Peter Checkland: trailblazer *extraordinaire*[1]

John Naughton[2]

When I first encountered Peter's work, several things struck me immediately about his approach. The first is that he was intensely committed to operating in the messy real world of organisations in which groups of people engage in purposeful activity but often find themselves enmeshed in complex difficulties for reasons that baffle even those most intimately involved in them. Secondly, he seemed interested not in imposing externally-generated 'solutions' on these people, but in helping them arrive at outcomes that might improve the situation and were both systemically and culturally feasible in their organisational context. Thirdly, he saw the systems practitioner as an actor in the problematic situation, with responsibilities that are generally shirked by external consultants. And finally, he held strongly to the view that some systems concepts were essential to this process of analysis and 'action' research.

Discovering Peter's work was a pivotal moment in my professional development. It seemed to me that here was something we could offer in good faith to our Open University students. It was a way of using systems ideas intelligently in the real world. There were, however, two obstacles to making this happen.

The first was that although Peter's writings seemed to me to be remarkably clear – unusually so for academic material – an astonishing number of people seemed to find his central ideas hard to grasp. I will come back later to why that was so, but for now all you need to know is that they did. Of course, the students who did the Masters course at Lancaster did seem to have a good grasp of Soft Systems Methodology, but that was only to be expected. After all, they were mature postgraduate students, many of whom had had significant industrial or organisational experience; and besides they had a whole year in which to come to grips with the great man's thinking. But at the OU we had only part-time undergraduates, many of

[1] Edited text of a contribution to a Systems Symposium celebrating the work of Professor P.B. Checkland held at Lancaster University Management School, April 20, 2012.

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whom came from indifferent educational backgrounds and were new to learning. The only time we saw these students face-to-face was at a one-week Summer School on a UK university campus. How could we hope to inculcate in them the essential principles of Soft Systems Analysis (or Soft Systems Methodology – SSM - as Peter christened it)?

Solving that problem became for me the first great challenge of my teaching career. What I set out to do was to distil the essence of the approach into a package that would be comprehensible to, and applicable by, our students. I approached this by creating a *Reader's Guide to Checkland*, which was originally a cyclostyled booklet that attained *samizdat* status in the OU community but was later properly published as a course unit in our Third Level Systems Course (T301).

In its various manifestations the *Reader's Guide* was a severely utilitarian publication; it was basically an attempt to answer the question: *what do you really need to know in order to understand and apply Checkland's methodology?* It involved a close, guided reading of his original article on SSM, plus an analysis of the *constitutive* and *strategic* rules of the approach together with some observations on the philosophy underpinning SSM. Together with some case studies created by my colleagues, the *Reader's Guide* formed the basis for a group project which all Systems students tackled at Summer School. In this way we introduced thousands of OU students to Peter Checkland's thinking, and in the process changed some lives and shaped quite a few managerial careers.

One of the things I learned from this, by the way, was what I had been put on this earth to do. I suddenly realised that there are three kinds of people. They are trailblazers, road-builders and travellers. Trailblazers are rare, precious, invaluable people -- thinkers who hack their way through an impenetrable jungle, leaving marks on the trees as they go. They create the rough outline of a way through the jungle. But most people are not fit or able enough to traverse that rough path. So they need road builders, who can take the trail and turn it into something that travellers can easily use. Peter is a trailblazer. I'm a road builder. And most of the rest of us are travellers.

Given this background, you can perhaps appreciate why, for me, the publication in the *International Journal of General Systems* of an autobiographical essay by Peter was such a landmark event. In this 24-page article[3], he looks back at his personal, professional and intellectual development – from childhood to his current eminence. It's a fascinating account of a personal and intellectual journey, and I commend it to you in its entirely. What I want to do here, though, is to draw out from it some points that I found particularly interesting or significant.

The first is the fact that it is a *personal* account of a personal journey. It opens with his childhood in Birmingham and takes him through school, National Service (in the RAF), University (Oxford), his career in industry and then in academia and as a sought-after consultant. Of course, it also covers his intellectual development and the evolution of his contributions to the Systems field, but it is rare to find in the pages of an academic journal any acknowledgement – let alone one as revealing as this – that the author of the article is anything other than a disembodied intellect.

Secondly, there are two interesting transitions in Peter's personal odyssey. One is the way he moved from being a distinguished product (he got a First) of a temple of laboratory-based scientific reductionism – the Chemistry Department of Oxford University – into a formidable exponent of holistic thinking in practical, everyday settings. The other is the transition he made from being a successful manager of R&D in a major industrial company to becoming one of his university's most distinguished scholars.

The two transitions were not unconnected, of course. They began after he was promoted to run an R&D department of about a hundred scientists. "In my new role", he writes,

> "I became aware of something called Management Science and went rather eagerly to its literature. After all, I was an ex-scientist, now a manager. Surely, this would be just what I needed? I was surprised to find it almost completely irrelevant to my day-to-day concerns as a manager. I had discovered a literature which seemed to focus entirely on the logic of situations which recur. Obviously, there are such situations,… But as a manager, I was normally concerned with the issues which made the situations I faced unique, rather than the logical structure of them which might be general. Obviously, the meaning of the word 'science' in the phrase 'Management Science' (or in the phrase

[3] Checkland, Peter, "Learning your way to 'action to improve' – the development of soft systems thinking and soft systems methodology", *International Journal of General Systems*, Vol. 40, No. 5, July 2011, 487-512.

> 'Social Science' for that matter) cannot be the same as the meaning when used in the phrase 'Natural Science'. Nature's world reveals regularities; human situations are always capricious… I realised I would have to supplement the way of thinking I had absorbed as a natural scientist if I was to cope with the buzzing confusion of the real life which goes on outside laboratories. But I was not dismayed by this. I found the glorious improbably variety to be observed in the human tribe fascinating very stimulating. My feeling was: 'hand on to the scientific ability to think rationally, but enrich it so that it is more relevant to human situations'."[4]

As I read that passage recently, I was catapulted back over three decades to the moment when I first came on Peter's work: it was that unique combination of trying to think rigorously while respecting the capriciousness of organisational situations that marked him out as such a distinctive thinker.

It turned out that Gwylim Jenkins's Systems Engineering[5] was useful as a starting point only in the sense that a diving board is useful to a diver. Once Peter and his students began delving into the *problematiques* encountered in real-world organisations, it became clear that a different approach was required. One (controversial) element of this was that the work had to be done as *action research* – where the researcher enters a problem situation and becomes a participant as well as a researcher. From a scholarly point of view, this raised two problems: the first is that of deciding on the dividing line between action and research; the other is that the researcher becomes *accountable* for the contribution that he or she makes. "This", Peter observes in his memoir, "is something most academics do not relish!"[6]

That's putting it mildly. The only way out of this dilemma was to become what Donald Schon later christened a "reflective practitioner"[7] – and that is indeed what Peter became, so that much of his later writing is really a sequence of increasingly meditative reflections on systemically-informed interventions in problem situations. This also explains why he found the work of Geoffrey Vickers so compelling, because Vickers – a celebrated City lawyer with wide experience of life as well as business – was himself the ultimate reflective practitioner. Peter is now working on an intellectual biography of Vickers, and I for one can't wait to read it.

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[4] Checkland, op. cit., 496.
[5] Gwylim Jenkins was the founder of the Systems Department at Lancaster, and the person who hired Peter from ICI.
[6] Checkland, op. cit., 500.
[7] Schon, Donald, *The Reflective Practitioner*, Basic Books, 1984.

Some years ago, I was invited to a one-day seminar convened in the Hague by Jaap Leemhuis, who I am delighted to say is here today. The event was convened to survey the impact of Soft Systems Analysis in the years since Peter first articulated it, and attendees included senior business executives, academics, consultants and others familiar with Peter's work over the decades. It was a fascinating day, but overhanging all the discussions there hovered a puzzling contradiction. On the one hand, it was clear from the views and testimony of those present that Soft Systems Analysis had played a major role in elucidating and ameliorating significant organisational and policy issues in the private and public sectors in a number of countries. It was clearly an approach that worked. So why then, after four decades of success, was it so little known? Why were there so few university departments teaching and practising it? And why was its originator less celebrated than the average purveyor of inspirational corporate happytalk from a third-rate US business school? Why, in other words, did Soft System Analysis never become *fashionable*?

We can all think of possible explanations. SSA is hard, unspectacular work, which doesn't lend itself to business-magazine sound-bites. It's not a glamorous, high-tech *technique*. No computers are involved. It doesn't provide magic bullets or comprehensive "solutions" to organisational problems, merely agendas of possible changes that the people involved can sign up to – changes that are, in SSM-speak "systemically desirable and culturally feasible". It's hard to explain to the non-initiated: there's no "elevator pitch" for Soft Systems Analysis. And, as if these handicaps were not enough, there is the crowning disability of all, namely that the leading scholar in the field insists on writing in plain English rather than in the unintelligible jargon that marks out a proper academic discipline!

It's not as though interest in *systems* waned through those four decades in which Soft Systems Analysis stayed below the radar. On the contrary: they were decades during which the world woke up to the importance and significance of the concept. But this interest was channelled *not* into using systems ideas to effect change in the way actual organisations work, but into new, highly-mathematical disciplines like network theory and complexity theory and the tools that they have spawned. So when you meet someone at a party nowadays and reveal that you're a systems

academic then the likelihood is that you'll be asked something about Facebook or the Web or – *in extremis* – the Stuxnet worm.

I say this with feeling because it has all happened during my working career. And I think I know who's responsible.

His name is Thomas Kuhn, the great historian and philosopher of science. We're just coming up to the fiftieth anniversary of publication of the book that really made his name: *The Structure of Scientific Revolutions*.[8]

In it, Kuhn set out an arresting picture of how scientific disciplines develop.

Most of you know it, I guess, but in case you don't let me summarise it crudely. According to Kuhn, the scientific process is not a linear or uniform one, but rather the alternation of two phases – one called 'normal science', the other periods of upheaval or 'revolutions'.

As its name implies, normal science is business as usual. A community of scientists, characterised by a collective commitment to a set of "shared theoretical beliefs, values, instruments and techniques, and even metaphysics" which Kuhn called a "disciplinary matrix" and, more famously, a "paradigm," engage in research which essentially involves exploring – and hopefully resolving – discrepancies between the paradigm and the aspect of the real world to which it supposedly applies.

Despite the name, no scientific paradigm is perfect. There are always things it doesn't explain. Normal science proceeds by exploring and seeking to resolve these discrepancies, using as its basic assumption acceptance that the paradigm is fundamentally sound. And in many cases this is justified: the anomaly is resolved by adjusting the paradigm is or incrementally extending it. But, over time, the number of unresolved anomalies builds up, until eventually some practitioners begin to question the paradigm, and eventually to propose an alternative. At this point the discipline enters a period of intellectual crisis which ends with the overthrow of the old paradigm and its replacement by a newer one – the 'paradigm shift' of popular usage. After the shift normal science resumes – until the next time.

[8] T.S. Kuhn, *The Structure of Scientific Revolutions*, University of Chicago Press, 1962.

This, brutally simplified, is Thomas Kuhn's picture of the scientific enterprise. It was controversial from the outset because it clashed with more idealistic, normative ideas about how science should proceed.[9] But it has also proved immensely influential because it chimes with many practitioners' experience of how science is actually done. And of course the history of science is littered with examples of revolutions: think of the transition from Newtonian mechanics to quantum mechanics, for example; or of the emergence of the theory of plate tectonics in geophysics.

Kuhn's book has had an extraordinary impact. It is, for example, the most cited 20th-century book in the Arts and Humanities index – above Joyce's *Ulysses*, Wittgenstein's *Philosophical Investigations* and Chomsky's *Aspects of the Theory of Syntax*. The citation indices give us one measure of the extent to which his picture of scientific progress penetrated our collective unconscious. But in fact the penetration goes deeper than we can measure. His account achieved a kind of metaphorical dominance which, I think, is only rivalled by C.P. Snow's trope about the 'Two Cultures'.[10] And just as Snow's thesis exerted more leverage on the arts and humanities than on science and technology, so Kuhn's picture had a similar unintended consequence. For while scientists – who in general tend to be unimpressed by the philosophy of science – generally paid little attention to Kuhn, in other parts of the academic forest people began to sit up and take notice.

Kuhn's insight had a radical impact on one area in particular – the social sciences, which in the 1950s and 1960s were still struggling to attain academic respectability. So whereas scientists looked at Kuhn and found his account banal or unremarkable, social scientists and workers in other, non-scientific, fields (for example, management studies) saw in it an important message – not to mention a hope of salvation. The message was that if you wanted your discipline to be seen as academically rigorous, then it must have a theoretical and methodological core. It must, in other words, have a paradigm. And these non-scientific disciplines set about acquiring paradigms like ostriches going at brass doorknobs (as PG Wodehouse would put it), with consequences that were as predictable as they were sometimes malign.

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[9] For example, Karl Popper's view as set out in his *Conjectures and Refutations: the growth of scientific knowledge*, 2nd edition, Routledge, 2002.
[10] C.P. Snow, *The Two Cultures*, new edition with an introduction by Stefan Collini, Cambridge University Press, 2012.

To see why, we need to look more closely at the concept of a paradigm or a 'disciplinary matrix'. Although it played a pivotal role in Kuhn's account, he was surprisingly – some would say maddeningly – vague about it.[11] But the essence of it is a set of theoretical beliefs, methodological principles and values to which a mature discipline collectively subscribes. A paradigm defines what an academic community believes to be true and valuable. It enables the community to decide what is important and what is peripheral. It sets the criteria by which professional work in the field is to be judged – the standards to be adhered to, and hopefully achieved. It defines what should be taught to students, what textbooks and readings are approved. It enables appointment boards to decide who should be appointed to teaching and research posts, who should have professorial Chairs and who should be promoted and honoured. A paradigm, in other words, is absolutely central to the functioning of a mature discipline.

And this is not just a matter of organisational exigency, by the way. In any field of intellectual inquiry, we cannot operate without such a disciplinary matrix. Otherwise the field would just be a cacophony of incommensurable beliefs. As Karl Popper used to say, all observation is drenched in theory. So there's no escaping paradigms.

The problem arises when they take on a life of their own. One sees this most strongly in the early stages of scientific revolutions: the paradigm is so deeply embedded in the way a discipline functions that challenging or abandoning it would open too many cans of professional worms. Too many senior people have too much invested in the old order – which is why, sometimes, we have had to wait for them to die off before a new paradigm could really take hold. So sometimes – even in science – paradigm shifts happen more slowly than they should.

But in science shifts happen eventually, for the simple reason that, in the end, there's no way of fudging the issue. There was no way of concealing the fact that Newtonian dynamics simply couldn't cope with what went on at the sub-atomic level. In science, ultimately, reality intrudes. That's why, despite the best efforts of the

[11] See Margaret Masterman, "The Nature of a Paradigm", in Imre Lakatos and A. Musgrave. (eds), *Criticism and the Growth of Knowledge. Proceedings of the International Colloquium in the Philosophy of Science*, Cambridge University Press, 1970.

Stalinist state, for example, Lysenkoism eventually bit the dust: it was impossible to ignore the evidence provided by the natural world.[12]

The problems start when we move away from the exact sciences and into fields where the concept of an external, objective reality is more problematic. In these, there may be no incontestable reality – no natural world – against which to judge the applicability of utility of a paradigm. What happens then?

Well, one thing that does not happen is that practitioners throw up their hands in horror and wail that it's all too nebulous and that it's impossible to think of having a paradigm in such circumstances and that the field is too immature and that basically everyone should go back to [fill in the blank] – naming a discipline that it allegedly more mature and respectable.

No -- disciplines do not turn their back on paradigms, for two understandable reasons. The first is intellectual – one cannot do rigorous inquiry without some agreed conceptual framework. The second reason is more disreputable and is rooted in what one might call the political economy of academic disciplines. Students have to be recruited, taught and examined. University departments have to be created and staffed. Jobs have to be found for the boys – and girls. Scholarly journals have to be edited, peer-reviewed, published and subscribed-to. Professors have to be appointed, promoted, given tenure – and, occasionally, dismissed for unprofessional conduct. Governments and legislatures have to be supplied with "expert" advice. And so on. So even if there were no intellectual case for the evolution and maintenance of a paradigm, the practical, pragmatic arguments for having one are generally regarded as unanswerable.

Now you might ask: where's the harm in all this? And the answer may be that in most cases paradigms provide as good a way of organising academic life as any other. But there are times when paradigms can be pathological. The processes by which this can happen are varied, but we've seen examples of it over the last three decades. Take, for example, the way in which the academic study of English literature metamorphosed from the close and attentive study of texts and their context into a

[12] See Loren Graham, *What Have We Learned About Science and Technology from the Russian Experience?*, Stanford University Press, 1998.

preoccupation with 'theory' – psychoanalytic and otherwise – and the rise of obscuranticism as a token of profundity. Or consider the way the discipline of 'cultural studies' was overtaken by postmodernism, deconstructionism and other 'isms'.

You could say that it doesn't matter if fields like these are taken over by obscurantist nonsense. But actually there are some cases where a dysfunctional paradigm can have really pernicious effects on society. This happens when the discipline which has congealed around a pathological paradigm is taken seriously by governments and policy-makers.

A prime case in point is economics, a field that was once a broadly-based subject which took in history, institutional studies and psychology but which since the 1960s has become inexorably more mathematical and abstruse, to the point where it has largely been about the study of abstract models based on axiomatic assumptions about economic behaviour. Watching it from the outside, one could see all the signs of a self-reinforcing paradigm whose power and status grew in direct proportion to its remoteness from the real world. Mathematical dexterity and ingenuity came to be prized over any interest in understanding how real economies worked and how real institutions functioned within them. But such is the status of mathematics in our societies that the more incomprehensible economics became the more governments and policy-makers stood in awe of the discipline -- and the more its practitioners became valued advisers on virtually every public-policy or investment decision made by the state.

Then came the global banking crisis. In November 2008, Her Majesty the Queen visited the London School of Economics and asked a simple question of the academics gathered to welcome her. Why, she inquired, had none of them seen the crisis coming?

Which, when you come to think of it, was a jolly good question. On June 27 2009, the British Academy, which is to the humanities what the Royal Society is to scientists, held a symposium on the subject, after which two of its more eminent Fellows wrote to the Queen, summarising the conclusions of the symposium.[13]

[13] http://www.britac.ac.uk/events/archive/forum-economy.cfm (accessed 24 April, 2012).

"Everyone seemed to be doing their own job properly on its own merit", they wrote, soothingly.

"And according to standard measures of success", continued the British Academy representatives,

> "they were doing it well. The failure was to see how collectively this added up to a series of interconnected imbalances over which no single authority had jurisdiction. This, combined with the psychology of herding and the mantra of financial and policy gurus, lead to a dangerous recipe. Individual risks may rightly have been viewed as small, but the risk to the system as a whole was vast".

If you're of a suspicious turn of mind (which, I'm sorry to say, I am), this smacks of cant. What it's basically saying is that everyone's to blame, which is another way of saying that nobody's to blame, Ma'am.

The Royal Academy's bland missive irritated not just yours truly, but also a number of distinguished economists, among them my friend Geoff Harcourt, one of the greatest living experts on Keynes and a life-long believer in the proposition that there's a lot more to economics than applied mathematics. So he and his buddies set to and composed another letter[14] to Her Majesty in which they pointed out that the British Academy letter

> "does not consider how the preference for mathematical technique over real-world substance diverted many economists from looking at the vital whole. It fails to reflect upon the drive to specialise in narrow areas of inquiry, to the detriment of any synthetic vision. For example, it does not consider the typical omission of psychology, philosophy or economic history from the current education of economists in prestigious institutions. It mentions neither the highly questionable belief in universal 'rationality' nor the 'efficient markets hypothesis' — both widely promoted by mainstream economists. It also fails to consider how economists have also been 'charmed by the market' and how simplistic and reckless market solutions have been widely and vigorously promoted by many economists."

"What has been scarce", they went on,

> "is a professional wisdom informed by a rich knowledge of psychology, institutional structures and historic precedents. This insufficiency has been apparent among those economists giving advice to governments, banks, businesses and policy institutes. ... We believe that the narrow training of economists — which concentrates on mathematical techniques and the building of empirically uncontrolled formal models — has been a major reason for this failure in our profession. This defect is enhanced by the pursuit of mathematical technique for its own sake in many leading academic journals and departments of economics."

[14] http://memex.naughtons.org/archives/2009/08/19/8707 (accessed 24 April, 2012).

The reason I've gone on at length about the failure of the economics profession to equip either their students or our society about the fatal flaws in the real-world system they are supposed to understand is that it demonstrates what happens if society comes to rely on a pathological paradigm. It reminds us that if the subject you profess relates to phenomena that are important for society, then you need to choose your paradigm with some care. And you need to be alert to its deficiencies.

Which brings me back to the discipline of Systems. Just as economics decided to escape from the messy complexities of the real world into the rigorous solipsism of mathematical modelling, so in the last few decades we have seen an analogous retreat in our field. The work that is now perceived as valuable and academically respectable is also increasingly mathematical – as for example in the intriguing abstractions of network analysis and complexity theory. These paradigms are seductive partly because they trade under the protective cloak of mathematics – still the Queen of the Sciences in the public and academic mind. And partly because they facilitate the business of "normal" science – the solving of small puzzles that are such good sources of peer-reviewed publications. This has been great for grant applications and Research Assessment Exercises, but it is, as Charles Lamb once observed of books, "a mighty bloodless substitute for life". And it is what Peter has fought against for the whole of his distinguished career.