

# On Building an Academic Research Profile

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*Abstract:* In academia the imperative to build a research profile is clearly conveyed, but not the means. The following is a starting point, and very much from my own perspective. It is, however, jargon free and intended to be accessible to all academics, no matter what their background. I will discuss tips on how to produce research, on collaboration, and on research management, from the perspectives of both the manager and the managed.

## 1. Introduction

What marks a desirable research profile? I suggest

- enough output to have an impact;
- output in areas of strategic importance;
- high quality.

Here I will focus mainly on advice on how to produce and sustain output. Clearly, what strategic means changes over time, and, like fashion, should guide but not dominate your activity. However, what is interesting to you could be irrelevant to your peers and your institution. You avoid that scenario.

Quality should always be the first goal. I have direct evidence that a quantity of low quality work can be detrimental to progress in a career. This doesn't mean that articles in lower impact journals that explain and promote your work is not valuable. It certain is sensible and important to make your work available to as wide an audience as possible, and to keep your name and that of your unit and university in the public arena.

Why bother to build a research profile at all? The first reason is because keeping your intellect challenged is a worthwhile life-long pursuit and immense fun. In many ways such activity defines who you are. Most researchers I've known are better people for being just that. Second, by one definition the most fundamental activity of a university is the creation of a learning environment by people actively engaged in scholarship and research. What marks academics as different from people who may be better educators, is the depth of vision that active research gives. Third, if you want to build a successful academic career, you have to perform in several areas.

Typically academic promotion is judged on factors such as teaching, research/scholarship, management, and professional activity. Research and scholarship are generally taken to mean something like the following.

*Research* includes systematic and rigorous investigation directed to the discovery of hitherto unknown facts; the construction of explanatory theory.

*Scholarship* is an activity directed to the construction of an analysis or interpretation of existing human products of human, scientific, and literary activity aimed at increasing the accuracy and depth of human understanding.

Both should result in *tangible output*. This may include editorship or editorial board memberships, refereeing, and contributions to conferences. Contributions to textbooks and scholarly articles on various aspects of teaching and learning are also relevant.

Of course, there is output that is neither research nor scholarship, not all scholarship is research, and not all research is valuable.

From a cynical point of view, for an academic research is part of what may lead to promotion. For a politician, research may mean finding out what they said on a particular issue in certain public forums, such as parliament, and also what the leader said, in order to see if they are in conflict. I suggest that in the broader community there are many meanings attached to the word 'research'. This is unfortunate, because the definition of research in academia will be modified over time by political imperatives from the broader community, and valid contributions at certain times in a career may be judged invalid at other times.

The following observations represent a very personal view.

## 2. Producing Research Outputs

There are several things I wish I'd known about academic research when I was much younger. From where do research ideas come? How can you set about improving your research performance?

- *Initially set out to do accessible research.*

Setting out without considerable background to find a cure for cancer is very likely to end in disappointment. Setting out to apply a particular technique to an area when this has not been done before is very likely to uncover new knowledge. The value of the discoveries made and not made in each case is, of course, quite different. However a good strategy is to work up to the difficult questions.

- *Expand what is accessible to you.*

Read widely in your chosen areas of interest. Write scholarly overviews. Learn new skills and new techniques, improve and polish existing ones. Write tutorial papers about these skills. If you find mistakes in papers or important books identify and correct these to the authors, and if appropriate, to the wider community through appropriate articles.

- *Start with the motivation.*

In statistics an interesting data set may be the beginning of a profound investigation into fundamental issues. Starting with a challenge is another approach. Good researchers tend to be quite passionate about what they are doing, and for new

researchers and when starting a new project it is important to instil motivation early and maintain it.

A colleague has asked, "from where do the challenges come?" Some obvious answers include, consulting, coffee room and other casual meetings, conferences, workshops and other formal meetings, reading the literature, and from colleagues in your research grouping.

- *Try to work on classes of problems rather than particular issues.*

The initial plan for the Rayner and Best (1989) goodness of fit book was a list, but eventually the core of the plan was a two-by-two table. The specifics aren't important here, but it became clear in setting up this structure that not enough work had been done on the (2, 2)th category. That generated some interesting work that was published in good journals.

- *Only address 'good' questions.*

I once spent six weeks while on leave proving three theorems. None of those theorems appeared in the ultimate journal publication of that body of work. The theorems related to 'bad' questions. Before solving any problem, contemplate your position after you have solved it: do you want to be there? Would your colleagues be interested in this work? Would journal editors want to publish it? Some academics are not gifted at choosing questions. If you recognise this, run your initial ideas past colleagues.

Struggling with difficult and bad questions isn't a complete waste. You often learn new techniques, acquaint yourself with additional background material, develop your problem-solving skills more than with easier questions, and, no doubt, develop intuition for what is and is not a difficult or bad question. However it is inefficient to ask and attempt to answer too many bad questions.

- *Work on several fronts rather than just one.*

This works for me, but not for everyone. It would be strategic to work on, for example, areas of (a) prime personal interest, (b) the unit's greatest strategic interest, (c) predicted strategic interest. Within each area address several questions simultaneously.

- *Have a medium to long term plan.*

It is valuable to have medium and long-term goals. For example, a grant application in three years time will have a better chance of success if in the intermediate period you establish your credentials in the area by expanding your background, making appropriate contacts and publishing articles in the area. Most of us tend to devote the bulk of our energies to what we enjoy most, and that means becoming expert in certain areas, and maybe losing competency in others. This may be a very poor strategy. Maintain and expand your skill base.

This year I hope to publish a focused users' monograph on new methodologies I have developed over the past five or so years, and published in a technical monograph last year. Next year I hope to produce a second edition of a monograph first published some time ago. I have set up a team to investigate ancillary questions, and they will start work this year. The year after that ... .

- *Write work in progress notes.*

I keep a study leave diary in those blissful times when it is appropriate to do so. This prevents me from spending too much or too little time on particular projects. It also makes the writing of the study leave report a very simple chore.

Similarly in the early stages of a research project, I write notes on progress made. These notes can be vital in collaborative work. In my own experience, these notes have often developed into published papers, and often were the draft modules that made the writing a pleasant and successful experience.

- *Develop strategies for dealing with problems you can't solve.*

The first strategy is to try again later, after the subconscious has had a chance to reflect on the problem. Make a lifelong commitment to developing new tools. A particular problem may provide the impetus to learning more about a particular technique. Third, ask someone with better tools in general, or if you recognise the tool needed, with more experience with that tool than you have.

- *Develop strategies for dealing with mistakes.*

Some of us have much to be humble about! We can all learn from our errors, and those who don't make them miss out on important growth experiences. But it is inefficient to go too far down the wrong path. Any check that lessens that waste is worthwhile. Importantly, research is a confidence game. Don't become despondent when you make the odd mistake, or when you don't seem to be making much progress.

### **3. On Research Collaboration**

There is a perception abroad that teams are a better way of producing research.

- *In joint research the personal relationships **must** work.*

I don't see how it is possible to have a successful professional relationship if the personal relationship isn't positive. One student I supervised had a body odour problem that progressed throughout the year of the supervision. That wasn't an enjoyable experience. Another student regularly passed on to me politically incorrect jokes, and that experience was very enjoyable.

- *Remote collaborations are unlikely to be fruitful.*

In any ongoing collaboration, schedule regular interactive meetings. These meetings are the for a at which

- projects can be proposed and discarded,
- approaches sifted and refined,
- difficulties quickly surmounted, and
- momentum established.

Nowadays my collaborators and I use phone, fax and email on a regular basis and attempt to meet in person as often as possible, up to several times a year. This is a reflection of our willingness to use such facilities, and having the resources to do so.

I also tend to drop in unannounced on my research students. This makes sense when you are working together on something, and progress seems to happen alternately: your colleagues make progress when you can't and vice versa. But if I'm bogged down on other things and can't get to research, I tend to seek out the stimulation of others' progress. It's a more harmless addiction than many!

- *Skills should overlap.*

In my area, a reasonable mix in a joint collaboration would be for the person with superior mathematical and writing skills to focus on those, while the one with superior computing skills and a wider view of the literature focuses on those areas. Of course we are each able to, and do make contributions in the other's areas of expertise, but it is more efficient to optimise outcomes by focusing skills.

Consistent with this view is to put together teams to work on particular projects. The members should have complimentary skills, energy and flair. The members will need to meet in the flesh regularly, especially at the beginning of the project. It is a good idea to write a project working document, and possibly with contributions of different sections from team member. Every member should have assigned tasks and a timeline. This may seem over-proscriptive, but I've seen teams fall apart because they missed one of these steps.

Workshops that I've been involved in seem to operate as a team meeting just once. This may work well as a think-tank whose prime function is to provide input into a team that will do the actual work.

As I get older I think my intuition and my ability to synthesise improves as my technical accuracy declines. I need to be in teams that utilise and compliment my skills.

#### **4. Constructing a Research Project**

Aggregate the above.

- *First, a 'good' question, or class of questions is needed.*

'Good' means accessible with the tools currently available, or reasonably so. It also means the questions are interesting: one's peers want to know about this question and this area. There is a school of thought that questions on the boundaries of disciplines are particularly fruitful, especially if one of those disciplines is evolving and 'hot'. Honours students choosing projects, research students and early career researchers will almost always benefit by genuine interactive discussion with experienced researchers before settling on a topic.

- *Build a team with the necessary skills to solve the problem.*

Sometimes the team is predetermined: the supervisor(s) and PhD student, or the research division of a particular company. The skills required to solve the problem need to be identified, and assigned to individuals within the team, or new members with those skills need to be introduced into the team. Of course, the interpersonal relationships between members of the team should be positive.

- *Construct a sequence of tasks that will solve the problem.*

The tasks should be assigned to the appropriate members of the team, timelines constructed for each task and the whole project. A protocol should be developed for achieving progress. So task teams and the entire team should meet regularly. Of course 'blue sky' research doesn't happen like this, and cannot!

- *Research is a confidence game; be supportive of and constructive with your colleagues.*

It is my experience, and others have confirmed the observation, that performance and output vary with confidence. It therefore pays to raise the confidence of your colleagues. A subsidiary benefit is that the positive environment will raise your own confidence with beneficial flow-on effects.

- *Ensure the new knowledge is appropriately disseminated.*

If the funding body thinks the project is worth spending money on, then it, like you, must think others will benefit from knowing the outcome of your endeavours. Likewise the examiner must think the research of the successful research student is worthwhile, and the assessors of your promotion application won't think it worthwhile unless journal editors have deemed it worthy to pass on to their readers. And of course, dissemination is essential if the student is to ultimately get an academic job, or if (further) grants are to be won.

In my area I construct new ways of analysing data. I consistently put this material out to peer review, and if that is positive, I try to produce an accessible version for users of the new methodology. Either dissemination alone seems pointless.

## 5. On PhD Students

Some readers are either PhD students or will soon be supervising same. Some good questions and conclusions follow.

What will convince *your thesis examiners* to award your PhD?

Conclusion: *Quality and quantity are necessary.*

The *supervisor* is highly important for a number of reasons.

Conclusion: *It is necessary to have both the right supervisor **and** the right topic.*

Obtaining a research degree is, in essence, a *project*, and like any project requires certain skills.

Conclusion: *Time management and project management are necessary.*

## 6. Research Management

A young colleague of mine, when starting his first academic job was required, in order to achieve tenure, to produce three papers per year until tenure was granted. The entire

department had one publication in the preceding year. It seems that the management maxim here was *"Do what I say, not what I do."*

At this establishment there was little support for research. So while there were start-up grants that could be applied for, there was no leadership: no indication of the goals of the unit or the university. There was no research infrastructure in the sense of funded groupings of researchers, and there was no collegial support. Leave was initially refused for my colleague to visit a more experienced research mentor, and there was no general academic mentor. It seems that the management maxim here was *"Don't provide leadership, infrastructure and collegial support for the objectives of the unit."*

- *Inexperienced researchers will benefit from having a productive and supportive mentor.*
- *Leadership, infrastructure and collegial support are essential.*

From the researcher's point of view, you should demand a productive and supportive infrastructure. If in your situation the model is less than desirable, be a part of the solution, don't contribute to the problem.

## **7. Conclusion**

In order to build a research profile, what can you reasonably expect of your employing institution? Identify and lobby for these things.

In order to build a research profile, what can you reasonably expect of yourself? After all, if you give nothing you should expect the same. And if you give a lot, and perform well, your institution cannot expect to retain you. So in answer to the question asked, and with the benefit of considerable hindsight I suggest that individuals should:

- produce research and scholarship of the highest quality of which I'm capable;
- embrace project/institute objectives;
- be active in their areas of greatest personal interest, and predicted strategic interest.