
Writing *for* Scholarly Publication



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SAGE Publications
International Educational and Professional Publisher
Thousand Oaks London New Delhi

Chapter 1



Writing as Conversation

This chapter introduces five basic ideas that recur in the chapters that follow:

- *Scholarly work is rooted in the lively exchange of ideas—conversation at its best.*
- *Written work is the most enduring and often the most influential contribution a scholar makes to academic conversation.*
- *Writing is also important to scholarship because it clarifies thought and thus the generation of new knowledge.*
- *Even procrastinators often begin writing before establishing critical parameters of communication, thus diluting these benefits.*
- *Seeking advice from others, from the beginning, can save time and firmly put writing into a conversational mode.*

Scholarship Is Conversation

Thomas Kuhn helped us see that scholarship requires and is the product of interaction within a scientific community.¹ The content and process of scholarship is learned from other scholars; they shape the way the individual understands the world, and define the issues worth attention. Scholars need this audience to assess and appreciate their work. Even the most iconoclastic individuals are rooted in a social setting.

An important form of interaction among scholars can be usefully defined as *conversation*. The word clearly goes beyond an exchange of information, but I was surprised that the *Oxford English Dictionary* defines it first of all as “the action of living or having one’s being in a place or among persons.”² Conversation is thus the essence of society.

It suggests ongoing dialogue that has the potential not only to add to each participant's store of information but to alter participants' opinions and priorities, even the way they go on to conduct their academic lives. In Appendix B, Jone Pearce suggests that we frequent the same intellectual café.

Let me give you a specific example. About nine months ago, my husband Jim was asked to be the discussant for a keynote speaker at his annual professional meetings. This is the kind of assignment you take seriously. He liked the paper but thought that the author might have missed an alternative explanation for the phenomenon that interested him. This insight was based on previous work that Jim had done in a different subfield, so it was relatively easy to go to the library, get some data, and do some preliminary analysis. This work suggested his hunch was right, and part of his comments about the presentation revolved around this addition. The speaker responded that Jim's line of inquiry would strengthen work in the area, although he remained interested in his major argument. Various members of the audience debated the relative importance of the two lines of attack and suggested a few more of their own. When Jim returned from the meeting, a doctoral student in his department who had been in the audience said she was working in a region that might provide additional insight into Jim's initial arguments. As they talked, they got excited about the possibilities and are now in the process of writing a proposal for government funding. If they get the grant, it will occupy their time for several years. Even if this particular proposal is not funded, Jim's knowledge and current interests have subtly shifted.

Academic conversations are often like this. They take place over time, draw in people from different institutions, at different points in their academic careers. They move from individual to joint work, from private to public settings. Any one conversation won't affect all participants' work—but our endeavors are indelibly shaped by ongoing interactions among colleagues.

It is impossible to anticipate this flow of influence. Surely you have been delighted by a conversation with someone you did not anticipate would be so interesting, and surprised by the impact of "exactly the right word at the right time." Scholarship is like that, and newcomers are an important source of its vitality. Conversation occurs in the classroom, among colleagues at the same institution, at conferences, by

e-mail. It occurs most of all via the written word. At least at the present time, journals, books, and their electronic substitutes are the basic means by which the scholarly community carries out its work. These written words are not just the repository of past and current findings. Through the subject matter covered, the methods used, the reviews written, the arguments and agreements presented, writing in journals and other outlets defines a field and shapes its future.

To share your ideas and fully participate in the scholarship of your field, you therefore must *write*. This is the first reason to pay attention to writing. Writing is not just a way of communicating conclusions from your scholarly endeavors, it is a more basic means of participating in scholarship itself. Our efforts to communicate, especially within the disciplining confines of the written word, help us develop an intrinsic understanding of the tacit norms and subtle nuances that characterize good scholarship.

It is important to recognize that many different conversations are taking place at the same time, even in relatively small fields. Each scholarly subfield understands the world in a somewhat different way and focuses on somewhat different issues. Some fields have a long tradition, with many accepted assumptions; others are still sorting out the basic rules of the game. These complexities merely underscore the importance of being an active participant in the field and having a knowledge of scholarly conversation that is rich and current. The more intricate the conversation, the more you need to clarify your ideas in writing and submit them to others for further refinement. The more the boundaries of a field are unclear, and the need for and nature of the work to be done is the subject of debate, the more you need to test your ideas in writing and submit them to others for their response.

Of course, "the scholarly community" is not always accessible. And it may not always be right. So individuals need to develop their own sense of the world and what new knowledge is worth having. Stories about influential scholarship often involve individual perseverance when other scholars did not recognize the significance of the work being pursued. But the "internal compass" directing our scholarship should not blind us to the importance of seeking out and connecting with a larger community interested in similar issues. Good scholarship, that is rigorous and tested and useful to others, results from interaction within such a community.

Before you begin to write, I therefore suggest you focus on your own scholarly community.

EXERCISE 1

Identify the people, topics, and specific works that provide the intellectual foundation of your project.

I have in mind a rather personal account, but one that other members of your scholarly community would recognize. The format might take the form of a family tree, but you can make connections visible in any way you like. Your overview should have a temporal dimension; think of work that excites you, look at the key bibliographic works those authors cite, perhaps go further back into the bibliographies of these works. Include significant works written by specific individuals. You should be able to show multiple influences on your work and may find that geographic centers of past accomplishments are of interest. The objective is to establish your location in the social space of scholarship as a first step toward becoming more actively involved.

Good Thinking and Good Writing Are Intrinsicly Linked

One of Karl Weick's famous aphorisms is, "How can I know what I think until I see what I say?"³ Initially enigmatic to many, this question is more easily accepted as a reminder that experience often precedes cognition.

But I suggest that the relationship is a circular one. Another side of Karl's equation is, "How can I improve what I write until I find out what I think?" In other words, we write to think better, and as we think better we write better, as shown in Figure 1.1.

Taking this point of view adds another dimension to the idea that writing is at the heart of scholarly activity. We do not write just to compete in the "publish or perish" game. Writing *is* the game because

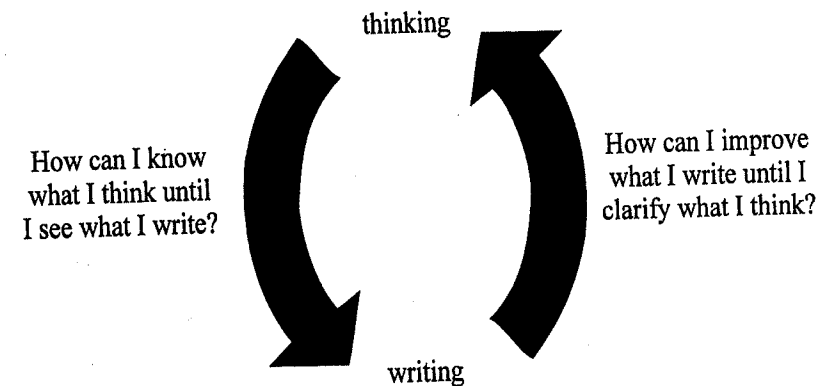


Figure 1.1. The Reciprocal Relationship of Writing and Thinking

it almost inevitably improves thinking about what questions to ask, and how to answer those questions. As I have worked to improve my writing, I am confident that my capacity to think through complicated issues, design more innovative and important research, and carry it through have also improved. My ideas are rarely well articulated until they are written down. More precisely, they are rarely well articulated until they are *rewritten*, subjected to detailed conversation with others, and rewritten again.

The first critical step in this process of needed improvement is to open a new computer file and begin putting words in a row. As I begin to write, I realize that the A-B-C-D progression of ideas that seemed so obvious in the night, in the car driving to work, in my mind as I read the literature, or even as I delivered the lecture, is not that simple. There are always mental gaps that have to be filled and sequences that must be reordered. Once I begin to write, I realize that I cannot convincingly make C follow B; I need to reorganize my thinking. Further, it often becomes obvious that D is not the logical conclusion. It needs to be followed by other ideas, which I have not yet developed. In grappling with the organization of ideas, I usually realize that I must make hard choices about purpose, audience, and outlet. I must clarify my intended contribution, especially to myself. The precision that can be

found in writing thus is critical from the very beginning of the scholarly process.

In Parts I and II of this book, I will talk about choices that apply to *both* research and writing. At the beginning of a scholarly project, writing can streamline research design and add elegance to analysis. Similar benefits can be gained when starting to write about a project that is essentially completed. When you are just starting a scholarly project, I encourage you to go through the first exercises in this book, even though I generally use them with people who arrive in a course with their research design, data collection, and analysis substantially completed. There are different ways to describe any project, and different audiences will be interested in hearing about different aspects of what you have done. These introductory chapters are written in the firm belief that it is better to explicate the framework for what you will write before you begin. It is very painful to be told after months of research and writing effort that the audience for your work is not obvious, that too many subjects are being considered, and that the contribution of the work is not clear. Chapter 3 considers these issues in more detail.

Deciding *When* to Begin Writing Is Critical

If you begin writing too soon, your thinking may be so embryonic that you do not find the effort very clarifying. If you wait too long to write, you are likely to have so many ideas they are difficult to organize and express clearly. Of course, the right time to begin depends upon your thinking and working habits, but in my experience most of us get into a double bind: We wait too long to begin but then waste time writing because we still have not made important decisions about the basic components of successful communication.

A strong organizing thread in this book thus is as follows:



*Think before you write.
Then, write to help you rethink.*



The critical questions to answer are these:

- Which conversations should I participate in?
- Who are the important “conversants”?
- What are these scholars talking about now?
- What are the most interesting things I can add to the conversation?

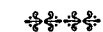
These are not easy questions. Typically, scholars are aware of and interested in more than one subgroup and quite a few conversations. At this point you must consider the literature in your field. Survey the many scholarly conversations that have been taking place over the last few years and identify those that seem most interesting and important to you. Also focus on conversations that you feel will continue for some time. Scholarship is subject to fads, and conversations run their course. To find conversations that are likely to continue long enough for you to make a contribution, pay more attention to what is being written and discussed at meetings, in working papers, and in electronic exchanges than in journals and books.

My advice is to find a few conversations and actively imagine yourself “talking back” to your colleagues from the very beginning of your career. As you define specific interests and conversations, begin to write. Even though you may not see your ideas in print for some time, the exercise will improve your thinking, your research, and thus your chances for ultimate publication.

Let me be very clear about the reason I am urging you to do all this work:



*You should anticipate making an impact on the
scholarly conversation of your field, from the very
beginning of your career.*



By suggesting that you pay more attention to scholarly conversation, I am encouraging you to make a worthwhile contribution to that conversation. Sometimes authors are too hesitant. They think they will

have to wait until they have more experience before joining the conversations that interest them most. Don't be that tentative. Begin by thinking in terms of the people and the ideas that interest you most. Assertive, well-grounded responses to those who have captured your interest are rewarding and fun to make. These responses are the best representatives of the time you are investing in your scholarly career. They are the most likely to be published, and cited by others.

Thinking, Writing, and Contribution Are Improved by Seeking Advice

You don't have to isolate yourself in your early steps toward ultimate publication, although far too many of us do. It seems obvious to me that I do my best thinking (and writing) when interacting with others. Why then do I wait so long to seek advice about the things I am writing? A major reason is that I'm reluctant to ask others to waste time on ideas that I myself know are still only partially formed. Another reason is that I am respectful of my colleagues' time.

But, basically, I'm afraid—afraid they will think that I'm not as smart as I want them to think I am, afraid that my emerging ideas could be jinxed by premature exposure to others, afraid these ideas are not as worthy as I hope they are.

I also do not *give* advice as often as I might. Here again, I am hesitant. I don't want to squelch my colleagues or their ideas. I am respectful of their efforts. But the major reason that I don't offer advice more often is that I don't have enough time. Many things impinge upon my writing; working on other people's writing keeps me away from what I want to do most, *my* writing.

Both dynamics stymie scholarly conversation, and thus the progress of scholarship. The desirable solution is easy to state but hard to do. First, get tough. People hesitate to seek or give advice because they do not sufficiently separate ideas from their carriers. But what I am thinking about today is a very small part of who I am, and what you are thinking about today says relatively little about who you are. When we have our most noble thoughts about scholarly inquiry, we say we are engaged in a joint endeavor to understand the world. That is a good

image to bring down to the everyday world of writing. Remember that we have a common commitment to expand understanding.

Second, minimize the burden. The chapters that follow are based on the confidence that the benefits of seeking advice far exceed the pain. The benefit received can be vastly increased if you follow this prescription:



Ask for help often, but keep requests to read an entire manuscript to a minimum.



It is relatively easy, and much less painful, to get a response to a title, abstract, outline, even introduction, than to an entire manuscript. Another advantage of focusing on these specific parts of scholarly writing is that their established function may lead to more explicit advice. Putting forth parts of a manuscript also makes it possible to move into the advice-getting mode much earlier in the writing process. You will be thinking in terms of conversation from the very beginning—testing and improving your ability to clearly say something of interest to others.

Community Is Important and Must Be Nurtured

Your chances of getting advice will increase significantly if you can establish a continuing connection with a small set of people who will converse about your field of inquiry (even though they may not be part of it) and make an ongoing effort to read your attempts to participate in that community. As a second preparatory step to working with this book, I therefore encourage you to name fellow travelers who are or might be involved in the process of improving both your thinking and your writing.

EXERCISE 2

Identify potential members of your "writing community."

I have used this as an early exercise in a number of workshops on writing and have been amazed at the variety of answers people provide. Peers interested in the same subfield typically are just the beginning of a long list that might include more senior colleagues, noted figures in the field, people in other fields, spouses, partners, parents, personal friends, and acquaintances who are not at all interested in the specific subject of study.

Reading such lists leads me to several general observations. First, everyone has contacts who might be helpful for improving their writing. Second, we need different kinds of readers at different points in the scholarly process. Third, it makes sense to be explicitly strategic about asking for advice. Fourth, we can learn from those whose motives are suspect, but these learning possibilities are unlikely to be the basis for an ongoing advice-giving relationship.

I believe the most sustaining sources of advice are found within a group that has a history and a commitment to the future. In this community, trust is desirable; it greatly broadens the scope of conversation if few topics are off limits. Honesty (leavened with care) then deepens the content of the exchange; again, the issue is maximizing the scope of what can be said.

Even when you are part of such a community, it pays to think about how you will interact with it. Some people are likely to give more caring and supportive feedback than others; some people (including some of the caring ones) are likely to be more honest than others. Early contact may provide "shaping" insight, while later contact could elicit more specific and tailored feedback. Sometimes I seek out people I can count on to be enthusiastic because they too love the questions that interest me, because they love me, or because they are blessed by a sunny disposition in general. At other times, it may be more efficient to hear an opposing point of view—if I can preserve the essential parts of my work with a strong internal compass. I'm also going to consider whether

it makes sense to reserve the input of a senior colleague who could affect my career for critiquing the things that I've already fine-tuned with the advice of others. In short, it makes sense to establish ongoing high-trust relationships, which is why I invite you to work on another exercise.

EXERCISE 3

Think of at least three things you can do to form and then sustain your writing community.

If you took some time with this exercise, you probably identified quite a variety of things that can be done to support community, from establishing a formal time for conversation to going bowling. What works to form and maintain a group is as varied as groups are varied; the key is to think innovatively about ways to maintain commitment. Reciprocity appears to be critical. If you are lucky, you will occasionally be able to ask your adviser, your spouse, or others in unique relationships to lend you a helping hand with your writing. A more reliable source of help can be had when both parties' writing benefits from the advice-giving process—in my experience, that means individuals who each believe they will genuinely benefit from the advice of the other. Often, but not always, they are at roughly the same level of experience and ability.

Good Advice Takes Care

Whether the request is long or short, it is helpful to know what would be most useful to say as a reviewer in an advice-giving relationship. At the end of this book in Appendix C, Kurt Heppard offers a checklist. It's a good list for those who are asked to provide formal reviews for journals, grant proposals, and other outlets. It is equally useful as a guideline for responding to members of your own writing community when they ask for your advice about the content of what they are

writing. The following more general observations about advice-giving also may be helpful:

1. *Make sure you know what the writer wants to accomplish.* Nothing stymies conversation more quickly than misinterpreting your colleague's intentions.

2. *Identify positive aspects of the current manuscript.* We learn at least as much from other people's opinion about what we do well as from their opinion about what we should change. Furthermore, positive reinforcement lays a very strong foundation for hearing subsequent suggestions for improvement.

3. *Organize and prioritize your ideas about needed change.* Reviewing another person's writing is hard work, but the work is likely to have much greater impact if you take a bit more time to consolidate and prioritize your observations. More specifically, flag the two or three actions that you think would *most* improve what you have read.

4. *Make sure you are not imposing your agenda on their work.* Even two individuals who share the same interests, have similar training, and are trying to talk within the same scholarly community will not write in the same way. It's a point worth remembering as a reviewer. The most helpful advice is not what *you* would do but what you think the *author* should do.

5. *Don't hoard your insights.* In the process of responding to the work that comes your way for comment, there is sometimes a little black voice that whispers, "Don't give away your best ideas!" My advice is to ignore that insidious voice. Ideas are cheap—making something of them is difficult. Ideas that are not exercised in the public arena are less likely to grow and make room for new and improved insights.

Once in a while I recognize an idea of "mine" that someone else has adopted without credit; typically, they don't even remember the source. So be it. Community is not possible for people who are hypersensitive about their own possessions. Furthermore, the way another person develops "my" idea makes it quickly theirs rather than mine. Often I wouldn't have made anything of it anyway, but if I want to pursue the idea further, I can in turn build on the work they have accomplished. If

the exchange of ideas does not seem to be mutually beneficial, I don't have to include Benedict Arnold in further conversation. But I have found that this is rarely necessary. In very rare circumstances, it may be smart to keep an insight "off the street" until you have developed it. In general, I believe that you are unlikely to develop such an idea to its full potential if you are paranoid about ownership. Good science is built through good conversation. Good conversation has few boundaries.

6. *Be supportive; this is hard work!* Although it is true that some people need favorable feedback more than others, I am amazed at how supportive words positively influence almost everyone. Research and writing are essentially lonely activities. We are required to define an agenda that will make a unique contribution to scholarship. There may not be many people working on the issues we choose. However many there are, we quickly become more expert on "our" subject than almost anyone we know. Positive interaction transcends loneliness and is very important. It builds trust—trust that allows members of a community to challenge each other with vigor, giving and receiving hard, honest feedback.

7. *Reflect back on your own work.* I have found that improving my ability to give advice has meant I give myself better advice as well, but the benefits of reviewing will not be gained by either party without effort. It is very easy for authors to move away from a painful exchange with too little learned. Quick distancing is even more likely for reviewers. To increase the rewards of reviewing, I suggest that one of the rules of your writing community should be that reviewers consider the ideas they offer in the context of their own interests. I have found that what starts as an effort to be supportive often ends by intellectually engaging me, potentially generating the kind of scholarly conversation we all seek.

8. *Develop and maintain an internal compass.* You do have to be careful when seeking and giving advice, especially if you do it early and often as I advise. An inept remark can easily derail an author from a worthy project. Advice from a group is often contradictory. Suggestions for change may not be on target; after all, they are of necessity based on much less time thinking about your project than you have invested. Perhaps most insidious, in my experience, bad habits are contagious.

As a reviewer, it is worth worrying about what might seep back into your own work.

Conclusion

Academic writing has its own rules and its own creativity. It is also like all other kinds of writing, and you therefore may benefit from the many books available to those who write in other genres. The bibliography summarizes a few of the many sources available.

I find it remarkably easy to identify with these books. Alongside our writing brethren, we choose our subject, try to develop content that will engage others, and work in relative isolation with vacillating confidence in our ultimate success. If writing is hard, perhaps it is because it forces us to “go public” with the inescapable responsibility of these choices. But if we do not engage in conversation, we ultimately have to give up the idea that we are scholars.

Annie Dillard describes writing as wrestling with alligators in her book *The Writing Life*.⁴ She once saw a real alligator win a contest with a man in the Everglades. Various studies indicate that the average academic paper is cited only a very few times by other scholars over the many years that follow publication. The more we think seriously about good conversation as the basis of scholarship, and learn to be better conversants, the more we can beat these dismal odds. The alligators should not win!

Notes

1. Kuhn, T. (1970). *The structure of scientific revolutions* (2nd ed.). Chicago: University of Chicago Press.
2. *The compact edition of the Oxford English dictionary*. (1971). Oxford: Oxford University Press.
3. Weick, K. (1995). *Sensemaking in organizations*. Thousand Oaks, CA: Sage, pp. 12, 18.
4. Dillard, A. (1990). *The writing life*. New York: Harper, p. 75.

Chapter 2



Managing Scholarship

Writing interacts with other scholarly activities to change a project's scope and focus. This chapter discusses the utility of deliberately:

- *Expanding understanding by exploring alternative paths of inquiry*
- *Cutting away alternatives to regain the concentration necessary to make a specific contribution*
- *Using joint authorship to expand/contract project definition*
- *Managing time and other writing inputs*

Scholarly Activities Tend to Follow a “Crooked Accordion Path” of Expansion and Contraction

Scholarship can have many aims, but all of them involve doing something that has not been done before. Even the scholar who replicates a study, or codifies previous work, aims to make a new contribution to understanding. This is not easy work. Typically, I start with an idea of what I want to accomplish and begin on one or more scholarly tasks—a literature review, an experiment, discussion with others, or possibly a very rough draft. Almost any task, especially at the beginning of a project, is likely to change the definition of interest considerably, either broadening or narrowing the idea I started with. A literature review, for example, will almost certainly suggest new ideas and variables as well as interesting models and propositions that haven't been considered.

Chapter 3



Choosing a Topic

It is not easy to find a topic that will maintain your interest over the full time period required to advance understanding. Then, too, many people write papers of personal interest that attract little enthusiasm from others. Two activities increase the probability that your topic is both personally absorbing and connects with an audience:

- *brainstorming techniques that increase the variety of topics to choose among,*
- *then juxtaposing your interests, the interests of others in the field, the demands of the subject, and your other commitments to decide among writing alternatives.*

Brainstorming Generates Alternatives From Which More Informed Choices Can Be Made

Many people do not seriously consider their alternatives before beginning a research or writing project. Although it is possible to become frozen in the headlights of too many choices, I think that it is well worth the time to generate and analyze alternatives before beginning work.

In fact, purposeful choice is *the* most important thing you can do to increase scholarly productivity and impact, although luck plays a part that cannot be anticipated, and intuitive leaps are to be valued. Unfortunately, planning is not emphasized in most graduate schools or in most conversations among scholars. Rather than planning, many scholars focus on the interesting intricacies of the tasks at hand. I tend to plan

less than I think I should, but when I do pause to reflect, I almost always think the break was worthwhile.

The bibliography lists a few books on brainstorming techniques that are well worth reading as a first critical step in more purposeful thinking about alternatives. I hope you are familiar with the basic rules; nonetheless, they are worth repeating.

1. *Autonomous idea generation without evaluation.* As you begin brainstorming, it is important to delineate your own ideas so that other sources do not overly determine the parameters of your thinking. You should move away from the literature describing previous work, and what well-known scholars in your field think should be done next, to give your own inherent creativity space to “play” with research and later writing alternatives. By extension, move away from what your adviser, your smartest colleagues, and other influential people in your life think. In fact, do not think about what *you* think. The cardinal rule of brainstorming is to generate ideas first, without evaluation.

2. *Varied inputs from other sources.* After you have sketched out quite a few alternatives for a research project or specific piece of writing, add the insights of others. Brainstorming activities are designed to generate a wider pool of ideas than you would normally create. One of the most reliable means of doing so is to interact with others—ideally in face-to-face fanciful conversation, although interaction with written materials can also be generative. Obviously, the more you expose yourself to sources you do not normally encounter, or find modes of conversation that are out of the ordinary, the more novelty is likely, and novelty is what you are seeking.

3. *Playful curiosity and development.* Once you have a pool of ideas, see what happens when you juxtapose, combine, and extend them. The objective is to generate a lot of alternatives to work with, including some “off-track” material that may provide an interesting twist that will increase your contribution to the field.

4. *Delayed evaluation.* Suppressing judgment includes censoring the internal doubts and external comments that inhibit pursuing a

train of thought into new territory. Having fun and being playful are the desired modes of behavior. Yet, ultimately, choices are made. Brainstorming ends as you specify criteria for evaluation and choose among the rich set of alternatives that have been developed. The outcome of this deliberate process of stretch and collapse has been shown to be very productive for some serious and profitable activities, such as new product development and the solution of vexing scientific problems.

Break the Task Into Components, Brainstorm on Each Component

A useful technique for increasing your pool of novel ideas is to break a subject of inquiry into its attributes. For an example adapted from Koberg and Bagnall,¹ consider Figure 3.1 as the first output of a conversation (with oneself or with others) on “designing a new writing instrument.”

With such a list, you can easily construct a whole range of new objects to write with. You could design, for example, a square paper box filled with beet juice. That first idea might not make it to the store shelves, but you have quickly freed yourself of preconceptions about ballpoints and graphite pencils. And I can imagine that some potentially marketable ideas might ultimately come out of fanciful speculation about who might be interested in a square paper beet juice pen and how they might use it.

It’s easy to anticipate what I’m going to suggest you do next. In my view, few scholars take enough time to think about the almost infinite range of opportunities for carrying out research before they commit themselves to one project. When it is time to write about a project, they similarly do not explore the wide range of alternatives available. Even when they pause to reflect, few are as creative as they might be.

I believe that the richer the “gene pool” of ideas, the more interesting, more varied, and more useful the products of scholarship are likely to be. Therefore I recommend that you think for a few moments about Exercise 7. It will probably be more illuminating if you can get one or two colleagues to do the same thing independently.

<i>Size</i>	<i>Shape</i>	<i>Color</i>	<i>Material</i>	<i>Marker</i>
small	cylinder	black	wood	graphite
medium	sphere	blue	plastic	ink
large	box	rainbow	paper	beet juice
	oval	marbled		

Figure 3.1. Design Alternatives for a Writing Instrument
Based on Koberg and Bagnall (1976).

EXERCISE 7

Identify basic categories and alternatives within categories that could be used to define your scholarly work.

There are obviously many different approaches to this assignment. If other people thought about the same exercise, you should have more dimensions than you personally identified, even if you thought you were being quite creative. Figure 3.2 suggests an initial list that can be easily changed or elaborated. (You might note that I deliberately did not try to make the alternatives mutually exclusive.)

You could go on to outline the many different paradigms that inform research and writing, from a logical-positivist perspective to postmodernism and beyond. Then, think about the various fields that could inform the kind of work you do, from economics to anthropology to neurology. Follow that list by outlining, in a few areas, the various theories that interest you. Move on to methodological alternatives. (I

<i>Audience</i>	<i>Format</i>	<i>Purpose</i>	<i>Approach</i>
Close Peers	Formal Paper	Inform	Extend Theory
Another Field	Presentation	Persuade	Present Data
International	Essay	Describe	Review Literature
Academic		Explore	
Practitioner			

Figure 3.2. Writing Alternatives

am following a quite structured view of scholarly choices here, as shown in Figure 3.3.)

By playing with permutations from Figures 3.2 and 3.3, it is possible to generate a large number of “beet juice” alternatives that are unlikely to lead to viable scholarly activities. But finding different ways of thinking about scholarly endeavors is an important early task to ensure that you are making an informed choice about your writing alternatives.

Identifying Your Paper Alternatives

All this has been merely a “warm-up” for the beginning of the real work. Now you have to consider your own research and training, your own interests, ambitions, and resources, and come up with specific alternatives for your next project. When I ask my students to do this task (even first-year PhD students), I suggest that they set as a goal generating *at least* one writing project a day for a week. They have courses, the literature, their own and their colleagues’ past projects, and many other sources to help them along the way.

Depending on the time available, it may make sense to generate alternatives rather systematically from lists like those shown in Figure 3.2, but ultimately you must just sit down with a blank computer screen

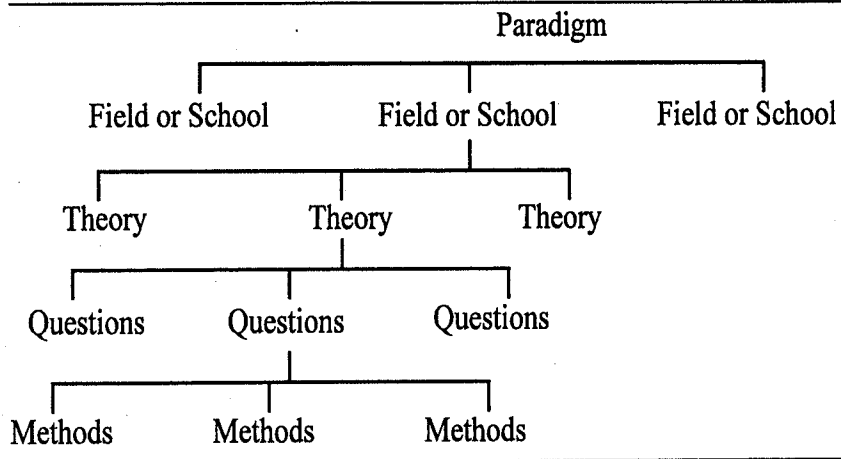


Figure 3.3. The Research Pyramid

or piece of paper and think of something you want to do that hasn't been done before, as far as you know.

EXERCISE 8

*Generate five to ten ideas for writing projects.
Reduce them to your three top choices, synthesizing
your best ideas from the larger set.*

Getting into a "rate busting" mentality is helpful to meet this assignment. Not too much detail is required. I ask that each idea be described in a paragraph or two. In addition to the basic topic, you should include at least three or four other project attributes: theoretic approach, population studied, ontological perspective, method of analysis, sources of data, audience addressed, and so on. Aim for variety on these dimensions as you move from one writing possibility to another. For example, think one day about great data sources; begin

the next day by thinking about the theoretic perspectives that interest you most.

As you work on generating a larger pool of alternatives than most people consider explicitly, you may need some help following the "no evaluation" rule of brainstorming. I give this piece of advice:



Establish a "bottom drawer" to file project possibilities. Wait until you have a half-dozen or so before you synthesize and evaluate them.



The bottom drawer of your desk has several attractive features as a research tool. First, you can and should keep the drawer shut most of the time, opening it only long enough to throw in your latest idea. It's not a very formal filing system, which is all to the good. You can even put in a book or picture or newspaper article that you find evocative.

Think about making this drawer a permanent part of your work environment. I find that I frequently have ideas for new projects throughout the research and writing process. They are not just research and writing ideas but teaching and other possibilities as well. Unfortunately, they often distract me from the work at hand. I take advantage of the intrinsic generative nature of scholarship by writing a brief description of each possibility as it intrudes upon me. Then, I file it in my bottom drawer for later consideration. Having done that, I can usually ignore what is otherwise an attractive nuisance.

You should wait at least until day five to open your bottom drawer and examine the results of your brainstorming efforts. Typically, I have to use a second critical research tool, the dining room table, to spread these papers out and categorize them. When I do that, I typically remain enthused about some possibilities that were in my mind all week, but I also see patterns that were not so obvious. I'm surprised, for example, by how often I return to a particular finding or refer to a particular article. Waiting to accumulate a set of possibilities thus lets me learn new things about myself.

Seeing my brainstorming efforts fresh, as a set, also makes it more likely that I'll see connections among my ideas. Brainstorming philosophy includes the idea that a first pass rarely produces a fully realized idea. You haven't finished with Exercise 8 until you consider combinations of ideas and select the most promising for discussion with others.

Four Parameters for Evaluating Research Topics

You move out of brainstorming into a very different logic when you begin to choose among alternatives. The more energy you put into generating research ideas, the more work you will have to do to reduce them to one choice and commit to writing one thing, now. The process of thinking about the merits of more than one genuine alternative, however, has multiple benefits. First, considering serious alternatives will give you confidence that you are choosing the best project to pursue as well as information about what aspects of this project are particularly important to you. Second, deliberately choosing among alternatives makes it less likely that another commitment or opportunity will derail you. Care in the choice of a project will also help develop a sense of your place in your field of scholarship, something that is harder to see as you move into the minutiae of writing. Finally, considering viable alternatives will help you establish a portfolio of research objectives that will both balance and diversify your work over time.

I do have an important suggestion for evaluating the alternatives you have chosen, but it is highly systematic. Indeed, I am following a very systematic template throughout this book. I have mixed feelings about this, because I know from my own research and writing experience that there are many aspects of choice, including happenstance, emotion, politics, and other forces that are not easily incorporated into a rational format. I'll discuss a few of these issues, but I urge you to focus on the "critical diamond" shown in Figure 3.4. It is the best tool I know to identify projects that are interesting, important, and likely to be published.

The scholar. Start evaluating alternatives by considering yourself. You must decide how much innate, not totally rational, enthusiasm you

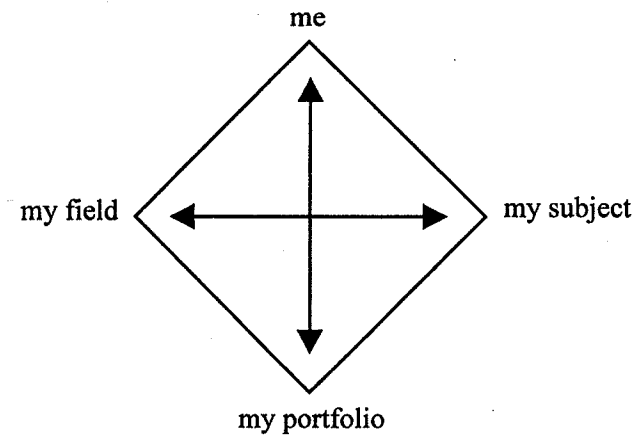


Figure 3.4. A "Critical Diamond" for Evaluating Writing (or Research) Alternatives

have for each serious possibility you have identified. Remember that scholarship is essentially a lonely activity. Most projects take longer than you estimate they will. Therefore, even when thinking about coauthoring, it doesn't make sense to work on a project if you don't like it. So, begin with your own interests.

There is an important balance to consider as you think about what you most want to do:

- Some projects are attractive because they fit your current skills and abilities. You are more likely to make a genuine contribution to your field if you emphasize your strengths. You also speed and simplify execution of the project.
- Other projects contribute to your development as a scholar, thus making subsequent projects more powerful. This is a key consideration for every scholar because it is our responsibility to invest in and develop our skills.

I don't have any easy metric to decide among projects, but I encourage you to weigh the merits of each project you consider along this difficult continuum, and I have this strongly felt piece of advice:



Do what really interests you—within reason.



Projects that codify or test what my mother already knows (and my mother knows a lot) are problematic. Life is short. Outcomes, including fame, are very difficult to predict. It therefore makes a lot of sense to me that each scholar should try to answer questions that genuinely perplex him or her. They will enjoy the process of scholarship and care less about the vagaries of publication. Projects that seek to “prove” something the author already believes are rarely of interest to me and probably are not of great interest to the author.

The field. You must also think about how interesting your possible writing project is to your field. This is as important as thinking about your own interest in the project. Consider a potential project’s fit with dominant paradigms, currently “hot” topics, questions pursued by leaders in the field, understood methodologies, familiar sources of data, and so on.

Again, there is no easy metric. A project that is too far from the center of a field will have difficulty attracting attention and being understood. One that is too close to what has been done before is unlikely to be considered an interesting contribution to conversation. I have three pieces of advice:

- Make sure you are aware of current conversations. This is more easily done by following Internet conversations, working papers, conference proceedings and specialty meetings than by reading journals and books. The works now becoming available in published form were done at least two years ago; often they have had a much longer gestation period.
- Don’t jump on today’s “bandwagon” unless you feel you have an innate understanding of why it is interesting and important. Fads in most fields move fast. If you have an intrinsic understanding of your subject, you will be able to reframe it in terms of new enthusiasms if necessary.
- Finally, be expansive.



Put all your relevant ideas into your current project; do not ration good ideas in the hope of second publication.



The publications that make an impact are rarely one thin slice from a larger pie. Furthermore, new ideas will grow out of working with the ones you have. You therefore not only write a more interesting paper by working without constraint, you create the ground for additional interesting papers.

The subject. Even if your project involves historical data, it is good advice to consider what can be learned from the subject of study itself. There are at least three things to consider before embarking on a project:

- The intrinsic need for the study/article
- Site access
- Availability of data

Many insightful scholarly contributions begin with the observation of an anomaly in the world, a question about why something doesn’t happen, or a revealing comparison among different data points. You can make a contribution if you codify what exists; you also can contradict current wisdom. You might ask a few people close to the subject what they think of your writing alternatives and how they would rank them. If you can capture their enthusiasm, you are far more likely to have an interesting story to tell.

Portfolio. Any one writing project is advanced at the expense of other alternatives, whether we pay much attention to them or not. When I get enthusiastic about a new project without thinking about the other things on my writing agenda, however, I often find that I lose time later. Often I have to suspend work to honor other commitments, or a cooler look at other options may push the interloper out of the picture altogether. Thinking in terms of a portfolio will help you avoid this misdi-

rected effort. Time is your most precious resource. Most of us can think of many more possibilities than we will ever have time to complete, even with the help of coauthors.

The criteria for managing a set of promising projects depend upon many things—your field, your nonwriting responsibilities, your aspirations, where you are in your career. Some people seek variety and are happiest advancing many different interests at once. Others are much more disciplined, seeking significant influence on a specific conversation, perhaps working on one manuscript at a time. At issue is not only the range of subjects you choose to pursue, or how many endeavors you keep going at the same time, but where you will carry out the conversations that interest you.

The criteria for tenure and promotion are an obvious guide when making such decisions, but choices are often simplified and distorted by the academic grapevine. Few employment or promotion decisions are made solely on the basis of the number of publications in top journals, for example, as often believed in North America. It is obviously desirable to have been accepted in such outlets, but the cumulative impact and future trajectory of scholarly work is more important.

My point is that it makes sense to have an agenda and a timetable. I made a smart move as an associate professor, for instance, by editing a book in a new area of inquiry. However, it took a lot longer than I expected, and I wouldn't recommend working on a book without careful consideration. This is just one complexity that leads me to strongly recommend discussing the course you are charting with other people—not only senior advisers but peers who will see issues of immediate relevance that older colleagues may not notice. A plan will help you make the best decision about whether or not you can afford to work on the one project that is in the center of your radar screen at the moment.

You may discover you have too few choices. I find that many people have difficulty coming up with more than one *real* alternative from Exercise 8. I know this is true because when I ask people, "Which one of these three alternatives do you really want to write about?" they can usually tell me without a moment's hesitation. My advice to them is to go back to the brainstorming process and come up with more than one real candidate. Typically, they do this by becoming more micro, developing variations around what they now know they really want to do. That is fine. Just remember that you haven't really chosen your research project until you've felt some anguish over deciding what to do.

But, if you have gotten this far, it is equally likely that you have more ideas than time and resources. You may feel frustrated having to choose among one or more projects that vitally interest you, one or more that you think would most interest colleagues, or are compelling in terms of the subject matter itself. One way to ease this dilemma is to remember that the writing (or research) project you choose now is not the only thing you will work on over the next period of time.

Thinking in portfolio terms should help you balance a relatively high-risk project with other lower risk projects. "Risk" is something you will have to define for yourself, although some obvious dimensions include the predicted length of time to project completion, distance from central interests in your field, sensitive or low data availability, high cost—the list can go on and on. My rather obvious advice will have to be tailored to each individual's circumstances and career goals:

- Keep the pipeline full by considering the time requirements of different projects.
- Mix coauthored projects with projects done alone.
- Mix projects of great personal but low field appeal with "sure hit" projects.

Choose to Begin

The basic idea is that you must consider and reconsider your interests, the current conversation in your field, the issues raised by the subject, and the portfolio of your other projects until you feel comfortable that the writing project you are about to choose makes sense from all four points of the critical diamond. The choice among viable alternatives will thus not be easy, but it will be a real one.

EXERCISE 9

Evaluate your top three possibilities for research or writing on the four points of the critical diamond. After discussing your alternatives with others, commit to one project as the focus of your immediate attention.

A small bit of advice from the brainstorming literature here: Make your choices an exercise in deletion. Combine and synthesize ideas that come out of your Exercise 8 efforts, then decide which is least viable, interesting, and so on. Take that out of the pool, then delete the next least attractive.

Why focus on three choices for more detailed comparison? Because it is hard to discuss many more than three with other people. Collapsing to two, on the other hand, invites dichotomous thinking that will tend to overly simplify your choices.

As you make your final decision, I have a last piece of advice:



Be interesting, but don't try to be avant-garde on every dimension.



In the early flush of enthusiasm, with relatively little real information to disconfirm hope, it is easy to design a high-risk project. People with a certain turn of mind are likely to be lured by their opportunities. Excited by a new innovative-but-untested set of ideas, they get in over their heads methodologically or theoretically or just in terms of project scale. Other people, driven by the need to be different, may choose the radical alternative on as many dimensions of their work as possible. If your aim is to find an audience, both inclinations are problematic; your most distinctive contribution can be easily lost in the "white noise" of other distractions.

Some middle ground makes sense, for most people, most projects, most of the time. You have the greatest chance of being part of a scholarly community if you strive to be interesting and innovative, adding something new that has not been done before but within sight of the current boundaries of the field.

It is fun to try to push those boundaries. If you want to do that, my advice is to look for fellow travelers but simultaneously define a project that will be more easily understood by mainstream scholars. This work is most likely to bring you into conversation, which can help you be a successful revolutionary.

Continuing to Manage Your Portfolio

Your portfolio of research and writing projects will require continuing attention because its very existence can slow or even stop publication. Most ideas in your bottom drawer must stay in your bottom drawer. You must always make careful decisions about what you will work on now and what you line up for subsequent attention.

A feasible number of projects is not the only thing you must manage. If you do not think about your portfolio, you may find that you inadvertently talk to groups outside of your field about subjects that do not represent your primary interests but nonetheless lead to writing commitments. Increasing pressures to publish make these or other "quick hits" alluring. Three things must be kept in mind before taking the bait. The work you do now develops reputation and skills for the work you can most easily do next. Your list of publications and work in progress is a signal that others use to make decisions that can affect your career—sometimes you are not even aware they are being made. Finally, if your true passion lies elsewhere, you are wasting time.

Even when you remain focused, writing itself can create imbalance. Almost every project encounters difficulties. Analysis may be difficult; writing about limitations may be discouraging; an inviting introduction may be elusive. With experience, I have found some of these problems quite predictable. Certain parts of writing are hard to do, and turning attention to another paper is very appealing, especially one that is easier and more fun to work on. If you do not resist this temptation, your portfolio will suffer. You will move further and further from publication as the papers you have in hand require skills you have never developed.

Other emotions can also lead to uneven effort. One of the biggest mistakes I made in my early career was not to revise two different articles for the most prestigious journal in my field. Reviewers of the first work did not seem to understand my purpose. Their suggestions confused me, and I had grown tired, so I gave the paper to a friend working on an edited volume. A year later, I submitted a paper that was even more important to me. One reviewer wanted me to consider another literature, and the journal editor agreed. That made me mad. I had looked at that literature and found it less useful than the one I used as a foundation for my arguments. So I sent the second paper to a less widely circulated journal. Both were bad decisions, made without managerial oversight, that reduced the "value" of my portfolio of

publications. With the 20/20 vision of hindsight, I should have asked other people to help me understand the first reviews. The second paper would have been stronger if I had added my impassioned argument for the greater utility of my literature over others and sent it back for reconsideration. I wasn't thinking in terms of conversation, and I ended up not reaching the audience that I most wanted to discuss my work.

A portfolio can help you initiate multiple conversations—but not too many. Although I am going to keep my attention focused on supporting your effort to get one manuscript to the point of submission, I encourage you to actively manage a limited and focused portfolio of scholarly effort so that other scholars have the opportunity to share and shape the knowledge you are gaining.

Conclusion

One of the reasons I had a hard time getting published after I received my PhD was that I could not decide *what* out of all the many things I might do, I wanted to do most. The critical diamond I've just described helped me balance the forces that pulled me in different directions. An ongoing portfolio of projects, some under way, others mere possibilities in my bottom drawer, has helped me work on *one* writing project right now, although in truth I continue to overcommit myself. In the hope that you can be more disciplined than I sometimes am, the rest of this book focuses on helping you “frame” and publish the project you chose in Exercise 9.

Note

1. Koberg, D., & Bagnall, J. (1976). *The universal traveler*. New York: William Kaufman.

Chapter 4



Identifying Conversants

The idea that scholarship depends upon interaction with other scholars is widely accepted but often forgotten in the research and writing process. This chapter therefore coins the term “conversants” to identify specific written works that the author hopes to directly engage with his or her writing. Identifying three or four conversants (ideally before research begins but certainly before writing begins):

- Will focus the questions asked, and influence the way they are answered, and
- Make publication more likely, if the rules of good conversation are followed

Definitions

The first chapter of this book argues that scholarship is socially defined. This idea is well established in the sociology of science; a few salient references can be found in the bibliography. My definition of conversation is consistent with that literature.

Conversation is an inclusive term that covers many different forms of interaction among scholars. Although face-to-face discussion is an important part of that interaction, and informal electronic communications are increasingly important, the written word is still the primary mode of scholarly communication. All forms of communication transmit ideas and organizing influences, but articles and books are the more formal, more definitive statements intended to influence other scholars. This is an author's best forum for making an enduring contribution to scholarship.