely deduction, induction, and what he called abduction, or the method of hypothesis, more recently named inference to the best explanation. Unlike the items of Crombie’s list, these are used in any kind of conversation whatsoever. They are also prior to any science, although we have honed deduction in the mathematical style, and honed induction using probable reasoning and statistical inference.

Present American undergraduate education implicitly accords with this understanding. Courses variously called ‘modes of reasoning’ or ‘critical thinking’ or ‘symbolic logic’ are forced on every student of the arts and sciences, on the grounds that logic is a prerequisite for clear thinking, independently of the subject matter. Sometimes the students are also taught to distinguish good from bad science; the bad is often, wrongly called ‘pseudo-science’.

1 The subtitle of Davidson’s book is Historical Epistemology and the Formation of Concepts. That may have contributed to the association of the styles project with historical epistemology. For the record: I cannot recall ever thinking of the styles project as filed under ‘historical epistemology’.
We may briefly point to a further reflection that must be developed at length elsewhere. Mercier (2011) argues that arguing is a universal human practice. Mercier and Sperber (2011) contend that it is not for finding out the truth or deciding on the best course of action. Arguing is for maintaining one’s own opinions against those of others. This sounds implausible to all of us brought up on logic since Aristotle and the syllogism. But if we had been reading his Topics (about dialectic and arguing back and forth with equals) and Rhetoric (about presenting an argument to an audience), Mercier and Sperber might have seemed less iconoclastic. (They themselves were unaware that Aristotle had been there before them.) They maintain further that their account of argument explains apparently innate human inadequacy in logical thinking, illustrated by the group of phenomena made quantitative by cognitive psychologists such as Tversky and Kahneman. What is inefficient at extracting truth from data is efficient for convincing other people. They go so far as to suggest that the practice of arguing may have had a good deal to do with the emergence of human speech. Thus arguing, on their analysis, is universal, ancient, atten
tive to persuasion rather than truth, and largely independent of scientific reason.

Why do we have such a different impression of logic? Perhaps what happened was this. Arguing had been going on forever, wherever talkative people gathered. Aristotle wrote the first textbooks about how to argue (or gave classes that became textbooks). Later he invented the idea of logical form, which he imposed on simple arguments such as the most important structure he devised, the syllogism. This was happening around the same place and time where demonstrative proof had been discovered. But logic and mathematics pursued different careers until the 19th century, when the likes of Boole, Frege, and Peirce began to absorb logic into mathematics. This sketch may be a prelude to a deeper understanding of the difference between scientific reason and logic.

In parenthesis: we take for granted that much of the actual discourse among scientists consist of arguing in the sense of Mercier and Sperber: trying to influence the opinions of colleagues, rivals and patrons. That is what Aristotle called dialectic and rhetoric, two words that have been debased since his time.

8. Whig history

Crombie’s template has an initial virtue. It does not suffer from metaphysical numerology. It is not monism (one), dualism (two), triads (Kant and Peirce) or waffle (many). Just plain old boring six. Admittedly that is an instance of ‘The Magical Number Seven, Plus or Minus Two’, George Miller’s (1956) claim about the limits of our ability to process information. (None the worse for that!) And Crombie did surrender to numerology, recasting his six as two groups of three.

The template is, nonetheless, an unsurprising description of distinct genres of inquiry deployed in all the sciences, though in varying proportions for each special science. It is often emphasized that Crombie was a Roman Catholic by conversion, and attached far greater weight to mediaeval Christian (but not Islamic) contributions than they deserve. Much more importantly, he was a whig historian, forming a list of basic types of scientific investigation in current use, and looking into the historical trajectory that ended us up here and now.

What are the principles of individuation for Crombie’s six styles? We start from now, with six rather visibly different methods, and work back, tracing lines of filiation with past practices.

We owe that wonderful canard, ‘whig history’, to Herbert Butter-
field. The most vicious whig historian in his bestiary was David Hume, the patron saint of analytic philosophers. Perhaps philosophers are, by nature of their interests, whiggish.

One could cloak the styles project in the mantle of a recently trendy phrase used for a short time by Michel Foucault, ‘history of the present’. That would be pretentious: the shoe does fit, but it is for dancing only. A self-conscious use of the past to reflect on the present has all sorts of dangers, but philosophers are in the business of living dangerously. Crombie was right to say, in efect, Look! Here are these six. So what six was he looking at? Three of them are tried and true, three of them are true but not so often tried.

9. The three obvious candidates for the template

Every once in a while Einstein said something perfectly obvious and on which everyone was already agreed, but which was still worth saying.

Development of Western science is based on two great achievements: the invention of the formal logical system (in Euclidean geometry) by the Greek philosophers, and the discovery of the possibility to find out causal relationships by systematic experiment (during the Renaissance). In my opinion one has not to be astonished that the Chinese sages have not made those steps. The astonishing thing is that those discoveries were made at all.2

Well, actually Chinese scholars did make many steps towards these achievements, but Einstein was a man of his time. And by implication I have just distinguished logic from geometry. The important insight is that these inventions and discoveries, as Einstein rightly calls them, were not discoveries of facts or technologies, but discoveries of how to find out. Compare Kant:

In the earliest times to which the history of reason extends, mathematics, among that wonderful people, the Greeks, had already entered upon the sure path of science. […] the transformation must have been due to a revolution […].

Natural science was very much longer in entering upon the highway of science. […] In this case also the discovery can be explained as being the sudden outcome of an intellectual revolution.3

These words are from the preface to the second edition of the Critique of Pure Reason. Between 1781 and 1787, Immanuel Kant, the Captain of Team Reason, noticed that reason has a history.

Kant’s examples of the second revolution make plain that he is referring to the same discovery as Einstein, namely ‘systematic experimentation’: ‘When Galileo rolled balls of a weight chosen by himself down an inclined plane, or when Torricelli made the air bear a weight that he had previously thought to be equal to that of a known column of water…’ Note that both examples, although motivated by speculation, are described as well thought out experimental exploration, and not as theoretical science.

Kant and Einstein used different words. One said ‘achievement’ and ‘astonishing’, where the other said ‘sudden outcome’ and ‘revolution’. They describe two signal discoveries differently, but they are pointing at the same two local events, one in ancient Greece, and the other late in the European renaissance or early modern period. Both men saw clearly that these two events marked radical

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2 From a letter by Einstein to J. S. Switzer, 23 April 1953; Einstein Archive 61-381.00. I owe the quotation to Rasmus Winther.

3 Critique of Pure Reason B x-xii. The emphases are Kant’s. He invented the very idea of revolution in science (Cohen, 1985). I most strongly urge reading the entire few paragraphs in the second edition (1787). They speak several times of revolution, and are of great interest for other reasons too.
innovations in human life. Both saw that we had to find out how to find out.

These two achievements are what Crombie picked for his first two styles, which I label mathematics and experimental exploration. He called his third style hypothetical modelling. Kant (and more interestingly Einstein, the greatest of theoreticians) did not, in the passages quoted, notice this as a different way of engaging in scientific research. Husserl did. Galileo is unique, in that he has been taken as an emblem of experimental exploration and of hypothetical modelling. Since these two have since been run together as constituting 'the scientific method', Galileo's double duty is not so often mentioned.

Kant rightly attended to the legend of Galileo the experimenter. Husserl, moved by the more modern spirit of Alexandre Koyré, took the great achievement of Galileo to be the mathematization of the universe. Crombie showed amply that hypothetical modelling long precedes Galileo, even if he was misguided in putting so much emphasis on mediaeval sources. Galileo's role model was Archimedes, not the scholastic dabbler. The relevant fact is that at the time of Galileo, and importantly through the man's own work and triumphs, mathematical modelling became a paramount tool of inquiry.

These images of mutations in thought and practice are both right and wrong. The discovery that we can make demonstrative proofs from postulates was not an 'immaculate conception', but a continuation of Mesopotamian and North African practice. On the other hand, there was a sharp break in the development of mathematical reasoning, the discovery of the possibility of demonstrative proof. In the case of mathematics, 'A relatively large number of interesting results would have been discovered practically simultaneously' (Netz 1999, p. 273). Netz suggests a period of at most eighty years.

We have no need to dismiss the Babylonians, Egyptians and others who taught mathematics to the Greeks, in order to see that a revolution in reasoning was wrought. This is a 'crystallization' of practices begun much earlier. The metaphor is far from perfect, but it allows us to acknowledge sharp breaks within what, in retrospect, is the long haul from the dim realization of a new way to carry on, to the present well-developed mathematical style of thinking & doing.

10. The laboratory

The styles project does not engage in historical research but is delighted by new historiographical ideas that it can exploit. Netz (1999) provides one such: the first analysis of what did happen delighted by new historiographical ideas that it can exploit. Netz (1999) provides one such: the first analysis of what did happen delighted by new historiographical ideas that it can exploit. Netz (1999) provides one such: the first analysis of what did happen delighted by new historiographical ideas that it can exploit.
12. Ludwik Fleck

Ludwik Fleck's theory of the *Denkstil* and the *Denkkollektiv* was the first use of the idea of a style of scientific thinking. Fleck has been picked up as a pioneer, perhaps a founding father, of social studies of science. Some say he was a deeper thinker than T. S. Kuhn, who himself acknowledged his debt to the man. The styles project would have been richer had it invoked Fleck thirty years ago. Today he can be used as a corrective to, a prophylactic against, the legends just invoked.

Those who read his book in English translation know its title as *Genesis and Development of a Scientific Fact* (Fleck, 1979). His German subtitle was mislaid along the way: *Introduction to the Theory of Thought-Style and the Thought-Collective* (Fleck, 1935). The two words, *Kollektiv* and *Stil*, were put to protein uses by German intellectuals in the early twentieth century. Art history, of course. In political economy and sociology, for sure. Also for example by Richard von Mises (1928) in his analysis of the uses of probability in statistics—and in 1931, in theoretical physics. These notions were ‘all over the place’ in those days, including some despicable ones (judische Denkstil). So beware!

As Fleck’s subtitle makes plain, Thought-Style and Thought-Collective go hand in hand. A Thought-Collective is a small network of investigators who address a family of problems that they understand in much the same way, and which they attack using a group of mutually intelligible methods. Fleck’s Thought-Styles were constituted by the types of questions asked, the range of possible answers that was envisaged, the methods which were useful, and the background information taken for granted. A Thought-Collective was a social unit identified by education, training, interests, and mutual communication. Thought-Collectives are local, cohesive, but relatively short-lived, for they tend to dissipate as questions become answered or problems prove to be intractable. People move on, and out of the collective.

We can extend Fleck and think of nested (also, interlocking) Thought-Collectives. Elwick’s study of British life sciences 1820–1858 (§5 above) can be parsed in those terms. A large Thought-Collective with shared assumptions spanned most of the workers he studied, while the competitions he describes were between rival Collectives within the British nest.

Now to the legends. Kant really did have the picture of a light bulb going on over the head of ‘Thales or some other’. It is never one person. It is always some small group, and there always has to be a larger uptake in order for any new way of thinking to stabilize. It is inviting to proceed using Fleck’s words: a small Thought-Collective emerges around a tremblingly new Thought-Style. We can use Fleck’s words without the implication that it is all in the head, ‘thought’. The reason that Fleck has been so attractive to sociologists of scientific knowledge is that they disregard the mental meaning of ‘thought’ and attend to the social relations between the actual protagonists, and their material experiments. Fleck brilliantly describes both in his study of syphilis. On another occasion, a Fleck-like picture may be offered for each of the crystallizations already mentioned. In *Leviathan and the Air-Pump*, the large Thought-Collective is none other than the Royal Society of London, within which Boyle’s team was nested. Hobbes wasn’t, and he tries to maintain an older Thought-Style.

13. Crystallization

The metaphor of crystallization is handy. Many readers of the *Emergence of Probability* (Hacking, 1975) have tried to undo its premise, that, to put it crudely, there was no probability before 1650 or so. A few of these helpful critics are mentioned in Hacking (2006). There is a long history of various kinds of probable reasoning—charted globally by Crombie himself, and locally by meticulous scholars (e.g. Maclean, 2002) in greater detail. There was, nevertheless, a radical change in the very feel of chance and of evidence that took place during the second half of the 17th century. It was a crystallization of ideas that had been inchoate for centuries, and which precipitated at that time to create the conceptions of probability that have come to dominate the modern world. That was not the end of the story (and here the metaphor of crystallization is defective). The *Taming of Chance* (Hacking, 1990) expounds some large scale socio-political events that helped bring us up to what we now call scratch.

The same metaphor of crystallization suits the discovery of demonstrative proof in the Eastern Mediterranean. There was no immaculate conception, for sure, but something radical did happen, a crystallization of old practices into a new way of finding out, namely by proving. This does not imply (as the metaphor of crystallization would do, taken strictly) that ‘mathematics’ became a rigid crystal. It is far from clear what makes mathematics mathematics (Hacking, 2010a), but it is certainly a long series of contingent events, which are still going on pace with, for example, the advent of computer-generated proof. There is much to be said for the iconoclastic opinion of Doron Zeilberger (2010): ‘Our mathematics is an accidental outcome of the random walk of history, and would have been very different with a different historical narrative.’ (Original italics.)

The metaphor of crystallization also fits the radical change when hypothetical modelling was mathematized by Galileo and his peers. Likewise, the transition from experimental exploration to the systematic creation of phenomena in the laboratory, at the time of Galileo and Boyle. This was such a radical transition that Hacking (1992) unwisely proposed that it was a new style of reasoning, a merging of experimental exploration and hypothetical modelling.

It is a strong temptation. Thus Bernadette Bensaude-Vincent (2009a, p. 18; cf. 2009b) proceeds in that direction, suggesting that we should ‘subdivide N2 in Crombie’s list in order to give more visibility to a specific style of thinking induced by chemical laboratory practices’. In my opinion, it is better not to multiply six beyond necessity, and instead to see the invention of the laboratory as making experimental exploration more ‘systematic’, to use Kant’s word. It is also to be noticed that the chemical laboratory is different from, and older than, the physical laboratory.

Mergers are often more interesting than splits. For example, the way in which historic-genetic reasoning in cosmology and life science is giving way to hypothetical modelling and experimental exploration in accounting for the origin of things. And although experiment has a life of its own, laboratory work without hypothetical modelling would be as worthless as the frantic collecting of Francis Bacon’s ant in his parable of *ant*, *bee*, and *spider* (*Aphorism XCV of the Magna Instauratio*). And hypotheses without experiment are like the spider. Scholars no longer appear to speak of the scientific method, but until quite recently it denoted the bee, who combines the talents of ant and spider.
14. The autonomy of Crombie’s styles

A central idea of LT&R was that scientific styles of thinking & doing are not good because they find out the truth. They have become part of our standards for what it is, to find out the truth. They establish criteria of truthfulness.

LT&R proposed that Crombie’s styles are self-authenticating. One point of the expression has long been trite. Epistemology used to search for foundations. Peirce and then Popper, and then a host of writers, have emphasized that knowledge has no foundations. But the quest for foundations goes on. The task of the logician, one might think, is to find a foundation for reason. ‘Self-authenticating’ might serve as an antidote. Scientific reason, as manifested in Crombie’s six genres of inquiry, has no foundation. The styles are how we reason in the sciences.

To say that these styles of thinking & doing are self-authenticating is to say that they are autonomous: they do not answer to some other, higher, or deeper, standard of truth and reason than their own. To repeat: No foundation. The style does not answer to some external canon of truth independent of itself.

It will be protested here: but the justification is pragmatic. These styles of reasoning work! That is the standard to which they answer. There is no external canon, of the sort demanded by correspondence theories of truth. But there are still standards, pragmatic ones.

Pragmatism is indeed a helpful first step away from foundationalism. But, in the constant quest for the final stop on the journey, it pulls up at that Victorian value shared by Marxist, Protestant, Capitalist and William James: the eternal value of Work.

Something works if it has results that were intended, or are to our liking. Technoscience has indeed enabled us to change the world, mostly to our liking. The maxim, that nothing succeeds like success, is a deeper saying than is usually understood. For success helps determine what will count as success. Success has a lot to do with future success because it helps characterize what in the future will count as success. We continue to change the world mostly to our present liking partly because what we like has been so profoundly affected by things we have grown to like and which are products of the sciences. The styles flourish in a complex web of interactions whose evolution they help determine. That is why the ecological metaphor already mentioned in §2 and §7 above, to be developed further in §20, is so inevitable.

15. Truthfulness

Bernard Williams’ Truth and Truthfulness (2002) is a work of profound originality on many fronts. Only one of its many facets will be used here. It suggests a way to explicate the autonomy of Crombie’s styles, in a way that is aligned to discussions of truth by analytical philosophers. That will make the notion more accessible to one class of readers, and less accessible to another. So be it.

Williams asserted that the concept of truth has no 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). Most analytic philosophers would agree. Many other serious thinkers, for example those rightly fascinated by Steven Shapin’s (1994) A Social History of Truth, would wholeheartedly disagree. I believe Williams might have thought the difference was more in words than content. He might have said of Shapin what he said of Foucault, that much of such ‘work addresses epistemological issues, of what at different times counts as establishing truth in different fields’ (Williams 2002, p. 300, n. 31). He denied that truth has a history, but said that criteria for telling the truth in a given domain change over the course of time. He expressed this by saying that truthfulness has a history while truth does not.

The styles project can follow Williams and say that truth has no history, while truthfulness—the possibility of telling the truth about a specific topic—does have a history. That is the idiom of analytic philosophy. One can recast the same thoughts using an alternative idiom. Thus for example where Shapin writes very clearly and naturally about a social history of truth, Williams can say that the criteria and practices of truth-telling have a remarkable social history: indeed he tells some of that himself in his inimitable way.

Williams did not find a distinction between truth and truthfulness; he made one. Contrary to the common self-understanding of analytic philosophers, most distinctions in analytic philosophy are made, not found. That is, they do not exist as facts in a pre-existing conceptual scheme which careful observation reveals. Here we can adapt the familiar Leibnizian image of the sculptor and his block of marble. No one seriously imagines that only one sculpture could emerge from a raw lump. What the analytic philosopher does is attend to one vein in the conceptual stone that is useful for a certain purpose, but there may be other veins in the marble that yield a different carving. Just as the sculpture is made by the sculptor, so a distinction is made by the philosopher. (Once again ordinary language—we say, ‘we make distinctions’—hits the right nail on the head.)

Williams took two ordinary words, ‘truth’ and ‘truthfulness’. One, together with its core cognate, the adjective ‘true’, is in use all the time. The other, ‘truthfulness’ is seldom used. He wanted a word connected with truth, but which reflects the way in which the truth in various domains can come into being. LT&R expressed very much the same idea by saying that with the emergence of a new style of thinking & doing, new sentences ‘came up for grabs’ as true or false. It would have been more prudent to add, ‘or are grabbed in new ways’.

This is connected with a related theme in LT&R. It urged that Quine’s notion of a conceptual scheme, which he explained as a set of sentences held for true, should be replaced by a set of sentences up for grabs as true or false. Thus emergence of a new style opens new spaces of possibilities—not an idea on which Quine would have looked with favour!

16. Shifts in conceptions of how to tell the truth

What use did Williams make of his new distinction? He claimed that Thucydides effected a fundamental change in the practice of history-writing. A new criterion for telling the truth about the past entered the field. Every event must be presented as occurring before, after, or at the same time as every other event. Many would describe this as a conceptual truth, but it is one that entered our system of historical thought at a specific time in the West (and at a slightly different time in China). It was a new criterion for truthfulness about the past.

Williams’ fundamental assertion is that there was, ‘most basically, a shift in conceptions of what it is to tell the truth about the past’ (p. 154). The styles project latches on to the logical form of this thesis, as expressed by the schema (⋆):

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

That sounds as if X is a given, X = the past, or, in the case of the emergence of authenticity, X = the self, the past and the self being timeless entities. Not so. 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 from what he says about history (pp. 170-1): This significant change that took place in the fifth century B. C.; It was the
invention in the West of historical time. It did introduce ‘an increase in explanatory power’. It does not, however, ‘mean that those who operate in the new style, who have the “objective” conception of time, are more rational or again better informed than the others’. Their predecessors were not confused; they did not believe something contrary to the later historians. Most importantly, ‘The invention of historical time was an intellectual advance, but not every intellectual advance consists of refuting error or uncovering confusion’.

The schema (*) can be expanded:

This significant change took place in the Y century, in connection with an emblematic figure Z.

In history, X = the past, Y = the fifth century BC, and Z = Thucydides. For authenticity, we have X = the self, Y = the eighteenth century, and Z = Diderot and Rousseau. Heroes in history! Legendary figures again.

Note how well the metaphor of crystallization fits Williams’ two cases. People had been writing histories for a goodly time before Thucydides, and some notion of authenticity was around long before Diderot. But it is widely agreed in both these cases that something significant in Western practice occurred around the two periods singled out by Williams’ analysis. In each case we have a crystallization within previous ways of going on. Whether Williams was right about history or authenticity, our concern is the general schema of a ‘shift in conceptions of what it is to tell the truth’ in some domain of inquiry.

If Williams is right, Thucydides is a legendary trailblazer, an icon around whom crystallized new ways of telling the truth about the past. There is an inviting parallel with ‘Thales or some other’. There was a shift in conceptions of what it is to tell the truth about geometry and of mathematics more generally. Proof became the gold standard for the truth of mathematical propositions. This significant change took place in the sixth century BC, and its emblematic figure is Thales. That’s Kant’s story. After reading Netz we might prefer to replace the legendary Thales by another figure, and move the date down a bit.

We are in effect asserting that there are distinct criteria of truthfulness in different types of domain. This well fits the verification principle (shades of Ayer, 1936 with which we began in §2). But it will sound repugnant to much modern analytic theorizing about truth. (I recall Donald Davidson thinking the project was amusingly ingenious but ludicrous.) Yet it has no problem with Tarski’s celebrated schema T, held to be the sine qua non of truth. It even gibes well with Crispin Wright’s (2003) pluralism, which holds that one can be a ‘realist’ about mathematics (platonism) and an anti-realist about history, or vice versa. Wright developed his pluralism from Michael Dummett’s notion of bivalence, deducing that truth conditions works out differently in different fields of discourse.

17. Ontological debates

There are innumerable questions about what exists, from the Higgs boson, to a slow leak in my bicycle tire. We know how to try to answer them, and in the first case have been willing to spend a fortune from national treasuries trying to do so.

Philosophical questions about existence are different in kind, for debate is interminable. New and sometimes fascinating things keep on being said, but impasses seem permanent. Checkable facts seem to have no bearing. Such questions, which will be called ontological, appear to be of three sorts. One type is sceptical. Does the external world exist? Are there other minds? These questions are asked by children about the age of six or seven, with little or no prompting. They may be called pre-scholastic.

A scholastic question arose in logic. Do universals exist, or only their instances? The positive answer was long called ‘realism’. In 1492 Spanish scholars named the opposite doctrine ‘nominalism’. This usage of the word ‘realism’ is no longer in fashion among analytic philosophers, having been replaced by realism as bivalence, as in Dummett’s school just mentioned.

Here we are concerned only with a third sort of ontological question, the one that arises in the sciences, and which preoccupies us here. The most longstanding example is about the abstract objects of mathematics, such as numbers, groups, topological spaces, or, in the first instance, shapes such as the circle, the sphere, and the polyhedron. Do such objects exist? The affirmative answer is called mathematical realism or platonism. (This species of ‘platonism’ is best written with a small ‘p’.) Do non-observable theoretical entities exist? The affirmative answer is called scientific realism.

The styles project may cast the third type of ontological debate in a new light. It suggests that the third type is a by-product of styles of reasoning. This may be therapeutic, but for those who want yes/no resolutions to each question, debate is interminable—one might almost say, incurable.

18. The by-product account of ontological debate

In §4 above, styles of scientific thinking and doing were said to be characterized by their objects and their methods of reasoning. Early on, the styles project maintained that each style of scientific thinking & doing introduces a new class of objects. Hence it is plausible to suggest that ontological debates of the third type are by-products of the introduction of a class of objects by a distinct style of scientific thinking & doing. But what is this ‘introduction’? This does not mean that objects of the class did not exist before there was a way to investigate them. That is nonsense. Each new style of thinking & doing introduced a new class of objects into discourse. We may bypass an explication of this sense of ‘introduction’ and say only that ontological debates of the third type are by-products of the emergence of a style of scientific thinking & doing. But it is important to avoid brief generalizations. Every style has its own personality: the way that arguments over the existence of species or of the higher taxa are engaged, are quite different from arguments about platonism.

In taxonomy, the reality of the higher taxa, or even of the species themselves, continues to be contested. Philosophical thinking about probabilities is littered with lesser debates about the reality of statistical entities. In at least these cases we may offer a diagnosis of the ontological disputes. It does not make them go away: there will always be platonists and anti-platonists. But for those who have difficulty taking the debates seriously, a diagnosis may be helpful.4 The ontological debates are by-products of the ways in which styles or reasoning introduce into discourse new classes of objects.

19. Cognitive history

Let us pass from the deserts of ontology where nothing flourishes but controversy, to pastures full of new growth. Netz subtitled his book about Greek deduction A Study in Cognitive History.

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4 Hacking (1983) took scientific realism seriously when it presented an experimental argument for realism about theoretical entities—as the best argument, not as a conclusive one. A remark on page 2 is seldom noticed: ‘Disputes about both reason and reality have long polarized philosophers of science [. . . ] Is either kind of question important? I doubt it.’ I continue to doubt it.
I take him to mean an attempt to understand how an organism with certain cognitive capacities developed sophisticated techniques of demonstrative proof in a corner of the Mediterranean 2400 years ago. In his own words, ‘proper cognitive history refers explicitly to the biologically cognitive facts out of which the particular bit of history is made, and provides an account for how such skills got to play out and be transformed through history’.5

Netz is frank about the current state of what he named cognitive history. When a student wrote asking him what he meant by the term, Netz replied by quoting Ghandi. When Ghandi was asked what he thought about Western civilization, he replied that he thought that it would be a good idea. Netz continued by saying that that’s what he and I think about cognitive history: it would be a good idea.6

In the case of mathematics, a cognitive history can draw on many recent results and proposals that can be identified in neuroscience, cognitive science, developmental psychology, anthropology, and history (including but not exclusively the history and prehistory of the sciences). There are whole new fields of great relevance, such as captured by a subtitle, Archaeology Meets Neuroscience (Renfrew, Frith, & Malafouris, 2009), going back to The Archaeology of Mind (Renfrew, 1982). But we have not yet begun to fight. What we have, even in the most favoured style of scientific inquiry? Much of ScottAtran’s unfashionable practice of the history of science. He began to describe himself as doing historical anthropology. That is a French rather than an English term, modelled on the work of the great classical scholar Jean-Pierre Vernant (1914-2007). In France, it has become a sub-discipline of historical studies, where numerous schools and institutes are so named. Hence it is a term not to be taken lightly and I shall not do so. Otherwise I regard all these disciplinary appellations as throwaway labels. Often on first sight they produce a sharp cry of recognition. Take the Archaeology of Mind just mentioned. It is the study of cognitive development of the human mind by reflection on the evolution of artefacts. We are to think not only about how humans made the artefacts, as in traditional archaeology, but also what the artefacts, once in use, did to the human mind. When the labels make us stop and think like this, that is a good thing. But later they tend to become slogans and should be put away. It is usually unwise to embed handles handy for a moment into the architecture of the human sciences.

There is no doubt that the styles project is anthropological in character. It is also ecological, for it is not just about what used to be called Man, but about what used to be called Man and his place in Nature. Styles of scientific thinking & doing in the European tradition are rooted in human biology and cognitive capacities. Each emerges in a tiny locale, and even then involves only a very few of the people who live nearby. The styles turn out to have had unparalleled effects on life on Earth. The study of these styles can thus be filed as a case of ‘ecological imperialism’. With an important difference: In the end the Europeans in the ‘European tradition’ do not matter, and will even cease to exist as an identifiable group. The styles of scientific reasoning do matter, and will continue developing long after Europe is just a relatively small land-mass.

21. On the cumulative character of styles

In the whig history used by the styles project, styles of thinking & doing in the European tradition are cumulative. This is a virtual necessity, for we define them retrospectively, in terms of how we got to here. But there is far more to accumulation than that. It is hinted at by phrases such as ecological history. The styles are adapted to their environment, but like any creature they also change the environment to their advantage. Latour’s riff on Archimedes is perfect. Give me a laboratory and I shall move the world. LTR&B offered Paracelsus and the Doctrine of Similitudes as a style of reasoning that we can scarcely comprehend in all its richness and what we take to be absurdity. It is no longer with us, except in pale and ignorant shadows that pass themselves off as homeopathic medicine. The pragmatist in all of us wants to say that the scientific styles persist because they work—and Similitudes did not. But Paracelsian medicine did not die because it did not cure people. A commonplace nowadays: doctors never cured people until late in the 19th century. We do not well understand the switch from (to use one possible description of what happened) what Foucault called the épistème of the Renaissance, to that of the era of Representation and what he called the Classical Period or what others call Early Modern Europe. But it certainly was not because something did not work.

But yes, the genres of inquiry central to the sciences do work. But work for what? In part, for purposes of their own devising. ‘Success’, as was said in §14, helps direct what in the future will count as success. In our case the effect is heightened, because success changes the world so that success of that kind is easier to achieve. This returns us to ‘self-authentication’. The styles in our list do not answer to any criteria of truthfulness other than their

5 Quoted by permission from an email of 26 January, 2010.
6 Paraphrased by permission from an email of 10 February, 2010.
own. They are not ‘chosen’ because they ‘work’. They help determine what counts as working.

But let us not continue to be so obsessed with that notion of work, which so captured William James, or even survival, which so captured Charles Darwin. There is also play, which is something that the survivors do in the world they are making, and in order to keep themselves sane.

22. Work and play

For all its ultimate horrors, Europe in the 1930s produced a lot of good ideas of a very general kind, some of which long lay fallow. One is Fleck’s Thought-collective/Thought-style, published 1935, but activated only much more recently. Johan Huizinga’s Homo Ludens (1949), dating from 1934, is still fallow. It is not about the role of play in culture, but about culture as in part constituted by play.

One of the reasons that the items in Crombie’s list—like so many other communal phenomena—have been taken up over and over again is their playfulness. People practice those genres of inquiry not just to find out more about the world, or how to change it to our liking, but also for fun. And here I mean good clean simple fun for its own sake, child’s play. Some cynics ridicule science as a game, but I do not mean anything highbrow, I mean fun.

Once again Reviel Netz is helpful. In Ludic Proof (2009) he argues that Archimedes, the greatest mathematician of the ancient world, made playful competition an integral part of his mathematical correspondence around the Mediterranean. That said, play could still be thought of as an element in proof—which nobody would deny. Following Huizinga, we may suggest that play is not only an enjoyable aspect of some proofs, but also is integral to the practice of proving, proof-following, and proof-seeking. Not just in the times of ludic proof, but right now.

Huizinga concluded his book with an aphorism: ‘civilization is, in its earliest phases, played. It does not come from play like a babe detaching itself from the womb: it arises in and as play, and never leaves it.’ Could we conjecture that proof was, in its earliest instances, played? And is this not an element in the origin of every style of thinking & doing, not only at the beginning but in the reason why people continue? Robert Boyle invested a private fortune and the Crown spent a public fortune on the Air Pump. Certainly not for its commercial benefits. Certainly out of curiosity, but the half of curiosity, a fundamental attribute of the human species, is play. As Shapin and Schaffer tell us, the Air Pump soon became a gentleman’s toy, which could be bought from a good maker for five guineas.

That may be an important corrective to characterizing scientific styles of thinking & doing as ways of finding out. An enormous amount of scientific activity is pursued because it is playful.

23. To do

Many topics arise. Some things to do have already been pointed at. Another is relativism. LT&R was written for Rationality and Relativism. It promised to introduce a whiff of relativism from the heartland of rationality. It did just that. Many are vexed by relativism, whatever that is. Those who most proudly and loudly assert foundationalism or relativism have been Big Problems for me, and I tend to regard them as ‘sophomoric’. At any rate, it is best to avoid sweeping generalities and get down to discussable details.

Barry Allen (1993) uses what he calls ‘denomology’ as a sort of counterexample to my approach. If Crombie’s styles are self-authenticating, why not witchcraft too? Others have used what they call ‘magical thinking’, be it in traditional religions or as subcultures of industrial societies. Relativism, or reductio ad absurdum of the styles project, take your pick. This is a sufficiently focussed question to which the project can reply. For reasons of space, it will have to do that elsewhere.

There is much else. Work on the project in print says almost nothing about the second group of three styles on Crombie’s list. They are not even discussed in the Taiwan lectures (Hacking, 2009) which are in any case not generally available for copyright reasons. They were taken up in a series of lectures in Mexico, some 40,000 words that need to be put in better order. This will bring out the extent to which every style is fundamentally different from every other. Each has its own ‘personality’. In §4 we quoted Crombie as saying that his second three styles concern ‘the science of the regularities of populations ordered in space and time’. Much more importantly they have very different relationships to truth than members of the first group. For example, we do not say that a taxonomy is true, so much as apt or explanatory or robust. Moreover, the ways in which at least some of the styles introduce new classes of objects into scientific discourse are fundamentally different from each other.

Then there are more speculative topics that derive from ideas of cognitive and then ecological history. For example, everything in this project takes for granted that there is such a thing as Nature studied by Man. Philippe Descola (2005) convincingly argues that this notion of Nature apart from Man is anomalous in human culture. Pierre Hadot (2006) takes a long view of the idea of nature from Heraclitus to now. It shows how potent has been the idea that nature has secrets—to be found out. And that is what the styles are up to: they are different ways of finding out.

To what extent have the styles of scientific thinking & doing determined our anomalous relationships with nature? How much of their fascination, in work and play, is the channelling of human curiosity into finding out the secrets of nature, whose history is narrated by Hadot? To what extent are the anomalous relationships a precondition for the emergence of styles of scientific thinking? These are among the more interesting topics to be addressed.

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7 The English title of the edition referred to here is Homo Ludens: A Study of the Play Element in Culture. It is translated from the German translation from the Dutch, 1940. Huizinga himself insisted he meant ‘The Play Element of Culture’. For the definitive Dutch edition of 1950 see the Netherlands Digital Library DNBL: http://www.dnb.nl/tekst/huiz003homo01_01/