



THE LONG AND WINDING ROAD: GETTING PAPERS PUBLISHED IN TOP JOURNALS

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ABSTRACT

This paper goes behind the scenes, from the author's point of view, in the messy process of publishing papers in top quality journals. It describes the tortuous history of a particular paper that was eventually published in *Information Systems Research* to provide some insight, especially for those new to the academic world. It goes on to draw more general guidelines for shaping papers for publication.

KEYWORDS: journal publication, confessional research genre

I. INTRODUCTION

When we open the latest issue of a journal and see the pristine papers within we may feel "gosh, what an excellent piece - I wish I could do that" or "how on earth did that get published?" What we rarely think about, and what is rarely ever made public, is all of the messiness that went on behind the scenes in going from some completed research to a published paper. Many of us learnt valuable lessons, sometimes through painful experience, over the years. But, the whole process can be quite daunting and surprising for those at the start of their academic careers, perhaps when it matters most [Applegate and King, 1999].

The purpose of this article is to open up this particular black box. In Section II, it presents a detailed history of the itinerary of a single paper published in *Information Systems Research* [Mingers, 2001]. From this story in the raw, I give more general guidance for getting papers published in top journals (Section III). This article can be seen as complementary to Lee's [2000] editorial comments which covered the more formal aspects of submitting manuscripts to journals. It can also be seen as an example of the confessional genre of writing [Schultze, 2000, Van Maanan, 1988] that presents a more self-revealing picture of the processes of research.

I should perhaps begin by posing the question why try to publish in the top journals? This question may seem rhetorical, but there are reasons not to: first, it usually takes a very long time, is risky because of the high rejection rates, and generally requires a lot of effort. Second, you may find that your particular research topic or approach does not easily fit into the mainline journals and so you may decide to publish in more of a niche journal.

There are, however, good reasons to take the risk:

- You *may* have a better paper at the end of the process – it will certainly be different.
- For some, it may be important for tenure; at the least it is very helpful on your CV

- recruiters are much more impressed by a single top-rated publication than by several minor ones.
- Your work (and your name) will be widely seen.
- It is a mark of quality.
- It opens other doors, such as refereeing and invites to conferences.

II. CHRONOLOGY OF THE PAPER

The research was undertaken between 1994 and 1997 when I was interested in different methods or methodologies for intervening in problem situations within management science. I argued that such real-world interventions required a combination of methods, in particular both hard and soft approaches. I called this approach “multimethodology” and wrote a paper and edited a book on the issue [Mingers and Brocklesby, 1997, Mingers and Gill, 1997]. At the same time, I felt these arguments also applied to research. Indeed, I do not see research and intervention as distinct but simply different ends of a continuum. I developed my ideas in the context of information systems research methods and presented a paper at the European Conference on Information Systems (ECIS) in 1997 [Mingers, 1997]. After the conference an editor of *Information Systems Journal*, who was also a colleague at work at that time, said he liked the paper and asked if I would submit it for a special issue on research methods. This offer seemed like a good opportunity, especially as I usually think that being invited to submit by an editor is a good start. I developed the paper and submitted it in July 1997. Table 1 shows the chronology of events.

Table 1. Chronology of Multimethod Research Paper

1994-1997	Developing ideas on multimethodology in the context of operational research methodologies and interventions. Write paper (and start edited book).
1997	Apply these ideas and frameworks to research (rather than OR interventions) specifically in the IS area.
June 1997	Paper presented at <i>ECIS</i> conference, Cork, Ireland
July 1997	Asked to submit the paper to a special issue of <i>Information Systems Journal</i> by the editor. Develop the paper and submit.
Oct. 1997	Receive two referees reports diametrically opposed to one another. One wants the philosophy section removed (first 22 pages!) and recommends rejection, the other wants it strengthened.
Nov. 1997	Resubmit with comments about the problems of responding to the two referees.
May 1998	Receive next referees comments plus AE recommendation. One referee still suggests rejection, the other wants further revisions. The AE, and editor, decide to reject anyway despite the editor saying “he liked” the paper!
June 1998	Decide that I do believe in the paper so revise and submit to <i>MISQ</i>
Oct 1998	Receive preliminary reports from referee and AE. They feel that there is some merit in the paper but want to split it in two and change the philosophical orientation of it. I feel that I don't want to go down that route so withdraw it.
Nov. 1998	Never daunted, submit to <i>ISR</i> . Allowed to nominate reviewers.
May 1999	Received first reviews, generally positive. Three reviewers, AE comments, and Senior Editors overview.
Aug 1999	Returned paper with quite major revisions. Gave separate detailed comments to all five reviewers.
Mar 2000	Receive second reviews from AE and three reviewers. Asked for more significant revisions but told it won't go back to the referees.
May 2000	Returned paper with several more changes. Again send comments to reviewers.
June 2000	Received further comments from AE only. Paper is accepted subject to minor changes.
June 2000	Sent revised version.
July 2000	Final acceptance.
Sept 2001	Publication!!

In October (quite quickly) I received two referees comments and realized I had problems because they were diametrically opposed to one another. The paper consisted of two main parts - general arguments about the desirability and feasibility of multimethodology, and some frameworks that I developed to help practically with choosing different methods. The first referee recommended rejection, saying that s/he was already convinced about multiple methods and so the first part of the paper was nothing new. All it needed was a discussion of some examples. The second referee, who was obviously interested in philosophy, recommended revision and requested a lot more detail in the philosophical section suggesting that I had not really made the case for paradigm commensurability well enough. S/he felt that the practical part of the paper was not worthwhile. Disagreements among referees are common, but not usually so stark. In response, I tried to address both sets of concerns by developing the philosophical arguments and adding examples to illustrate the use of the frameworks. These changes inevitably made the paper longer but would not necessarily satisfy either referee. I wrote a response to each referee as well as the associate editor to whom I pointed out the problem.

In May 1998, six months later, I received the referees' responses. One referee was still not convinced by the first part of the paper. "My reaction to pages 1 to 22 is that this portion (the majority) is largely rambling. As a person already convinced of the merits of combining methods in IS research, I do not see the relevance of this section." The second referee said, "I welcome the clarifications that have been made, but still feel ... that the argument remains unconvincing." S/he provided three pages of detailed comment and suggested revising again. At this point, the AE, backed by the editor, took the decision to reject the paper rather than request further revisions. The main concern was that I had not succeeded in clarifying my arguments especially with regard to combining methods from different paradigms.

The AE summarized, "As I said at the outset, I believe there is some merit in what you are trying to do. However, the paper in its current form is unsuitable for publication in ISJ." The editor said, "we {ed. and AE} discussed if the paper might be rescued by further changes but he is convinced that this is unlikely as the referees fundamentally disagree about certain aspects. ... I'm sorry the news is not good (I rather liked the paper)."

This decision did not please me! I was convinced that the paper was quite strong; that the particular position I was arguing had not been put forward before; and that one of the referees was being quite unhelpful. Having put a lot of time into the paper, I felt that the editors should have let me have a further try at clarification. However, what was I to do? I could just leave it, or I could send it to an "easy" journal where it would surely be published reasonably quickly, but instead I decided to take a risk and send it to a top journal - *MISQ*. My reasoning was that I did believe in the paper as a significant one; it had already had two lots of revisions done on it; and as I was not short of publications, I could afford to take a chance.

I made further changes and submitted it in June. In October, I received a reply from the AE. S/he had not yet sent it out to referees but discussed it with a colleague. They suggested major changes before taking it any further - splitting the paper into two - philosophy and practice - and re-orienting the philosophical position that I was advocating. After considerable thought, I decided that I did not want to go down that route. I believed that the combination of theory and practice was a strength, particularly as some of the frameworks developed directly out of the theoretical arguments, and I did not agree with the philosophical approach they suggested.

I felt I should try one more time and so made some further small changes and submitted it to *Information Systems Research* in November. One helpful procedure of ISR (and other journals) is to be able to nominate possible referees, although apparently many people do not make use of this opportunity. I think it is valuable not because the referees will be easier but because at least you are assured that some of the referees will presumably be in sympathy with what you are doing. I received the first reviews in May 1999, three detailed referees' reports, an AE's, and a note from the chief editor. In general, they were all very positive. At last, I was getting somewhere. The main points, with my responses, were:

- Lack of consistency in usage of terms such as "method", "methodology", "paradigm", and "discipline". This variation was partly a difficulty of inconsistent usage in the literature. I

- put considerable effort into creating a clear set of distinctions, the hardest being about “methodology”, and then sticking to these distinctions throughout the paper.
- Concern about the practicality of researchers adopting multiple methods. This concern was expressed in a variety of ways by the reviewers, for example requesting exemplars of multimethod research, mentioning the difference between single pieces of research and research programs, and a concern with time and funding. I addressed these comments with significant extra material: an evaluation of three more multimethod case studies (although I did not consider them exemplars); a new section on the practical, pragmatic difficulties; and a table of different types of generic multimethod research designs.
 - I was asked to remove some empirical data showing the lack of multimethod research in IS journals (ironically I had put in this data in response to an earlier referee) as it was felt to be inconclusive. Reluctantly I did so after a brief discussion with the senior editor.
 - The AE did not feel that the underlying philosophy of critical realism [Mingers, 2000] was appropriate: “Critical social theory (CST) in general enjoys an uneasy acceptance among North American academics despite its popularity in Europe, ... {t}ying your arguments to CST will not win you any new converts ...”. I did not feel that this was an appropriate comment. A paper should be judged on its intellectual integrity not on the popularity or acceptability of its theoretical base. I decided not to make this change, especially because one of the referees had asked for an expansion of the critical realism section (perhaps a European?).
 - Various other structural changes were suggested, including shortening several sections, with which I complied.

I resubmitted the paper in August 1999 and received the next set of reviews in March 2000. The AE recommended that the paper should be accepted subject to significant revisions but the senior editor just wanted me to revise and resubmit again at that stage. The referees all agreed that the improvements were significant but were still not entirely happy with the overall argument. They were still not clear if I was arguing for multimethod research or multi-paradigm research or both; whether multi-paradigm research was possible; and whether I accepted current paradigms or wanted to replace them with a new one. I was by now becoming somewhat disheartened, as this comment was a continuing theme throughout the history of the paper. It seemed very clear to me but I obviously could not get it across to others.

The AE was very helpful, giving me three pages of detailed suggestions about how to restructure sections of the paper, centering it more clearly in the IS literature, and using different empirical examples. I could happily comply with most of these suggestions. However, the AE also suggested several sections were distracting from the main argument and should be removed. These included the section on critical realism as well as the section that contained the practical frameworks I developed to help with choosing methods. These suggestions were much more problematic because I felt that the sections were an integral part of the paper. In the end, I reluctantly accepted them. The realism section did not really do the subject justice so I replaced it with some references; and some, although not all, of the frameworks had been published in a previous paper. To a degree, though, my decision was one of expediency - the paper had been dragging on for so long I really wanted it published.

Finally, in June 2000, I received confirmation that the paper was accepted subject to yet more minor revisions! One revision is worth mentioning because it again reveals cross-cultural differences. One of the research examples was work done by Ormerod [1995] who used a variety of soft methods in helping to define an IT strategy for Sainsbury's supermarket. I saw this work as a clear example of action research since it tested out methodological possibilities and Ormerod reflected on the outcomes. The AE said “...it seems to be an applied IS development project rather than an attempt to build or test theory. ... Many ISR readers may not regard it as a true ‘scientific’ study.” As can be seen in the paper, I argued that this example should remain.

The paper was finally published in September 2001, four years after its original submission.

III. GENERAL ADVICE ON GETTING PUBLISHED

SELECTING THE JOURNAL

Status vs. Risk

In general, there is a clear correlation between the perceived status of a journal and the expectations they will have, and therefore the risk of rejection.

- Be confident that you do have a *good* piece of work. Either very sound empirical work investigating a topic of relevance; or some theory/methodology that makes a substantive and original contribution. It should *not* be:
 - *ad hoc* research on patchy data;
 - a speculative idea that is not well grounded; or
 - a simple replication of what has already been done.
 It needs to be original, rigorous, and significant or interesting.
- Be able to afford rejection, It is not a good idea for those needing publications quickly.

One common concern is knowing how well a journal is ranked. At the top level I think it is clear which are the most prestigious journals although it depends on whether you only count IS journals or also include more general ones like *Management Science* or *Academy of Management Review*. Lower down it becomes very subjective depending on who you ask, and what criteria you use. *ISWorld* lists various IS journal rankings at

<http://www.bus.ucf.edu/csaunders/newjournal.htm> [Saunders, 2001]

although it is heavily North American. Another useful source is Harzing [2001] which lists several different rankings of general management journals including IS ones.

Generalist vs. Specialist

This judgment is difficult. If you work in an area that publishes its own specialist journals, publishing in a top one is very good and will enhance your reputation in your research community. However, it will not get your work widely read and will not be rated as highly by outsiders (e.g., for jobs). Ideally, go for a mixture over time.

Subject Area/Methodology of the Journals

Most journals tend to focus on particular areas of the discipline; or implicitly or explicitly privilege particular research approaches; or are theoretically or empirically inclined. It is clearly better to send your paper to a journal it fits with but fit is not always easy to tell, especially if you are new to the area. You can:

- look at what the journal itself says;
- see who the editor is and what they publish;
- ask people who have been around for sometime;
- look at past articles, and
- look at empirical reviews of what journals actually do publish.

For example, MISQ offers a lot of information on its website [MISQ, 2001], particularly editorials outlining what is expected of authors.

Journal selection can be difficult because journals do change over time, especially as editors change (see MISQ), and often what they publish is not necessarily what they would espouse. Introna [2001] presented an interesting analysis of the history of MISQ in terms of the power it exerts over the IS discipline. One good test is to look at relevant papers that you rate highly and see where they were published. Also, look for journals that recently published several papers relevant to your topic.

Special Issues/Conferences

Often special issues are easier to get into. The competition will be less; someone other than the usual editors will organize it; and you know that your paper should be relevant. Make sure you understand the theme, and where the special issue editor will be coming from. Often editors will solicit papers they heard presented at conferences. Invitation is usually (!) a good starting point.

TAILORING THE PAPER

Style

- It is important to put your paper in the house style, including headings and references. Using bibliographic software (e.g., *Endnote* or *Procite*) makes this task very easy. For style, it is often easier to look at a current paper rather than the instructions in the back of the journal.
- Check if there are general structures the journal expects. For example, most US journals expect an explicit “Methodology” section for empirical papers and often a “Theory” section as well. It makes for rather standardized papers, but does force you to address these issues. Many papers in non-US journals skate over them.
- Make sure your English is clear and direct. There can be cultural differences in style. In my experience, US journals especially like short punchy sentences and active tenses¹. Use spellings that are appropriate for the country, and rumor has it that submissions on A4 (European) paper to US journals are binned² immediately!
- Make sure the paper looks properly finished off. Neither editors nor referees like receiving sloppy work – if you cannot be bothered why should they?

Content

It is difficult to generalize, but I think the following are important:

- A strong grounding in the (IS) literature. Certainly, US journals expect a significant discussion of existing theoretical and empirical work, and a clear statement of how your paper relates to it. They are not usually keen on non-IS material. Make sure you include many references from the journal to which you submit.
- A clear statement of the methodology for empirical work, or justification for theoretical work. It's not enough simply to say “case study” or “questionnaire”. Be aware of the methodological bias of the journal if any. Look at recent papers published in the journal doing similar work to yours.
- A clear statement of the original contribution of your work and why it is significant for IS. Convince them that yours is an important topic and with interesting conclusions.
- A strong concluding section summarizing the paper and discussing limitations and further research.
- If you can, link you paper into an on-going debate, especially one in which the journal/editors are involved, even if this link means re-orienting what you want to say. Rather than saying “This is my work – take it or leave it”, think how the work can be shaped into a contribution to an ongoing discussion.

One major thing I have learnt is not to agonize over particular sections or sentences in the paper. Notwithstanding what I said about making sure the paper is in good shape, I used to spend ages mulling over particular bits: did they say exactly what I wanted? Could they be misinterpreted? Had I covered all the arguments that might be put against them? Should I put in more or less? What I then discovered was that the referee would either ignore them totally;

¹ Editor's Note: CAIS is an example of such a journal.

² “binned” is the UK equivalent of being put in the trashbin.

suggest that whole sections be removed; or have quite different concerns about the paper than I did. All the agonizing was quite wasted.

So, what I would suggest is writing a paper as quickly as you can, preferably without long breaks. Then, going over it to check for all the points I mentioned above. But then, just submit it and not worry too much. Wait to get the referees comments and regard them as setting the agenda for what you actually need to do to the paper.

In other words, rather than seeing the submission of the paper as the end point of some long search for perfection, see it as the starting point for molding your material into something that is actually published. This advice relates to the next section on referees.

RESPONDING TO REFEREES – RE-WORKING THE PAPER

The First Comments

Any reputable journal will send you copies of referee's reports, and any comments from editors, along with a decision. If you do not get them then you should ask for them. If decisions were made after reviewing, especially to reject, then you are entitled to see why.

The amount of comment can be very variable. Generally the better journals, and especially US ones, give very good reports. In my case, at the first stage I received three referees comments, an AE's comment, and the editor's summary. But sometimes the referees' comments may be quite cursory, only a line or two, and the editor may simply say "respond to the referees' comments".

The main points I would make are:

- In my experience, it is unusual *not* to have to make significant revisions to your paper. I do have a colleague who had two consecutive papers accepted by a journal without significant modification. The editor wrote to him saying he had never heard of that before. Do not become dispirited or see it as a criticism of you or your work in general. It may be that there is some significant problem with your paper, but more likely, it is just experts in a subject with their own particular views and biases. Often the referees think they are not doing their job if they don't make recommendations for change. The way to see it is free advice on your paper, even if you disagree. All of us, full professors or not, have had and still do have major re-writes and even rejections, as my example shows.
- You generally receive one of three responses – accept, reject, or resubmit with revisions. In my experience, 70% are the last. I suggest that you treat revise and resubmit as a success, and regard it as the first step towards being accepted. If you treat the comments seriously, and make a decent effort to accommodate them, you should eventually be accepted. This outcome is not guaranteed, again as my example shows.
- If you are rejected, do not throw the paper away. Think hard about why that particular journal rejected it, and choose another one that may be more suitable, or easier. Tailor the paper for that journal and submit it. You have lost some time but that is all – when it is published the rejections will be forgotten. I experienced several rejections at various times but all of them were published eventually. For me, the basic test of a paper is: if I saw this paper in a journal would I be interested in reading it? If so, then it is worth trying to get it published. If you do resubmit, make sure you update the references otherwise it is very easy to spot that the paper is recycled.

Responding

The main point is to take seriously what the referees say and recognize that you have work to do. You may need to remove material, even treasured material; you may need to do more, possibly significantly more; or you may need to rewrite or re-orientate your paper. There may be some things that you are not willing to do.

If you decide to revise and resubmit, then you should:

- Address as many of the comments as you possibly can, especially trivial things such as references (referees always want you to reference their articles – a good way of spotting the reviewer), English, and style.
- Write a detailed response, *to each reviewer*, addressing each point.
- Where you do not want to do something explain clearly why not.
- Write separately to the editor (or AE) explaining your general response and particularly highlighting any difficulties such as disagreements among referees.
- If there are comments you are not clear about ask, do not just ignore them.

Regard the whole process as one of negotiation. You do not have to do everything, but you have to do enough to let them know you have taken them seriously. This advice is especially true of any comments by the editor or AE. Editors are not simply neutral. They have their own views and often want to write your paper for you. Have the confidence to resist them if you disagree with their suggestions.

Problems with Referees and Editors

We all have many horror stories of dreadful referees and editors (and also, of course, very helpful ones).

- A referee rejects the paper with very little comment, or clearly disagrees fundamentally with your whole approach. An example from a paper of mine submitted to a UK journal: “This paper is thin and self-indulgent. It could be regarded as ‘publishable’, in that many papers like it do get published. But they add nothing useful to knowledge, given the unrealistic stance in many such papers. The authors don’t seem to realize that ‘methodology’ is of interest and concern only to a narrow group of academics.” The referee went on to suggest that knowledge was only worthwhile if it was “argued from experience in real management situations”. The referee is clearly a practitioner who sees little value at all in scholarly activity. If the other referees are similar, give up the journal and go elsewhere. If it’s just one, and the others are constructive, contact the AE and ask what to do.
- Two referees strongly disagree with each other. This situation is difficult. Either contact the AE or try and appease both.
- A referee wants major new work, perhaps empirical research, and you do not want to do this. Contact the AE and try and negotiate something less.
- The referee gives a response that you think is inappropriate. I can give an example of this from another paper of mine submitted to a top US Operational Research journal. The whole review consisted only of two paragraphs with no mention of my paper as such. The only substantive comment was, “In my experience, the use of these soft methodologies only serves to mask the preconceived notions and prejudices of the investigator. Soft methodologies are soft primarily because they are not objective. I believe they are at the root of current cynicism that has surfaced about segments of our profession.” Here, the referee made no attempt to review the paper at all but simply dismissed the whole area of soft OR/systems in a totally prejudiced way. Contact the AE and discuss it.
- The referee says the work reported was all done before, by them, and references many of their own papers. First, decide if they are actually right. If you do think you have something new then you may have to re-orient the paper to contrast what you did with what they did. Make sure you reference them a lot!

IV. CONCLUSIONS

I tried in this article to lift a corner of the veil that usually surrounds academic research publishing and show that it is a messy, and often frustrating, business but one that is ultimately rewarding. The main message, especially for those just starting in the academic world, is that completing a research study and writing a paper is not the end of the process, but just the beginning of another stage that will require significant scholarly effort. Do not let yourself be put

off or discouraged by reviews that sometimes can go beyond the bounds of polite discourse, cloaked in anonymity. In addition, do not give up even if your cherished paper is rejected. If you are confident that you have something to say you will be able to publish it somewhere. You just need the right outlet. And you will be published in high quality journals if you make the effort.

Editor's Note: This article is based on a talk presented at the UKAIS Research Roadshow, Bath University, 29 Nov. 2001. It was received on March 15, 2002 and was published on March 28, 2002

REFERENCES

Editor's Note: The following reference list contains the address of World Wide Web pages. Readers who have the ability to access the Web directly from their computer or are reading the paper on the Web, can gain direct access to these references. Readers are warned, however, that

1. these links existed as of the date of publication but are not guaranteed to be working thereafter.
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