

# The Science of the Unknowable:

Stafford Beer's Cybernetic Informatics

Andrew Pickering  
University of Illinois  
Department of Sociology  
Urbana, IL 61801  
pickerin@uiuc.edu

Cybernetics and New Ontologies:  
An interview session with Andrew Pickering

Kristian Hvidtfelt Nielsen  
khn@si.au.dk  
The Steno Institute  
University of Aarhus

Working Papers from Centre for STS Studies  
Department of Information & Media Studies  
University of Aarhus

Published by The Centre for STS Studies, Aarhus 2006.

Editorial board: Peter Lauritsen, Simon Kiilerich Madsen, Finn Olesen.

Andrew Pickering: *The Science of the Unknowable: Stafford Beer's Cybernetic Informatics*

Kristian Hvidtfelt Nielsen: *Cybernetics and New Ontologies: An interview session with Andrew Pickering*

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Printed at Fællestrykkeriet for Sundhedsvidenskab, University of Aarhus.

Cover design: Annette Bjerre Design.

ISBN 9788791386121 (print)

ISBN 9788791386138 (web)

The Centre for STS Studies

Department of Information & Media Studies

Helsingforsgade 14

DK-8200 Aarhus N

Tel: +45 8942 9200

Fax: +45 8942 5950

sts@imv.au.dk

<http://imv.au.dk/sts>

# The Science of the Unknowable:

## Stafford Beer's Cybernetic Informatics

Andrew Pickering  
University of Illinois  
Department of Sociology  
pickerin@uiuc.edu

This essay derives from a larger project exploring the history of cybernetics in Britain in and after World War II.<sup>1</sup> The project focusses on the work of four British cyberneticians—Grey Walter, Ross Ashby, Stafford Beer and Gordon Pask; here I focus on Stafford Beer, the founder of the field he called management cybernetics, and his work in informatics.<sup>2</sup>

Anthony Stafford Beer was born in London in 1926. He joined the British Army in 1944 after just one year as an undergraduate in London, and served in India and Britain. He left the army in 1949, and between 1949 and 1970 he worked in the steel and publishing industries and ran his own consulting company. From 1970 until his death in August 2002 he worked as an independent management consultant (*Times* 2002).

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<sup>1</sup> An earlier and shorter version of this essay was presented at the Second Conference on the History and Heritage of Scientific and Technical Information Systems, Chemical Heritage Foundation, Philadelphia, 16-17 November, 2002, and is to appear in Mary Ellen Bowden and Warden Boyd Rayward (eds), *Proceedings of the 2002 Conference on the History and Heritage of Scientific Information Systems* (American Society for Scientific Information/Chemical Heritage Foundation). The present essay is a revision of a talk presented at the Dept. of Information and Media Studies, University of Aarhus, Denmark, 10 April 2003. I am grateful for comments received at both of those meetings, and also for suggestions from Raul Espejo on the final draft. Other studies deriving from this project include Pickering (2002, 2003a, b, c, forthcoming a, b). During 2002-3 the project was supported by the National Science Foundation under Grant No. SES-0094504, and I was based at the Science Studies Unit, Edinburgh University. I thank the Unit Director, David Bloor, and all of its members for their hospitality.

<sup>2</sup> Beer provided me with a considerable amount of information before his death. I thank Beer's partner, Allenna Leonard, and his daughter, Vanilla Beer, for very important assistance and encouragement in my research. I also thank Eden Miller for enlightening discussions of Beer's work in Chile (below) and for allowing me to read and cite her unpublished work.

I begin with an overview of Beer's general perspective on information science and information systems, intended to bring out the singularity of the cybernetic approach.

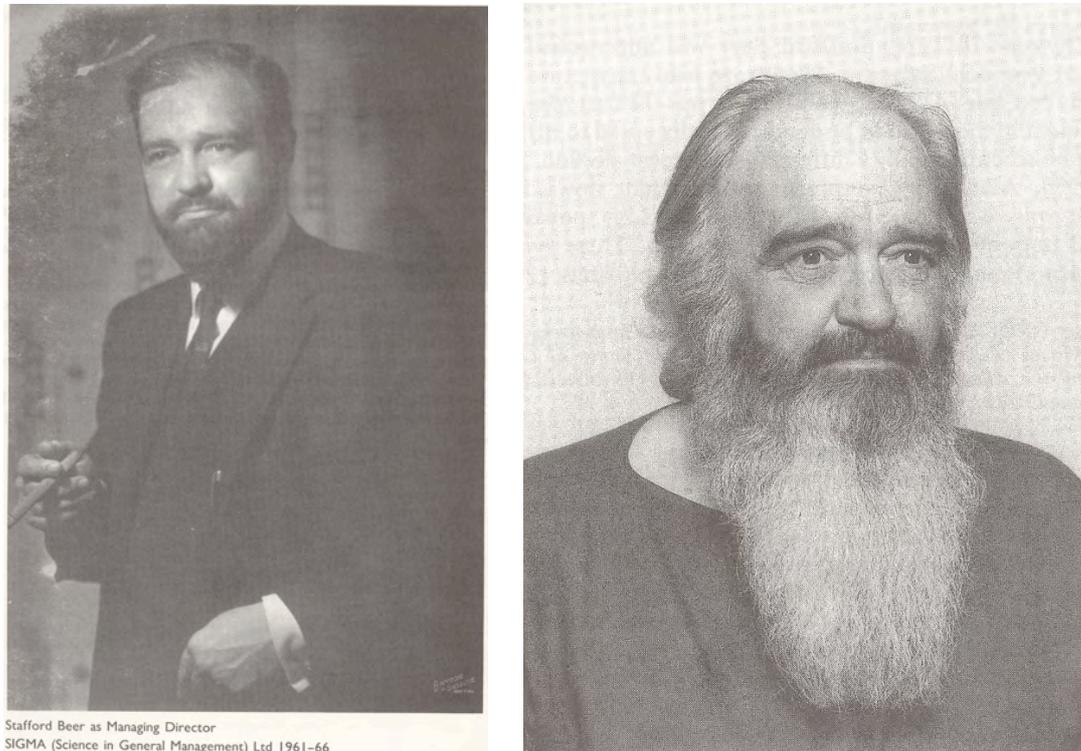


Figure 1: Stafford Beer (a) in the early 1960s, (b) in 1975.

From the 1950s onwards Beer was a remorseless critic of the ways in which computers were being deployed in industry, essentially to replace existing paper systems. He felt that this did nothing to change existing organizational forms, and that something more imaginative was required. His argument was that the postwar world was a new *kind* of world. Specifically the pace of change had increased markedly since the war, and the important thing for organisations was thus that they should be *adaptive*—light on their feet and ready to accommodate themselves to the new situations which would arise faster and faster as time went on.<sup>3</sup>

To render organisations adaptable, according to Beer, required reorganising them to make possible specific patterns of information flow and transformation.

We can turn to some examples shortly, but first I want to emphasise the gap between the mainstream vision of informatics and the cybernetic one. What we need to think about here is *ontology*—the question of what the world is like.

Mainstream informatics presumes a very familiar ontology. The world is a regular, law-like place that can be *known* more or less exhaustively. It is a place that can therefore be controlled and dominated through knowledge. That is the logic behind the creation of bigger databases and faster information systems. Of course, this ontology does recognise the existence of the unknown, but only as something to be conquered, to be drawn into the realm of the known.

Cybernetics turned this picture inside out and exemplified a different and much less familiar ontology. Beer (1959, 17) argued that there exists in the world a class of ‘exceedingly complex systems,’ including the brain, the firm and the economy, which are in principle *unknowable*. However much data we gather on them, we can never know them completely, they can always surprise us. Such systems can never be dominated by knowledge, and instead we have to learn somehow *to cope* with them. And cybernetics was, then, the science of dealing with the unknown, the science of adaptation—an extremely odd sort of science.<sup>4</sup>

This ontology of the unknowable is the key thing to grasp in thinking about cybernetics. And two corollaries of it are worth mentioning. First, it thematises *time*. By definition one has to deal with the unknown in time, as it happens. No amount of information about the past, as stored in conventional information

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<sup>3</sup> See, for example, Beer (1959)—his first book.

<sup>4</sup> On the affinity between the cybernetic ontology and that at which I arrived in my analyses of scientific practice (Pickering 1995), see Pickering (2002). My particular interest in the history of cybernetics is to see how this ontology engaged in a wide variety of real-world projects. I emphasise these ontological considerations because it seems to me that an ontology of representation and knowability exercises a certain hegemony over our imaginations. We (academics especially) often find it hard to imagine the world as a place of unknowability and becoming, in which, as it were, actions speak louder than words. My ambition in examining the history of cybernetics is to help revive our ontological imaginations (my own as much as anyone else’s)—to help us see the world differently and to exemplify the sorts of practical projects that that might suggest.

systems, can ever prepare us for genuine unpredictable novelty. As Beer (1972, 199) ironically put it: 'Look straight ahead down the motorway while you are driving flat out. Most enterprises are directed with the driver's eyes fixed on the rear-view mirror.' This real-time/retrospective contrast is an important angle on the specificity of cybernetics.

The other corollary is this. Conventional informatics is, as I would say, *representational*—meaning, again, that it is all about the accumulation of data and knowledge. One might eventually want to draw upon that knowledge for action, but that is not the defining feature of an information system. The information system is, as it were, detachable from the action. Cybernetics viewed information systems differently. If we have continually to deal with the unexpected as a practical matter, then the accumulation of representational knowledge seems less relevant. What one wants instead is a *performative* information system, geared straight into the action, not detachable at all. One would not care exactly what information was flowing through the system and how, as long as its output was an adaptive transformation of the organisation to its environment. This contrast between the representationalism of conventional information systems and the performativity of cybernetic ones is very important. In 1962, in one of his more visionary moments, Beer described electronic computers as dinosaurs, looking forward to the day when they would be supplanted by another class of information processing devices that simply would not have representational intermediate states at all (Beer 1962a, 220).<sup>5</sup> When I came across that idea I was amazed. Something beyond the computer? What is this man talking about? Is he mad?

Beer was not mad. Now we can turn to history, starting with a brief detour through the work of Beer's friend, W. Ross Ashby, born 1903, died 1972, the doyen of the English cyberneticians.<sup>6</sup> We need to think especially about a device Ashby built in his spare time in 1948, his famous homeostat (Ashby

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<sup>5</sup> The page citation here, as below, refers to the more accessible 1994 reprint of this article.

<sup>6</sup> For more on Ashby, see Asaro (1998) and Pickering (2003a).

1948, 1952). This was an electromechanical device intended to mimic the biological process of homeostasis—the ability of organisms to maintain ‘essential variables’ such as blood temperature constant in the face of fluctuations in their environment. Without going into details, in Ashby’s homeostat the essential variable was the electric current flowing through a moveable needle dipping into a trough of water on top of the device, and the machine’s environment was constituted by electrical interconnections to other homeostats. The trick in maintaining homeostasis was that when the current within a given homeostat went beyond some preassigned limit, a relay would trip, operating a stepping switch which would change the electrical resistance of the homeostat’s inner circuitry, with the sequence of different values for the resistance being determined from a table of random numbers. The homeostat would thus, as it were, *randomly reconfigure* itself. If the current were to continue to go beyond its limit, the machine would reconfigure itself again and again until homeostasis was achieved. The homeostat was thus, as Ashby called it, an ultrastable device; whatever one did to it, it would eventually find its way back to homeostatic equilibrium with its environment. It was a device for staying the same. Another British cybernetician, Grey Walter (1953), sarcastically referred to it as *Machina Sopor*.



Figure 2: Ross Ashby.



Figure 3: The homeostat: photo & circuit diagram.

I need to make three remarks on the homeostat. First, I hope it is clear how it fits in with my earlier remarks on ontology. The homeostat was a device that *dealt with unknown*. It did this in real time—it reacted to fluctuations in its environment as they happened. And it did so in a performative rather than a representational fashion: it did not seek to know the world representationally; it simply materially reconfigured itself as the occasion arose. If you have the hang of that, then you have the hang of what was most distinctive about British cybernetics. If orreries—those beautiful early-modern models of the solar system—were the mechanical emblems of the ontology of the knowable, then the homeostat was the emblem of the cybernetic ontology of unknowability.

Second, the homeostat was the centrepiece of Ashby's first book, *Design for a Brain* (1952), and Ashby intended it as a model of the brain inasmuch as it *learnt* to cope with its environment. But, again, it was a performative brain—as distinguished, for example, from the rational representationalist brain that was later exemplified in symbolic AI.

My third remark is this. We can think of the homeostat as a controller, and much of Ashby's cybernetics focussed precisely on questions of control (Ashby 1956).<sup>7</sup> His key result here was the Law of Requisite Variety—Ashby's Law, as Beer called it. Variety is a measure of the number of states a system can take up—25, as it happens, in the case of Ashby's first homeostats, with their different possible internal electrical resistances. And the Law of Requisite Variety stated that a system could succeed as a homeostatic controller only if it disposed of as much variety as the environment in which it existed. A homeostat could maintain its ultrastable condition when connected to another homeostat with the same number of internal states, but might fail against one having twice that number.

That is enough to get us back to Stafford Beer. If Ashby did cybernetics as a 'pure science,' Beer was an applied cybernetician, which is what interests me—I am especially interested in what cybernetics looked like when it was put to work in the world. Ashby's homeostat was at the heart of all of Beer's attempts to conceptualise and design adaptive organisations, and now we can run through some of these as they emerged in the period from the 1950s to the 1970s.

In the 1950s, Beer's cybernetics revolved around the contemporary fantasy of the 'automatic factory,' in which all operations were to be controlled by computers rather than people. Beer likened current visions of the automatic factory to a 'spinal dog'—a dog whose nervous system had been severed below the brain (1962a, 164). Such an animal can, apparently, continue to live and display bodily reflexes, but it cannot learn and adapt to changing circumstances. To move from the automatic factory to the cybernetic factory thus required adding a brain, and this, Beer argued, should be an Ashbean homeostat.

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<sup>7</sup> For further discussion of the cybernetic conception of 'control' see Pickering (2003b).

I cannot go into detail here, so let me instead discuss some figures from a major paper Beer wrote in 1960 by way of an overview (Beer 1962a). Figure 4 is a logic diagram of the cybernetic factory. The T- and V-machines are what we would now call neural nets: the T-machine collects data on the state of the factory and its environment and translates them into meaningful form; the V-machine reverses the operation, issuing commands for action in the spaces of buying, production and selling. Between them lies the U-Machine, which is the homeostat, the artificial brain, which seeks to find and maintain a balance between the inner and outer conditions of the firm—trying to keep the firm operating in a liveable segment of phase-space. Figure 5 is a more suggestive figure, a painting by Beer, labelled 'general picture of the whole theory' (the T-U- and V-machines are indistinctly labelled in the smaller painting at the lower left).

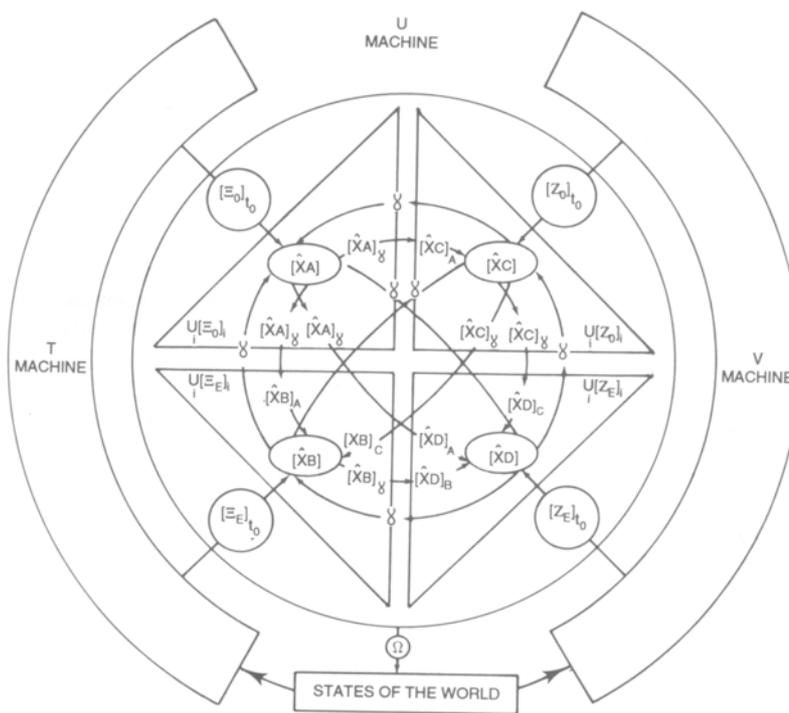


Figure 4: The cybernetic factory.

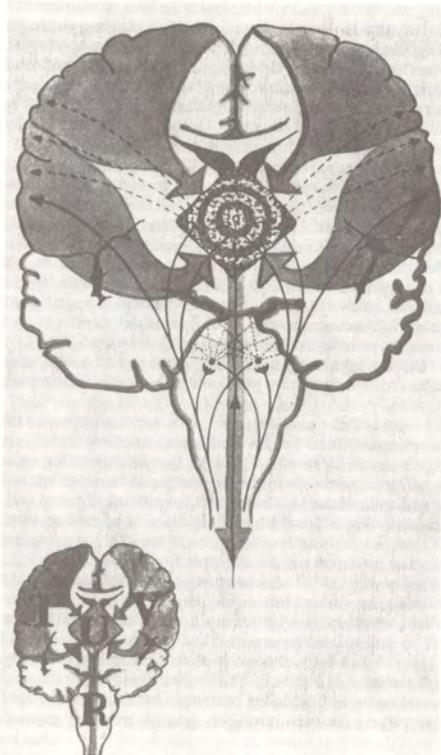


Figure 5: The factory as brain.

The cybernetic factory was not pure theory. By 1960, Beer had at least *simulated* a cybernetic factory at Templeborough Rolling Mills, a subsidiary of his employer, United Steel, and the next figure might help us understand things better. In figure 6, the lines of circles and squares marked ‘sensation’ to ‘judgements’ correspond to the numerical inputs to the T- Machine: ‘tons bought,’ ‘cost of raw material,’ ‘cash at bank,’ ‘value of credits,’ etc. At Templeborough, all of these data were statistically processed, analysed and transformed into 12 variables, six referring to the inner state of the mill, six to its economic environment. Figures were generated at the mill every day—as close to real time as one could get—and each day’s figures were stored as the ‘generalised gestalt memories’ indicated at the lower left and right of the figure. Beer claimed to see how all this data collection and processing, including changes in the classification system, could be accomplished automatically, although in fact it was still done clerically in the mill according to protocols devised by OR scientists—this was one sense in which the mill had become a simulation of a fully cybernetic factory.

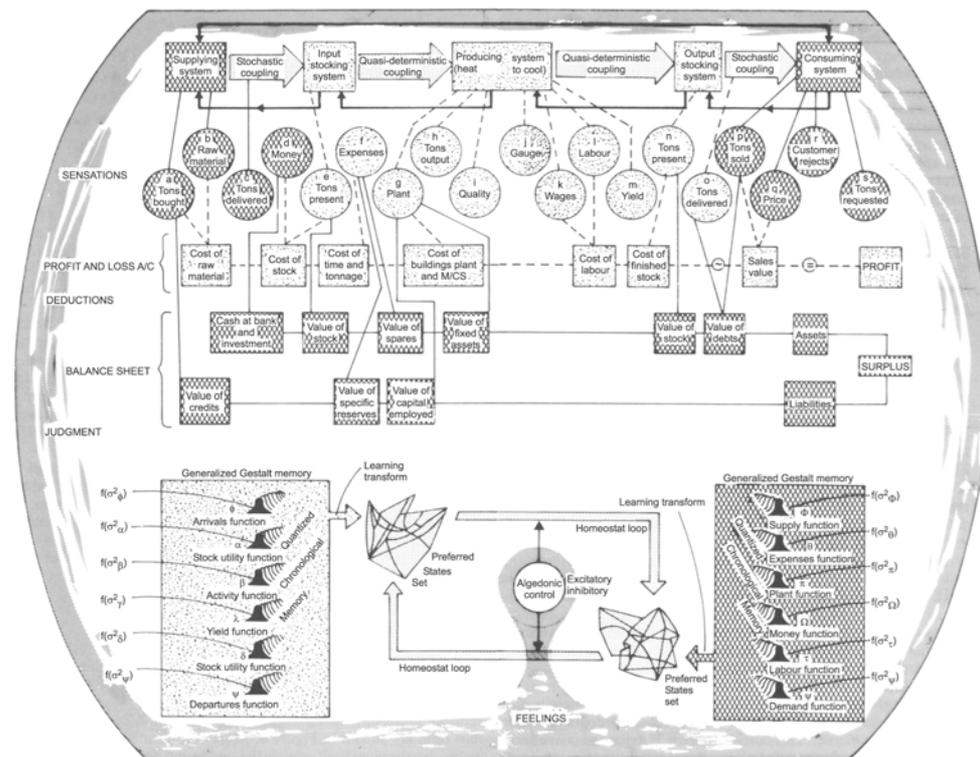


Figure 6: Simulation of a cybernetic factory.

The other sense of simulation concerned the U-Machine. As indicated in the lower centre of the figure, the two gestalt memories of the factory defined two phase-spaces in terms of the relevant parameters, and the job of the U-Machine was to strike a homeostatic balance between them. But nothing like a functioning U-Machine had yet been devised. The U-Machine at Templeborough was still constituted by the decisions of human managers, though now they were precisely positioned in an information space defined by the simulated T- and V-Machines.

So, by 1960 Beer had constructed a simulation of a cybernetic factory that promised to dispense entirely with human personnel, though humans in fact still filled the gaps for machines which were not yet in place. Beer could see how to complete the automatic T- and V-machines, though the U-Machine remained unspecified. Nevertheless, he wrote, 'Before long a decision will be taken as to which fabric to use in the first attempt to build a U-Machine in actual hardware (or colloid, or protein)' (1962a, 212).

The vision of the adaptive factory not just running smoothly but also evolving and changing all on its own without any human intervention is itself amazing, but Beer's attempts to construct the U-Machine homeostat are where the story gets really interesting. The requirements for the U-Machine were that, first, it should be able to internally reconfigure itself, like Ashby's original homeostat, and that, second, in accordance with Ashby's law, it must have high variety, in order to have a chance of coping with the complexity of its environment. These days, we might think of somehow programming a computer to fulfil this function, but Beer argued that this was not necessarily the way to go. Computers were extremely expensive in the 1950s and 1960s. And besides, Beer had come up with a different idea:

As a constructor of machines man has become accustomed to regard his materials as inert lumps of matter which have to be fashioned and assembled to make a useful system. He does not normally think first of materials as having an intrinsically high variety which has to be constrained. . . [But] We do not want a lot of bits and pieces which we have got to put together. Because once we settle for [that], we have got to have a blueprint. We have got to design the damn thing; and that is just what we do not want to do (1962a, 209, 215).

What is all this about? Ashby had built an electromechanical equivalent of a homeostatic biological system and called it a brain. Beer's idea was to turn Ashby's idea through another 180 degrees: he wanted somehow to enrol a naturally occurring homeostatic system as the brain of the cybernetic factory. He had conceived the idea, I would say, of a nonrepresentational, adaptive, *biological computer*. This was the machine which he hoped would supercede the electronic computer; the referent of his remark about dinosaurs. And, during the second half of the 1950s, he embarked on 'an almost unbounded survey of naturally occurring systems in search of materials for the construction of cybernetic machines' (1959, 162).

In 1962 he wrote a brief report on the state of the art, which makes fairly mind-boggling reading (Beer 1962b). Let me just mention some of the systems he discussed there to convey the flavour of it. The list includes a successful attempt to use positive and negative feedback to train young children to solve

simultaneous equations *without* teaching them the relevant mathematics—to turn the children into a performative (rather than cognitive) mathematical machine—and it goes on to discuss an extension of the same tactics to mice! This is, I would guess, the origin of the mouse-computer that turns up in both Douglas Adams' *Hitch-Hikers Guide to the Universe* and Terry Pratchett's *Discworld* series of fantasy novels.<sup>8</sup> Beer also reported attempts to induce small organisms, *Daphnia* collected from a local pond, to ingest iron filings so that input and output couplings to them could be achieved via magnetic fields, and another attempt to use a population of the protozoon *Euglena* via optical couplings. (The problem was always how to contrive inputs and outputs to these systems.) Beer's last attempt in this series was to use not specific organisms but an entire pond ecosystem as a homeostatic controller, on which he reported that, 'Currently there are a few of the usual creatures visible to the naked eye (*Hydra*, *Cyclops*, *Daphnia*, and a leech); microscopically there is the expected multitude of micro-organisms. . . The state of this research at the moment,' he said in 1962, 'is that I tinker with this tank from time to time in the middle of the night' (1962b, 31).

In the end, this wonderful line of research foundered, not on any point of principle, but on Beer's practical failure to achieve a useful coupling to any biological system of sufficiently high variety. I do want to note, however, that I admire Beer's imagination enormously in this phase of his work.<sup>9</sup> I also want to mention that it is clear from subsequent developments that the homeostatic system Beer really had in mind was something like the human spinal cord and brain. He never mentioned this in his work on biological computers, but the image that sticks in my mind is that the brain of the cybernetic factory should really have been an unconscious human body, floating in a vat of nutrients and with electronic readouts tapping its higher and lower reflexes—something

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<sup>8</sup> In the *Hitch-hiker's Guide*, the earth is a giant analogue computer built by mice-like beings to answer the Ultimate Question. On the earth as an analogue computer, see Blohm, Beer and Suzuki (1986).

<sup>9</sup> One could develop this point further in a discussion of hylozoism, the idea that matter is active and thus that we should enrol it in our projects rather than bending it to our will. A key text here would be *Pebbles to Computers* (Blohm, Beer and Suzuki, 1986).

vaguely reminiscent of the movie *The Matrix*. This horrible image helps me at least to appreciate the magnitude of the gap between cybernetic information systems and more conventional approaches.

Now we can return to something more like normality. Beer's dreams of biological controllers came to an end in the early 1960s but this provoked a transformation rather than an abandonment of his vision of the cybernetic factory. His 1972 book, *Brain of the Firm*, laid out a new vision of what he called the Viable System Model—VSM for short (Beer 1972).<sup>10</sup>

The VSM took up Beer's earlier plan for a cybernetic factory and transformed it along two axes. First, the simulation of the cybernetic factory just discussed became, in effect, the thing itself. Beer dropped the ambition to dispense entirely with human beings and instead argued that human managers should be positioned within purposefully designed information flows at just those points that would ideally have been occupied by homeostatic ponds or trained mice. Second, Beer extended and elaborated his conception of information flows considerably. The aim of the firm had to be to survive in an environment that was fluctuating and changing. How was this now to be accomplished? The place to look for inspiration, according to Beer, was, once more, nature. Biological organisms have already mastered the trick of survival and adaptation, and Beer's idea was therefore to read biological organisms as exemplary of viable systems in general—we should transplant their key features to the structure of the firm. In particular, as I hinted a minute ago, Beer chose the human nervous system as his model. If his original idea was that the firm needed to contain an artificial brain (made of magnetic *Daphnia* or leeches), the idea of the VSM was that the firm should *become a brain*, a cyborg brain with human brains lodged within it. (Another weird image, if you think about it too hard.)

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<sup>10</sup> This book was significantly extended in its second edition (Beer 1981) and eventually formed part of a trilogy with Beer (1979, 1985). For more on the VSM, see Espejo and Harnden (1989).

The spirit of the VSM is nicely expressed in the juxtaposition of two figures from *Brain of the Firm*: one a schematic of the human body; the other of the firm. Very briefly, Beer argued that one needs to distinguish, at minimum, five levels or systems of control in any viable system. In this figure, System One consists of four subsidiaries of a larger organisation, labelled A, B, C and D, analogous to arms and legs, the heart, kidneys, etc. System Two, the equivalent of the sympathetic nervous system, connects them to one another and to System Three, and seeks to damp out destructive interactions between the subsidiaries. System Three—the pons and medulla of the VSM—consists of a set of Operational Research (OR) models of production that enables management to react to fluctuations in Systems One and Two—by reallocating resources, for example. System Four—the base of the brain itself—was envisaged as a decision-making environment for higher management, modelled on the World War II operations room. It would collect and display information from the lower systems and from the outside world and, very importantly, it would run a set of computer programs that higher management could consult on the possible *future* effects of major decisions. At the same time, this operations room was intended to function as a club-room for senior management—a place to hang out, even when major decisions were not at stake. Finally System Five was the location of the most senior management whom Beer regarded as the cortex of the firm. Their vision of the firm and its future, whatever it was, was to be negotiated into reality in reciprocally vetoing homeostatic interactions with System 4.

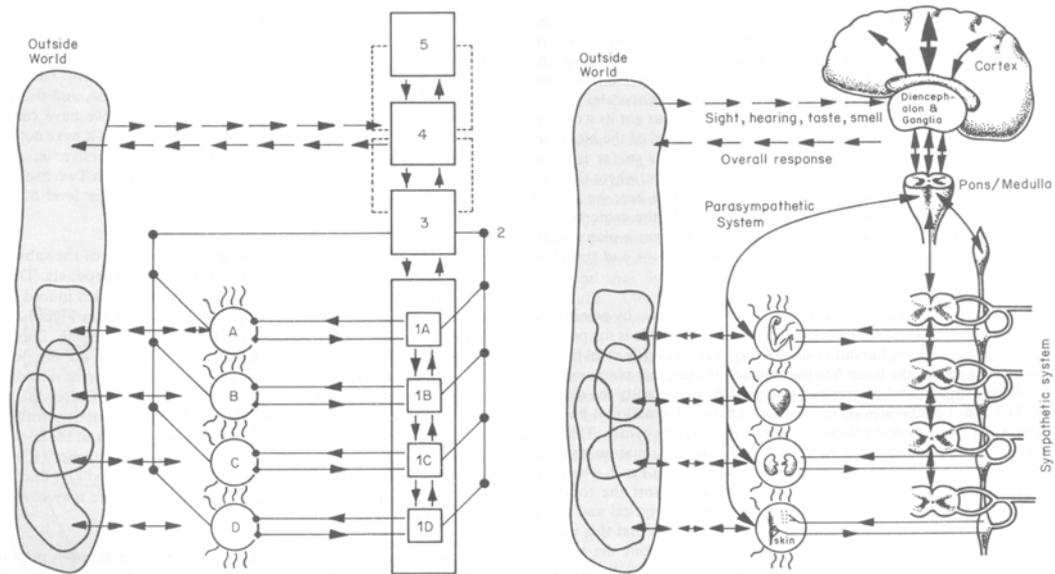


Figure 7: Control systems in (a) the firm, and (b) the human body

Here I should return to the question of ontology. Despite my earlier emphasis on performance vs representation, it is clear that the VSM *did* incorporate significant representational elements, especially the computer models running in Systems Three and Four. But one should note that one function of the programs running at the System Three level was statistical filtration—that is, to junk almost all of the information that arrived there rather than to store it. And, second, in the VSM the models at levels Three and Four were to be *continually updated* in comparisons between their predictions and the actual performance of the firm and its environment. This updating recognised even in the realm of representation that the world remained an unknowable place; the utility of the models had continually to be found out in real-time experience.

It is important to emphasise that the VSM was not a theoretical conceit. All of Beer's consulting work was based on it, and by the early 1980s he could already list amongst his clients small businesses and large industries, publishers, banks and insurance companies, transportation, education and health organisations, and governments and international agencies (Beer 1989a, 34-35). I cannot go into examples, but I can note that in much of this work, the VSM functioned as a *diagnostic* tool—comparison with the VSM diagram was a way of singling out organisational problems that needed to be addressed. Beer

claimed that one could go very quickly to the heart of an organisation's problems in this fashion—though addressing the problems took much longer.

Only on one major occasion did Beer have to chance to implement the VSM from the ground up—when he was invited to help design and implement a control system for the entire economy of Chile, under the newly elected Marxist regime led by Salvador Allende. From 1971 to 1973 Beer threw himself into Project Cybersyn as it was called (for 'cybernetic synergy'); a lot was done in a very short period of time, and I can just summarise what was accomplished.<sup>11</sup>

By installing telex facilities, a real-time communication network called Cybernet was established, linking much of Chile's industrial base to one computer in Santiago. A set of programs called Cyberstride were written to process and filter the incoming data at the System Three level, and another program, CHECO, was written to simulate the overall behaviour of the Chilean economy at the System Four level. The System Four operations room was also getting into shape by 1973, as shown in figure 8. This cybernetisation of the Chilean economy was an extremely ambitious project which, alas, never had chance to go into full operation. On September 11 1973 General Pinochet launched a successful coup against the Allende government, and Allende himself died that day. Some members of Beer's group fled the country; others were jailed.

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<sup>11</sup> The second edition of *Brain of the Firm* (Beer 1981) includes a long history and discussion of the Chile project. Miller (2002) is an important historical study of Cybersyn.



Figure 8: Operations room of project cybersyn

Chile and Cybersyn are as far as I want to go in tracing out Beer's development of the Viable System Model in management cybernetics, and I want to pause now to take stock of where we have got to, before branching out in different directions.

I can say this: what we have been exploring so far is Beer's implementation of the cybernetic ontology of unknowability in the construction of adaptive information systems, and what I have been trying to show is, first, the intriguing and imaginative singularity of this work—how different it is from conventional informatics. The key contrast here is Beer's emphasis on real-time performance rather than data-processing as a self-contained activity, thematised for me by his idea of biological rather than digital computation. And, second, I have been trying to show that, despite the seeming paradoxicality of it, one can indeed construct adaptive systems, systems that adapt to and transform themselves in the face of the unknown, as in Beer's implementations of the VSM.

I could finish the essay at this point—there is much to think about in those aspects of Beer's work already discussed. But there are further aspects of his work that also deserve attention here. Beer's daughter, Vanilla, recalls that 'Stafford and I generally ran Jesus and Marx together in an attempt to produce metanoyic possibilities,' so the last sections of this talk make some observations on Beer's politics and spirituality.<sup>12</sup>

First, politics. Beer sometimes described himself as 'an old-fashioned Marxist' (Miller 2002) or even as 'somewhat to the left of Marx.'<sup>13</sup> Hence, no doubt, his enthusiasm for the Chile project. But his writings cannot be construed as contributions to orthodox Marxist debates, and we need to come at them from a different angle.<sup>14</sup> Running through Beer's cybernetics is a profound concern for *democracy*, at the level of both theory and practice. The theoretical concern followed directly from the Law of Requisite Variety. In our dealings with others it is imperative that we dispose sufficient variety to accommodate their variety, and vice versa, so decision-making should take the form of a homeostatic reciprocal reconfiguration of all parties concerned—rather than, say, the imposition of the will of some on others. But what interests me most is not Beer's democratic *theory* but his attempts to, as it were, *engineer* democracy, to make it real.

What is at stake here? An ideal democracy might be one in which everyone discusses any given issue with everyone else until a conclusion eventually emerges that all are willing to abide by. The trouble with this ideal is that it quickly becomes practically impossible as the number of people involved increases. There are many ad hoc solutions to this problem to be found in the

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<sup>12</sup> The quotation is from an email to the author, 3 April 2003. The Oxford English Dictionary defines 'metanoia' as 'the setting up [of] an immense new inward movement for obtaining the rule of life; a change of the inner man . . .'

<sup>13</sup> Email from Vanilla Beer to the author, 3 April 2003.

<sup>14</sup> 'Stafford was fond of telling the story about Marx that had him saying "Thank God I'm not a Marxist." He didn't usually describe himself in this context but Stafford had a great deal of admiration for Marx, especially his early writings on alienation. He wasn't much of a fan of Das Capital mostly on the grounds of dull and repetitive' (email from Allenna Leonard, 5 April 2003).

world today, ranging from committee meetings of small numbers of representatives up to simple voting in national elections. Beer was very critical of all of these in their handling of variety. The agenda of committee meetings is easily rigged to define the possible outcomes, for example, leaving little space for open-ended adaptation. And Beer's problematic can thus be understood as one of trying to design better arrangements for democratic and adaptive decision-making. If conventional politics is about advocating specific plans of action, then Beer's work has to be seen as a species of *sub-politics* or *infra-politics*—the attempt to establish a suitably democratic and adaptive ground on which conventional politics can be conducted.

So much for generalities. What did this mean in practice? We can start by thinking again about the Viable System Model. Beer repeatedly stressed various aspects of this. First, just like the comparable levels of the human body, the various components of System 1 of the viable system were supposed to be quasi-autonomous. Most of the time, the heart and the lungs do their own thing, quite independently of any conscious control, and Beer thus envisaged the individual divisions of the firm in System 1 going their own way, in charge of their own destinies, for most of the time. This was one sense in which the VSM could be said to be democratic by design. Of course, the various levels were also interconnected and responsive to one another—they were not entirely autonomous—but again Beer thought of the coupling as essentially homeostatic. Experiments at each level could be thought of as analogous to the random reconfigurations of Ashby's mechanical homeostats, and the evolution of the entire system would be determined by a process of reciprocal vetoing between such experiments at all levels, just like an array of homeostats coming into equilibrium with one another. Again, then, the VSM envisaged democratic relations, here *between* levels, rather than unidirectional control.

Beyond this coupling of levels lay the question of what the viable system was for: what did the organisation as a whole seek to hold constant in the face of its unpredictable interactions with its environment—what were the system's goals?

Again, Beer stressed the importance of not thinking of such goals as hierarchically determined, as simply given by the organisation's 'brain.' In Chile, in parallel with the technicalities of Cybersyn, Beer worked to develop what was called the 'People Project,' intended to provide real-time feedback from the people on the conduct of government (Beer 1981). One endearing aspect of this was Beer's idea that people should be equipped with 'algedometers'—dials that they would set to indicate the magnitude of their pleasure or displeasure. Beer imagined, for example, a situation in which the integrated result of everyone's individual algedometer settings would be displayed on a giant algedometer next to a politician giving a speech on TV. Not only could the people see their own collective response to the speech, but they could also see that the politician could see it, and they could expect him or her to act accordingly—and then they could respond to that. This was one way in which everyone could contribute to 'design the nation,' as Beer put it.

In such ways and more, then, the VSM is a kind of *techno-social diagram* of an adaptive democracy—a map of how people might be arranged and connected to involve them all in their collective adaptation to a fluctuating and ultimately unknowable world. I find this very interesting. I am especially struck by the concreteness and specificity of Beer's approach. I have read many books of political theory which go through fascinating and complicated arguments only to reach the obvious conclusion—that democracy is a good thing and we need more of it; Beer is one of the few thinkers I have come across who had anything new to say at this subpolitical level about how democracy might be made in practice. Again we see the characteristic cybernetic concern with performance rather than theory and representation.

Not everyone saw the Chile project the way I have just described it, however. In Chile itself, as well as the US and Britain, Cybersyn was criticised, from left and right, as technocratic (Miller 2002). Beer rejected the charge along the lines I have just discussed, but it is easy to see what was on the critics' minds. The organic liveliness that Beer wanted to foster within the VSM structure could

readily be denatured into a command structure—homeostatic couplings could be replaced by a one-way flow of orders—and indeed some factions in Chile hoped to operate the Cybersyn apparatus just that way after the coup. It is therefore interesting to look at another of Beer's projects that evolved over the years alongside the VSM.

This was what Beer called the syntegegration approach to decision making, which implemented a quite different organisational diagram (Beer 1994b). The question here was how to bring together a group of people to discuss their collective future without specifying any detailed agenda, and the solution this time was an appeal to geometry. Team Syntegegrity was based on the figure of the icosahedron—the regular three-dimensional structure that has 20 triangular faces, 12 vertices and 30 edges. Participants would be assigned to one edge or another, and loosely defined discussion topics initially assigned to each vertex. Each participant would then take part with the appropriate others in discussions of the two topics at the ends of their edge. The discussions might come to some conclusion on given topics, redefine them, suggest new ones, or whatever, and then the whole cycle of discussion would start up again. In this way novel thoughts and proposals would eventually 'reverberate' around the whole structure from vertex to vertex and return to the original proposers in modified form, and the upshot of the procedure would ideally be a consensual understanding of the group and its purposes that could not have been foreseen by anyone in advance.

Syntegegration figured ever larger in the consulting work of Beer and others in the 1990s, and Beer (1994b, 12; 1989b, 122) described it, with some justification, as a process of 'perfect democracy'—clearly it was a process with no privileged centre whatsoever. Again, what I find striking about it is its specificity: syntegegration was not a theoretical argument; it was a practical sub-political set-up in which democratic deliberation and planning could be conducted in a completely open-ended fashion.

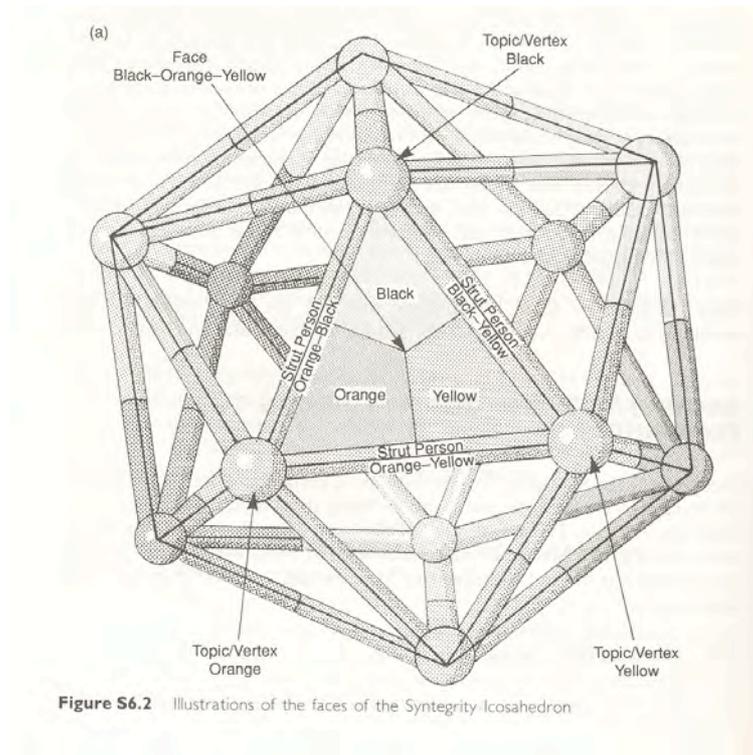


Figure 9: The syntegration icosahedron

So far I have been talking about what one could call the inner sub-politics of organisations. How could a firm or a nation arrange its own internal affairs to be democratically adaptive? But Beer was also concerned throughout his life with inter-organisational and international conduct: how should systems conduct their interactions with one another?

Here, from the 1950s onwards, Beer's rhetoric was always one of 'crisis.' Since World War II the world has been changing faster and faster in unpredictable ways, but our institutions cannot recognise this, they are not adaptable. Instead of coping with the inexhaustible variety of the world by deploying their own variety, they seek to fix their environments. And since the world is ontologically incapable of being pinned down and fixed, this necessarily has disastrous results—typically manifest in the Third World, the environment and so on. And hence we need to redesign our institutions cybernetically, precisely as adaptive systems.

When I first encountered this rhetoric, I wanted to ignore it. It was both self-serving and dated. We all used to talk like that in the 1960s but, in fact, the world has not come to an end since then. Oddly enough, though, just while I have been writing about Beer, his stories have started to seem very relevant indeed. Everything that has happened since those planes flew into the World Trade Centre and the Pentagon speaks of an Anglo-American attempt to freeze the world, to stop it displaying any variety at all—running from endless ‘security’ checks and imprisonment without trial to the invasions of Afghanistan and Iraq. Global politics has collapsed into one-bit discriminations (Beer 1993, 33) between ‘us’ and ‘them,’ the goodies and the baddies—and you would have to be mad to believe that things will get better because of this instead of worse. As Beer wrote in October 2001, ‘Last month the tragic events in New York cybernetically interpreted look quite different from the interpretation supplied by world leaders—and therefore the strategies now pursued are quite mistaken in cybernetic ways.’

Of course, many interpretations of recent events are possible, involving, for instance, the concealed interests of Texas oil money. The interesting thing about Beer’s macropolitical analysis—worked out at great length in his reflections on the Chilean experience—is that it again revolved, literally and metaphorically, around a diagram of information flows, in which media and government models systematically reduce the variety recognisable in crisis situations and thus exacerbate the very crises they seek to represent and manage. Here, then, Beer’s subpolitics extended itself into the field of intersystemic and international relations.<sup>15</sup>

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<sup>15</sup> See also Beer (1993, 37-42) on the World Syntegration project.

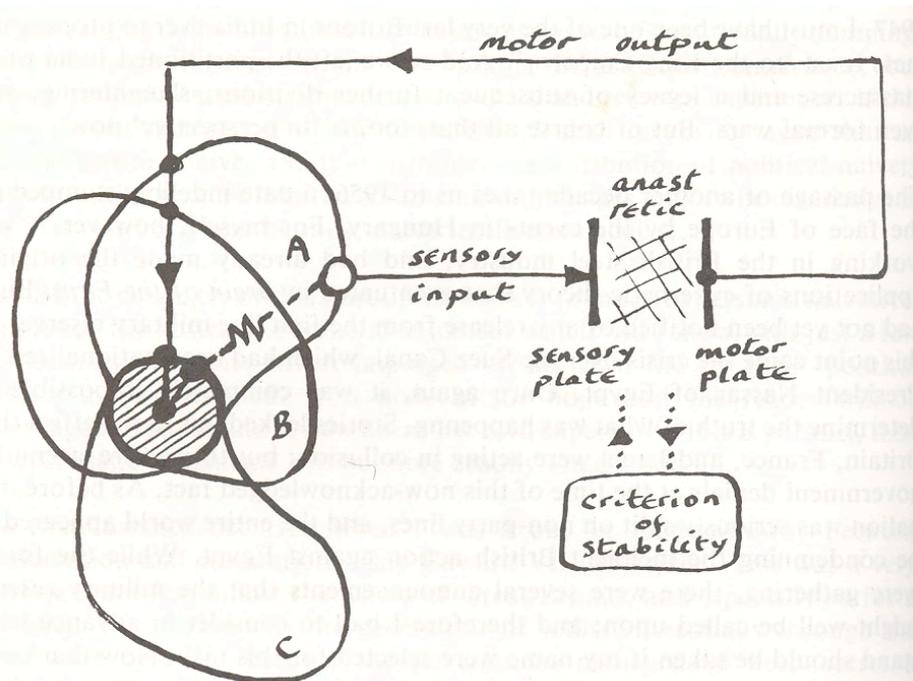


Figure 48. (Compare Figures 6 and 7)

Figure 10: The cybernetics of crisis

Where have we got to now? One might think of informatics and politics as two distinct projects, taking place in quite different and disjoint social arenas. What fascinates me about Beer's work is that in it informatics and politics were continuous with one another, with his distinctive diagrams of information flows and transformations fusing the two together, with one another and with the cybernetic ontology of unknowability and with a wild, if unrealised, vision of biological computing. Now we can move to my last topic, the spiritual aspect of Beer's work.

Beer grew up in the Anglican church, converted to Catholicism for 24 years and ended his life as a self-described Tantric Yogi. The affinity between cybernetics and Eastern mystical religion, especially Buddhism, is widely recognised. It is enough to note that one of the best popular introductions to recent scientific work on self-organising systems is *The Web of Life*, written by New Age guru Fritjof Capra, also the author of the famous *Tao of Physics* (Capra 1975, 1996). But these connections are usually made in a rather generic fashion. Capra's argument, for example, is that humanity is entangled in a complex system of

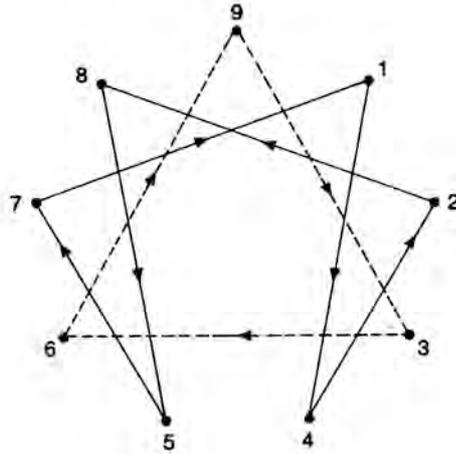
relations with all of the plants, animals and minerals that comprise the planet Earth, and because of those entanglements it is in our interest to respect and care for nonhumans and humans alike, much as the Buddha encouraged us to do. In Beer's case, however, the connections between the spiritual and technical aspects of his work are much more specific and concrete than that. I do not feel that I fully understand any of these yet, but let me just mention three of them.

(1) The icosahedron. At a mundane level it would appear that Beer could have chosen any of the regular polyhedra as the basis for his syntegegration approach to collective decision-making. It is true that the icosahedron accommodated a relatively large number of participants relative to other polyhedra, but what made this figure particularly attractive to Beer and others was a species of number-mysticism. Especially, under a certain geometrical projection the icosahedron gave rise to a figure known as an enneagram, whose mystical significance Beer traced back to Sufism and to the Vedas. During the Chile project, a Buddhist monk gave Beer a mandala which turned out to include an enneagram, and which Beer used in his meditational practices thereafter (Beer 1994b, ch 12).<sup>16</sup>

(2) The viable system model. An important aspect of the VSM which I have not mentioned before is that it supposed viable systems to be recursive. Each component of System 1 of any viable system was supposed to be itself a viable system. Thus, under higher magnification, each system 1 in fig. 7 was supposed to consist of its own five element system, and so on, both up and down the scale. Since the body has mind and consciousness, this implied, for Beer, that different levels of consciousness could be traced down to the individual cells of the body, and upwards beyond the body, to a kind of group consciousness that arose in syntegegration and eventually to the cosmos itself.

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<sup>16</sup> In this connection it would be interesting to explore further the connections between Beer's work and that of Buckminster Fuller (discussed in Beer 1994b, *passim*). The same Buddhist monk reappears in Lilly (1972).

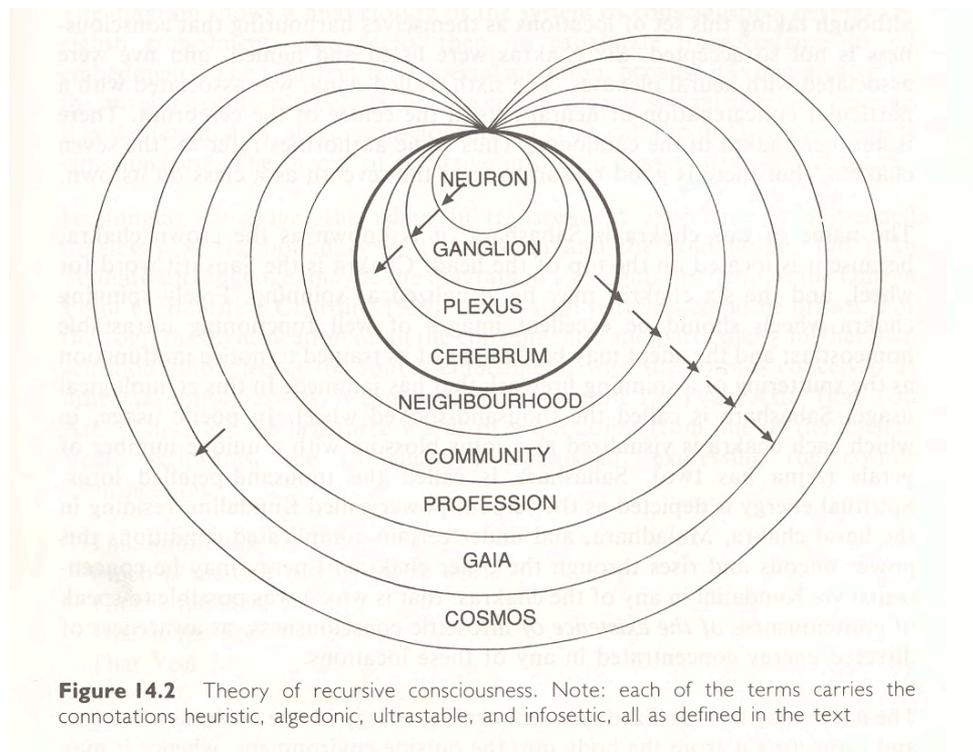


**Figure 12.4**

Consider:

- one seventh = 0.142857 (1...)
- two sevenths = 0.285714 (2...)
- three sevenths = 0.428571 (4...)
- four sevenths = 0.571428 (5...)
- five sevenths = 0.714285 (7...)
- six sevenths = 0.857142 (8...)

Figure 11: The enneagram



**Figure 14.2** Theory of recursive consciousness. Note: each of the terms carries the connotations heuristic, algedonic, ultrastable, and infosettic, all as defined in the text

Figure 12: Recursive layers of consciousness

(3) The VSM again. While this originated as a map of the physiology of the human nervous system, Beer also regarded it as a map of the mystical, spiritual body. The yogic chakras could themselves thus be mapped onto the elements of the VSM and ascribed their own consciousness along the lines just mentioned, a consciousness to which, as Beer put it, 'I attest from yogic experience myself' (1994b, 247). The previous mapping would then connect us to the divine at the cosmic scale—to divinity as the ultimate unknowable with which its constituent elements, including the human, endeavour to cope and live in the presence of.

Again, what interests me most here is that Beer did not attempt to separate his spirituality from his technical work, along the lines of the Modern settlement as Latour (1993) calls it, where the spiritual and the scientific, say, are thought to exist in separate realms. Nor were the two realms connected by a sort of parallelism. Instead, like the technical and the political, they were fused together. The icosahedron was at once a map for organising mundane social relations *and* a meditational device. The VSM was a map of finite human organisations *and*, at the same time, of the spiritual order of the cosmos. It is worth noting that what emerges here is a very 'earthy' view of the spiritual as continuous with the secular, a view in which, presumably, all of the elements of the spiritual world themselves become open-endedly in time in relation to one another. This is a very different theology from the one I was taught at school, with the Christian God as eternal, unchanging and quite apart from His earthly creation.

We have travelled along way from the Templeborough steel mill, via biological computers, the organisation as a performative brain and subpolitical diagrams of democracy, to arrive at the Yogic chakras and cosmic consciousness. What interests me so much about Beer's work—and British cybernetics in general—is the distinctive character of its interventions in so many fields that we usually think of as disjoint: informatics, management, computing, politics and spirituality (and this list goes on). I am also struck by the unity of these

interventions, which can all be seen as the working through of the ontology of unknowability or becoming in a way that breaks down modern disciplinary distinctions. We can perhaps find some inspiration here for our own work.

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## Figure captions

Figure 1. Stafford Beer (a) in the early 1960s, (b) in 1975. *Source:* Beer (1994a, xii, 315). Reproduced by permission of Allenna Leonard.

Figure 2. Ross Ashby. *Source:* I thank Ashby's daughters, Jill Ashby, Sally Bannister and Ruth Pettit, for providing me with this photograph and for permission to reproduce it.

Figure 3. The homeostat: partial photograph of four interconnected homeostat units. *Source:* de Latil (1956, facing 275).

Figure 4. The cybernetic factory. *Source:* Beer (1962a, 192, fig. 2). Reproduced by permission of John Wiley & Sons and Allenna Leonard.

Figure 5: The factory as brain. *Source:* Beer (1962a, 198, fig. 3). Reproduced by permission of John Wiley & Sons and Allenna Leonard.

Figure 6: Simulation of a cybernetic factory. *Source:* Beer (1962a, 200-1, fig. 4). Reproduced by permission of John Wiley & Sons and Allenna Leonard.

Figure 7: Control systems in (a) the firm, and (b) the human body. *Source:* Beer (1981, 130-31, figs 22, 23). Reproduced by permission of John Wiley & Sons and Allenna Leonard.

Figure 8: Operations room of Project Cybersyn. *Source:* Beer (1974, 330, fig. 12.1). Reproduced by permission of John Wiley & Sons and Allenna Leonard.

Figure 9: The syntegegration icosahedron, *Source:* Beer (1994b, 338, fig, S6.2). Reproduced by permission of John Wiley & Sons and Allenna Leonard.

Figure 10: The cybernetics of crisis. *Source:* Beer (1981, 354). Reproduced by permission of John Wiley & Sons and Allenna Leonard.

Figure 11: The enneagram. *Source:* Beer (1994b, 202). Reproduced by permission of John Wiley & Sons and Allenna Leonard.

Figure 12: Recursive Layers of Consciousness. *Source:* Beer (1994b, 253). Reproduced by permission of John Wiley & Sons and Allenna Leonard.

# Cybernetics and New Ontologies:

An interview session with Andrew Pickering

Kristian Hvidtfelt Nielsen<sup>17</sup>

khn@si.au.dk

The Steno Institute

University of Aarhus

## Introduction

In April 2003, the Centre for STS Studies at the Department of Information and Media Studies, University of Aarhus, hosted a two-day seminar, *Cybernetics and New Ontologies*, with Andrew Pickering. The idea of the seminar was to have Andrew present some of his recent work on cybernetics and new ontologies, but also to open for a broader debate on the methodological and philosophical perspectives of his work within science studies.

The program of the first day included an afternoon interview session in which I posed Andrew some questions about his academic background, his way into STS, his work and its implications. The following is an edited transcript of the interview session. The interview took place in front of the rest of the seminar participants, most of whom eagerly joined the discussion. Since I don't know the names of the people who raised their voices, I have chosen just to write as *questions (Q)* and *answers (A)*. Andy answers, while I and other participants are responsible for the questions.

**Q:** Let's start out with your own academic history in terms of 'the mangle of Andrew Pickering'. In your book, *The Mangle of Practice*, you play with the idea of the mangle as a kind of theory of everything. You say that the mangle is a conceptual tool that can be applied to, well, in theory, everything. So, let's

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<sup>17</sup> I would like to thank Randi Markussen of the Centre for STS Studies at the Department of Information Science for inviting me to conduct the interview with Andrew Pickering, and Andy, of course, and the rest of the audience for making the interview session a very interesting afternoon hour.

apply it to Andrew Pickering. The mangle of practice, of course, is the open-ended dialectic of resistance and accommodation. If we look at your academic career in these terms and start out with the beginning, with you working as a physicist and then turning to the sociology of scientific knowledge (SSK). Could you please tell us a little about what resistances you met in physics and how SSK accommodated you?

**A:** Oh yes, the story of my life. When I was a little boy, I was fascinated by the mystery of it all. At school, that fascination turned first into a fascination with chemistry – I liked all the pretty-coloured chemicals and all the strange things that happen when you mix them together. However, I went off chemistry when we started doing organic chemistry – it's not a very sensual business doing organic chemistry except everything smells awful – and, so, I got interested in physics. It seemed clean, elegant, and non-smelly. Those beautiful equations fascinated me, and I just went into physics without really thinking about it very much.

I was glad to be able to go from school to university. In those days, the British educational system was expanding which made it easier than ever to go to the university. When I was doing my undergraduate degree, I thought the big thing to do was a PhD. And if you were a real scientist, you would do particle physics. That was the hot subject in those days.

I discovered along the way that I was no good at doing material things such as performing experiments. So, without really thinking, I became a theoretical physicist. Which meant that at the age of about twenty-three my life had gone in very straight line, and I had ended up with a PhD in theoretical particle physics. Without really wanting one. Yet, I carried on and the most obvious thing to do was to get a job as post.doc. Which is where I came to live in Denmark, and had a post.doc. position at the Niels Bohr Institute in Copenhagen for a year. Then, I went back to Britain for a year, but at the same time, and this is the resistance, I suppose, I became more and more dissatisfied with what I was doing. Both the kind of technical work and the social environment of physics. I didn't enjoy being a physicist all that much. I thought it a very solitary and

peculiar way of going on. I don't know if other ex-physicists feel this. You know, your work very much cuts you off from the world. If you're thinking about what quarks are doing when you are working and technical problems like that, when you leave the office, you can't talk to people about it. You're disconnected. It's just one of those very long detours away from the world, especially theoretical physics not even handling material objects.

It was also – and now this is becoming an autobiographical talk – a very bad time to find a job in physics. Afterwards, when I wrote the history of particle physics, I discovered that during the ten-year period while I was learning to be a physicist there was one job in physics in British universities. And I knew the guy who got it!

So, for external and internal reasons, I decided that I was going to leave physics. And as this kind of story of becoming goes, it was 1975, I think, when I stopped being a physicist, and it was spiritually still part of the 1960s in which one could imagine 'dropping out' as people used to call it. If you didn't like physics, you could just leave – it didn't seem such a mad thing to do in those days as it probably seems today. And, so, I just left. I finished my post.doc. and went on holiday, in Morocco I think it was, and never came back.

I kind of hung around for a while, unemployed, and then decided to get a job. I found it very exciting that one could go and work in the real world and make a lot of money, which was also an attracting proposition to me. And then I discovered that all I was qualified to do in the real world implied working for the military. I'd learned how to program a computer in those days and the only places that hired computer programmers were the Ministry of Defence in Britain and various defence contractors. So, I found that my only useful skill, it seemed to me, was to be working for this world that I didn't really want to work with – the military!

Well, I kind of wandered around for a long time when a friend of mine who just finished his PhD in physics a year before me told me that there were jobs going in academic life in this field called science studies. Somebody had told him that, and I believed him even though it turned out not to be true. But, because

of that, I looked into the world of science studies, and this is all. A tale of open-ended resistance and accommodation.

There were two great centres of science studies in Britain in those days, probably in the world, the university in Bath, where Harry Collins was, and the science studies Unit at the University of Edinburgh, where David Bloor, Barry Barnes, and Steve Shapin were just inventing what was called the Strong Programme in the Sociology of Scientific Knowledge. These were the ancient of days; these were the glory days of science studies when Bath and Edinburgh were creating a great sensation in the world by having big controversies with philosophers of science. Basically, the philosophers were either rationalists or realists, i.e., they believed that either scientific knowledge was special because it conformed to some rule of rationality, or that it was true because it grasped on to the world in some unique way. Of course, the argument that was being pushed in Edinburgh and Bath was that scientific knowledge is socially constructed; it's actually a social thing. What people believe has to be understood in terms of their social background rather than the correspondence to the world or the special rationality. Philosophers were up in arms, there were great debates going on; it was very exciting times.

And I decided to go back to Edinburgh. I decided to rejoin the academic world by going to the science studies Unit in Edinburgh. At the time, I thought I could just do a Master's Degree and then I would go on and have a lecturer job in the university. But that didn't work out. I ended up living on a succession of research grants as people do today, I suppose. I had to make some connection between my old life and my new life, and, so, I did studies of my old discipline, particle physics. At first, I did various case studies, and then I wrote *Constructing Quarks*. So, I did manage to recycle some of my previous existence. And that gets you up to 1984, but I should probably... Ask me another question!

**Q:** In "The Mangle of Practice", you say that your first book didn't quite match the theoretical setting in which you found yourself, i.e., The Strong Programme in the Sociology of Scientific Knowledge. Would you say that the empirical material that you were working on offered you some sort of resistance that

didn't accommodate well into SSK? If so, what other accommodations did you then find? And was it a conscious choice, or, rather, something that just happened?

**A:** I do believe that empirical research is an important thing. I also believe that doing empirical research is something different from kind of imposing a set of theoretical prejudices or some explanatory scheme on the material. I think if you really seriously try to deal with empirical material you should be learning about it, not reproducing what you already know. So, how does that go here? Well, when I went to Edinburgh, the basic story in the Strong Programme was that we should understand scientific knowledge as being somehow a product of the social in some sense or other. The only version of that that I could understand was the one that I associate with Barry Barnes, i.e., the so-called 'interest model'. You look at a body of scientific knowledge and ask: Why do these people believe that? And you try to account for it in terms of their social interest, that is, something to do with the group that they belong to, what is to the advantage to that group. There were many studies that kind of demonstrated that, e.g., the classic study by Donald McKenzie called *Statistics in Britain*, where he traces the different ways of developing this technical field in 19<sup>th</sup> century Britain and associated them with the interests of the different groups, which espoused the different kind of statistical formulas. The kind of statistics we do now is associated with the group of professionalizing middle-class people who wanted statistics to be... well, a profession. And the other way of doing statistics had something to do with a more conservative, organic view of society. So, you plausibly make this correlation between interests and bodies of knowledge.

Unfortunately, when I tried to look at the history of particle physics I couldn't see any of that kind of thing going on whatsoever. Maybe I was a bit naïve, but it seemed to me that particle physics was a kind of self-enclosed little universe that didn't seem to be very susceptible to these big outside social interests.

Those interests didn't explain the kind of controversies that were going on in the field. And I thought, one had to think otherwise.

The favourite kind of methodology of the Strong Programme was to look at a controversy in science. The idea was that, in controversies, because scientists disagree about how the world is, you couldn't appeal to the world to explain the diversion in these positions. And you couldn't appeal to rationality, because, presumably, all parties in the controversy were rational. Therefore, when you looked at a controversy, the argument went, you would be able to see the social roots of various beliefs cleanly exposed independently of realism and rationalism.

When I looked at a few controversies, it seemed to me that the best explanation you could give was that somehow controversies were structured by people's expertise. If you're an expert in this technique in science, you would tend to use that technique to construct your knowledge about the issue at hand. And, vice versa, if you're an expert in a different technique you would use that one. And it didn't seem surprising to me in that instance that controversies arose. So, the explanation of controversies had something to do with the expertise and the technical resources, more generally, that people brought to bear in the production of knowledge. This was a kind of social patterning of knowledge; it was a kind of Strong Programme approach, except for the fact that I couldn't help noticing that the fact that expertise itself was developing in time in the production of knowledge. Expertise wasn't a fixed thing that explained everything else; it was something that grew up in the process producing knowledge. That's where I started thinking that there wasn't anything fixed, reliable social that you could use to explain why people believed what they did. That model, then, seemed different to me from the standard sociological models that were then in existence. And that model was the one that I kind of blew up and elaborated in to the story in *The Mangle*.... That's the kind of resistance and accommodation, you asked me about. And I repeat, because this is something important, that the analyses I gave in the books grew out of just trying to explain what I found empirically in the world, you know, looking at documents, reading people's papers, looking for the history of experimentation, and things like that. It wasn't that I had some kind of preconceived view of how the world was that I, just using this information, tried to give some flesh to. It's really important to

understand that going through empirical material should change your understanding of how the world is.

**Q:** To understand you correctly, you say that the empirical material offers you resistance that you try to accommodate by means of new understandings, new theories?

**A:** Technically, a resistance is to some kind of project, or, at least, this is the way I use the word in *The Mangle*.... Of course, you always come from somewhere; I mean you don't approach empirical material without having any ideas whatsoever. But, again, the great thing about doing something like history or ethnography is that there's something for your preconceptions to bump into. It happens all the time when you're trying to write up any kind of study. It just doesn't work – what you think you want to say, it just didn't come out that way when you write it out on the page. The resistance is just the sense that something's gone wrong. What you thought in the beginning is not going to work in laying out your material. The accommodation is just to change your ideas, change the material, get some data, think about what the project is, fiddle around with that... This is a nice story of becoming, really, when you think about it.

**Q:** Also, it reminds me of your idea about scientific practice: Physicists encounter resistance when they work with their machines, trying to get the material to do certain things, and then stand back and try to accommodate whatever comes out of their experiments into their theories.

**A:** Exactly. The thing that real scientists have that I don't have anymore is equipment, apparatus. A lot of scientific practice is precisely just fiddling around with machines, not with ideas. If you want to make a difference between the social and the natural sciences, it would be that the natural sciences have something else to fiddle around with apart from ideas and documents. They've got the material world to play with.

**Q:** That's also Latour and Woolgar's conclusion in *Laboratory Life*: What's the difference between them and us? It's really that they have a laboratory and we don't!

Let's move forward in time and have look at some of your more recent work on cybernetics and the parallels to science studies in general. It seems to me that one of the more obvious resistances that your studies of British cybernetics stump into is the idea of a representational idiom in science. The cyberneticians you study choose performance and activity to representation and theory. In a recent article in *Social Studies of Science* called "Cybernetics and the Mangle", you accommodate this resistance into science studies by concluding:

"Theory in Science and Technology Studies need not rest at the level of theory. Taking my cues from the homeostat, for example, I can see now that there is a mangleish style of engineering, distinctively different in approach from the classical approaches in engineering most of us are familiar with, and, likewise, following Beer and Pask, there can be mangleish approach to management, the arts, politics, and spirituality."

Now, what I would to ask you is simply this: Do you think that there's a mangleish approach to science studies and what is it?

**A:** It's the kind of thing I do. What can I say? I'd be interested in if you or anybody else has got any clear ideas on where this should take us within science studies itself. My way of thinking, since I wrote that article, is going in a different angle. Actually, the study that's in *Social Studies of Science* is a last version of a series of talks that I gave. Whenever in the past years somebody invited me to give a talk on my work, I would give this kind of talk about my three cyberneticians. When I first gave the talk, which is kind of an entertaining talk – look how amusing and interesting and imaginative these people were! – I remember saying in the end: "And I don't why I am talking about this. I just thought it was interesting. What do you think?"

I gave several versions of this talk, and before the last one, at the Virginia Tech, I thought I would have to work out why I am interested in these people. Because I hadn't really worked it out – you just kind of plunge into these projects. That's when I realised that the homeostat was just like a little engineering model of the picture of the world that I tried to develop in my book *The Mangle of Practice*. That's why I liked them; they were simply doing the same thing as me. They were me, in a sense, and I was them.

It seems like a pretty stupid conclusion. Why study somebody because they are the same as you? So, then I had to say to myself: Why aren't they me? And the answer is: Well, me I wrote that book – it is words, it is representation, and it was funny to write it – the representational book which argues against representation in favour of performativity! So, the reason why the cyberneticians were not me is that they somehow made this ontology flesh. They built little machines that somehow exemplified it, instantiated it, and elaborated the ontology as well. They constructed things that were engineering devices that could do things in the world. And my fascination ever since I saw this point is not actually to do something new in science studies per se – because I reckon the analysis I gave in *The Mangle of Practice* is a pretty good analysis. I'm not sure what to do with it except that I should slow down...

Of course, one thing that I did after *The Mangle of Practice* was to try and see if the analysis would go through another instance. One project I had was to write the history of agency, which was to go from microstudies of laboratories to conceptualizing the entire history of the world as a kind of mangleish process. To try and see big social transformations and big transformations in technology and the sciences as being locked together in certain periods. So, I became very fascinated in World War II as a time with big interrelated transformations in science, technology, and society. And, so, your question "Where does this take us?" – Well, maybe it takes us to things like that. We could change the scale. We could try to look at big things. We could take the sensibilities of open-endedness, becoming, resistance and accommodation, and the dance of agency and see how concretely these things play out in information science, for example.

Now, there are a few people around the world that seem to be interested in this kind of open-ended reconfiguration of the social, technological and information sciences. There are approaches to programming that actually look very mangleish too. There are endless ways of taking these sensibilities and putting them to work in other empirical studies on different scales.

My contribution was, as I said, trying to do this large-scale, macro-mangling, and I published a few papers on that. My favourite paper is still not published, and it's about the history of the synthetic dye industry and organic chemistry in the 19<sup>th</sup> century and the way the structure of industrial Britain changed in the same process as the structure of modern chemistry changed. Knowledge of the benzene ring somehow goes with the history of the dye industry, and vice versa. I find that very interesting. But I guess, as I was saying before I went on that detour, that the thing that I find most exciting, as I also write in this paper, is the realization that one can bring this ontology down to earth in all sorts of concrete, constructive projects like engineering, like management, like doing information science as in the example of Stafford Beer, or strange ways of considering spirituality and the arts etc. etc. I think, I might have answered your question somewhere along the line.

**Q:** Well, not quite, I think. You've talked about performativity in the arts and the sciences, but the question was: What is the consequence of a performative turn in science studies?

**A:** So, I haven't answered? Well, in a way I've implicitly answered the question by showing you Stafford Beer so far. Stafford Beer exemplifies the way that you could interfere...

**Q:** But, performativity in science studies, wouldn't it mean that, instead of studying Stafford Beer, you might somehow work with him in what he's doing? That would be performativity, wouldn't it?

**A:** Yes, like Moses glimpsing the Promised Land but never quite making it, right?

**Q:** I believe that you find many examples in science studies of the kind of performativity we discuss here. Of course, there's the work done by Michel Callon, Bruno Latour and colleagues at the Centre de Sociologie de l'Innovation in Paris where they do science studies but also engage in managing science and technology projects. Ideally, they try to apply the conceptualisation of science that comes out of their particular way of doing science studies in evaluating and managing big scientific projects.

Also, there's a unit at the Dibner Institute at MIT where they do the history of recent science and technology. They've discovered that, in order to make scientists and technologists living and working today interested in their project, they had to engage with them in new and other ways. Simply, in order to get the material they needed for their project, they had to give the scientists an opportunity to decide what material was important, which topics were interesting, and how the history of different disciplines ought to be written. So, in a way, in this particular project, scientists and technologists are actively engaging in the history and sociology of recent science, and, vice versa, historians and sociologists are no longer distant observers of science and technology, but engage with scientists and technologists in performing contemporary science and technology. These, I think, are two ways in which performativity may play a role in science studies.

**A:** Of course, the accusation is that I'm not practising what I preach.

**Q:** Rather, we shouldn't be preaching so much. Are you preaching?

**A:** I don't know whether I'm preaching. This recent science project at MIT is interesting in this connection (if it's the same one that I know about), because it doesn't actually work. They didn't actually succeed in getting scientists to contribute to writing their own history. I was just over there two weeks ago and they're just disbanding the project...

I know what you mean – as for myself, I would love to be out trying to build robots, for example, trying to take what I've understood from science studies and build that into robots – well, maybe, I could do that. But, of course, there are all sorts of difficulties in moving from one field to another. I keep being nice to Rodney Brooks at MIT, hoping that he'll say: 'Why don't you just come and work in my lab for a year?' He hasn't caught on to this yet.

I'd like to be a management consultant, like Stafford Beer or other people in this field, and get a fabulous consulting fee for just talking about Heidegger for one day at IBM. But, again, it's a practical problem of how to move into that field.

**Q:** Even if you work with performativity, like you do, you have to be representative. You represent performativity in your work. Perhaps we shouldn't be preaching performativity so much because we'll end up being caught up in our own critique of the representative idiom. No matter how much we like the ontology of performativity we have to work within an ontology of representation. We represent performativity.

**A:** I stick to my ontological point of view, this ontology of becoming. I think that's right. What I argued in *The Mangle of Practice* was not that we should completely forget about representing things. I didn't argue that representation was a waste of time or that it was misleading or anything like that. I argued against representation as being some kind of self-enclosed, autonomous activity. What I said was that we have to rebalance our understanding of science to play up the performative aspects of it, and then we have to think of representation as something that happens in relation to performance, rather than something to be understood just on its own terms. This is an argument against, for example, traditional philosophy of science, which says that all we need to do is just examine scientific knowledge and the relationship between the bits and that's all we need to think about. It's against internalist history of science which suggests that science is a purely autonomous endeavour that has got nothing to do with the outside world.

I wanted to beef up the idea that science is something that really does engage with the material world and the social world all the time. Its representative components have to be understood in relation to that, with the way of getting on in the world, rather than something which is either true or false. So, the argument wasn't "away with representation!", because then I would be a total idiot writing books that said... Of course, Wittgenstein did something like that, didn't he? Climb up the ladder and then you throw it away.

**Q:** There's a funny separation going on between words that we immediately think of as being representative and then doing things in engineering or science which we connect with performativity. With Wittgenstein and also Austin in mind, at least we have to stay open to the idea that words also perform.

**A:** I don't like these references to Austin very much. Part of what is at issue here is the dreaded 'linguistic turn' in philosophy and the social sciences that took off somewhere around the turn of the 20<sup>th</sup> century and in which all problems about the world were rephrased as problems about the word, about language. So, instead of saying "how is the world like?" we say "how do we speak about it?", or, "how do we produce knowledge about something like that?" The effect of the linguistic turn is to produce this kind of ontological effect that we're trapped within language. You can't get out of it. Steve Woolgar and his program of reflexivity is a perfect example of being trapped in the mirror maze of words and never getting to anything and not being able to do much there. It's interesting that Woolgar couldn't carry on when he started to talk about reflexivity. Part of what I want to do in talking about performativity is to capture the agency of the material world. When I hear people saying that language is also performative, which it is – to me that is to remain within the mirror maze of words. It doesn't remind people of what I want to remind them of which is: we aren't just doing language. We are also doing material things in the world – even spiritual things, since I read Stafford Beer... [Silence.] Have you all run out of questions?

**Q:** Well, I would perhaps also like to talk a little bit about the difference between classic SSK and people like yourself and Bruno Latour who speak about material agency. Of course, this is a debate that's been around at least since the 'epistemological chicken' debate between Harry Collins & Steven Yearley, on the one hand, and Bruno Latour & Michel Callon, on the other, in the book you edited, "Science as Practice and Culture" (1992). If possible, I would like to take up this difference again, inspired by the discussion about "The Mangle of Practice", which occurs in "Studies in the History and Philosophy of Science" vol. 30, issue 1 (1999). Interestingly, it's the exact same issue in which we find Bruno Latour's discussion with David Bloor based on Bloor's article "Anti-Latour". Now, I found this juxtaposition between the Latour-Bloor discussion and the critique of your work quite intriguing. I detect, in these more recent debates, a slight change compared to the chicken debate in that the debates are becoming less constructive and more personal. Do you think that this whole debate about sociality and materiality is still a fruitful way to go on in science studies?

**A:** If you look at the debate between Bloor and Latour, I actually think you could talk like Thomas Kuhn about this: It seems to me that there are two incommensurable paradigms in play. The people in Edinburgh like David Bloor, who I see every day, has got a certain way of constructing the world, a way of paying attention to it, picking out certain features, so that you can tell a reliable, causal, modern, explanatory story. It's a certain way of setting up the problem. I actually doubt, and I've said this to Bloor a lot of times, that he can understand what people like Bruno Latour is doing.

Latour is coming at things from a completely different angle, a completely different problematic. So, what David Bloor can see in Bruno Latour's work is precisely David Bloor. You always see yourself, right? I said that before, didn't I? So, he sees in Bruno Latour the kind of things he himself picks out everywhere and says: "Bruno, that's very good." And then he sees all the other bits, which make no sense to him whatsoever in terms of the classic, modern approach to

the sociology of knowledge, and he dismisses them as being some kind of French frivolity. [Laughter.] You'll have to edit the tape on this.

For me, it's very interesting historically because I started off in Edinburgh and I feel as if I've understood exactly what Bloor is saying. And, somehow, in the course of my own work, I have drifted over to having much more sympathy to the way in which Bruno Latour comes to things. Because it includes things; sociology of scientific knowledge excludes. The actor-network can talk about transformations of the social. The social isn't the causal centre anymore; it is something that is being transformed in the history of science and technology. And this seems to me like the way in which we should understand it, if we want to be sociologists of science and technology. What an awful lot of people want to know is: How is the social world changing as the material world changes? So, I think, I can now understand what Bloor is talking about, and I can understand what Latour is talking about, and I can see these two things are being incommensurable. I think, well, if we say that the Strong Programme is a Modern sociology with a capital M that means we can all understand it 'cause we've all grown up in the modern world. But, what Bruno is doing, and even what I'm trying to do, is much harder to understand because it doesn't go well with all these projects of modernity.

There is a kind of asymmetry here. In the chicken debate, Latour & Callon are arguing with Collins & Yearley, and Bruno says something like: "I can get you in my sights, but you can't get me in yours". I think that's true. From one side, you can understand the other side, but not vice versa. This is very frustrating to everybody. Bloor is probably quite angry with Latour. Look at Latour's reply, he says something like: "I give up, David. I've been arguing with you for twenty years now, and we haven't moved an inch."

What can I say? It might be that arguments like that don't go anywhere as far as the people that are seriously having them are concerned. But, it might be that they are informative for the people that are standing around watching them. They might be useful in that sense.

**Q:** They are useful as an entry to science studies.

**A:** The thing that delighted me when I was trying to put together this volume called *Science as Practice and Culture* was that Harry Collins wrote an article attacking Bruno Latour, and then Latour and Callon replied. This is the great virtue of this volume that it crystallizes the fact that there are two rather different approaches in science studies. Before, that was always being covered up. We're all personal friends, and it was somehow agreed not to criticize each other in public.

Of course, I say two, which is a kind of dichotomizing that I'm not allowed to do, but I have a weakness for such things...

**Q:** I would like to take up performativity again and ask about the current state of doing STS from where you are. Why are you writing your stories about quarks, cybernetics etc., apart from the fact that they are interesting? What drives you in writing? What changes are you aiming at?

**A:** I would be very unwilling to say that there is any single drive behind what I'm writing. If I said that, I would be doing a traditional sociology of knowledge in relation to my own work, wouldn't I? The drive would then explain what I was doing. And it would be a drive that pre-existed and therefore didn't become. Then, I wouldn't actually be learning from doing my research, which I would like to think I am doing.

I could say part of my project is an anthropological one: Here's this strange tribe and I want to study it because I think it's fascinating and that's what anthropologists do. But, another part of the answer would be one that I foreshadowed this morning. At some sub-political level, I believe ontology is important. It's no coincidence, as they say, that modern science is a science of representation and domination and, out there in the real world, we're representing and dominating nature with often disastrous results, and we're representing and dominating Afghanistan and Iraq. We live in a time where the ontology of representation and domination has gone mad. Now, if we could take seriously the ontology of becoming (and I myself find it very hard to take

this seriously – I mean, it's utterly alien to the world I grew up in), but, if, somehow, we all took it seriously, we would go on differently. This is a sub-political thing. This is why I talk about Beer and how do two systems relate to one another. You could just kill the variety of the other one, but you could also experimentally come into coexistence with whatever that system is doing and vice versa. It's a very different way of being in the world. And that's why it's interesting to look at these concrete projects that these cyberneticians did. Some of them are overtly political; some of them are just engineering, building robots. Some of them are fun, almost art installations. If you were to multiply all these projects and understand in terms of ontology of becoming, the political idea is that the world would be very different. And better!

Or, at least, we would have another option. We would subconsciously be able to conduct ourselves in different ways from the ways that we do now. And they are not that great, are they? In my lifetime, things seem to have got worse rather than better. Any alternative is better than that.

**Q:** It seems to me that you are introducing another aspect now. You have, on the one hand, the ontology of being vs. the ontology of becoming and, on the other, also the good vs. the bad. It's not clear to me that the ontology of being maps directly onto the bad and vice versa; that if we stick to the ontology of becoming rather than that of being we would have a better world. I'm not sure. If you want to, you can surely conceptualize warfare and write about what is going on in Iraq in the context of ontology of becoming. Ontology of becoming is in fact a means with which to dominate others.

**A:** You're complicating the issues splendidly. What I'm doing is laying out these naïve thoughts. Last week an ex-student of mine back in the States send me this email saying: Have a look at this website. And the website belonged to the U.S. Dept. of Defence and was about this "netcentric warfare". You should be very interested in that within the world of information science. And the site was all about having people informed about everything everywhere – rather like Stafford Beer – for precisely the point of killing Iraqi terrorists or something like

that. And then, I actually read in the Sunday paper that they're just sending this high-tech battalion out to Iraq and these are the people, the netcentric warriors. So, you're quite right: There's nothing magically good about this ontology and its project. And it does get complicated at this point. But, the argument might be: Well, at least when I talked about Stafford Beer I showed you how this ontology could be implemented differently in this democratic sub-polity. So, it is something which is non-modern and which points in a certain direction, which is not netcentric warfare, and which is profoundly democratic – democracy is a boring topic, but here's something new, I think: It's a way of arranging people. And, certainly, there's faith in people in Beer's work. If you can arrange people to come together, constructively and experimentally, you could do no better than having giving them the chance to come out with something they think is better.

The question is, is there any external criteria of 'better'? You're saying there is, because you had the second axis. I rather incline to think that what better is is what emerges; that there is no platonic essence of 'good' and 'bad'. The better we can hope for is a certain kind of experimentation, which gives things a chance to emerge in the world, and which you can think about. My favourite example is the old "What people learned to do with electric guitars in the 1960s". At the beginning of the 60s you could never imagine making the sound of Jimi Hendrix. And, yet, it just turns up, and I think it sounds fantastic. Any pre-existing criteria would not tell you that Hendrix or Neil Young would sound fantastic. It just had to be found out in this open-ended experimental process.

**Q:** This whole discussion seems to imply that you can choose between different ontologies. That is not the meaning of ontology that I learned in philosophy.

**A:** Finally, I've sorted out what's going on here. I subscribe to this ontology of becoming. I think that's how the world really is. And I base that on everything I said in my book *The Mangle*.... I also notice that the scientists I'm studying don't think about the ontology of becoming. They construct the world differently. What can we say about that? We can say that that's a way of standing in the

flow of becoming. As a matter of fact, the modern sciences are standing in the flow of becoming in a way that effaces becoming. Scientists don't talk about it; nevertheless, they are plunged into it. That's a project.

And, then, there must be this other project, a different way of standing, which is to actually recognize the flow of becoming. So, a lot of the time when I talk about ontology, I'm talking about the way in which people think about ontology. I'm encouraging you to think becoming. Whether you like or not, the world is going to become, but you can stand in that flow one way or another. Or, you can choose all sorts of hybrid, muddy positions between these two poles.

There's one ontology, how the world is, and the other is ontological imagination; how we imagine the world to be like. I think that resolves that question.



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