Prometheus, Vol. 23, No. 2, June 2005

Book Reviews

Deep Smarts: How to Cultivate and Transfer Enduring Business Wisdom

Dorothy Leonard and Walter Swap

Boston, MA, Harvard Business School Press, 2005, xii + 289 pp., US$29.95, ISBN 1-59139-528-3

The book is of good parentage. The authors (husband and wife) are respected US academics, she from Harvard Business School and he from Tufts University, and the back cover has glowing testimonials from such names as Rosabeth Moss Kanter and John Seely Brown ('… a must-read for anyone interested in high-performance organisations'). An extract appeared in the September 2004 issue of the *Harvard Business Review* by way of a trailer, which I read at the time and enjoyed, so I was already favourably disposed towards the book when asked to review it. Now, having read it, I feel rather disappointed. The book does not (for me) add much to what was in the HBR article, so if you have read the latter, the book is probably not worth the additional investment of money and, more importantly, of time.

Nevertheless the theme of the book is very important to business and is a core challenge for Knowledge Management: how do you transfer business expertise to other people, particularly that deep, experience-based knowledge of your organisation's recognised experts? Such expertise is probably critical to most businesses, and were those with that expertise to leave unexpectedly, taking their knowledge with them, the consequences could be devastating. With the baby boomers about to retire, this problem is becoming acute, and it is pointed out that by 2006 half of the entire workforce of NASA will be eligible for retirement.

Leonard and Swap have invoked the term 'Deep Smarts' for this expertise, no doubt looking for a punchy title, but for me it doesn't work. It is too much of a glib Americanism that does not travel well, and I feel it trivialises something important. Even worse, they don't really define the term. They do state 'Deep Smarts are as close as we get to wisdom' (p. 2), but this does not help, as 'wisdom' is another poorly-defined concept that to my mind seems to correlate with old age and little else. Leaving aside the matter of precise definition, the term raises a number of other related questions. For example, how many people in an organisation are thought to possess 'Deep Smarts'? Is it something associated with only a tiny minority, or is it something more widespread? Bill Gates has apparently identified about 20 people at Microsoft and then noted 'we would be in trouble if they left', but

Prometheus ISSN 0810-9028 print/ISSN 1470-1030 online © 2005 Taylor & Francis Group Ltd
http://www.tandf.co.uk/journals
DOI: 10.1080/08109020500099370

does this imply these are the only people with 'Deep Smarts' in that organisation? What about Bill Gates himself, who is in the fortunate position of being in an organisation of such clout that even inferior decisions might be made to succeed, decisions that might fail if made in less powerful organisations. What about Carly Fiorina, the recently sacked CEO of HP? She may have been seen to possess 'Deep Smarts' six months ago, but does she now? If she has lost her 'Deep Smarts', where might they have gone? Might 'Deep Smarts' not be something automatically associated with an organisation's senior long-serving employees, at least while they are still successful?

In contemplating what is special about 'Deep Smarts' and who might and who might not possess them, I was reminded of a story that W. Edwards Deming, the late American Quality guru, liked to tell. During the Manhattan project to build the atomic bomb the physicist Enrico Fermi had asked General Groves, the head of the project, what was the definition of a 'great' general. Groves suggested that a general who won five major battles in a row would be 'great', and went on to say that only three in a hundred generals would qualify. Fermi then pointed out that as most battles involved forces of similar strength there was a 50:50 chance of winning each battle. The chances of winning five in a row was thus $1/2^5 = 1/32$, or three in a hundred, so the incidence of military greatness was no more than a consequence of random variation. Might it not also be so with 'Deep Smarts'?

Leaving aside such pedantry, let's adopt a more pragmatic view. Let's assume that, as with pornography, although difficult to define, we are willing to accept that 'Deep Smarts' exist and we would recognise them were we to come across them. How then does this book help the practising manager? Structurally the book comprises nine chapters and the argument develops from discussing what 'Deep Smarts' are and why they are important, through how they develop initially, the influence of beliefs, assumptions, and the social environment, to how they might best be transferred to others. It is argued that 'Deep Smarts' can only develop over time. Extrapolating (rather uncritically!) from the world of chess, Leonard and Swap claim 'Most evidence suggests that it takes at least ten years of concentrated study and practice to become expert (as opposed to merely competent). The Ten-Year Rule places some inescapable limitations on the development of expertise in management …' (p. 48). They also claim that 'Deep Smarts' are made up primarily of tacit knowledge—knowledge, like that of riding a bicycle, which cannot easily be articulated. Both these claims seem reasonable, but the consequence is that it is very difficult to transfer the 'Deep Smarts' from one person to another. Leonard and Swap devote two chapters to discussing how such a transfer might best be facilitated and come down firmly in favour of the necessary experiences being recreated in a controlled manner in a process they call 'guided experience', using someone having 'Deep Smarts' acting as a knowledge coach. They suggest four forms of this guided experience—guided practice, guided observation, guided problem-solving and guided experimentation. I found this part both interesting and practical, and for me it was the most useful part of the book. I also thought the various examples were well-chosen and informative. Many of these dealt with start-ups during the Internet-boom, where expertise had to be developed very rapidly within a number of fledgling organisations. The 'Ten-Year Rule' had to be bypassed, and this proved impossible in a number of cases.

Whilst I found the discussion on transferring 'Deep Smarts' valuable, I was less impressed with other sections, some of which did little for me. This was particularly so with the chapters on Beliefs (Chapter 5) and on Social Influences (Chapter 6)

which I found superficial and unpersuasive. I had the constant feeling that Kuhn had done a much better job 40 years ago in his *The Structure of Scientific Revolutions* (which I have seen listed as one of the great books of the twentieth century). I was disappointed there was no reference to Kuhn in *Deep Smarts*, but I think this is a common problem with business books. It is generally the case that each new business book that comes along, advocating some different approach, or even some panacea, fails to identify other similar and possibly rival approaches, or to explain what these alternative approaches have in common and how they differ. In this respect *Deep Smarts* is no better or worse than the others, or at least those I have come across. Although there is brief mention (but only in a footnote) of Nonaka and Takeuchi, and a recognition that some of Leonard and Swap's techniques are 'more specific examples of the four mechanisms … socialisation … internalisation … externalisation … and combination' (p. 265), this relationship could have been discussed in more detail. I would also have liked to see reference to Senge's *The Fifth Discipline* which I felt had resonances with *Deep Smarts* in several respects. For example, what is the relationship between Senge's 'Personal Mastery' and Leonard and Swap's 'Deep Smarts'? Similarly, why no mention of Goldratt's *The Goal*? This seems to me to be a case study on the transfer of 'Deep Smarts' from the Jonah character to Alex Rogo (Latin for 'I ask'), using a number of the 'guided experience' techniques (as well as Socratic questioning) advocated by Leonard and Swap.

Has *Deep Smarts* anything new to say, or is it simply a restatement of existing good practice? I am inclined to feel it is the latter. Surely being aware of where your expertise resides, thinking through how to protect yourself from losing that expertise, and doing something about developing or acquiring expertise you are lacking, are all part of 'good management' and always have been. Early on (p. 2) Leonard and Swap quote from *Managing by Results* by Peter Drucker: 'It is only in respect to knowledge that a business can be distinct, can therefore produce something that has value'. However, elsewhere in that same 1964 book, in a chapter entitled 'Knowledge is the Business', Drucker points out the following: 'Few questions force a management into as objective, as searching, as productive a look at itself as the question: What is our specific knowledge? Few answers moreover are as important as the answer to this question. Knowledge is a perishable commodity. It has to be reaffirmed, relearned, repractised all the time. One has to work constantly at regaining one's specific excellence'. Surely this is close to being a summary of the general theme of *Deep Smarts*. Nor does the solution of 'guided experience' seem particularly novel, given that this has been the primary mechanism for transferring knowledge from master craftsman to apprentice for centuries, and is today's preferred method for the training of professionals such as surgeons and lawyers. What goes around, comes around?

Nevertheless, if *Deep Smarts* helps to remind managers of the need to understand and steward their key knowledge resources it will be of value. I don't believe you need to read the full book, the HBR article may be sufficient, but it won't do any serious harm if you do. You may not learn much that is new, but it might catalyse you into necessary action. To be ignorant of, or to ignore, the message would be fool-hardy. Taking a quote from Deming (from another context)—'you don't have to do any of this; survival is not mandatory'.

Finally a minor quibble. On p. 193, Leonard and Swap quote a riddle, as evidence(!) that you have to struggle with something to understand it. The riddle is, what is being described by the following: *The notes were sour, because the seams split*? If you are successful, if you have the 'aha! effect', you are expected to come up with

bagpipes as the answer. Speaking as someone who has played the bagpipes, on and off, for over 40 years, I can say this is unequivocal piffle—it makes absolutely no sense at all. Not only is the adjective 'sour' one I have never heard used in the context of bagpipe playing (even from those who might be hostile!), but were the 'seam to be split' (itself unlikely), you simply could not get any sound out of the pipes. You might get a hernia trying, but nothing else, I assure you.

Fortunately the other examples quoted in *Deep Smarts* are much more believable.

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Breakthrough: Stories and Strategies of Radical Innovation

Mark Stefik and Barbara Stefik

Cambridge, MA, MIT Press, xiii + 294 pp., $29.95, ISBN 0-262-19514-3

Innovation is the holy grail of modern technology policy, and in this book, we are promised not merely innovation, but radical innovation. To add to the excitement, Mark Stefik, the first author, is no theoretician, but a manager and researcher at Palo Alto Research Centre (PARC), known throughout the world as a hub of information technology inventiveness.

The authors are Californians, at least by adoption, and proud of it. No European would pen such a grim list of acknowledgements as we find here. The authors thank everyone from the school Mark Stefik attended 40 years ago to the family who looked after Shadow the dog while the book was being written.

But matters improve once we reach the meat of the argument. Although the Stefiks seem not to have read Michael Porter on competitiveness, they make the same point as he does when they point out that brilliant inventors on their own are useless. They need a proper ecology to flourish, including an education system, sources of capital, a healthy government, an adventurous private sector, and at some point, a final consumer to buy the invention.

The Stefiks take a purist view of the role of the university in innovation. Academics make discoveries, as they see it, but should not get involved in anything too close to the coalface thereafter. Universities are there, instead, to produce future generations of bright graduates to toil in research laboratories such as, well, PARC. They regard such labs as a key twentieth century invention in their own right. In the US these labs gained in budgets and influence after the Soviets launched the space age. As well as a host of useful innovations, they became involved in basic science such as the discovery of superconductivity (but not, as the Stefiks say, dark matter).

The question which is not addressed here is what the future holds for these enterprises in a world where US economic dominance is on the wane and where large corporations are under threat from nimbler competition.

The issue, as these authors see it, is about how firms that fund genuine innovation can cope when they get what they wished for. A truly fundamental innovation can strike at the heart of a firm's existing business, or present opportunities in a new field with which the company is ill-equipped to cope. Both are threats, if only to capital allocation, which can be as disruptive as the invention itself.

However, the Stefiks do not claim to solve this problem. The core of their work is a range of interviews with present-day inventors and innovators which give the book a bitty feel but which do present new information on successful ways of running research. Many of the examples are from the US and from IT, and a suspicious number are from PARC, but there are others from parts of the world such as Japan and on topics such as the invention of Velcro. If there is one group of people who could benefit from this book, it is the finance director who wants a feel for what those money-guzzling and badly-dressed people over in the research centre are up to.

The Stefiks find that inventors come in a range of colours. Some are driven by theory; some by methods; some by data; and some by needs which they want to meet. This seems simple enough, although the overlap between data and methods is probably greater than they acknowledge, and provides a good typology of invention, which many will find valuable.

If there are generalised lessons from all the information that the authors provide, one is that inventors each have their own style and cannot be rushed. In the nineteenth century, the solution to this problem was to have private means, or failing that a rich patron. Now, indulgence on behalf of a multinational will do instead.

In addition, it turns out that like sports teams, or indeed nations, good research centres have their own myths and legends. The MIT Media Lab, for example, attaches high status to demonstrating products—a rarity for an academic institution where publishing is usually everything. At SRI International, the myth is that a small group left to itself can change the world. And perhaps most bizarre at first sight is Intel. Its ethos is minimal information. It does what it has to do to solve the problems it has got, but does not mount research for the sake of it. As a result, it makes use of most of the ideas it produces, a claim that most high-technology firms cannot make, and produces few spin-off firms to distract attention from the task at hand.

These pages contain many heroic accounts of innovation against the odds, for example of UNIX at ATT, where the project was concealed from management after earlier fiascos and had no funding.

These war stories are fun, but do the Stefiks have a significant reasoned account of how innovation works and how it can be encouraged? Here the scorecard is less complimentary. The book's case studies, based on interviews (including some Stefik-on-Stefik encounters), are many but they break the flow of the continuous argument that might otherwise have developed. (They are also illustrated with nasty hand-drawn artwork that a major publisher, in this case MIT Press, should have suppressed.)

Despite these problems, *Breakthrough* does make some fair points. One is on Open Systems, a substantial fashion item in the world of software and one that is regarded by many as the way of the future. Well, perhaps. But as these authors see it, the ethos of Open Source can produce incremental improvement for commodity products but will not bring about major breakthroughs. Considering all that is said about the Open Source culture as a new machine for energising innovation, it would have been fascinating to see this argument developed in more detail.

They also argue that the scale of science, especially in biomedicine, calls for new models for its structure and organisation, typically involving both universities and the private sector. In the UK, the US and a few other countries there is a long history of biomedical research in which universities and hospitals work together,

but the addition of the private sector adds a new layer of interests and conflicts, plus claims on profits when they emerge.

One lesson from these pages is that these new ways of working will have to do more than provide money, equipment and other resources. They will have to provide time and space for creative thought that is hard to channel or predict. Universities are already putting more pressure on staff to be productive in the short term, while the venture capitalists who fund many new businesses are not famous for their patience. We all think that we live in an era of breakneck innovation. But if the context for it to take place is removed, could the pace slow instead of increasing continuously as most observers predict?

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American Genesis: A Century of Invention and Technological Enthusiasm, 1870–1970—Second Edition

Thomas P. Hughes

Chicago, The University of Chicago Press, 2004 [first edition 1989], xxxii + 529 pp., US$22.50, ISBN 0-226-35927-1

This second edition of Thomas Hughes' modern classic on technological innovation and revolution presents the reader with a paradox. The author is potentially radical in the revisions of the argument he presented in 1989. Rather than providing a straightforward update of socio-economic change driven by technology and industrial systems by incorporating the information revolution into the body of his analysis, it appears that Hughes has decided that the industrial capitalism is revolutionary in a permanent and continuous way that will always continue, most probably driven by the US—and that this cannot be challenged from 'outside'. As the author notes in his extensive and somewhat contrite foreword, it seems that there is now little prospect of overturning or even challenging the spread of large scale technological systems such as the military, natural sciences, engineering or 'Amerikanismus' in management (interpretations of Fordism and Taylorism outside the US). This new material and the vision of history that underpins it are questioned towards the end of this review. First, however, an outline of the book and Hughes' arguments.

The first edition of this book ended with predictions in three areas: first, that a series of accidents (Three Mile Island, *Challenger* space shuttle, Chernobyl) would lead to a widespread questioning of reliance on technological systems; second, that this might in turn suggest a hostility towards and turn away from such systems and towards smaller scale situationally appropriate uses of technology; and third, that the next significant technological revolution might have its roots outside the US. Over the 15 years between editions, however, Hughes notes that the 'information revolution' underpinned by increased availability of computing power has its roots firmly in the US. In addition, Hughes seems to have lost the 1960s countercultural idealism that informed his previous conclusions, revising those with a tired sounding acceptance of the inevitability of the contemporary US-dominated organisation of society and work.

There are two great strengths to both this and the first edition's broad vision of technology, innovation and invention. First, the author continually emphasises that

any understanding of our development of power, engines, and industrial applications of the natural sciences must take account of political, demographic, and cultural contexts. Second, Hughes provides a rare historical perspective on industrial and technological invention, enabling fruitful comparisons to be drawn between for example the spread of electrical power and Internet provision networks. It is helpful to explore in depth one of the areas that Hughes analyses, to illustrate his approach and the insights it generates.

The longest and most detailed chapter takes the linked stories of the Tennessee Valley Authority (TVA) development and the World War Two 'Manhattan Project' on atomic research. Hughes, through careful collation of archival documents and contemporary reports, is able to trace the political, social, economic and cultural paths that actors followed during two very large scale state-sponsored projects—one designed to defeat poverty and regional under-development, the other to aid Allied efforts towards winning a world war. Hughes notes the many parallels between the two projects: managerial challenges, political resistance and enthusiasm, the need to draw on experience in industrial development while simultaneously organising in radically different ways to Fordist or Taylorist approaches, and finally the military–industrial takeover of both TVA and atomic research. Given his conclusions in the original text as to the hope that resistance from individuals and groups to military and industrial interests can provide, it is especially noteworthy that Hughes emphasises the reaction of the scientific community to the news that a viable atomic bomb had been developed. Before the two devastating explosions in Hiroshima and Nagasaki, a number of the Manhattan project team recommended either that the technology be kept as a theoretical threat to the enemy, or that the bombs be dropped in uninhabited areas of Japan simply to demonstrate their destructive potential. Subsequent to the destruction of the two Japanese cities, this small group was joined by many others from the scientific community, arguing that this new human power over such fundamental technology and power should not be ceded to military and industrial interests. However, the movement proved to be short-lived and ineffective, as US military and industrial organisations began to dominate further development and application of the innovations. Following his somewhat a-theoretical perspective Hughes does not pursue the *why* of this, preferring to emphasise micro-political action and individual self-interest, and always the role of the new dominant system of organisation that President Eisenhower warned the US population against in 1961: the rise of the 'military–industrial' complex.

In publishing this as a second edition, Hughes must have been faced with a difficult decision. As an analysis, his 1989 publication will remain forever rooted in its moment; perhaps this is as it should be. After all, Hughes' historical approach emphasises for us the centrality of temporal location to understanding technological innovation. It would in that sense be contradictory to substantively revise the story that the author tells. However, the foreword hints at the potential that exists for revising this form of historical analysis in the light of continuing social and economic change—but theoretical developments are important in this context too. Clark and Rowlinson[1] suggested recently that organisation studies would benefit from a 'historic turn' that acknowledged historically informed debates concerning the viability of industrial corporations during advanced capitalism, or explorations of the power that global corporations have access to. Hughes' analysis could be seen as an example of historical engagement, and therefore as a support to such a turn. However, one of the endorsements on the cover of the second edition

undermines this, suggesting that 'readers who don't look for theoretical argument in history won't regret its absence' here. If Hughes had been able to engage with historiographical debates or historical theories of interpretation then an already strong analysis would have been further supported.

Hughes may however be too harsh on his 1989 conclusions when he suggests that his anticipations of countercultural challenges to large scale technological systems were 'flawed' (p. x); he suggests that those first edition conclusions were influenced more by his own desires than evidence or analysis. There is considerable evidence and numerous academic accounts of both social movements (such as anti-globalisation protest groups) and individual withdrawal from the systems Hughes analyses. Indeed, Casey[2] goes so far as to suggest that an increasing number of people are reasserting their agency in relation to the organisation of production and consumption in order to seek re-enchantment in work and everyday life. Individual and small collective actions to construct identity, searches for emotion, meaning and spirituality all contribute to this. Casey's notion of re-enchantment may only be work-related, or, as she notes, it may eventually become political in a wider sense. However our individual relationships with technological systems such as modern science, large scale engineering projects, or the organisation of work unfold, we (and Thomas Hughes) should have confidence that resistance and counter-dominant actions and values will continue to be enacted and articulated. Thus, despite the protests of the new foreword, it may be that the key message of this book remains unchanged, and that it is one that continues to make the book an important aid to understanding the sociology of innovation and technology.

Notes and References

1. Peter Clark and Michael Rowlinson, 'The treatment of history in organisation studies: towards an historic turn', *Business History*, 46, 3, 2004, pp. 331–52.
2. Catherine Casey, *Critical Analysis of Organizations: Theory, Practice, Revitalization*, Sage, London, 2002.

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Creative Knowledge Environments: The Influences on Creativity in Research and Innovation

Sven Hemlin, Carl Martin Allwood and Ben R. Martin

Cheltenham, UK and Northampton, MA, Edward Elgar, 2004, 256 pp., £59.95, ISBN 1 84376 5187

Even among the most ardent advocates for managing creativity, there must be some nagging question whether this is an oxymoron, that creative production is the result of serendipity, chance, hard work, and working against the odds. To some extent this edited book would confirm that this is so. For example, if it is true that creative researchers are typically ambitious and motivated, it is also equally true that they can be dominant and aggressive (p. 22). However, if there is to be any semblance of a chance that creative production can be managed, then

the book also provides some principles for managing creativity intelligently and sensitively. It is concerned with developing a creative product—something 'not only novel and imaginative, but also judged to be useful and of good quality'—not a process. Despatching the epistemological implications of 'knowledge products' fairly quickly and shallowly in Chapter 1 (though this is not important in this context), the book then sets about developing our understanding of the issues involved.

Firstly, the environment needs to be classified into micro (individuals and teams), meso (institutions and companies), and macro (national, international) levels, each of which is 'nested'. Secondly, the physical, social and cognitive environments need to be considered. Because there is already a reasonable literature on the effects of the micro-environment, this book concentrates more on the macro level. Thus two chapters address the micro environment, while five of the nine chapters deal with the meso and macro environment. Perhaps most pivotal to the book's usefulness is Table 1.1 (p. 7), which sets out eight components of knowledge environments and their characteristics, as this becomes the basis of recommendations in the final chapter.

The comparative international study by Mason, Beltrano and Paul (Chapter 6) shows that the UK had a faster rate of building new relationships than France because of a greater flow of qualified scientists and engineers (QSEs) between companies. However, it remains undetermined as to whether this advantage is offset by greater costs in setting up networks and trust building. Although Bengtsson and Lind's chapter on developing a science park (Chapter 4) is more descriptive than analytical, it nonetheless offers some useful insights into such a venture involving the Triple Helix of government, university, and industry. Perhaps the most important of their findings is the need for quality 'relational entrepreneurs' to enhance the links between the various actors.

Kaiser's chapter (Chapter 7), which looks at science–industry relations during times of paradigm shift, I found to be one of the more useful contributions of the book. Using the paradigm shifts in telecommunications (to digital and optical) and in pharmaceuticals (to biotechnology), he shows that new modes of coordination are wrought by paradigm shifts in technology and other changes to the macro-environment. Paradigm shifts are defined as 'strong prescriptions on the directions of technical change pursued or neglected by innovative organizations' (p. 154). German industry, he claims, was shown to be caught short because of its orientation to traditional technologies (paradigm shift) and market liberalisation (macro shift), but adapted by outsourcing much R&D previously done in-house and by collaborating in the case of pharmaceuticals with US companies. Importantly, these two German industries adapted to paradigm change in different ways. Telecommunications developed stronger links with universities and specialist research organisations, while biotechnology adapted at the innovation phase by allowing specialist small and medium size research firms to lead the way (p. 168).

At the micro level, it is possible to create an appropriate environment by ensuring that there is an appropriate level of autonomy and cooperation, a common vision, sound project management, as well as attributes of openness, tolerance, and trust, according to the two chapters (Chapters 2 and 3) provided by Magnus Gulbrandsen and Mika Nieminen. University administrators and research funding authorities would do well to learn that bureaucracy and inadequate core funding are most likely to destroy creative environments. Perhaps, the

most subtle finding is the need for a degree of tension. For example, autonomy needs to be balanced by the need for research teams to coordinate; and some degree of bureaucratic auditing may actually provide the opportunity for research teams to reflect on their practices. Because creative environments need to be open to external sources of knowledge, the forms of interactions and collaboration are also a crucial factor.

Though the contributions are rather uneven in standard, there is much to be gained from the book by managers dealing with creative goods production. An aspect I found a little annoying is that the opening chapter didn't really segue well into the structure of the book. The closing chapter, however, did summarise the findings very effectively. The book's aims are relatively modest: 'it would seem possible to shape, at least to some degree, and to stimulate CKEs and their development' (p. 205). Usefully, the authors then summarise the ways in which those implementing creative knowledge policy might create the most efficacious environment by summarising the findings of the eight components of knowledge environments set out in Chapter 1. Some of these suggestions are unexceptional and would most likely apply in any organisational environment. For example, under Task Characteristics they suggest that 'it is probable that simple and routine tasks have a negative effect on creativity' and that 'it is important to have staff suited to the task characteristics and able to deal with the specific tasks involved' (p. 206).

The more important guidelines are those that deal with Discipline/Field as this involves the challenges of Mode 2 Knowledge Production. In particular, it is crucial for policy makers at national and institutional level to assess whether disciplinary knowledge production or inter/trans-disciplinary problem solving approaches are best suited. Other useful guidelines are those that consider the tricky issues of dealing with individuals: how to balance personal autonomy with institutional requirements; and, if Type A behaviour is associated with successful research in males how does one guard against potential harassment and bullying, though this is not discussed. It is pleasing also to see that adequate funding is listed as a 'basic aspect of organizations' creativity' (p. 211) as impoverished researchers have become almost emblematic of university research.

I would have liked at least a chapter on the effects of neo-liberal government policy on knowledge production and creative environments. For example, at the Second Conference on the Triple Helix of University–Industry–Government Relations,[1] Richard Nelson of Columbia University argued that such policies (especially the imperative to patent and to create products) adversely affect scientific communication by encouraging secrecy rather than publication, and create unnecessary 'transaction costs' to knowledge transfers from academia to industry. As well, one might ask how smaller countries such as Sweden (and as an Australian, I'm interested in my own country) effectively network and develop strategic alliances in emerging technologies without being swallowed by larger economies, particularly the US. I would also be interested to know about another issue raised at this conference, namely the appropriation of intellectual property by industry and whether universities, many of which are at least partly funded by government, are providing free or cheap product development for industry.

Nonetheless, this book will provide useful reading for policy administrators at national and organisational level, even if they do no more than read the first and last chapters.

Notes and References

1. Reported in: L. Leydesdorff and H. Etzkowitz, 'The triple helix as a model for innovation studies (Conference Report)', *Science & Public Policy*, 25, 3, 1998, pp. 195–203.

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