

Five things I learned when I slowed down

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A Fellowship

There is an old conversation that goes back and forth between scientists and those who hold the purse strings. Scientists want more freedom to discover, and they fortify their position by drawing on examples where serendipity unexpectedly turned new discoveries into benefits for society. Those who control the purse strings need to streamline scientific activity with the goals of those who provide the funds, and need an objective way of dividing limited resources amongst many hopeful bidders. Land and Water Australia (LWA) added an intriguing twist to this conversation by throwing the ball back into the scientist's court. When shortlisted for an LWA Senior Research Fellowship, you were given just one A4 page to describe what you would do and why, if you were given complete scientific freedom for 12 months.

As part of the scientific camp bleating for more space to operate, LWA had called my bluff. Did I really have a crucial project ready to go that needed no plan, no budget and no milestones – just freedom to operate? These thoughts troubled me on the first few days after I took up my fellowship. In my one-pager I said I would write a book about turning water into food that would cross over from a specialist to a lay audience. But how to begin?

I got permission to write from home, away from the phone and the internet. For a while I was lost without the constant interruptions I claimed to abhor. I needed distractions from the empty white page in front of me, just to convince myself I was actually doing something. But soon I realised the way out of this malaise. Before writing, I needed to start reading again. Of course I read the scientific literature, but I had learnt to read very superficially and selectively. Apart from formally reviewing the work of others, I tended to skim the papers from which I could lift the bits that bolstered or reinforced my existing position. I hardly ever used to read outside of my field, or things that would stretch or challenge my current understanding. There were certainly people in my network throwing fascinating stuff my way, but this was buried in the dark depths of my email in-tray. My mind was too cluttered to ever make it a priority.

The book “Out of the Scientist’s Garden: a Story of Water and Food” was duly published in February 2010. The writing part itself turned out to be almost therapeutic. It developed a gentle momentum and I felt like I could have gone on and on, were it not part of the deal I made with the publisher to keep the book under 200 pages so the RRP would stay under \$30. The time between final draft and holding the finished product in my hand was unbearably long. Responding to reviewers’ comments, negotiating with the publisher, selecting photographs, layout, editing and printing all took more time than I had allocated.

It is not my purpose here to write about the book itself. It is available in all good bookshops, as the saying goes, or through www.thescientistsgarden.com. The story I want to tell in these pages is the story behind the book – what I call “the five things I learned when I slowed down.” The five things I learned all came to me from friends or colleagues, who send me an

article with a note something like “read this – you’ll really enjoy it”. And though I intended to read them someday, they had languished forgotten in my in-tray.

The five things

Here they are: i) how to think across scales, ii) how to understand cause and effect, iii) how to plan, iv) when to push through and v) when to simplify. None of these five things could be considered a Eureka moment for me, but together they have fundamentally shifted my perspective as a scientist. They underpinned the way I approached the task of writing a science book for a general audience and they underpin the way I am doing my subsequent work.

1. How to think across scales

I saw Passioura’s visual parable over 20 years ago, and took it then as little more than a stimulating curiosity. At first he shows you some dots on a page, and these dots represent the detail of a problem we are trying to solve. There are big dots and there are small ones, and they are arranged in different ways, and I’m sure you could find an algorithm that can describe the way these dots are arranged on the page (left). But if you were to stand back a bit, take in more of the picture and drop some detail, you will find that those dots slightly merge into one another, giving us the illusion of light and shade (middle). And if you stand back even further, you will see that the light and shade actually form shapes that we recognize (right).

The point of this visual parable is that when you change scale, you are not simply extrapolating. We do not understand the man by looking at the dots, even though those dots

ultimately produce the expression on his face. For those who missed the warning in 1979, Passioura concludes the visual parable 31 years later with a sharp critique of the modern propensity to believe that we can alter plant adaptation by tweaking a few genes (Passioura 2010).

The problem with changing scale is that there are constraints and interactions at higher scales, which the scale below knows nothing about. This means I cannot say for sure how my discovery at one level will play out in the bigger picture above. Passioura says that changing scales is a bit like learning a new language, and if you want to converse with those working at different scales you have to learn their language. Language learning takes a long time and, according to Passioura, is best conducted as hundreds of unstructured conversations around the coffee pot in the tea room, rather than trying to institutionalise it through formal arrangements.

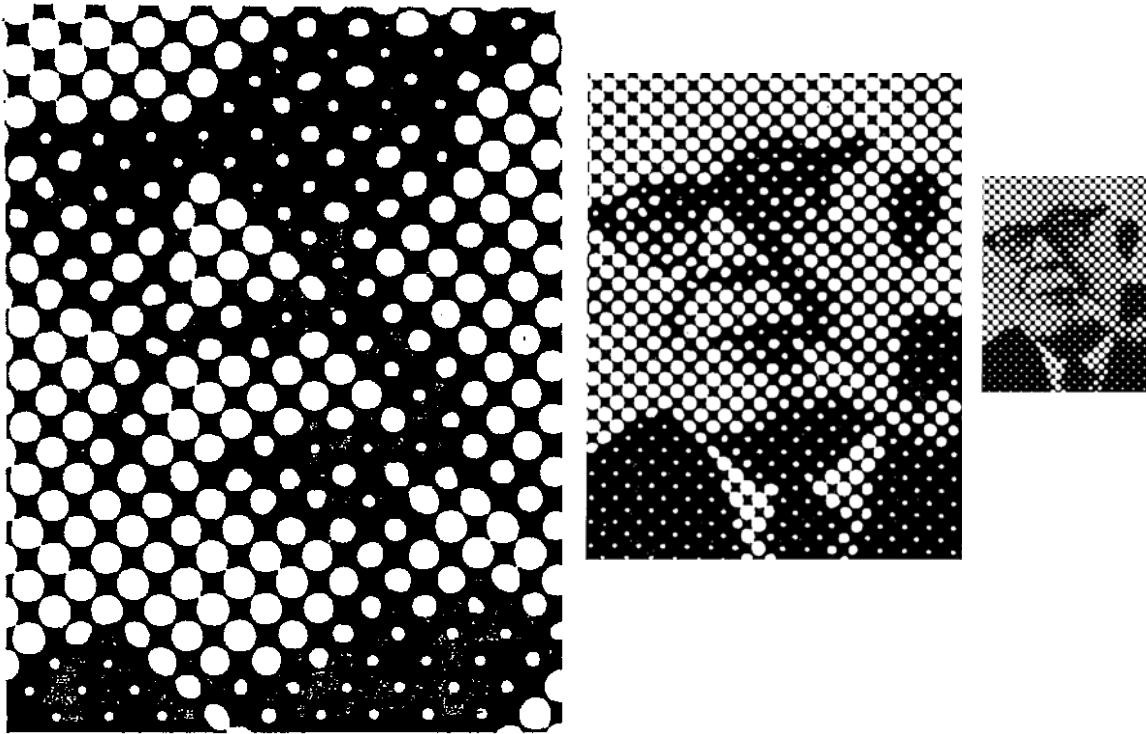


Figure 1. To see the bigger picture you have to drop some of the detail. We will never understand the expression on the man's face by concerning ourselves only with the dots.
(from Passioura 1979)

2. How to understand cause and effect

Understanding cause and effect is the core business of science. Sir Isaac Newton laid the foundations in *Principia*, showing that the planets in the sky and bodies on the earth obey laws of motion when acted upon by a force. Laws give the physical world predictable or deterministic behaviour. Biology and ecology are not quite as simple, and the reason we carry out the controlled, replicated experiment is to find out what causes what amongst numerous possibilities. We vary one or two factors called treatments, and try to remove any other factors from impacting on our observation. Statistics then helps us to formally separate the signal (variation due to the treatment) from the noise (variability from factors we could not control).

For much of my scientific career I thought there were two kinds of problems. There were those things we understood, which filled the pages of text and reference books (the known), and the things we did not yet understand (the knowable). The job of the scientist was to apply the robust methodology of science to the latter group. Snowden (2002) introduced me to two other classes of problems, the complex and the chaotic. Much has been written on these two subjects, and I cannot do justice to either here.

The more I read about complexity, the more I realized that many of the issues I worked on resided in this domain. Complexity is about how *whole systems* respond to stimulus or change, and by nature complex problems have an intrinsic uncertainty about them. The whole is sometimes less than the sum of the parts and sometimes more, but it is *not* the sum of the parts. We can no longer just say that x causes y when there are a whole lot of factors

that combine and interact across scales in a myriad of ways. It just might be that biology and ecology are not entirely reducible to physics (Ulanowicz 2009).

Once we grasp the idea of complexity it will cause us to proceed with more modesty or humility, because so often today's solution becomes part of tomorrow's problem. Certainly we would talk less about silver-bullet solutions and more about learning. The idea of learning appeals to me more and more as I progress in my career, because we will never know enough to live wisely in a complex world. Since a scientific career is forged in a competitive culture, it's so easy to filter out those who hold different or opposing positions, and to ignore the knowledge that comes from other fields of endeavour. If we can cast off the mantle of 'expert' and the threat of competition, we may truly be able to engage our knowledge with that of others.

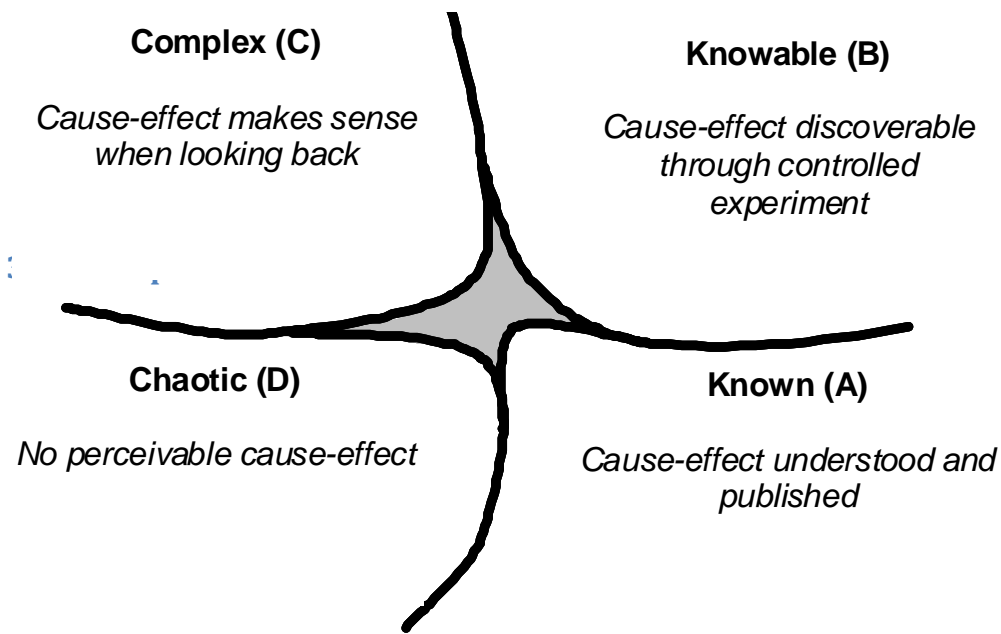


Figure 2. I thought my job was to apply scientific method to problems in the Knowable Domain B until they were captured into the Known Domain A. When dealing with complex problems we cannot explain everything from the bottom up.

(from Snowden 2002)

3. How to plan

I started to write “Out of the Scientist’s Garden” with no plan. I knew where I wanted to end up but had little idea how to get there. This did not worry me unduly because I have always operated this way. If I started with a rigid map, it became more a limitation than a guide. Then a friend sent me a wonderful typology of management approaches, which, although coming fairly and squarely from the business world, maps splendidly onto the scientific endeavour (Sarasvarthy 2005).

Essentially there are three types of goals and you need different managerial styles to attain them. There are defined goals, like building a bridge over a road, in which case it is obvious when and how well the goal has been attained. Then there is a desired goal – the kind of thing we see in vision statements – which is in no way as concrete as the bridge. Finally there is the ‘imagined end’, for people who know where they want to get to but have little idea how they will get there.

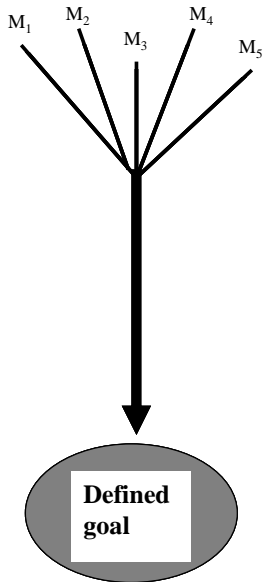
To attain the *defined* goal, we need managerial thinking. Someone has to make sure the bridge is built on time, within the agreed budget, and that it will not fall down. Although we know how to build bridges, the task requires considerable technical and logistical skill and we employ highly qualified people to do it. This is rather different from the *desired goal* where we are charting new territory. The board of the company cannot simply select the means to attain the goals like a bridge builder selects the engineers, steel and concrete. They need strategic management, which means they create the means to get to the goal, such as fostering unusual combinations of skills and embarking on some higher risk projects. So a defined goal needs managerial thinking but a desired goal needs strategic thinking.

It is plain common sense for a manager to count the cost of the materials for building a bridge and the cost of putting the materials together. But those seeking their imagined end are not swayed by the pre-project cost-benefit analysis. They start with just what they have got, and with their eyes on the distant goal ask themselves “what can I afford to lose?” The managerial and strategic thinkers ask “who is my competitor” and “how do we best exploit our pre-existing knowledge”? The entrepreneurial types ask “who could be my partner” and “how can I exploit the inevitable surprises that will pop up along the way” (Sarasvarthy 2005)?

I’m not for a moment saying that we want to fill our scientific organisations with people searching for some imagined end. We desperately need the managerial and strategic skills. But what we don’t need is some bland ‘one size fits all’ approach foisted upon us under the guise of efficiency and good practice.

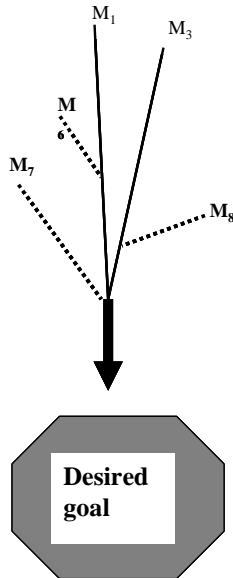
Managerial thinking

Select among existing means M_1 to M_5 and plan to meet defined goal



Strategic thinking

Generate new means (M_6 - M_8) to strive towards desired goal



Creative thinking

Start with what you've got and find the means to an imagined end

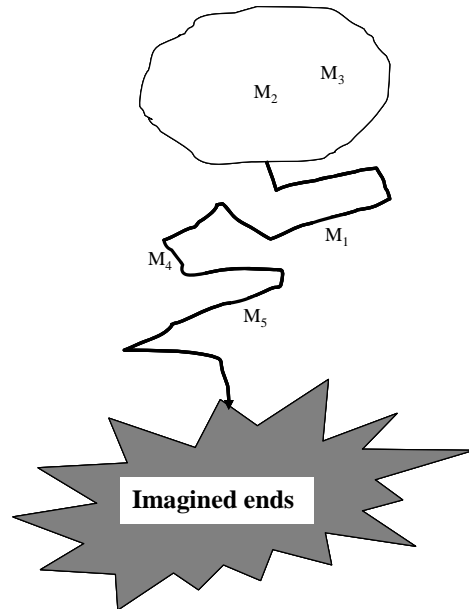


Figure 3. The nature of the goal determines the type of management style we deploy.

(from Sarasvarthy 2005)

4. When to push through

All worthwhile endeavours require perseverance, but science requires a special variety. The journey depicted in Figure 4 comes from inventor Dean Kamen, and wonderfully captures the lifecycle of a good idea. We start out cautiously and we may have to overcome a bit of scepticism from those around us. But the initial trials give encouraging results and some wider enthusiasm develops around the idea. We eventually secure the research funding and get the project underway. This marks a high point in our optimism, and only time is needed to deliver on the promise embodied in the idea.

As the work proceeds, we start to get results which contradict the earlier ones, and we find there a few more twists and turns than we anticipated. After a while we realise that things are not turning out according to the best case scenario we laid out in the research proposal or business plan. In fact things are taking much longer than expected and some pessimism creeps into the venture.

Not many scientists have all their eggs in one basket. We have some core business that keeps the lab finances afloat, but now we find that even our core business is suffering with all the resources that have been poured into the new idea. We get more pessimistic about the merits of the idea and ask ourselves “is it worth it?” At this point we have two options. Option one is the most favoured. We return to the original idea, dress it up in different clothing, and sell this new version, usually to different people. We push through the scepticism, secure the funding and do slightly different experiments, because this allows us to operate in the optimistic space in Figure 4.

Option two is more difficult. Kamen believes that most good ideas must travel through a dark night before coming back into the light. It is below the line, in that rather more pessimistic space, that we are forced to confront the limitations to our current way of thinking. It is only here that we can let go of those ideas we have grown so comfortable with, and which are preventing us from seeing further. We can no longer just tinker with the old idea when it needs a major overhaul. It is below the line where our minds undergo the preparation to learn something new.

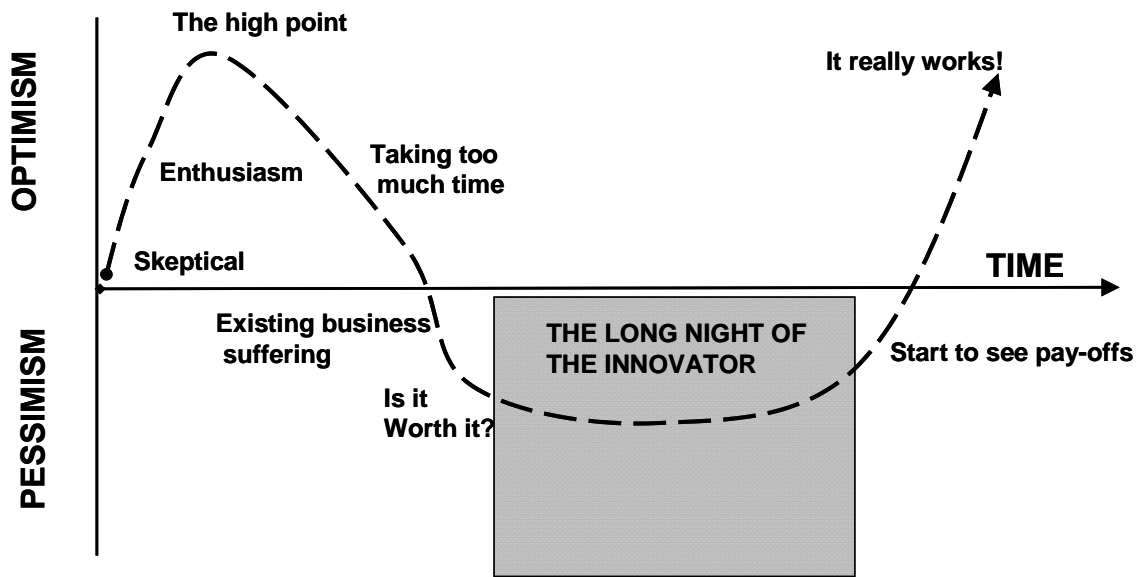


Figure 4. The progression of an idea through the dark night

Adapted from Dean Kamen <http://www.eurekamagazine.co.uk/article/8478/Innovation-in-a-nutshell.aspx>

5. When to simplify

There is a world of difference between simplifying a problem and finding that we have been simplistic. When we simplify we strip the problem down to its essential ingredients and this allows others with different perspectives to contribute their insights. To be simplistic, is to omit aspects of the problem that give the appearance of simplicity but eventually lead us into error. So how do we tell the difference?

The diagram below shows us the relationship between how useful our idea is and how much detail we must negotiate based on the original concept of Ward (2005). To develop something useful, we must engage the detail of the particular subject area. We start at position 1 in Figure 5 and make our way to position 2. At position 2 we have a useful idea (or product), but decide it is not good enough. We add more detail (or attributes to our product) because up to now there has been a positive correlation between adding detail and getting something more useful.

Unfortunately we find that after position 2 the game has changed. Our idea (product) has become overcomplicated and its utility has actually diminished as we head for position 3. The trick when we reach position 2 is to try a new approach. It may be that further progress comes not from adding more detail, but from taking it away – in other words, by simplifying the problem. Simplification arises when we reframe the original question in such a way that some of the original detail becomes redundant but the way forward comes into sharper focus.

This ability to reframe the question is central. There is no benefit in discarding detail we need, i.e. the simplistic road back to position 1. Moreover I'm not saying that that detail

between positions 2 and 3 is superfluous, or that there are simple answers to complex problems. The point is we do not need all the detail, until someone has asked a new question. This reframing helps us move to a new position where we can more usefully benefit from new knowledge (detail) – the road to position 5.

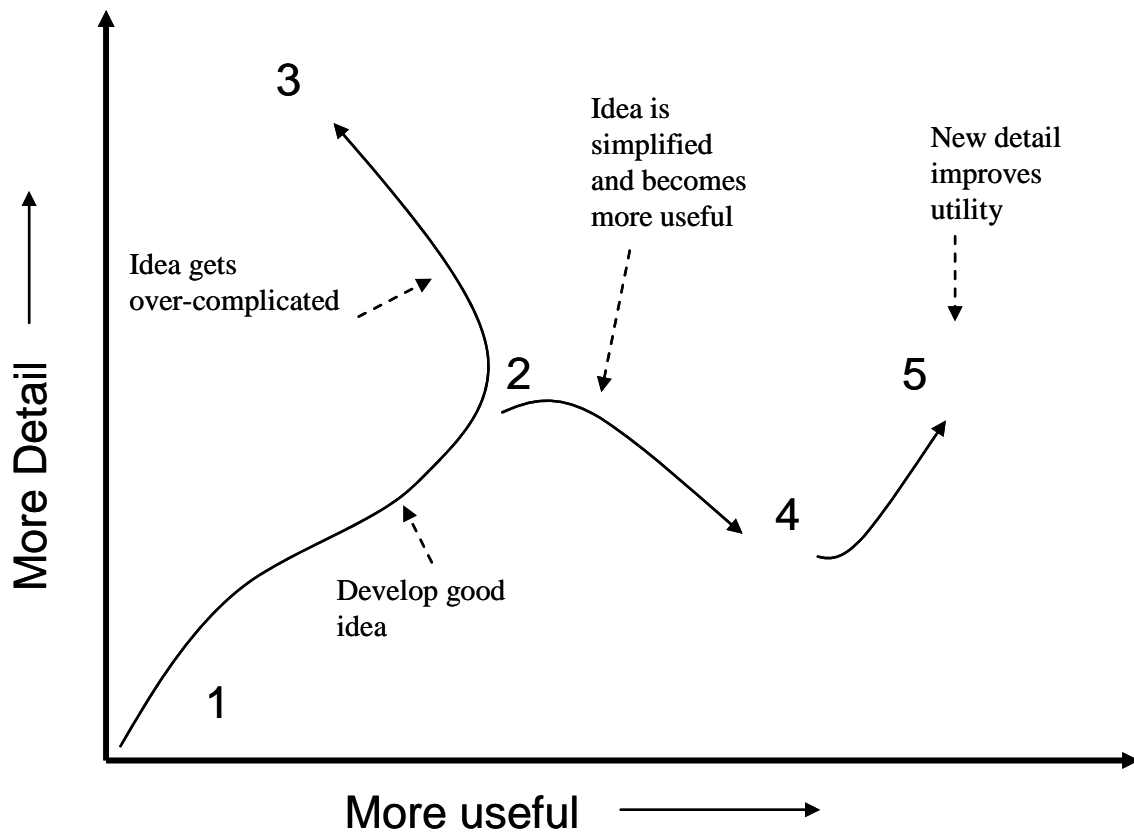


Figure 5. The relationship between utility and detail as an idea develops

(From Stirzaker *et al.* 2010)

Why tell stories?

At the early stage of writing ‘Out of the Scientist’s Garden’ I came across the proposition that we normally learn by accumulating new facts, but our understanding moves ahead in leaps when we ask new questions and see the old facts in a new light (Stankey *et al.* 2005). This was the stimulus I needed to depart from the usual scientific way of writing. Science claims to deal only with the facts, but to isolate the facts we tend to cut the problem up into little bits. It is by no means clear that we recreate the real world when we try to cobble the bits together again.

The idea of writing stories seemed to be the way around this. In the telling of a story we create a familiar world where the reader touches base with their own experience. New facts are woven in, but essentially the story is crafted to get the reader to make sense of their existing knowledge, and to perhaps see things in a different way. The story mode allowed me to put into practice the five things I learnt. The excursion into management styles was to remind me of the vast array of skills that have contributed to a world that produces enough food, even if it has not yet found a way to eliminate hunger (Figure 4). Similarly the subject of perseverance reminds us that some of the most important lessons are learnt when travelling through the dark night (Figure 3). The guiding principle of simplicity was drawn from Hollings’ challenge to simultaneously retain the lucidity that connects us to people and the rigour that connects us to science (Figure 5).

I dealt with the issue of scale (Figure 1) by starting small in my own fruit and vegetable garden but always pointing to the principles that underpin food production. The book closes by looking at what the whole world eats, how it produces this food and why, and connecting this back down to the tangible scale of a garden. The middle section links these two scales by

investigating how things work – soils, plants, rivers and aquifers. The idea of complexity (Figure 2) was easily contained within the story mode, because readers could draw different conclusions as their own experiences intersected with the story in different ways.

It remains for me to reflect on LWA's gamble: to see if a scientist could remain accountable to a project that had no formal mechanism of tracking accountability. My experience was that I had to rise to a level of accountability which could never be specified in a contract. But by far the most vital experience for me was to have the time to immerse myself in other peoples' ideas, and to weave them into my own imagined end.

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