THE LABORATORY STYLE OF THINKING AND DOING
Ian Hacking
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The third in a series of lectures in Taiwan, the preceding ones being “The roots of scientific reason” and “Where do mathematical objects come from?” at the Experience and Truth Conference, National Taiwan University, 10-11 November, 2007.

This lecture comes in two parts. The first, long, and thought-out part develops themes in my previous two lectures, especially the one on “scientific reason”. The second part, called “The Biotechnical Laboratory” is a response to Professor Chen, my host at this STS workshop. He wrote me that “Some STS friends suggest that I should ask you to talk a little about your ‘imagination’ on East Asian techno-science at the end of your lecture to the STS community. They are looking forward to hearing your vision on this topic.”

I intend that Part I of this lecture will stand on its own, so that it will make sense to people who were not able to come to my lectures in Taipei last Friday and Saturday. On the other hand, I do intend it as a development of ideas set out at some length in my first lecture, on “scientific reason”. I am not sure whether Part II will stand up at all. It consists of half-baked ignorant thoughts.

I. THE ORIGINS OF THE LABORATORY STYLE

One starting point for the first lecture was a philosophical adaptation of the idea of a historian of science, an Australian who pursued his career in Oxford, England. He was primarily a scholar of European sciences in the high Middle Ages and the Renaissance; his hero, like that of many other scholars, was Galileo. But contrary to popular wisdom, he thought that the so-called “scientific revolution” of the seventeenth century was not an autonomous event of that period, but rather an evolution of developments that began to flower in the 12th century. At a time when the influence of Bachelard and Kuhn was dominant, when philosophers and historians saw the past as a sequence of mutations and revolutions, he saw plenty of continuities and resumptions. His life work was summed up in 3 enormous volumes, Styles of Scientific Thinking in the European Tradition, published near the end of his life, in 1994. In each case he traced the trajectory of some six styles of scientific thinking from the ancient Mediterranean world to modern Europe.

We can distinguish in the classic scientific movement a taxonomy of six styles of scientific thinking […] distinguished by their objects and their methods of reasoning. Notice that we have three words here, styles, which are distinguished by their objects, and by their methods of reasoning. In the case of mathematics, we are familiar on the one hand with a distinction between mathematical and other methods of reasoning, and on the other, with a distinction between the abstract objects of mathematics, and the objects of everyday life.
I made the claim that each style of thinking introduces a new class of scientific objects. I proposed, moreover, that the seemingly unrelated ontological debates about abstract objects, about theoretical nonobservable objects in physics, or about the taxa of systematic biology, are all by-products of the introduction of new types of objects in the course of the emergence, acceptance, and use of a new style of reasoning within specific communities. Do not get the idea that first we have a style of thinking, which then introduces a new type of object and a new method of reasoning. The styles are constituted by their methods and the type of object with which they deal.

**Experimental exploration and hypothetical modelling**

Crombie’s first style was mathematical, the topic of my lecture on Saturday. In that lecture I noted some fundamental differences in the developments of mathematics in Ancient China and Ancient Greece, and proposed that the latter gave rise to a Western obsession with mathematical knowledge, truth, and objects, that is unknown in other philosophical traditions, I suggested this was partly due to the focus on *perspicuous proof* in canonical Greek mathematics, while Ancient Chinese mathematics was more concerned with process, and developed a remarkable series of techniques of approximations.

Crombie’s 4\textsuperscript{th}, 5\textsuperscript{th}, and 6\textsuperscript{th} styles have to do with populations and classes; we could call them taxonomic, probabilistic, and historico-genetic. I shall say nothing about them today. But there is lots to say!

I first heard about the “six styles” from Crombie at a conference in 1979, and that started me thinking anew about the sciences; the first sign of my uptake was published in 1982, and I have been thinking about these matters from time to time ever since.\footnote{Ian Hacking, “Language, Truth and Reason,” *Rationality and Relativism*, ed. M. Hollis and S. Lukes, Oxford: Blackwell, 1982, 48-66. “’Style’ for Historians and Philosophers,” *Studies in History and Philosophy of Science* 23 (1992), 1-20. Both reprinted in *Historical Ontology*, Harvard, 2002.} Today I shall be concerned with his second and third styles of thinking, and with a combination of the two. I call it the laboratory style of thinking and doing. Here are the brief descriptions of the second and third styles that I heard almost 30 years ago:

1. Experimental measurement, and exploration of more complex observable relations.
2. Hypothetical construction of analogical models.

**Recapitulation of 3 cautions**

Before proceeding, I shall repeat three cautions from previous lectures.

(a) Styles of scientific thinking are not sciences or scientific disciplines, and they are not mutually exclusive. Evolutionary biology uses lots of (1) mathematics, (2) measurement and experimental exploration, (3) hypothetical modelling and analogy, (4) taxonomy, (5) probability and statistics, and yet it is our most viable example of a (6) historico-genetic science. Most modern sciences use most of Crombie’s styles of scientific thinking.
(b) I postulate that each of Crombie’s styles is grounded in innate human capacities, which are discovered, exploited, and developed in specific historical situations. Thus they are the product of both cognition and culture; they are the products of interactions between on the one hand unique human endowments which are in turn the results of our evolutionary heritage, and on the other hand, specific historical events and developments. In virtue of the innate cognitive element, it follows that even if a style of thinking evolved in the first instance in a unique historical culture, it can later be acquired by people of any other culture who choose to do so.

(c) Styles of scientific thinking change, evolve, divide, and unite, in complex historical ways. We do not do mathematics today in the manner of the so-called “Nine Chapters” of classical Chinese mathematics, with their commentaries, worked out over centuries between 2000 and 1000 years ago. Yet at the same time we recognize that work as “mathematics”. Moreover, Crombie’s list of styles is not the last word. Thus I myself, working within the Western tradition, distinguish two core mathematical styles of thinking, one geometrical, whose early roots are handily located in or near Greece, and another, algorithmic, whose origins are conveniently located in or near Baghdad. There I divide. Today I unite. I want to emphasize the putting together of styles (2) and (3) to generate a new style of scientific thinking. It turns out to be the one that has had, and is continuing to have, the most important influence on human interactions with nature.

**Exploration and analogy: styles (2) and (3)**

Neither of Crombie’s styles (2) and (3) is peculiarly “European”. One way to think about the history of Chinese astronomy, for example, is to regard it as a deployment of these two styles of scientific thinking from earliest times, namely measurement and observation of complex relations on the one hand, and, on the other hypothetical modelling. The modelling was typically of the movements in the heavens, but I also count speculations about the atomic nature of the world as hypothetical modelling. The difference is that that astronomical modelling was controlled by observation, while atomism in earliest times was uncontrolled speculation by imaginative philosophers of nature.

Whereas only a few civilizations have developed much that I would dignify by the name “mathematics” I believe that a great many have engaged in various kinds of measurement, often using parts of the human body as the standards for transportable measuring devices, for example the foot, the arm from elbow to finger tips, or the length of the human thumb. Likewise many civilizations made models of their environment by analogy and guesswork. But a particular way of combining the two emerged, I suggest, in a quite specific time and location, in the 17th century in Europe.

It is not until page 1087, in the second volume of his book, that Crombie states in a single sentence what made this event:

The particular intellectual and artistic ambience of early modern Europe came to make (3) the method of hypothetical modelling a characteristically effective scientific combination of theoretical and experimental exploration.

I shall say little about the artistic aspect, on which Crombie lays much weight. But we must not forget that an important part of the local, contingent, historical sequence of events lies in the developments in architecture and in the development of specific techniques of perspective
representation developed in Italy and Flanders. It is also connected with a theological vision of the world. God is the divine architect, and Man, in trying to understand the world, can do no better than try to figure out how God did it. Leibniz called theoretical modelling *architectonic* reasoning. The Frontispiece to Crombie’s three volumes shows a drawing of “God the measurer,” God as the architect of the universe. This is taken from a late 13th century French Bible, illustrating the first line of the Old Testament, “In the beginning God created Heaven and Earth.” God is standing over an ill formed spherical universe holding a standard architect’s tool, what is called a pair of compasses, a device for drawing circles and measuring or transferring distances.

**The hypothetico-deductive method**

It may sound as if Crombie is describing something very familiar to logicians of science, namely hypothetico-deductive reasoning. Twentieth century textbooks on the philosophy of science, especially those of a positivist or empiricist inclination, spoke of the hypothetico-deductive method as the core of scientific reasoning. One makes a hypothesis or conjecture $H$, which may contain references to theoretical non-observable entities. One deduces observable consequences, often in the form of a conditional proposition: If circumstances $C$ obtain, then result $R$ is produced. One arranges an experiment so that $C$ obtains, and sees whether $R$ comes about. If yes, then the hypothesis is confirmed (Carnap) or corroborated (Popper).

In my opinion this account completely ignores most interesting aspects of experimental work, but it has the great merit of being logically transparent. The logical form applies well in the most banal circumstances, when no theoretical or unobservable entities are mentioned. It is the common sense of humankind, not science. For example, I hear a noise every evening behind the walls of my bedroom. I conjecture that there are mice living there. I leave some cheese out, and reason that if there are mice in the walls, the cheese will be gone by morning. The cheese disappears, all but a few crumbs. I conclude I have mice indeed. The conclusive verification comes when I set a trap and catch the observable dead mouse. Of course the inference may be mistaken. Exactly the same noise continues after I have killed some mice. Oh dear, I have birds nesting in the walls ….

Hypothetico-deductive reasoning, as thus far explained, is certainly not a discovery of early modern Europe. I imagine that people have been using that method of reasoning for as long as they have been able to talk, and arguably it is used by animals. It is what Charles Sanders Peirce, the great American pragmatist, called “abduction”, and more recent writers have called “inference to the best explanation”. Peirce thought that logic has three parts, deductive, inductive and abductive. This is a statement of fundamental importance, not original with Peirce, but more clearly understood, and more succinctly stated, by him than by any predecessor. As I said in the first lecture, I do not count any of these three as a style of scientific thinking; I hold them to be universally practiced by human beings, even if their codification in systems of logic is relatively recent. I file them with our evolutionary heritage. Inference to the best explanation is not what Crombie was talking about. It is not a cultural discovery of early modern Europe, or of ancient China or ancient Egypt. Logic, to repeat what I briefly asserted in the first lecture, is a human universal, and what is universal has three aspects, deductive, inductive, and abductive.
The Galilean style

I shall slightly reorganize Crombie’s account of styles (2) and (3). First I shall propose that around the time Galileo the method of analogy and hypothetic modelling underwent a remarkable sophistication that amounts to a mutation. Then I shall argue that a new style of thinking and doing emerged, what I shall call the laboratory style. The first event produced what I shall call the Galilean style.

I did not make up the name. Steven Weinberg, the Nobel prize-winning cosmologist, recalled Husserl speaking of a Galilean style for “making abstract models of the universe to which at least physicists give a higher degree of reality than they would the ordinary world of sensation.” Weinberg found this astonishing, “for the universe does not seem to have been prepared with human being in mind.” I am going to pass by that observation, which points to a nagging question. How come we are so good at modelling the complex processes of nature? I once made too strong an argument, that we have evolved processes that are what I called “self-vindicating.”

To return from metaphysics to cosmology, the grammarian Noam Chomsky picked up Steven Weinberg’s remark, urging that “we have no present alternative to pursuing the ‘Galilean style’ in the natural sciences at least.” The historian I. B. Cohen then went on to compare the Galilean style, with reference to Husserl, and to what he called Newton’s style. Despite the references to Husserl, this very specific sense of a “Galilean style” seems to have been devised in Harvard Yard in the late 1970s, rather than by Husserl in 1936. But although Husserl did not use the label in quite this way, his long discussion of Galileo in the Crisis gets to the heart of the matter, namely the use of mathematical models to comprehend the universe, both of the heavens and more importantly on the earth, and finally to guess that the heavens and the earth work by the same mechanics, as described mathematically. Husserl took this to be a fundamental moment in the history of European civilization: the mathematization of the world. He had the idea that only if we could recover the moment in their history could European break free of the disaster that was about to befall them. I cannot share that idea, but it reminds us how seriously Husserl took it.

To hark back to Crombie’s emphasis on the theology, never forget that Galileo insisted that God wrote the Book of Nature in the language of mathematics. Galileo saw himself as engaged in what Leibniz was later to call architectonic reasoning.

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2 “The Self-Vindication of the Laboratory Sciences,” in A. Pickering, Science as Practice and Culture (Chicago: University of Chicago Press, 1991), 29-64. I am told this paper was recently translated, in Taiwan, into Chinese.

3 References to Weinberg, Chomsky and Cohen’s discussions are found in my two papers on styles reprinted in Historical Ontology, Harvard, 2002, specifically on pages 162 and 179.
The fact that Weinberg and Chomsky invoked the Galilean style is instructive. For they are not simply great “scientists”. They are scientists in wholly different fields, who share one thing in common. To exaggerate, neither can tinker with the phenomena that they investigate, they can only observe. In that respect they are like the astronomers of ancient times. One man is a cosmologist, the other a grammarian. Traditionally, all you can do in cosmology is make models: you can not make experiments on the cosmos. Weinberg is the author of a wonderful book, *The First Three Minutes* – that’s the first three minutes of the universe, and we cannot experiment on that, although our cosmological models of course are informed by innumerable experimental results. But to exaggerate and even to parody, in cosmology all you can do is make models and compare the consequences of your models with observations. Likewise, in grammar, you cannot experiment (again I exaggerate) and you can only compare the predictions of your grammatical models by observing what people say or are willing to say. In short, cosmology and grammar are paradigms of non-laboratory hypothetical science, Crombie’s style (3).

I. B. Cohen, the historian, made a more detailed observation that we can tie in with Crombie’s idea of a seventeenth century combination of methods of reasoning. Cohen spoke of “two levels of ontology”, one of mathematics, the other of mensuration. Recalled that Crombie’s style (2) is the method (in part) of measuring. His style (3) is the method not only of modelling but also, with Galileo, as Husserl insisted, the method of mathematical modelling. I do not much care, for a moment, whether one says, with Cohen, that this is a combination of two levels of ontology, or with Crombie, a combination of the methods of two styles of scientific thinking. The point is combination.

**Forms of Life**

As this is a workshop in Science, Technology, and Society, everyone will be aware of the work of Bruno Latour. In my Saturday lecture on mathematics, I quoted an as yet unpublished essay by Latour, in which he discusses a book about ancient Greek mathematics by Reviel Netz. Latour said that,

This is, without contest, the most important book of science studies to appear since Shapin and Schaffer’s *Leviathan and the air pump*. I take it that a workshop on science studies is also well acquainted with this book, whose subtitle is *Hobbes, Boyle, and the Experimental Life*. I said on Saturday that I completely agree with Latour about the two most important books of STS published during the past 21 years: yet that I do so for reasons completely opposite to his. I explained that in connection with mathematics. Today it will become clear why I also so admire this work about experiment, but use it for ends entirely different from Latour’s.

You can get a glimpse of Latour’s interest in the book from the subtitle of the translation of the book into French, which Latour enabled through his own publisher. No longer is it subtitled *Hobbes, Boyle, and the Experimental Life*. That is turned into, *Hobbes and Boyle between Science and Politics*. There is nothing wrong with that, but it does redirect attention. The

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English title, by the way, gives a hint of a central theme in the book. The authors make heavy use of Wittgenstein’s phrase about forms of life. They see themselves as telling how and when an experimental form of life came into being. They teach that Boyle's experimental programme was, in Wittgenstein's phrases, a new “language game” and a new “form of life.” They say that, we will make liberal, but informal, use of Wittgenstein’s notions of a “language-game” and a “form of life.” We mean to approach scientific method as integrated into patterns of activity. Just as for Wittgenstein “the term ‘language game’ is meant to bring into prominence the fact that the speaking of language is part of an activity or a form of life,” so we shall treat controversies over scientific method as disputes over different patterns of doing things and of organizing men to practical ends.” (p. 15.)

The experimental program was, in Wittgenstein’s phrase, a “language-game” and a “form of life.” (p. 22.)

I am too cautious a reader of Wittgenstein to follow them in using his words, but it is a valuable direction to contemplate. By the “experimental programme” they mean more than Boyle’s lab in Oxford; they imply the programme of experimentation developed in the European seventeenth century. Experimental exploration, in a more loose sense of the words, evolved in many societies at many times around the globe. If I were to choose to use Wittgenstein’s words for my own purposes, I would suggest that the introduction of the laboratory style of scientific thinking deployed a new language game, within a novel form of life.

As the style evolved, other “language games” came into use. If we think only of the published texts in scientific journals and the like, there are excellent studies of that evolving genre. The Internet has changed everything again. Pre-publication online is the norm, and even journals of record pre-publish articles online, sometimes months before they are in print. I doubt that Wittgenstein would have called these language games, but his phrase is now out there, for anyone to use. The trouble is that it is so powerful a phrase, that it conveys a false impression of deep understanding, and of knowing what you are talking about. Some, like S&S, use the phrase with precision. Most do not. I prefer to leave it to the historical Wittgenstein.

Likewise one may say that the “forms of life,” in which the laboratory style is practiced, have changed. We may tend to emphasize research laboratories too much. The laboratory has, since the eighteenth century, been an arm of commerce and industry. It has effect ed radical changes in geopolitics: witness the development of German chemistry through the mastery of synthetic dyes in the nineteenth century, which, with parallel developments in steel and munitions processing, would, were it not for political error, have made Germany a far more dominant player in world history than it became.

Even the research laboratory has undergone immense changes. Much has been written about “Big Science” epitomized by the Manhattan Project and its consequences to the administration of American science. More recently biotechnology has totally changed the scientific landscape and of the forms of life experienced therein. I shall return all too briefly to that point at the end of this lecture. Now I take up the narrative of the Air Pump where I left off.

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A new actor: not a person but a piece of apparatus

Bruno Latour wrote an important review of *Leviathan and the Air Pump*, which was a prolegomenon to his own book soon to appear, *We Have Never been Modern*. I too reviewed the book the next year, drawing a very different lesson from his. I want to reemphasize that the lessons I draw from the two most important contributions to science studies in the past 21 years, are not incompatible with Latour’s. Not at all. They are profoundly different emphases. It is the mark of greatness in an author that different readers can learn different things from the same words.

In my review, written nearly two decades ago, I was absolutely astonished by what I learned from Shapin and Schaffer’s book. I shall so often speak of their book in this lecture that I shall refer to them as S&S. I admired them because their book was really new; as I put it then, they emphasized “a new kind of character, a new kind of place, a new kind of writing, a new kind of fact and a confrontation whose denouement was revolutionary”.

“The new protagonist is not a person but a piece of apparatus. There had been plenty of instruments for measuring or examining phenomena more minutely. The hero of this book is something new, a device that creates effects that didn't previously exist in isolation. It inaugurated laboratory science. Before the air pump, one aimed at solving the phenomena given in nature, usually in the heavens. Afterwards a new kind of science answered to a new master, the phenomena that fleetingly exist by artifice.

“The new place discussed in this book developed into the laboratory itself, the site for the manufacture of phenomena -- or, if that sounds too paradoxical, for their purification. The laboratory was to be a space at once open and shut. It had to be public because according to the doctrine that evolved, any work done in a laboratory can be done by anyone with adequate skills and checked by anyone who is a good observer. It had to be private because only a self-selecting few could know what was going on, make anything work, or even tell when apparatus was working. The nascent laboratory engendered the future style of scientific prose, writing that had to stand in for witnesses. At first it had to make you think you were really there. The effect was achieved not by adding local colour but by deleting it. The new writing was persuasive because it was presented in a plain and unadorned way, as if it were an exact description of what anyone could have seen, without interpretation. It dealt in matters of fact, and did so by making us think that matters of fact just come with the territory, beyond controversy. This book is in part about how it became settled what counts as "matters of fact". They really do come with the territory -- viz., the laboratory floor.”

I was primed to read S&S in this way because of my book *Representing and Intervening*, published in 1983. The second half of the book, *Intervening*, was a plea for philosophers to take experiments seriously. Philosophy of the sciences, especially the physical sciences, had for decades been totally dominated by theory. Just think of Carnap, or Popper, or Kuhn, or van Fraassen. Experiment was a mere adjunct to theory. Popper merely asserted openly what every contributor tended to take for granted, that the experimenter cannot even begin until the theoretician has done his work. Experiments were for verifying or corroborating or articulating


theories, or for showing that they were empirically acceptable. I saw myself as starting a “Back to Francis Bacon movement,” little knowing that it was well under way. Latour and Woolgar had already published their ethnography of the laboratory, and Shapin and Schaffer were completing *Leviathan and the Air Pump*. Next year Peter Galison was to publish *How Experiments End*.

**The new place: The laboratory**

Words are often useful signals of events. In European history, the word “laboratory” entered major European languages soon after 1600, perhaps 1605 in English, and 1620 in French. It is first of all a place, a piece of architecture, to quote the first definition in the *Oxford English Dictionary*, which arranges its definitions chronologically, it is “a building set apart for conducting practical investigations in natural science, originally and especially in chemistry, and for the elaboration or manufacture of chemical, medicinal, and like products.” The equivalent French dictionary, *Le Trésor de la langue française* says, “Premises equipped with installations and apparatus necessary for manipulation and experiment, performed in the framework of scientific research, or for the analysis of medicines of materials, for technical tests or for scientific and technical education.” S&S note, among other things, the rapid increase in the number of places identified as laboratories in London soon after 1600. This indicates that a new word was in use, but also that a new kind of place was coming into being. It is an outgrowth of alchemical research. S&S contrast laboratories with alchemical cabinets. The new laboratory is supposed to be a public space (p. 61). It is not a cabinet within which men explore the secrets of nature and keep their findings secret. Yet at the same time, as S&S emphasize, these new places were not open to any public, but only to an elite whose membership became defined as the members of the new scientific societies, their associates, and their employees, and aspirants hoping to join their ranks.

I like certain words in the French definition of the laboratory just quoted. A laboratory is a place equipped with installations and *apparatus* necessary for *manipulation* … The part of my book dedicated to experimental science was called *Intervening*, and intervening is close kin to manipulating. As I went on to emphasize in chapter 13, it is also a place for the “creation of phenomena” using apparatus especially constructed for that purpose. That is exactly what Robert Boyle was doing with his Air Pump. He was creating a phenomenon almost never in existence in a reliable reproducible form before, namely a vacuum in a container. His was not the first contained vacuum. Torricelli had reasoned that if you take a column of liquid contained in glass up a mountain, and observe the level of the liquid fall, you are observing a void, an empty space above the liquid. There were the famous spheres of Magdeburg.

It is quite astonishing how much effort and money early Europe was prepared to invest in creating a vacuum, which had no conceivable practical value. Boyle put his own fortune into it, and the British Government treated it as a major research enterprise and funded it generously. Other instruments also were richly funded, for example the chronometer, but that was of straightforward commercial importance. It would enable British ships to navigate the globe more reliably. They would reach distant parts and return with loot from what they saw as the West, the South, and the Far East. The prosperity and industrial development of modern Europe depended on trade and then a colonial empire, and hence on the chronometer for determining latitudes. But it is hard to see the practical point of spending a large amount of national treasure in making a better vacuum.
The creation of phenomena

When I wrote about the creation of phenomena I realized this would seem like a gross exaggeration. I said, that we bring into being phenomena that did not exist anywhere in the universe before. But I had to temporize, allowing it might be better to say that we purify phenomena, or we realize phenomena. I said that the laser was a new kind of phenomenon that simply did not exist before 1950 anywhere in the universe. Numerous physicists protested, but I think there is a growing awareness of how much this way of looking at things makes sense. In my own present hobby, very cold atoms, and what is called Bose-Einstein condensate, we can now read statements like this: “This state of matter could never have existed naturally in the universe. So the sample in our lab is the only chunk of this stuff in the universe, unless it is a lab in some other solar system.” Well, that was in a proud press release from the first laboratory that produced this new state of matter, Bose-Einstein condensate, in July of 1995.⁹ To call a phenomenon that exists for such a short length of time in extraordinarily artificial conditions a “new chunk of matter” sounds like an exaggeration, even to me. Within the year another laboratory had produced the same state of matter, and by now, twelve years after the event, many labs are doing so, for example Professor Yu’s laboratory at National Tsing Hua University, or Professor Han at Chung Chen University.

This may seem a long way from Robert Boyle and the air pump for creating a vacuum. I see it as its confirmation, its culmination. The old alchemists dreamed of transmuting base metals into gold. They made many empirical discoveries in the course of their vain experimental explorations. Boyle transmuted the dream of transmutation into the creation of new phenomena. And now we have achieved the alchemists’ fantasy. In 1995 we were able to transmute a substance, in this case the element rubidium, into a new state of matter, Bose-Einstein Condensate, not a gas, not a liquid, not a solid, but something new in the history of the universe.

What Thomas Hobbes saw clearly

Sometimes it is the first step that counts. Boyle is my iconic representative for the first step. Many, although not very many, workers around the same time, in different parts of Europe, were doing the same thing. I choose Boyle for two reasons. One is wholly fortuitous, that S&S have written that book. The other is not. One man, and one man only, saw just what Boyle was doing, and protested vehemently. That man was that old curmudgeon, the philosopher Thomas Hobbes.

S&S show that Boyle’s work was contested at the very outset, on both scientific and philosophical grounds. The figurehead for the confrontation was the other man in the subtitle of their book, Hobbes, Boyle and the Experimental Life. Hobbes was the aging philosopher, the veritable creator of the European theory of the state. His book that inaugurated modern political science, Leviathan, furnished part of the title of the book by S&S about the air pump. Now Latour, like all sensible people, sees Hobbes as the author of Leviathan, and, as such. the spokesman for a new age, a new kind of political society. In Canada, and most of the English

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⁹ “Physicists create a new state of matter at record low temperature. Joint release by the National Institute of Standards and Technology and The University of Colorado, 7/13/95”, still to be found on-line.
speaking world, every freshman studying political science has to try to read Hobbes, which is by no means easy, for the language now seems archaic or unintelligible to children weaned on television. Latour brilliantly read S&S as displaying the way that so-called modernity began, with a division between the social and the natural, with one body of expertise and intervention pertinent to one, and a quite different one germane to the other. Latour rejects the distinction in favour of what he calls Cosmopolitics, and he campaigns for a Parliament of Things. Here I have a much more modest project in hand, and make much more modest use of Hobbes.

In 1660, Boyle was 33 and Hobbes was 72, which happens to be my age. If I disagreed, on fundamental principles, with a brilliant, well-endowed, and well-supported young man of 33, how could I possibly win? I would be dismissed as an old fogey, and probably rightly so. Boyle was the wave of the future. One important contribution of the S&S book is that in an appendix Schaffer has translated what had previously been an obscure Latin pamphlet written by Hobbes, denouncing Boyle.

Hobbes saw exactly what Boyle was doing, and he hated it. He foresees that laboratory apparatus for generating phenomena was radically new. He was dead against it. This was not a quarrel about the relative weight of empirical evidence as against deductive proof. The question was more profound and more consequential. What shall be evidence? Is it to be what we find among us, bring home from abroad, chart in the skies -- or is to be what we make with laboratory apparatus?10

Are there not enough, phenomena Hobbes asks, already “shown by the high heavens and the seas and the broad Earth?” His interlocutor replies that “There are some critical works of nature, not known to us without method and diligence, in which one part of nature, as I will say, by artifice, that is, produces its way of working more manifestly than in one hundred thousand of these everyday phenomena” (p. 351). “Artifice” is exactly the right word. Boyle was inventing clever apparatus, artificial devices, to achieve a vacuum. The importance of mechanical devices in the scientific imagination of early modern Europe has long been emphasized. There was a famous clock in the city of Strasbourg in which artificial men paraded around telling the hours. In my telling of the story, the Air Pump put artifice to new ends, namely the creation of new phenomena. Hobbes saw that, feared it, and hated it.

The interlocutor states one fundamental, and almost never stated, rationale of laboratory science. It was as valid in the 1660s as it is in our present decade, 450 years later. “Such are our experiments, in which one discovered cause can be fitted to an infinite number of common phenomena.” Hobbes cynically asks, “and what are they,” these common phenomena infinite in number? Today we might add, as sardonically as Hobbes, “and tell us more about how you perform this wonderful act of fitting a common cause to your wonderful new phenomena!”

In the same dialogue, Hobbes was equally prescient about the authority of the laboratory itself. Gresham College, which became the Royal Society of London, the mother of all modern scientific academies, prided itself upon the open demonstration of phenomena to witnesses. Thanks to that, everything was public in order that there could be no doubt about what happened. Hobbes nastily asks, “Cannot anyone who wishes come, since, as I suppose, they meet in a

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10 Hobbes posed this question in Dialogus physicus, published in 1661, a retort to Boyle’s 1660 New Experiments Physico-Mechanical (and Boyle in turn confuted Hobbes in 1662). This dialogue was not translated in the French edition of S&S, hence adding to the shift away from the material side, which I emphasize, and in the direction of the social side of the book.
public place, and give his opinion on the experiments which are seen ...?” – “Not at all” Boyle is made to reply (p.350). Only the elite few could enter these laboratories, these colleges and societies. Closed clubs are hardly new. Hobbes’s point is the tension between the rhetoric of public verification and the fact of private and heavily controlled membership.

This aspect is developed at length by S&S, and has become one of the best known contributions of their book to social studies of knowledge. It is further developed by Steven Shapin in his important book mentioned in my first lecture, A Social History of Truth.11 Shapin contends that it was the nature of the presupposition of mutual trust and reliability, among a tiny elite of the population of London, namely the gentry, that made modern science possible. Sincerity was a presupposition of the enterprise, and it required a social setting to be in place. It is striking how the present practice of peer review for scientific journals 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.

The Vacuum

Boyle did two things. He made a device that produced a partial vacuum in a container, thereby defeating nature, if we believe in the saying, “Natures abhors a vacuum”. He also convinced everyone, that that was what he had done. STS tends to emphasize the social, and so attends to the second, so brilliantly analysed by S&S. I am very much a materialist, so I am interested in the material object, the actual Air Pump, and its clones, which soon became cheaply available all over Europe. And I am interested in the phenomenon it created, the (partial) vacuum in a container.

There is a remarkably instructive irony here, so powerful that I sometimes think of nature playing tricks upon us still, always hiding what she has up her sleeve. The idea of a secret of nature is a very deep and powerful one, and is arguably at the very core of the emergence of the scientific tradition in the ancient Mediterranean world, Egypt, Mesopotamia, Greece. I have been much impressed by a book by a Paris colleague of mine, Pierre Hadot: The Veil of Isis: An Essay on the History of the Idea of Nature.12 I believe that it is a version of the story of what I call “finding out,” which began long before the human race embarked on anything recognizably scientific. It begins with an obscure saying of Heraclitus, “Nature likes to hide.”13

The irony is that Boyle, trying to produce a vacuum in a container, seems to have been making an idle gesture to produce absolutely nothing. It was important in his day, for sure. He strongly advocated the so-called corpuscularian philosophy, which conceived of air being composed of little balls, the atoms, bouncing around in the void. It was important to show that the idea of a void was coherent. But after that, the void is nothing, is it not?

Classically speaking the void is nothing. Classically speaking, the void at absolute zero is where absolutely nothing is going on in nothing. But the classical picture of the universe is false.

13 See my review in the London Review of Books, 10 May, 2007, which explains some of the reasons why I think this book is so important Online at http://www.lrb.co.uk/v29/n09/hack01__html.
Quantum mechanically, the vacuum is full of quantum fluctuations. The classic textbook on the vacuum says in its preface:

According to present ideas there is no vacuum in the ordinary sense of tranquil nothingness. There is instead a fluctuating quantum vacuum.\textsuperscript{14}

Perhaps there is a certain truth in the statement of a physicist who writes about science with enthusiasm and who is not disinclined to invoke a Creator of the Universe in some of his popular writing. In a book with the characteristically hyped title of \textit{Superforce}, P. C. W. Davies writes,

The vacuum holds the key to a full understanding of nature.\textsuperscript{15}

Maybe he is right! Even more extraordinary than what I have just reported, is that the quantum vacuum is a hive of quantum activity at absolute zero. That is where Bose-Einstein Condensate lives, trapped by laboratory apparatus in a thermal cloud of other atoms within nanokelvins of zero. That is, within $10^{-9}$ degrees of zero, what is rightly called the ultracold. This may take us back to Heraclitus. What a wonderful place for Nature to hide her secrets, in a vacuum at absolute zero!

\section*{II. THE BIOTECHNICAL LABORATORY}

I am able to make fun of myself: I have now performed the feat of connecting, into a seamless whole, the mystical ancient Heraclitus and the most recent work in the laboratory. I should conclude by getting real. It is time for something about contemporary laboratories. Professor Chen, my host at this STS workshop, wrote me that “Some STS friends suggest that I should ask you to talk a little about your ‘imagination’ on East Asian techno-science at the end of your lecture to the STS community. They are looking forward to hearing your vision on this topic.”

I am sorry, but I do not have a coherent or well-informed vision. I simply do not know enough about East Asian technoscience. I am taking the opportunity to visit two cold atom laboratories that create Bose-Einstein condensate here in Taiwan. But they are not, yet, East Asian technoscience. They are doing western technoscience in East Asia. Their leading researchers were trained for example at MIT, which has perhaps the greatest of BEC laboratories. They may not be so different, yet, from two comparable laboratories in Toronto with which I associate, where the leading researchers were trained for example in Paris, by workers who include some of my Parisian colleagues, and who also operate one of the great cold atom laboratories. These visits, one to a lab here at National Tsing Hua University tomorrow, and one on Friday to Chung Chen University, will prove fascinating, but they will probably teach me little about specifically East Asian technoscience. (Perhaps I shall be proven wrong.)

It is biotechnology laboratories that I should visit. I know nothing about biotechnology. I think like a physicist. So when I was writing \textit{Representing and Intervening}, I talked to physicists. Physics was still, in 1983, the heartland of the natural sciences. I had known, since 1962, where the future lay, but it was not my future. My first job after graduating was in Cambridge, England, and the first year I was a fellow of a Cambridge college, two older fellows of that college shared the crop of Nobel Prizes awarded that year in biology and medicine, for DNA and RNA. I


\textsuperscript{15} New York: Simon and Schuster, 1985, p. 104.
remember being thought off-the-wall, when, a very few years later, in a crowd of humanists, I said, that is where the world’s action is. Forget the past: These new ideas and techniques will change the world!

By the 1970s everybody of good sense, humanist or not, knew that. But I did not retool. So today I know less about biotechnology than an able high school student. In this, unfortunately, I am not so different from most of my younger philosophical colleagues.

The world is being changed by biotechnology. When, decades ago, Bruno Latour and Steve Woolgar did their ethnography of a biochemical lab that shared a Nobel prize, they were working in a very traditional academic sort of place. Latour said the chief product of the lab was inscriptions, which in the end generated the stability of the science. I protested that the chief product of the lab was a new substance, a synthesized peptide, Thyrotropin Releasing Factor. My blunt Materialism and his visionary Idealism were apparent even in those days! But those were the old days. By 1974 there was a conference of all the recombinant DNA workers in the world, held at a nice resort on the California coast. They wanted ethical guidelines to determine what types of research were permissible to avoid creating monsters, bacteria, say, that would destroy the world’s rice crops. In those days they still worked with experimental samples that might occupy a litre. In a few years, as one of the participants (who was one of the Cambridge Nobel prize-winners whom I mentioned) recalled, they were shipping such material around in tanker trucks. New techniques interposed, and the speed of research speeded up ten-thousand fold. There are places all over the world where ten story buildings are occupied by nothing but gene-sequencing machines.

The initial research was done in academic laboratories. Venture capital companies began forming in the early 1970s. Now they can be said to dominate the field. This is where STS researchers of today should be learning, studying, analysing. In fact, it is an anthropologist who has led the way. He is a classical anthropologist, who trained with Pierre Bourdieu, and who later played a large part in introducing the work of Michel Foucault to an American audience.

I am referring to Paul Rabinow, a Professor of Anthropology at the University of California, Berkeley. I most strongly recommend the work of Rabinow to anyone in the STS field, who is interest in recent biotechnology. He is a model to us all.

Rabinow collaborates often with Nikolas Rose, a sociologist at the London School of Economics, who has created a major research centre there called BIOS, which describes itself as a multidisciplinary centre for research into contemporary developments in the life sciences, biomedicine and biotechnology. I met him a week before writing down these thoughts – thus about three weeks ago now. That was just after he had returned from a week in Shanghai meeting with front line researchers in biotechnology there. I asked him what I should say to you about East Asian technoscience.

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I am now going to build on some remarks that he made to me in conversation. He is absolutely not responsible for what I am about to say. If I say anything false or misleading or stupid now, it is my fault, not that of Professor Rose. I should also emphasize that I shall try to be purely descriptive. The phenomena to be mentioned are very different from the experience of conservative Western intellectual such as myself, but that does not mean that I criticize them because they are not what I do.

Nikolas Rose was, like so many visitors, bowled over by what he saw in Shanghai. This was not news to me. I recall speaking a few years ago to my Paris colleague Philippe Kourilsky, who was then head of the Pasteur Institute in Paris, France’s premier establishment for fundamental biomedical research. He had just come back from Shanghai where he officiated at the opening of a sort of clone of the Pasteur Institute, erected on the site of the old French quarter in Shanghai, preserving some of the elegant nineteenth century facades. That is mere appearance; within that complex they were, when he was there, creating a massive research enterprise, in remarkable new buildings, and in no time at all by French standards. “They will completely surpass everything we do within less than ten years,” said Kourilsky.

In America, biotechnological research continues to be done in traditional academic laboratories. But increasingly, it is the venture capital companies that are doing the cutting-edge work. Some of them have grown to become major corporations in their own right. There is a big difference between the capitalist laboratory and the academic laboratory. It goes right back to what I mentioned in connection with trust and seventeenth century English gentlemen – the system of peer review. Everything that happens in the academic setting is governed by peer review. Not only everything that is accepted as knowledge and published, but also all research that is funded. Writing and re-writing grant proposals is a prime occupation of academic researchers. Moreover, what is called peer review, at the funding level, is typically not peer review at all. It is not review by equals, which is the meaning of “peer”. It is more often the review of young researchers by successful old former researchers. They do indeed furnish all sorts of advice. They are wise old men determining who in the next generations will be supported. I exaggerate. They are not all wise or old or men, but certainly most of them are men, and many are old.

Private companies are different. There the measure of success is a patentable product. I spoke of the creation of new phenomena four hundred years ago. If only Boyle had been able to patent his Air Pump in those days, he would have recovered his investments one hundred fold. Compared to the academic laboratory, the world of research capital may seem cutthroat. Many traditionalists deplore it. A “generation” is sometimes counted as a span of 33 years, I do not know why. Within one such generation private capital has transformed biotechnical research. The Universities frantically try to keep up, establishing an office of patents. This department is usually on what is (in fact if not law) at least equal footing with ordinary academic departments, within the University pecking order.

So how does technoscience in East Asia fit in? It will affect the present generation of bioscience as much as venture capital changed the last one. And here, to connect with my earlier theme of the origin of peer review, we should be well aware of the potential of the transformation of the capitalist world into state-managed research with immense resources. It is most noticeable under what is officially the “communist” or at any rate socialist regime across the Straits, the Peoples’ Republic, which was where Rose had just visited before he talked to me. The difference from Western capital is not capitalism but the almost unlimited body of actually
or potentially trained scientific labourers, and a method of labour management that has already taken over the lion’s share of Western basic manufacturing. The transfer of manufacturing, which began in Taiwan, Korea, and of course Japan, has been multiplied beyond measure in the People’s Republic. Virtually all the children’s toys sold in America are “Made in China”; the same methods of manufacture, that is, very large enterprises organized in small production units, with a local team spirit, are now being applied to biotechnological research.

There are just two criteria of success in this research, as opposed to the development or production side of the new technoscience. Peer review is wanted, yes, and this is generously provided by chiefly American premium scientific journals. If a paper is accepted there, then a researcher and the team have established their credentials. The second is the patentable product.

In a state system, money can be thrown at research groups. Most of them will fail to meet the two criteria I have just mentioned. They will be jettisoned, the labourers assigned to minor tasks for the rest of their lives, little differently from what happens in a ruthlessly efficient capitalist company. The differences lie (1) in the potential workforce of researchers, on which there is no upper bound, and (2) in the organization and morale of teams within larger structures. I have mentioned the phrase “form of life”, and have declined to use it much. Yet here we have a new form of science performed within what it seems natural to call a new form of life. There is nothing essentially “communist” about it; it is a question of immense resources, combined with, in effect, not much accountability. That can at present be marshalled more easily by a hegemonic state than by any other kind of regime. We do have a standard of comparison, the Manhattan project, when the U. S. government threw untold sums of national treasure in a pretty indiscriminate way, in order to achieve results that a decade earlier might have been predicted to require half a century of research. The immediate result was the bomb. The long term effect on the organization of science is well known to all students of STS, namely “Big Science.” Perhaps that is a new scientific life form. But “Big” may prove to be miniscule in comparison with what may take place in South-East Asia within a generation.

I conclude my poorly informed response to Professor Chen’s request. You may not be pleased, that I have moved the proposal, to address East Asian techno-science, from here to the mainland. But what I have said will be something to discuss, and I am anxious to hear what you may have to say in reaction to it.