will always observe actually occur.

Science policy, of course, is a future-orientated field of study. The relevance of the past is often underplayed, perhaps because we expect simply to read off lessons from the history of science and apply them wholesale to current dilemmas. As Bud points out, "arguments of the past cannot, and should not, determine how disputes should be resolved, or biotechnology regulated" (p.220). Instead, *The Uses of Life* illuminates the dynamics of the policy process and highlights the complexities in the emergence of new areas of research. Above all, it draws our attention to what is at stake when we define the boundaries and contents of our policy categories.

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The Golem: What Everyone Should Know About *Science* by *Harry Collins and Trevor Pinch* (Cambridge University Press, Cambridge, 1994), pp. xvii + 164, A$15.95, ISBN 0-521-47736-0 (pbk).

Three years ago I was in a quandary. I had spent the best part of twelve months chasing down a novel property of nerve cells which, if true, would suggest new areas of memory research and enhance my scientific reputation. Then one day the effect I was studying disappeared; experiments that had worked for a year abruptly stopped working. For two weeks I laboured over my apparatus, varying every parameter that seemed relevant – and some that did not – while the dread word "artifact" loomed in my mind. Just as suddenly, and just as inexplicably, the effect returned; experiments were hurriedly concluded; and the paper was published with scant mention of the problem I had encountered. This problem I came to regard as an epiphenomenon, the consequence of some unknown flaw in those particular experiments, that did not detract from the force of my conclusions. That fortnight of unsuccessful experimentation I dismissed as an anomaly. By behaving in this way, it seems that I was doing my bit to animate the Golem.

Harry Collins and Trevor Pinch have written an entertaining, provocative book with a sting in its tail. It is, in their own words, a "straightforward" book for the citizen, displaying science as it really is, stripped of the historical revisionism that so frequently imparts an heroic gloss. The book contains seven short chapters, each describing a case study in controversial science. Some of the examples are famous (the tests of general relativity early this century), some are obscure (the sex life of the whiptail lizard), others just plain embarrassing (the story of cold fusion). From this disparate cross-section, Collins and Pinch draw their thesis: that science is an untidy, blundering creature, like the Golem from Jewish mythology, that rests upon craft knowledge, incomplete data and the all-too-human motives of ego and power. The sting comes in the concluding chapter, which draws together the lessons of the case studies. The most important purpose of the book, the authors say, is to change the public understanding of the political role of science and technology. Scientists are merely a particular kind of expert. Their area of expertise – the physical world – may evoke greater awe than that enjoyed by other experts, but "their knowledge is no more immaculate than that of economists, health policy makers, police officers, legal advocates, weather forecasters, travel agents, car mechanics or plumbers." (p. 145).

This is strong stuff, bound to raise the hackles of the scientific establishment. A review of the book by a member of the scientific aristocracy in the journal *Nature* is revealing.[1] It blithely sidesteps the book's thesis and reiterates the triumphalist view of science, claiming that unpopular views are vanquished by being "proven false". Nevertheless, Collins and Pinch have obviously been affected by this and other critical reviews. The edition of the book that I reviewed contains a Preface to Later Editions in which the authors feel compelled to respond to critics by emphasising that their conclusions apply only to controversial science. As they say, "the average scientist would be lucky indeed (or unlucky!) to be personally involved in the kind of excitement represented here" (p. xiii).

And yet, as a practising scientist (and a member of the post-Kuhnian generation) I read the case studies with more than a twinge of recognition. Cutting-edge science can be like this. Experimental data can be ambiguous. Seemingly robust phenomena can, for unknown reasons, be capricious (vide my experience described above). The believability of a result is often refracted through the prism of one's opinion of the competence and credibility of the experimenter. The dynamic of the social enterprise of science is such that powerful, highly-visible personalities stand a much better chance of getting their data and theories accepted than lower-profile practitioners. None of this is very surprising. Witty commentaries on this state of affairs have been written by scientists themselves.[2] Graduate students quickly learn that their chosen vocation can be messy and political. Indeed, it is interesting that, the more senior the scientist and the more remote she is from the day-to-day realities of the lab bench, the more inclined she is to overlook the vagaries of real experiments. If all this is common knowledge to scientists, Collins and Pinch are to be congratulated for bringing it to the attention of the public, and doing so in such an absorbing, clearly-written style that conveys so well the humanity to be found in scientific research.

What, then, is wrong with the conclusions that the authors draw from these case studies in controversial science? The hazards are those of extrapolating from the particular to the general. By their own admission, the case studies are not statistically representative. Most science is uncontroversial. Uncontroversial science, the authors say, does not need to be understood by the public, because public decisions based on this kind of science are not compromised by the credibility of the research that gave rise to it. Only controversial science needs to be understood by the public. This distinction is clearly made in the Introduction; but by the time the Conclusion is reached it has evaporated. All science is tarred with the same brush of overwhelming social contingency. Public decisions based on, say, a well-established forensic technique are implied to be just as suspect as those based upon cold fusion research. Whilst this makes for a good read, the caricature is regrettable. If members of the public are to be told that scientists in general are like plumbers, they should be shown more than just a controversial minority who look, sound and act like plumbers.

Collins and Pinch are especially interesting while discussing the factors that determine whether a piece of controversial research gains a coveted place in the textbooks, or is consigned to oblivion. An example of the latter is provided by the story of the chemical transfer of knowledge. In the 1960s Georges Ungar began experimenting on rats, training them to avoid darkened boxes. After training, the rats were killed and an extract of their brains was injected into untrained mice. These mice showed a greater fear of the dark than control mice that had been injected with brain extract from untrained rats. These findings were immediately disputed, and the confrontation continued for over a decade. Experiment alone seemed unable to settle the issue; reasons could always be found why this group produced a positive result, or that group a negative one. When Ungar died in 1977 the field died with him, having been propelled more by the personality of its chief advocate than its inherent interest to the scientific community. As Collins and Pinch emphasise, this area of controversial science was not defeated by unambiguous experimental evidence: rather, it faded away because more interesting problems came along

and because its principal supporters lost credibility.

An example of the opposite outcome – acceptance of an ambiguous result into the scientific canon – is provided by the story of Eddington's confirmation of Einstein's theory of general relativity by measuring the bending of starlight by the gravitational field of the sun. In 1918 two parties from Eddington's group set out to photograph stars during a complete solar eclipse, one from Sobral in Brazil, the other from the island of Principe off West Africa. When the data was analyzed it became apparent that the different measurements were ambiguous and contradictory. If the results were to be taken to support Einstein's theory, a large part of the data set would have to be regarded as 'anomalous' and be arbitrarily discarded. This, it seems, is exactly what was done: in 1919 Eddington announced that the observations had confirmed the theory of general relativity.

From examples such as these, Collins and Pinch propose the notion of the experimenter's regress: a good experiment is one which gives the right answer, but since one ordinarily does not know the right answer, one cannot know what is a good experiment. There are two ways of breaking out of this impasse; either one 'knows' the right answer (as in the case of Eddington), or else one extends the definition of what constitutes a good experiment to include, for instance, the credibility of the experimenter (as in the case of Ungar). Either way, experimental data alone is insufficient to establish the legitimacy of a scientific result.

Ask any scientist what he thinks of this, and he will quaver at the spectre of relativism that it invokes. Do not the case studies in this book describe contentious, underdeveloped fields that more experimentation will eventually remedy? Is not established science a massive, interlocking scaffold of mutually supportive results, beyond the reach of social contingency? Collins and Pinch offer little guidance on such questions, preferring instead to poke sticks at the collective Golem Science.

Given its professed goal of revealing some of the more bizarre features of science to the general public, *The Golem* succeeds admirably. Its crisply written and altogether fascinating case studies are alone worth the cost of the book. Whatever one makes of its conclusions, none can deny that they are provocative and, for a citizenship that needs to be interested in science before it can be informed, that is surely a good thing.

NOTES AND REFERENCES

1 W. Gratzer, 'Grappling with the Golem', *Nature*, Vol. 364, 1994, pp.22-23.
2 J. Klein, 'Hegemony of mediocrity in contemporary sciences', *Lymphology*, Vol 18, 1995, pp. 122-131.

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Organisational Decision Making and Information *by Mairéad Browne* (Ablex, Norwood, New Jersey, 1993), pp. xiii + 256, A$61.95, ISBN 1-56750-017-X (paper), A$125.95, ISBN 0-89391-870-9 (cloth).

The declared, and certainly ambitious, aim of this book is to bridge the gulf between managers who use information to make decisions in organizations and information providers. Of the theory by which this might be achieved there is ample consideration. Yet despite the analysis of