

**AUSTRALIAN ASSOCIATION FOR RESEARCH IN EDUCATION ®**

**ANNUAL CONFERENCE 1999, MELBOURNE**

**TOWARDS A SOCIOLOGY OF ACADEMIC PUBLISHING**

**Professor Robert Crotty**

**University of South Australia**

[Robert.Crotty@unisa.edu.au](mailto:Robert.Crotty@unisa.edu.au)

It is a simple fact of life that knowledge acquired by research, no matter how excellent, that has not been published in the appropriate form, does not exist as far as the academic world is concerned. Academic publishing is the recognised form for the dissemination of research knowledge and any other form, for example word of mouth or private correspondence, is considered inappropriate. It is possible for me, as an academic, to observe that X has gone through the accepted research process: X has outlined an acceptable theoretical field, has formed a feasible hypothesis, has adopted a realistic methodology and determined a method to follow, has made observations and gathered data, has analysed this data and formed conclusions. But unless X has actually published an account of this procedure and its outcomes then it is hearsay; it is inadmissible evidence in academic debate. Even if X informs me privately of the research outcomes, there is no advantage, except in those rare instances where X happens to be an Einstein or Wittgenstein.

It is academic publication in an appropriate mode that renders the research outcome legitimate, raising what was a personal insight to the level of academic 'fact'. However, not all published 'facts' have the same rating; a hierarchy of publications is recognisable in any discipline. There are two principal determinants by which this hierarchy is established. The first is the name and reputation of the journal or the publisher. In each discipline area, there are certain book publishers recognised as more prestigious, certain journals recognised as more rigorous in their selection of articles. Oxford University Press gives its impeccable *imprimatur* to any academic book it publishes; the *Harvard Educational Review* is known to publish only the very best in education articles. It becomes common knowledge that a particular journal or a particular publishing company is at the pinnacle, for example, in sociology, in history of education or in religious education.

The second determinant is the employment of refereeship. Some journals are refereed, sent out to anonymous assessors, while others are dependent on an editor or an editorial board. Articles in refereed journals are more prestigious than those in non-refereed journals. Books, which are published commercially, are sent out to editorial advisers and, while their criteria may be different from journal referees, the decision of editorial advisers determines whether the book will be published at the expense of the publisher. The alternative is for the writer to subsidise, in whole or part, the publication. Publisher-funded books are more prestigious than self-published books.

Astute academics, when reviewing the publications list in the *curriculum vitae* of those applying for employment, promotion or research grants, are aware of this hierarchy and make allowance for it. The Australian Vice-Chancellors' Committee determines its funding base on, among other things, research publications. It recognises a descending order of importance as regards research books, chapters in books, refereed articles, reviews, conference publications, A-V recordings, computer software, technical designs and models, patents and 'other creative works'.

### *Publishable Academic Knowledge*

What is the 'knowledge' that the academic world considers worthy of publication? It is not absolute truth. There was a time when Academe felt that it knew what the real world was all about, and that the real world was under human control. Human knowledge, it was confidently asserted, consisted of the empirical grasp of the truth, the knowledge that could be outlined in cold facts.

For most people in the western world this modern worldview is still their familiar way of thinking; science is still generally believed to be an objective search for the facts, carried out impersonally and in a detached fashion by dedicated scientific scholars. What has been 'scientifically proved' is thought to be absolutely true.

During the modern period after the Enlightenment, science became the queen; scientific facts were unassailable. It was a cumulative thing: Newton built on Ptolemy and Einstein built on Newton. The language of science was neutral and objective. Science was unified: sociology and psychology was based on biology which was based on chemistry which was based on physics and, at this point, it came close to the really real. Science claimed to bring humans by incremental stages closer and closer to The Truth.

However, earlier in this century, Albert Einstein's theory of relativity had demonstrated that space and time, the ultimate markers of stability, were themselves relative. Space and time measured in one frame of reference, he discovered, need not be identical to measurements in another frame of reference. The world is relative; nothing exists in and of itself. Quantum physics and chaos theory have subsequently claimed that the universe is not composed of things at all, but of relations. Chance and indeterminacy are part and parcel of the fundamental nature of things. Einstein's theory of relativity eliminated the notion of absolute space and time; quantum physics eliminated the notion of a controllable measurement process and chaos theory eliminated the idea of absolute predictability. Scientists are no longer confident that the world is open to full description. If this is so, then no one can predict what the future will be, not because of insufficient information, but because there is no certainty as to where the universe's processes are leading.

In such a new worldview, what is knowledge? Knowledge is culturally determined, socially located. Nothing can be known unless it is in a cultural context, and that context is an inseparable part of what is known. There are no things-in-themselves, no *noumena*, no facts in nature, which are independent of some particular observer. There is nothing known that is neutral and separate. Particular interests and particular values inevitably play a role in what is selected for study and how that study proceeds. This is true of all academic research. Research knowledge is inseparable from the research community within which it has been formed and is nurtured. In fact, this distinctive knowledge unites the group and gives it a group identity.

### *Sociology of Knowledge and Sociology of Science*

These considerations gave rise to new thinking within the sociology of knowledge and, in particular, within the sociology of science. Rereading some of the works from decades ago, I have been struck by the fact that references in that literature to academic publication have been overlooked. I think that there is value in reviewing the texts. In 1962/1970 Thomas S. Kuhn, in his epoch-marking *The Structure of Scientific Revolutions*, challenged the objectivity of the scientific enterprise. He put real people back into science and made it a very human exercise. Even if there was such a thing as The Truth we could not examine it directly, Kuhn was to say. We all wear glasses to see reality, glasses formed by our experience of life, our culture, our language, our worldview. No one sees the world as it is in itself, not even the scientist. Scientists look at the world and puzzle over it with a collective pair of spectacles. They have been well trained to share common assumptions, a common way of doing things, common expectations. All of these common assumptions, methods and expectations made up what Kuhn called a 'paradigm': the accepted way of solving a problem, using acceptable methods, standards and generalisations.

A paradigm is a fundamental image of the subject matter within a science. It serves to define what should be studied, what questions should be asked, how they should be asked, and what rules should be followed in interpreting the answers obtained. The paradigm is the broadest unit of consensus within a science and serves to differentiate one scientific community (or subcommunity) from another. It subsumes, defines and interrelates the exemplars, theories, methods and instruments that exist within it. (Ritzer, 1980)

The paradigm is the glasses, constraining scientists to see reality only in certain ways. Were earlier scientists wrong? Not exactly, they simply wore different glasses.

How does science progress for Kuhn? It does so not by logical and progressive development but by a scientific revolution or a 'paradigm shift'. There is a scientific revolution

when one paradigm is replaced by another. There have been scientific revolutions at the onset of the premodern, the modern and the postmodern periods. Final causality, the search for purpose in the universe, was replaced by mechanism, the Newtonian search for cause and effect; mechanism was replaced by quantum-mechanical randomness. Once a paradigm has taken hold of a scientific community it is difficult to displace. It establishes 'normal science' where all the assumptions and methods of the paradigm are taken for granted, and scientists proceed happily with their business of solving problems. Scientists are comfortable within 'normal science'. Scientists want to use the same pair of glasses; they have grown accustomed to them; they like them.

A paradigm shift is like a religious conversion. It usually does not happen of a sudden; it is rarely a purely intellectual affair; it is a very upsetting experience and is brought about only because there are too many anomalies in the practice of 'normal science'.

This can be indicated by the flow chart:

**Paradigm A (Normal Science) → Anomalies → Crisis → Revolution → Paradigm B (Normal Science)**

Some sociologists of science were not content with Kuhn's rather static social unit of research scientists, with its stable membership apart from the gradual replacement of older researchers by younger ones. Diana Crane did a sociometric survey of scientists within the research area of diffusion of agricultural innovation (1969: 335-352). Data on different types of scientific relationships were obtained from scientists, all of whom had published in this particular research area. The social group was found to be neither tightly knit nor closed to external influences. In order to conceptualise such a social group, Crane made use of the term 'social circle'.

Crane's work had been carried out on a group of scientists involved in applied work and, as a result, the application of her social structure to the wider field of science was called into question. M. J. Mulkay (1972), basing his research on W.O. Hagstrom (1965), dealt with the wider scientific community. He claimed that any scientific research community is divided into a number of social groups. There are, in the first instance, broad academic disciplines. Initiates who enter any one of these disciplines undergo socialisation within a firmly fixed intellectual framework. The framework would be largely unchallenged and unchallengeable. However, as the initiates advance in their personal research, they are increasingly involved in ever more specialised intellectual concerns. Within the parent discipline they find that they belong to a more select specialist group.

Graduate education, in Mulkay's view, gives these students the sort of inside information that has not yet been incorporated into the standard textbooks. It also socialises the graduates in acceptable ways of pursuing knowledge, making use of experimental and mathematical tools common in their field of study. Gradually, the graduates come to appreciate exactly what aspects of the existing intellectual framework may still be legitimately regarded as problematic.

It is at this point that the discussion becomes relevant for academic publication. This graduate education, in Mulkay's estimation, is controlled by social exchange. Scientists typically supply information to their colleagues by means of publications. In return they are rewarded by recognition, but this recognition is granted only to those contributions which conform to the current cognitive and technical norms. Hence, there is a close control over the work of the specialist group.

This specialist group has been described by Mulkay as dominated by the aforementioned 'cognitive and technical norms', and he himself draws the parallel between such a constellation and Kuhn's 'paradigm' (1972: 33). However, he also recognises certain advantages in making use of his own broader terminology. In the first place it leaves open the degree to which the basic assumptions of a community are inter-connected. It allows for partial and gradual change of intellectual commitment that would not be possible with Kuhn's idea of a paradigm, especially as Kuhn first formulated it. Mulkay also sees the specific use of 'norm' as drawing attention to the parallel between radical innovation and social nonconformity.

Mulkay describes the social structure of research communities as a complex web of 'problem networks', which are the most fundamental social units in the field of scientific research. Within a problem network a specific problem limits the scientific competence and interest of most researchers to the few networks bordering their own. However, unlike Kuhn, Mulkay sees little stability in such networks. This is because of Mulkay's wider view of the social processes involved in a great variety of scientific innovations. Behind such processes there lies the interpersonal competition for professional recognition. The writing of scientific communications, the planning of research, the selection of problems and methods are motivated and controlled by this latent function of seeking personal recognition. But in the course of normal science the most profitable areas of investigation are soon exhausted, breeding unease and instability within the scientific community. There is no longer a goal for open competition.

When the current problem area has been gradually filled in, social exchange as a mechanism of social control obviously becomes ineffective. The very effectiveness of normal science brings about the dismantling of the mechanism of control. Those working in the field, motivated by the latent function of competition for personal recognition, look to new fields and more interesting and rewarding problems. The end result is migration of some sort and consequent cross-fertilisation. Normal science thus lacks, in Mulkay's view, the presumed stability of Kuhn's paradigm community.

Mulkay indicates several dynamic processes for the growth of science by migration. In the first instance there is migration to new fields of ignorance. Secondly, there is movement by scientists into already established networks, bringing with them preconceptions and techniques that they have learned elsewhere. Those in possession in these other networks would initially resist the newcomers. A distinct, co-existing network might result, populated by the newcomers. If the newcomers take control within the network, the change would be revolutionary. Thirdly, there could be the Kuhn-like change in which 'migration' is internal, but this would only take place under special conditions. If few significant problems remain to be solved, if there are few opportunities for recognition, if research skills are not easily transferable to another field while, at the same time, cognitions are very precise, limiting any possibility of redefinition, only then might an internal revolution in thought, such as Kuhn described, take place.

While Kuhn's theory and the later refinements were confined to the functioning of natural sciences, the discussion has been extended to social sciences. Social sciences have conceptual models which act in much the same way as paradigms, although concurrent competing models are much more usual in the social sciences (Smolicz, 1974). Evidence that would lead to the universal discarding of a social science model rarely occurs. However, both the paradigm of the natural science and the conceptual model of the social science are very difficult to dislodge once they have been accepted.

In short, turning to the social science area, a conceptual model will define the field of work for a generation of researchers for the present time, indicating which problems are legitimate

for investigation, which techniques should be employed. Adherence to a conceptual model ensures that there is an orderly academic community, with unanimity and standards, that charlatans and mavericks can be easily distinguished and excluded.

Each social science has its own paradigm or short list of paradigms. They are limited in number. Knowledge generated within a paradigm community is considered to be legitimate and new knowledge is considered to be worthy of academic publication.

#### *Academic Publishing within the Academic Community*

One feature of research knowledge is, as stated earlier, that its legitimate dissemination is by academic publishing. If we accept a Kuhnian like stance towards the development of academic research then editors, referees and editorial advisers represent the 'authority' who protect the discipline area and the legitimate academics within it at any one time. They have a vested interest in preserving the present shape of that community, in suppressing novelties that might challenge it or cause others to reflect unfavourably on its achievements.

Legitimate academics are considered to be those who have learned the paradigm and now support it. They are not expected to expose to public view data that would undermine its presuppositions. We have become aware of the feminist claim that women have been taught to uphold the intellectual bases of patriarchy and have, at least in the past, wholeheartedly gone about supporting the very structures of their own domination. On a much broader scale, academics have learned the parameters of their own imprisoning, and cooperate with the 'authority' in maintaining it.

Within the confines of their paradigm, academics jockey for position and mark off their own territory. They do so primarily by cooperating in the academic publishing venture. The nicety of distributing offprints of published academic articles is precisely to indicate territorial possession. In order for an academic to move beyond the confines of the paradigm, positive action may be necessary. When feminists first began to submit for publication articles and books, which transgressed the boundaries of accepted paradigms, they were refused access to standard academic journals and publishing outlets. They found it necessary to establish their own publishing houses, to set up new journals, to nominate their own editors.

#### *Criteria for publication*

Valid academic knowledge is, according to this sociological perspective, that which is legitimated within a paradigm community. By what criteria then can any decision to publish this research knowledge be made? It would be an obvious error to think that the criteria are based exclusively on objective truth and discernment of scholarly excellence.

We can take the case of the journal article. The first criterion is whether its form, not its content, is acceptable. Form refers to presentation, length, mode of referencing (See examples such as *Publication Manual of the American Psychological Association*, 1995; *Chicago Manual of Style*, 1993). The seeming trivia of form, the details found in the inevitable 'Notes for Contributors' on covers of journals, provide the initial gatekeeping device. While seeming to require uniformity, they impose conformity. The non-conforming presenter will not even be read, but will receive a manuscript returned with, at best, a polite request for redrafting in the accepted form.

The second criterion is a good review from referees. While the referees always remain anonymous, the author is sometimes anonymous and sometimes not. If the author's name and university affiliation are handed on then women, younger academics and those from less prestigious institutions should realise that they are immediately disadvantaged even before the paper is read.

Journal referees are not paid. The importance and dignity of their role is reward enough. They are well aware that the journal they represent has more articles than can be published for the next year or even years. They are not expected to accept all articles; in fact, they are expected to reject more than they accept. The referees in fact are the official representatives of a paradigm, its gatekeepers. Journals do not appoint freelance researchers as referees; they appoint reputable academics who have demonstrated fidelity to the paradigm.

The matter is complicated by the fact that these referees could well see the article before them as a challenge to their superior position within the paradigm community. To admit that this article has said something new, and said it well, would imply that the author is a competitor. Not all would welcome that admission.

In short, the referee method is flawed (Moxley, 1992:183-184). It is rather chilling to read, for example, the work done by Donald Fiske and Louis Fogg (1990) on refereeship and its outcome. They concluded that 'in typical cases, two reviews of the same paper had no critical point in common'.

Even more telling was the research of Douglas Peters and Stephen Ceci from Cornell University. They selected twelve articles, already published by professors from prestigious universities, changed the contributors' names to nonentities and gave less prestigious university affiliation, and resubmitted the articles to the very same journals that had first published them. Of the twelve, only three were recognised as frauds by a total of thirty-eight referees. Of the remaining nine, only one was accepted for publication, with referees rejecting eight that had previously been accepted. By one of those ironic twists of fate, Peters and Ceci had difficulty getting their own results accepted for publication (reported in Couglan, 1989).

Conformity to the form required by a journal and the referees' assessment are the main criteria by which a decision on publication in an academic journal is made. These criteria can be dismissed as arbitrary and inconsistent. In fact, they fulfil a real purpose in defending the boundaries of the discipline paradigm. Slightly different criteria, including economic viability, apply to academic books, but there is the same basic principle.

From this sociological perspective, academic publishing can only be understood if the intentions of the author and the forces that are at work on the author are taken into account. The author has research knowledge to share with others. But why? Because s/he is aware that there is a limited paradigm area and that only those who stake out territory will succeed. Academic competition is the driving force; it is a very potent motivation to publish. There are forces at work on that same author, the forces of social control. Limitations are placed on what can be written and how it can be written. Social control ensures conformity, the protection of the paradigm territory. Otherwise the entire research community might be damned to drastic change and loss of territorial rights.

At some point academic publication has become the recognised yardstick of the compliant academic community. It seems apparent that a list of publications reflects industry and so it does - industry within the confines of a particular paradigm. It is equally apparent that extensive publications reflect academic ability - ability to manoeuvre within the confines of

such a paradigm. This is the yardstick that has been accepted as prime measurement of university activity and excellence in academic performance.

This is not an attempt to downplay publication neither as an important aspect of academic life or as a yardstick for measuring competency. It is an attempt to understand the nature of the yardstick, and to ensure that if modification to accepting academic publications as a criterion of excellence comes about, it does so for the right reason.

## REFERENCES

- American Psychological Association (1995), *Publication Manual of the American Psychological Association*, American Psychological Association, Washington.
- Couglan, E. (1989), 'Concerns about fraud, editorial bias, prompt security of journal practices', *Chronicle of Higher Education*, 15 February, pp.4-7.
- Crane, D. (1969), 'Social structure in a group of scientists: a test of the "invisible college" hypothesis', *American Sociological Review*, 34, 3, 335-352.
- Fiske, D. & Fogg, L. (1990), 'But the reviewers are making different criticisms of my paper', *American Psychologist* 45, 5, pp. 591-598.
- Hagstrom, W. O. (1965), *The Scientific Community*, Basic Books: New York.
- Kuhn, T. (1962/1970), *The structure of scientific revolutions*, University of Chicago Press, Chicago.
- Moxley, J. (1992), *Publish Don't Perish: the scholars' guide to academic writing and publishing*, Greenwood Press, Westport, Conn.
- Mulkay, M. J. (1972), *The Social Process of Innovation: a Study in the Sociology of Science*, Macmillan: London.
- Ritzer, G. (1980), *Sociology: A Multiple Paradigm Science*, Allyn and Bacon: Boston.
- Smolicz, J. (1974), 'Fragmentation in science and education; An analysis of the community structure of science', in *Melbourne Studies in Education*, ed. S. Murray-Smith, Melbourne University Press, Melbourne, pp. 1-58.
- University of Chicago (1993), *Chicago Manual of Style*, University of Chicago Press, Chicago.