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Learning to be a Successful Writer

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OVERVIEW

My efforts to be an active, publishing scholar are facilitated by four gradually won insights.

- *Scholarship is based on community and conversation.* In retrospect, the first papers I wrote had an intended audience of one. Not surprisingly, these papers did not get published. They bristled with academic references, but the references reflected an idiosyncratic search to satisfy my idiosyncratic interests. Now, I know that articles and books get published because they grow out of and contribute to shared interests. A cornerstone of every project therefore must be a decision about the scholars I intend to join. There are always many attractive alternatives. Over time I have involved myself in different conversations. That evolutionary process is critical, but each new involvement comes at a high price. I understand more of, and can more fully use, the efforts of fellow travellers when I have been following their linked ongoing exchanges. Similarly, my colleagues are more likely to pay attention to what I want to contribute if I have been in the conversation for a while.
- *Writing is a form of thinking, and thus a tool to use from the very beginning of a research project.* Early in my career, I did not begin to write until my library research was completed, data gathered and conclusions reached. I start much earlier today, because I have discovered that each aspect of scholarship – the way enquiry is framed, the literature that is brought into the argument, the research design, data analysis, etc. – is improved by the discipline writing brings. When I put ideas on paper, it is easier to both simplify my arguments and increase their detail. Even more important, formally articulating an argument gives me needed distance from 'my' ideas. What I am thinking at this moment is a *possible* contribution to scholarly conversation; it is not (at least until very late in the game) a corporal extension of myself that I need to protect.
- *It makes sense to seek advice throughout the writing process.* I sought advice in graduate school, though not as often as I should have. Once an assistant

professor, I assumed my job was to independently identify research ideas, carry them out and write up the results. That typically meant I h'ad little direct input for several years on each project I undertook. By then I was not only heavily committed to a perspective I had established with considerable effort, I was so pressed for time that reviewers became my (very expensive) advisers. Once I started thinking about the community and conversational bases of academic work, I realized that I was foolishly independent. Successful papers, with very few exceptions, are honed by interaction with others before formal submission.

- *Management is required.* I started my scholarly career with a 'sherry-sipping' view of academic life. As a consequence, it did not occur to me to draw on the managerial skills I used outside of the ivory tower. I hoped for inspiration, and tried to develop insight. Experience taught me, with difficulty, that inspiration and insight depend on preparation, organization and persistence. Today, I try hard not to schedule classes or appointments in the morning, for example, because that is the time when I write most easily and quickly. I leave home, so the laundry will not sidetrack me, but I don't go to my office, because other chores wait there. A local coffee house often is the destination, and I make sure that I have enough material to support a long stay. Each scholar similarly must decide how to organize their writing lives, including a decision about financial investments that will improve the quantity and quality of what they write.

When I became a part-time faculty member at Cranfield School of Management, I used these four ideas as the basis for a book called *Writing for Scholarly Publication* (Sage, 1999). It is an overly rational but practical guide that outlines many steps in the writing process, from developing a title to responding to reviewers. I will not summarize it here. Rather, I want to outline four additional insights that have grown out of my subsequent writing workshops and seminars.

I enjoy teaching in this area because it generates conversations on issues I still need to address in my own writing efforts. The intended audience of this chapter is thus a broad one, from current students to established scholars. However, I would like to thank the students in my current University of Colorado seminar, whose writing projects are used to illustrate several points that follow.

This is a semester course, on the cognitive foundations of strategic management, but the students' primary output does not have to be on a cognitive topic. They are enrolled in several different departments, and they each work on a paper that will advance their career goals. Participants turn in a writing assignment almost every week, culminating in formal submission to an appropriate conference or journal.

Formal output may seem to be an overly demanding requirement, especially for first year students, but at the end of every course, even shorter Cranfield Writing Workshops, a number of papers are accepted. A well-thought-out start is the key, followed by persistent development up to the point of submission.

THINK BEFORE YOU WRITE

A number of pitfalls lie at the beginning of almost every writing project. For example, I sometimes begin working on projects that I have to drop as the deadline for another commitment looms. Unfortunately, once I stop working on a paper it takes time to get back into the mindset completion requires; sometimes I never get back. I find myself in another difficulty if I begin writing with a particular audience and contribution in mind, but then lose focus and begin to follow other attractive but tangential topics.

We fall into these and other traps because we begin writing too soon, before thinking adequately about the full portfolio of our obligations and interests, and before thinking adequately about the audience we most want to attract. Thus, in what follows I emphasize what writing should do.

Commit to and draw on an intellectual identity

I wish I had said more about the importance of intellectual grounding in the Sage book. Too many people are unclear about their intellectual 'home'. (I am talking here about identification with a field of enquiry, rather than a more specific, often interdisciplinary, conversation on a particular topic.)

People who are grounded in a broader area of enquiry are more likely to make forceful and credible contributions in their writing. The broader context might be a recognized discipline (like sociology, or organization theory), a strong theoretic base (like systems theory), or an area of enquiry that is just developing disciplinary status and theory (like entrepreneurship). This broader context provides ontological and epistemological grounding. It offers tool kits for enquiry, and exemplary articles that help focus and elaborate contribution.

Sometimes the work already done in an established academic field becomes a strait-jacket that overly restrains subsequent efforts. But an equally confining position, and one that is increasingly likely in an era of interdisciplinary training, is to work with superficial knowledge from many fields, but insufficient grounding to really benefit from any one.

Fields of enquiry, broadly defined, provide an institutional basis for sustaining conversation over time. Meetings are held on a regular calendar, often subdivided into sub-fields that use the occasion to advance their own identity. Journals grow in and around these associations. This context makes a difference in finding an audience. Participants in an intellectual conversation are able to communicate because they have some common background. They are able to interest each other in new ideas because they know what will be considered surprising and interesting.

Because the field influences the sense people can make of a phenomenon (Weick, 1995), I now begin writing courses by asking participants to 'map' their location in *the*, one, larger area of enquiry they identify with most. This can be done in many different ways. One person might put themselves on a historical timeline of developing theoretic paradigms and offshoots. Another might develop a kind of Dewey decimal system that focuses on logical rather than temporal connections. A third possibility is to take a genealogical

approach, listing advisers, and their advisers. A variant would map networks more generally, perhaps in an institutional framework. The breadth of the effort varies with the proclivity of the individual, their career stage and the time we are willing to give to the effort. I am happy to have people work on informal or fanciful drawings, but even a casual map is often more difficult to draw than people think it will be.

The exercise can only be considered a success if participants become more aware of and committed to a broader intellectual effort. Jeanne McPherson, for example, started a PhD in communications after a number of years as a consultant and science writer. That background is helping her move fast. In her first year she took an overload of theoretical courses, from several different faculty in her department, and chose an adviser for her dissertation. In her second year, she is already gathering data for a larger project she has under way. It is action research that draws on Jeanne's consulting skills, and she expects to use data collected as a participant observer for her dissertation. Jeanne's current intellectual map is a set of six separate islands. Each is a topic that interested her in coursework, and appears very relevant to her fieldwork. She already has a rather extensive set of names and articles to provide topical detail in each of these areas. This list is sub-divided by whether the writer is within the communications area or not. But at this point she sees either too many or too few links between islands.

I think the current state of this map is a primary reason why she is having difficulty defining a topic for her paper in my course, and a focus for her dissertation. As she begins to analyse her field data, she has too many possible categories in mind, and they do not easily coalesce. Even if she makes a hard decision to restrict herself to one or two perspectives for the paper (which so far has proven to be difficult, because she finds more topics compelling), her next research project is likely to be drawn from another perspective, which will diminish her ability to draw effectively on what she is learning now.

Thus, my strong advice to Jeanne is that she put *one* (and only one) of her islands in the foreground, connect it with one or two other areas of enquiry that most interest her, and then firmly delegate other subject matter to the background. She is reluctant to do that, because she feels affinity with each area and does not want to relinquish possible help early in her fieldwork. (A more established scholar might be similarly reluctant because they are aware of the political pitfalls of aligning oneself with one set of scholars rather than another.) These are both valid concerns, but indecision poses its own difficulties. I urge people to make a commitment. If the fit is not good, it can be changed. Aligning oneself with a specific intellectual territory increases a sense of connection with academic endeavours. As long as she feels like a foreign visitor, she is less likely to feel like a person who can dare write. It is much easier to think of an interesting contribution to conversation if one feels at home.

Make a purposeful choice from a portfolio of writing projects

Though mapping intellectual territory takes time, in my experience it is worth taking yet more time for a second management task. That task is to develop

and compare a set of writing alternatives before choosing *one* paper to work on now. I used to begin writing with the assumption that many important details would reveal themselves as I progressed. The problem, as I have already mentioned, is that I too often began things I could not finish, and ended up moving in directions I had not anticipated. In both cases, the initial frame I was using became a trap I could only escape by cutting and abandoning a lot of written material. I still feel that writing is a useful way to work out detail, but now I try to make a more purposeful start, so that I waste less effort.

When I wrote *Writing for Scholarly Publication*, I suggested that people identify three writing alternatives, then compare them in terms of four criteria. I still like the criteria, but feel the number of papers to compare is too artificial. Better to start by listing all the papers one is writing, or thinking about writing. Then, compare the set along the following dimensions:

- *Your passion* for the subject. 'Passion' is a strong word, but writing is a long and often lonely business; it takes a lot of interest to complete a manuscript.
- The project's *relevance to an academic field*. As said above, I strongly urge writers to connect their intellectual effort to a specific, established area of enquiry – an area they see as the 'home base' of their ongoing intellectual efforts. The competition for journal space is going up; it is more and more important to deeply understand the issues that a given field thinks are important. It also is more important to know how to use popular methods of enquiry. Establishing an intellectual identity allows the writer to say something about a specific subject that a larger group will find interesting, something that challenges received wisdom.
- *Relevance to practice*. Writers in a professional field are often asked to justify a proposed project in terms of practitioner interest. I believe it is our obligation to make this connection, but virtue tends to be rewarded by greater access to data, and more involvement with people who can ground the academic's necessarily more removed insights.
- *Importance to career goals*. If I make the decision to commit to a project after explicitly considering all the other things I must, should and would like to do, it is more likely that it will continue to be my priority. I will devote what is always too little writing time to a project I am most suited to do well, one that I am most likely to learn from.

The last point deserves further elaboration. It is not easy to decide on the single paper that most deserves writing attention. For example, Cecil Peterson, who is looking for a job as he finishes his dissertation, brought five possible papers he might develop to my class:

1 Conference paper delivered last month.
2 Conference paper to be delivered in two months.
3 Dissertation results chapter.
4 Job talk for an academic audience that relates fieldwork to the dominant paradigm in the field.
5 Job talk for a practitioner audience.

Each of these paper possibilities is compelling, but when he came to class, Cecil was inclined to work on either paper 1 or paper 2. Both presentations are in a field where Cecil has worked for some time. He already has a published track record in this area. He not only knows some key players, they know him. Although he has decided he is more interested in other subjects, he recognizes that some employers will be attracted to his skills in this area. Furthermore, a lot of organizing work has been done to prepare for the presentations; therefore he is confident he can complete either paper in the time frame of the course, even though he has to give most of his attention to finishing his dissertation and getting a job.

That is the reason I was inclined to support paper 3. Finishing the dissertation would seem to be the single most important thing he should do now; anything else is a distraction. Furthermore, an important way to speed publication from a dissertation is to write results chapters with publication in mind. However, Cecil is not working in my college, and it quickly became apparent that he is committed to a dissertation format in which results are first summarized with minimum researcher interpretation. He has to write at least one more chapter after the one he is working on now before he adds his own analysis, thus, it is hard for him to imagine completing either a conference presentation or a journal article in the next few months.

Presentation or journal submission is the output required for the course. It might seem that I should relax this requirement for a student like Cecil, but I am reluctant to do so, because I think almost all academics today are pressured to publish an article or two a year, and we must accept this target as a permanent fixture in our lives. Midway in the class discussion of this reality, Cecil changed his emphasis to paper 4, which relates his data to the dominant paradigm in his field. He is confident that faculty in different specialty areas will recognize and appreciate it, even though he has decided he is personally more attracted to another, more contemporary theoretic approach. Unfortunately, this preferred framework is much more complicated than the dominant paradigm, and he is not quite sure how easily he will be able to use it to analyse his dissertation data.

There is a last complication: Cecil is also considering possible employment in a consulting firm. As he pursues that option he needs to talk about his dissertation work in a way that is more action-oriented and practical. He has already had informal conversations with one private sector employer, and this is the most exciting opportunity he has yet identified. Paper 5, which does not have to be that long, might be critical in actually landing such a job. Perhaps that paper should be his focus. He could also draw on past work in the subject field of papers 1 and 2 as a reminder of other skills he can offer to an employer.

That is a short, but complicated story. I find it easier to follow because its general outline is so familiar. I am perpetually pulled in multiple directions. When I step back, I see the same problematic dichotomies over and over again. I wonder if I should:

- Further investigate topics that I know something about, or work on more attractive but higher risk new projects?
- Develop practical frameworks, or work on more abstract, theoretic ones?

- Respond to immediate demands, or work on projects that might be more rewarding in the long run?
- Work on something that I really want to do, or respond to external interests and opportunities?

There is no easy answer to these alternatives. Over time, most of us move back and forth to satisfy different career pressures, and develop different competencies. Trying to be multi-faceted is laudable, though it can be distracting. External pressure also causes attention to oscillate, often with less positive developmental results.

The successful scholars I know minimize unnecessary bounce. They decide what they are going to work on now. (I underline 'ci' to emphasize that a decision literally cuts away alternatives. The same root can be found in 'incisors' and 'scissors'.) Interestingly, Cecil ultimately decided on a sixth paper project, one that he had not considered before specifying and analysing the portfolio of papers he had in progress. This paper focuses on the new, more avant-garde theoretic framework that really interests him. Although he does not yet have extensive empirical data to present, he can discuss several compelling examples from his dissertation data that convinced him to go beyond the dominant paradigm.

Paper 6 will be of immediate use as the basis for a job presentation to academic audiences. That makes sense because he does not want to jump into a consulting career without exploring academic possibilities. Time on the paper is doubly valuable, because he wants to conclude his dissertation by applying this framework. Hopefully he will feel confident enough to distribute the paper he is working on now to academic employers.

This choice of paper 6 makes sense to me, and prompts me to offer several other pieces of advice:

1 *Work on what is most important to you.* If you do what is easy, or merely interesting, as Cecil was tempted to do when he thought of turning a conference presentation into a paper, you risk never getting to the work that will make a difference. A similar risk accompanies the decision to work on a project that logically precedes the paper you really want to do, as Cecil was tempted to do when he thought about first using an accepted paradigm to look at his dissertation data. The problem is that academic projects take on a life of their own. They almost always take more time and energy than you hope. They may change your trajectory by inviting consulting work, or generating offers to speak or write on a subject that is not central to your interests. Even when you do get to what you wanted to do, it may not go as well as it would have if you had given it more time.

2 *Draw writing projects together.* One attraction of Cecil's ultimate project is that it 'kills two birds with one stone': it is a document that will be of immediate use in the job market, and an input to the final chapter of his dissertation. I have observed that successful scholars often take advantage of such overlaps, in fact they make them happen. If Cecil had decided to develop papers 1 or 2, for example, I would have asked him whether a stronger paper could be written by combining both presentations into one

(adequately focused) paper. Quantity is sometimes a measure of success among academics, however, and he might be reluctant to follow this advice. I would counter that remembered articles and books tend to be rich in detail. Given how little time there is to write, and how long writing takes, the ultimate project is more likely to be memorable if it draws on all the resources you have available.

3 *Consider dropping projects in less important areas.* 'Triage' is a method of dividing medical patients into three groups – those who will die anyway, those who will get well anyway and those who are most likely to benefit from medical attention. A similar approach can help focus attention when evaluating a writing portfolio. Cecil had already established his credentials in one area. He recently decided that he didn't need to do any more in this area, rather, it was more important to develop other interests. He can choose to develop papers 1 and 2 at another time; they could be very attractive to him as a young faculty member who needs to show productivity quickly, for example.

The overall message is that thought and planning before beginning to write can save a great deal of time. Time is a scarce resource. It does not make sense to begin a writing project until you are convinced that the project you are working on deserves your priority. Different people have different styles, and almost all of us are working on more than one writing project at any given time. Still, it is easy to be lured beyond our multi-tasking limits. Clear decisions not only minimize wasted time, they use career priorities to highlight the most important project to work on right now.

Stand back from work in progress

Once writing begins, scholars often find themselves totally immersed. That involvement is the great pleasure of academics, but it also makes sense to vary this by connection with a more distanced, managerial perspective. Two ideas make it easier for me to do that.

Focus attention on a limited, but compelling message

Academic projects typically begin with an internal monologue, often in multiple voices. Part of me wants to begin an interesting new project, for example, while another part of me cautions that I already have too many commitments. Inner conversation gets more interesting as it becomes more substantive. I wonder if an empirical study would be the best way to explore a new subject of interest, but I can also see a compelling argument for first working out a theoretical argument; or, perhaps I should begin by systematically reviewing existing literature? But which methodology? Which theory? Which literatures? How should they be combined? The thing I hope to hear is an internal voice that says, '*That* is interesting!'

Delving into the literature with a specific project in mind shapes my emerging thoughts and continues the dialogue. Does this paper advance theory? Does

the empirical design match the research objectives? Does it support or contradict my ideas? Does it lead into directions I have not considered? The most compelling questions grow directly out of what I want to achieve as a scholar.

Other ideas come from conversations with friends and advisers. If they are interested in an idea I am working on, I become more interested, and the ideas they add shape my own. More questions and interesting ideas develop in these conversations.

In short, the topic that becomes a formal research project almost always has survived the winnowing process of conversation – its content has been shaped by many voices. My early career insight, as mentioned at the start of this chapter, is that successful research projects and publications continue this conversation in a more public forum. Typically there are several choices available.

Dawn Detiene, for example, is writing a paper for my current seminar based on a data set that has already been collected. She is interested in explaining performance across three high-tech industries, and has data on a number of variables, including innovation decisions, CEO characteristics, organizational processes and organization culture. Her analysis could be framed in a variety of ways. To keep it simple, consider two alternatives: she could write a paper addressed to those interested in innovation in high-tech environments, or to those interested in the relationship between culture and performance. If she clearly makes a choice, it will help shape many subsequent writing tasks. What is an interesting finding in a high-tech discussion might not attract much attention among those interested in culture, for example, and vice versa.

Choice is not the only issue in continuing a research project. Even when I have made a clear decision about the conversation I want to join, and have identified a specific set of articles and books as an immediate focus (two things that help prune detail), my papers tend to be too complex. The problem, I think, is that they still carry too much weight from previous conversations with myself, and with various friends, advisers and literatures. Many of these are not from the primary conversation I hope to join. Thus it is necessary to establish and re-establish focus. Detail and complexity overburden the paper, especially for less intensely involved readers. The papers I most admire have breadth and detail, but only where it matters.

Persist, but know when to stop

Scholars who bring their writing projects to closure get the time and space they need from those around them. When my writing is going well, the things I want to say often seem fairly clear. That vision fades with amazing speed if I am interrupted. Distraction can bring new ideas, but it is more often the enemy of writing well.

As a reminder of the need for time and space, participants in the writing workshops I run receive a symbolic coffee mug. They are made so that the outside face shows the message on the door of the seminar rooms we use. That message is: 'Please do not disturb'.

Often, writing does not seem like 'real work' when compared to more tangible scholarly tasks, like teaching. Writers sometimes have to remind

themselves that writing is indeed work with significant time and space demands. The importance of time and space has to be conveyed to department heads, programme organizers and others. Administrators in many universities *say* they want writing output. In practice, however, they undermine sustained writing with various requests and distractions. It often seems to outsiders (and even to the writer) that writing is something that can be postponed. In fact, interruption is often very costly.

Once we get to work, we must think about how work can be sustained, and when it should stop. Workshop participants see a second message on their side of the mug, which is:

I will DECIDE:

1. To WRITE
   One paper from a portfolio
   Offering a few 'shiny things'
   To specific conversants
   In a particular forum
2. To KEEP WORKING
3. To STOP WORKING & SUBMIT

It is as simple as that. But that is not simple. Annie Dillard, a novelist, says that:

> Writing a book is like rearing children – willpower has very little to do with it. If you have a little baby crying in the middle of the night, and if you depend only on willpower to get you out of bed to feed the baby, that baby will starve. You do it out of love. Willpower is a weak idea; love is strong. You don't have to scourge yourself with a cat-o'-nine-tails to go to the baby. You go to the baby out of love for that particular baby. That's the same way you go to your desk . . . Caring passionately about something isn't against nature, and it isn't against human nature. It's what we're here to do. (Dillard, 1987: 75–6)

I am delighted with this passage, which I think applies as much to scholars as to novelists. It is not too far-fetched to think of what I am writing now as my baby, part of a larger loving family who share my interest in seeing it develop.

But as I began to quote this passage to others, I realized that my enchantment with Dillard's words revealed how long it has been since I, in fact, have got up to feed a baby. Love was not always my driving emotion when I got up for our young children. Furthermore, I did not leave a warm bed as easily for our second born, and he learned to become a civilized, sleep-through-the-night-child more quickly as a result.

I still agree that successful writers love their work. That is what drives them to find the time and space to work on their current paper. If they do not love it, they end up paying more attention to teaching, to administration, to consulting, to the theatre. Whatever.

But successful writers also keep working on those days when they are less enchanted with their baby. Persistent productivity is based on habit and duty

as well as love. If successful writers are not inspired, they work on tasks that require less inspiration. When love is low they work on bibliography, format tables and the like. The baby lives because it gets regular attention, flourishes because it is loved, because it gets priority over other attractive alternatives.

Eventually, however, writers must decide they have done what they can and send their child out into the world. Some scholars have their greatest difficulty at this last transition point. After the planning and research, after the framing and connecting with the literature, after the analysis and footnotes and charts, they still keep polishing their manuscript. They seem to love their progeny too much. If we take Dillard's words to heart, and start thinking of a manuscript as an entity that deserves a life of its own, it may be easier to let go. Submission is critical. We keep scholarly conversation alive only if we send our work to others.

CONCLUSION

Above my desk I have a brief quote attributed to Charlie 'Bird' Parker, the famous jazz musician. It says:

> I could hear the music before I could play it, and I could play it before I understood it.

A very similar feeling motivates me to write. Initially, I only partially perceive the nature of a subject that interests me. My curiosity begins to be satisfied through sustained enquiry, but it is only as I write about a topic that I begin to understand it.

Of course, I don't ever 'really' understand the complicated things that interest me. That is why I continue to choose a scholarly career, and more specifically, why I choose to keep writing. I want to get as close to understanding as I can, and I feel that I am making progress. I suspect Parker felt the same way about jazz. My objective is to reinforce the same passion in writing-workshop participants and readers of this chapter. The music comes into being as we play together; scholarship is a community activity.

Study questions

1 What is the 'map' of the intellectual territory I most identify with, most want to be a part of now and in the future? Where are my particular interests located in this territory?
2 What writing projects do I hope to achieve over the next few years? Which ones are most important to me? Which ones are most likely to appeal to the field I have just mapped? Which ones are likely to interest practitioners? Which ones should I work on now, given my stage in career, and the other things I have to or want to do?
3 What should I decide to write now?

Recommended further reading

Huff, Anne (1999) *Writing for Scholarly Publication*. Thousand Oaks, CA: Sage.
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