5

Research Design

❖   ❖   ❖

Research design balances activities likely to generate a few interesting/significant claims with activities that support them as trustworthy contributions to knowledge. You need some understanding of the design components outlined in the next six chapters in order to make an initial design, and (as Chapter 12 suggests) it is important to occasionally review these decisions as you carry out your project. Four definitions may be helpful. Design is

- *A related set of decisions that link your activities, your scholarly purpose, and its outcome*
- *Activity that is both organic and mechanistic*
- *A systematic plan of action improved by communication to others, especially sources of funding*
- *Thinking that requires plain, effective language.*

❖ THE PURPOSE OF RESEARCH DESIGN

My husband, Jim, got a pilot's license several years ago. I was astonished to learn that the number one reason for small plane crashes is lack of fuel. In some cases, an emergency situation like bad weather required the pilot to stay in the air until fuel was expended. In many others, the pilot left the ground without adequate preparation.

It is only a little less astonishing to me that a large portion of the high rejection rates at our best journals is the result of an editorial **desk reject.** In these cases, the editor

decides that a manuscript is so obviously unready or unsuited for the journal that formal review is not warranted. Some of these rejections may be due to genuine misunderstanding on the editor's part or the author's, but again, in many cases the fault is clearly inadequate preparation before submission.

The first part of this book suggests that the definition of "publishable" depends upon coordinating your interests and the interests of an audience. The second part of the book discusses subsequent options for connecting what you want to explain, who you want to converse with, and how you will discover and refine your explanation (your research activities). Six areas of research design are covered. Figure 5.1 provides an overview.

Herbert Simon describes design as the achievement of "what can be."[1] As you know, the outcome of scholarly work can take many forms, including a literature review, model, simulation, theory, report of empirical investigation, or other product. The chapters in this section of the book provide more detail about how your outcome can attract the attention of a scholarly audience.

My overall claim is that specific research design decisions in the areas listed (and others as well) must help you depart from what is currently known to your audience, while staying close enough to their interests that your contribution is recognized and
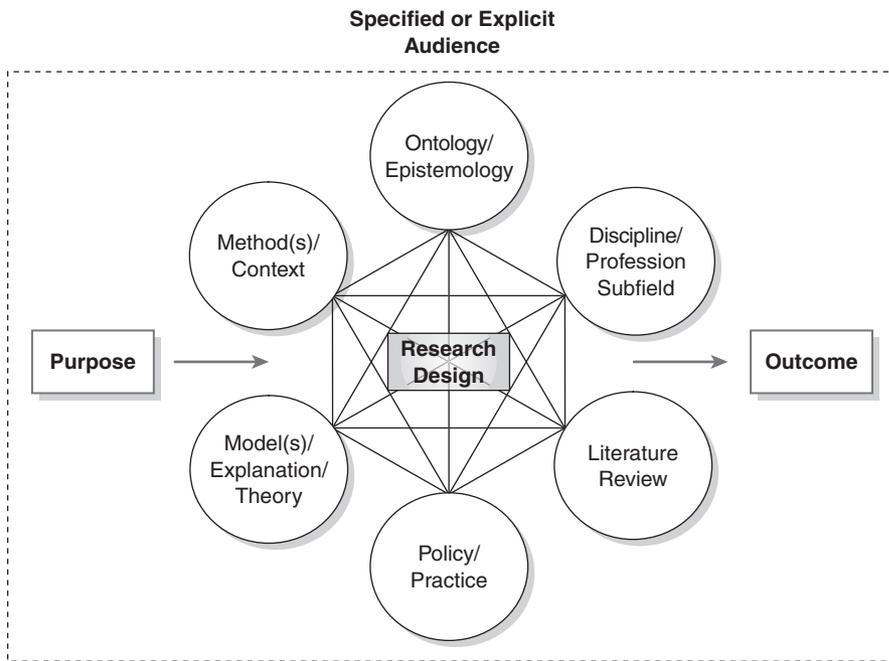
Figure 5.1   Design Decisions Connecting Research Purpose and Outcome

valued.[2] Figure 5.1 provides an A + B = C logic. Given your purpose and design, editors and other evaluators (dissertation chairs, funding committees, as well as colleagues) are very likely to ask about your outcome:

- Does it make sense? (Can I trust it?)
- Is it authoritative?
- Is it interesting/engaging?
- Is it significant/of enduring importance?

Of course, these questions should be supplemented by other questions related to the specific conversation you evoke. They are particularly detailed when considering journal submissions, a subject discussed in more detail in Chapter 12 after the details of research design have been given more attention.

You must also remember, as the circles in Figure 5.1 are meant to indicate, that not all evaluators will come from within the conversation that interests you. Promotion decisions, for example, involve inputs from people with many different disciplinary backgrounds. Though they will not understand the details, they will judge the overall logic of your work. You need to be as convincing as possible, and familiar decisions are helpful.

❖ DESIGN AS A MECHANISTIC AND ORGANIC PROCESS

Design is unlikely to be a big issue if you are in the midst of conversation where many choices have been previously debated and an overarching agenda has been established. As discussed in Chapter 2, it makes sense to undertake projects that enjoy these straightforward design options because the path to publication tends to be relatively clear. Sometimes, however, the connections may not be obvious. Newcomers must learn about design. Scholars beginning work in a new area must reconsider their previous assumptions. Those who choose more risky projects for personal growth and potentially higher payoff explore untested designs by definition.

It is not easy to specify a research design for projects that do not follow a clearly established model, and I have found that the **mechanistic** advice offered by many textbooks is insufficient. Especially for more interesting and significant research, **organic** activities are required as well. A book in my discipline offers this description:

Significant research is characterized by a particular kind of duality . . . characterized by both organic and mechanistic processes, by both linear and nonlinear thinking. Organic processes characterize the investigator's immediate world, and include . . . letting things happen that can converge and be exciting. The choice process for selecting research is often nonlinear, and is based on intrinsic interest

and intuition. The research outcome, on the other hand, might be characterized as mechanistic. The successful project . . . ends up as a clearly defined, logical, rational product for diffusion to colleagues . . . [and the broader public]. Perhaps this is why so much theoretical effort is required . . . [to translate] the poorly understood to the well understood.[3]

The following pages describe research design from this perspective. I draw on my own experience to outline four stages in the attempt to come up with a satisfactory purpose/design/outcome for a new research project:

1. Looking for an interesting subject of new research and the specific audience that might be interested in that inquiry

2. Becoming more logical

3. Formalizing design

4. Redesign

I hope that the alphabetical list, which starts below and continues through the chapter, helps you recognize some design issues you are encountering in your own work, and facilitates discussion with others. The list is initially presented as a way of *finding* a subject and receptive audience. Many dissertations follow this path, but I was rather surprised that I had to carry out so many familiar tasks when that project was completed and I had to find another subject of inquiry. An alternative starting point is also familiar: *responding* to an assignment, a funding opportunity, a collaborative effort, or some other project you do not initiate. In a few pages, you'll find a checklist that shows how the same steps might be helpful, with a few critical modifications.

Of course, different kinds of projects have different starting points. I will describe a process that revolves around finding a subject focus, but scholarly projects can begin in any area of design. For example, interest in developing expertise with a new method can drive research design. But here again, many of the same steps must be followed with modification. Keep the image of needless plane crashes in mind and attend to all aspects of Figure 5.1!

First Category of Design Activities—Initiating the Project

Often the basic insight for a new research project is a satisfying "aha," but holding onto that promising moment and transforming it into a concrete research project takes time and effort. As I contemplate an attractive but unshaped possibility, talk rather incoherently with my closest colleagues, and collect varied references that seem more or less relevant, three things seem particularly important:

A. *Testing vocabulary and short descriptions.* I need words and ideas. They have to be somewhat new to me if this is going to be a new project. I have to find them before I can think very effectively or start talking very effectively with others. In the beginning, I am like a bat sending out sonar signals. I discover the possible nature of my new enthusiasm, and the way others might see it, by the echoes that come back from colleagues, strangers, and my own reflections as I listen to what I say.

B. *Seeking interesting/interested conversations.* Though my early efforts are clearly shaky, I am discovering who might be interested in the topic I am trying to define. This is rarely obvious, in my experience. The excitement of many possible connections is often destroyed when promising leads seem irrelevant on closer inspection. Yet a single statement or discovered paper can speed me forward by making a compatible observation.

C. *Assessing opportunity.* Even at this early stage, I try to be a sensible (more mechanistic) planner because I know that serious research consumes my most precious resource: time. So I compare the "aha" with more objective estimates of my possible contribution and the time that might be required to achieve it. A number of ideas are abandoned at this point because the gain/pain ratio is not high enough, yet a few notes are saved because they might be resuscitated in another project.

In a few cases, early assessment triggers more focused design processes. I am not sure where I am headed at the beginning of a new research venture, though the level of uncertainty varies dramatically from project to project. The key objective of the design process, as noted above, is to put together a series of activities that will deliver "a clearly defined, logical, rational product for diffusion to colleagues and the broader public."[4] That's pretty mechanical, and there is a mechanistic formula for research design, which can be summarized as follows:

- Specify a gap in current knowledge that needs to be filled
- Provide information that fulfills the current gap (or makes a significant step toward doing so)
- Offer supporting explanation, including details about the process of discovery.

The journals I know tend to ask for a presentation that follows this formula even for inductive research projects. Note the translation: You and I have to move from the first "aha" to a statement recognized by a larger audience. The result has to be stated in their terms, not ours. In the process, we have to shift from the typically organic process of discovery to a more mechanistic presentation that will be accepted by sophisticated readers. As an editor, I feel this critical transformation is often missing, or poorly stated. Your best instructors are the authors of successful publications in your field.

EXERCISE 12

Identify Common Research Designs in Your Field

1. Examine every article in several volumes of the journal you would most like to publish your research.

2. Outline the research design of every article, except those that are not instructive for your project.

3. Based on this evidence, specify a design for your project that is likely to be accepted by your intended audience.

When people take the time for this assignment, they are typically surprised to find a rather small number of widely used design "recipes." I hope you make the same discovery, and that it speeds your research decisions. Before going further, however, note the hidden test in this exercise: if you set aside too many articles as "not instructive," you may have your sights set on the wrong journal!

Before moving on, it is important to emphasize the continuing interaction between the organic and the mechanic. More specifically, while I have just described moving from an organic search to more mechanical considerations, the opposite path is also possible. Scholars who begin with an assignment from an advisor, a possible source of funding, or other authority, may initially work on mechanical questions, but success will depend upon their creatively/organically finding something less obvious to add.

Second Category of Design Activities—Attempting to Be Logical

The projects I decide to advance get increasingly systematic attention, though more creative activities continue to play a role.

D. *Connecting with specific literatures.* Articles and books are ordered. Folders are created on my computer. Shelf and file space is cleared in my office. As a visual person, I begin thinking in terms of overlapping bubbles—the fields and smaller, more specific conversations, that potentially define my new project. I also start connecting a few constructs, as Dave Whetten advises in Chapter 11 on modeling. As key questions begin to emerge, some speculations become central while others fade.

E. *First "maps" of the subject.* With a better grasp of relevant literature and the ideas and vocabulary they provide, I can be more specific about what I want to study. Still, I usually find that focus is not stable. At one point I think "x" is the outcome I want to understand. Further reflection makes x a contribution to another outcome, "y." Perhaps x isn't even necessary. This is another of the realities of scholarship that does not get enough discussion,

in my opinion. The mechanistic form of publication hides considerable fluidity in under-standing subject focus, and this is sometimes a problem even when the "subject" is exter-nally defined.

F. *Early experimentation.* The nature of your field as well as your personal learning style will influence how quickly you become more active. Perhaps your experiments will be thought experiments, but I want to do something more tangible. I find myself carrying out informal interviews at this point, making cognitive maps, drawing models. In the process, I revisit design activities described in A through E: clarifying my subject, testing vocabulary, thinking about a conversational home, and so on.

G. *Increasing clarity about needed resources.* Though I am primarily using organic processes in the early stages of defining a new research project, the voice inside me that wants more mechanistic security is increasing in volume. So I begin to give more serious attention to research methods, the kind of data needed and where they could be gathered, colleagues who might contribute as coauthors, conferences that might be attended, and so on. My situation reminds me of friends in Los Angeles who have purchased a movie option on a novel. They are spending enormous amounts of time comparing actors who might play leading roles, thinking about who might write the screenplay, possible loca-tions for filming, and so forth. Possible actors and other key contributors are contacted to see if they would be interested in being involved if sale to a studio is achieved; in fact, sale depends upon having a preliminary network in place. It all sounds very exotic to me, but very familiar. They are doing what I've described so far. By analogy, data sources have to be assured, appropriate methods of analysis must be available, available time has to be considered—all this before knowing whether or not the project will really get underway.

H. *Managing commitment.* While I hope that the design activities I've just described are leading to a viable research project, I try to avoid becoming committed too early. Far better to decide this is not the project to pursue than move it along with insufficient evi-dence. This is particularly important if coauthors are involved, because different needs or interests are likely to lead to different "go/no go" assessments.

I try to remind myself at this point that the first reason I design is to convince myself about the possibilities of a positive outcome of research, but other observers are becoming increasingly important. A departmental presentation, for example, helps solidify my thoughts and generates ideas from others, but it is also a public performance that is harder to subsequently walk away from than a few more quickly forgotten conversations in private.

Third Category of Design Activities—Formalizing Design

As my thoughts mature, design now involves processes that most textbooks describe. Because these resources are widely available and can be chosen to fit your field

and interests, I will merely list four generic but critical activities: clarifying purpose and a plan of activities; choosing theory, methods, and resources to be used; anticipating evaluation of outcomes, including potentially negative consequences; carrying out a pilot project.

I. *Clarifying purpose and linking it to a sequence of activities.* This task is the heart of design, but it is not easy to know when a design is mature enough to proceed.[5] Formal publication often hides the difficulties involved. The common rhetorical structure of almost all articles and books describes purpose as the foundation for subsequent design. In fact, there are many entry points to research design. Choosing one will lead me in a different direction from other choices. It therefore makes sense to spend some time brainstorming different combinations of audience, subject, purpose, output, and the design alternatives they imply, as graphically interrelated in Figure 5.1.

I take at least several weeks (often much longer, given other responsibilities) before starting a substantial project to generate and evaluate these alternatives. My primary tool is an empty drawer that collects but keeps my efforts out of sight. I briefly outline and put in the drawer different possible projects, waiting until I have generated at least a dozen that significantly vary in terms of intended audience, subject, purpose, output, and design decisions. Then I examine the set, synthesize as inspired, and discuss the two or three choices that seem most feasible and enticing with others.[6]

The expected output of this organic activity is a mechanical, time-specific list of activities that are expected to culminate in a set of significant outputs. I have learned what is expected to be on this list by looking at research designs that led to journal awards and other honors, and suggest that you do the same in your area of inquiry. However, the mechanical list cannot be completed until I have cycled through three other activities.

J. *Choosing a theoretic framework, specifying methods to supplement current explanation, and gathering needed resources.* "Theory" is defined in this book as an abstract, generalized explanation that is independent of the thing being explained (as already described in Chapter 3). But what can be further explained depends upon available and understood methods. When in design mode, I draw ideas about theory and method together by using one of my two favorite research tools: electronic highlighting or paper Post-It Notes. Active reading shows me how theory and method can be united. I tag combinations I might use, and occasional ones I intend to avoid.

Thinking about methods at an early stage is important so that I don't lose data. Here is one frustrating example: I have often wasted time searching for a reference from a news report or an article in a journal I don't usually read. After moving away, I suddenly realize the idea could be a key piece in the puzzle I am struggling to assemble. Equally frustrating: more than once I have futilely wished that I were carrying a tape recorder when what I thought was a casual conversation turned to rich detail. I didn't think I had "started" my research, but useful data was at hand. (Take care, however, to follow human subject guidelines when responding to serendipitous opportunity. That's another resource that must be acquired as soon as practical.)

K. *Anticipating evaluation of outcomes and guarding against negative consequences.* Too often, evaluation is thought of as something that occurs after a project is completed. Design helps me think about evaluation before and during my activities rather than later, when insight is less useful. I try to sternly ask myself, "What is the best I can hope for from my own project?" Do those results justify the effort I am outlining? What about less auspicious (but more likely) results? Is there anything I can do to increase the impact of what I am planning? These questions suggest a mechanical comparison with the work of others, but a good answer almost always requires organic insight as well.

I have discovered in teaching and my own project planning that many scholars have a tendency to start too far behind the most interesting things they could achieve. My advice to myself and others is to avoid designing a "textbook" but boring project. Rather,

———❖–❖———

*Design for positive evaluation by emphasizing contributions
that would attract the attention of published authors.*

———❖–❖———

People resist this advice for two reasons, I think. First, many of us have a strong desire to be unique. Second, many of us (often the very same people who show up in group 1) feel it is presumptuous to identify with success. I want to remind both groups to remember that scholarship is not primarily about you or your trip to Hawaii; it is about what a broader community is interested in and needs to know.

Aiming toward positive evaluation does mean sharpening design. For example, it may be more useful to compare data from the ends of a continuum than to wade through data from the middle of a distribution. Finding a more engaging context may lead to more interesting insights. The overall point is that you and I must use our intuition, and ask for ideas from others, so that our research starts with theoretical ideas, methods, and a context for data collection that show promise even before evidence has been collected.

One additional point is also important. I recognize my ethical responsibilities, as briefly described in previous chapters, and know that design attention is required to avoid sins of omission as well as commission. The time to think about possible harm and potential gain is before research begins. Do informants need protection? What about the students who work for me? Are they learning the most they could from the experience? Have I thought about how I can learn from these participants and other research experience?

L. *Improving design via feasibility studies and pilot projects.* The discussion so far is too cerebral. My preference is for action. Table 5.1 lists a number of benefits to be gained from a small scale pilot study while still in the process of design. Some test the feasibility of research activities themselves; others explore the likelihood of support, from funding agencies or regulatory groups (like Human Subjects Committees), for example. Once

Table 5.1   Reasons for Conducting Pilot Studies

- Developing and testing adequacy of research instruments
- Assessing the feasibility of a (full-scale) study/survey
- Designing a research protocol
- Assessing whether the research protocol is realistic and workable
- Establishing whether the sampling frame and technique are effective
- Assessing the likely success of proposed recruitment approaches
- Identifying logistical problems that might occur using proposed methods
- Estimating variability in outcomes to help determine sample size
- Collecting preliminary data
- Determining what resources (finance, staff) are needed for a planned study
- Assessing the proposed data analysis techniques to uncover potential problems
- Developing a research question and research plan
- Training a researcher in as many elements of the research process as possible
- Convincing funding bodies that the research team is competent and knowledgeable
- Convincing funding bodies that the main study is feasible and worth funding
- Convincing other stakeholders that the main study is worth supporting.

SOURCE: Van Teijlingen, E. R., & Hundley, V. (Winter 2001). *The importance of pilot studies. Social research update.* Guildford, Surrey, UK: Department of Sociology, University of Surrey.

basic details are worked out, a pilot study can explore how much time a promising design will take, and help estimate the potential value of the results collected. Even a little experience, in other words, is likely to improve both the efficiency and the effectiveness of a research design, as suggested in Table 5.1.

The list in Table 5.1 readily supports a research design step worth underlining:

—❖–❖—

*Take the time for one or more feasibility studies or
pilot projects. The experience almost inevitably leads to
better design, and it is a sign of careful process that is likely
to reassure external evaluators.*

—❖–❖—

In a further summation, Figure 5.2 suggests that research design and testing relies on four activities that require organic insight, yet these interact to produce a public and more mechanical account of what the scholar is doing and why.
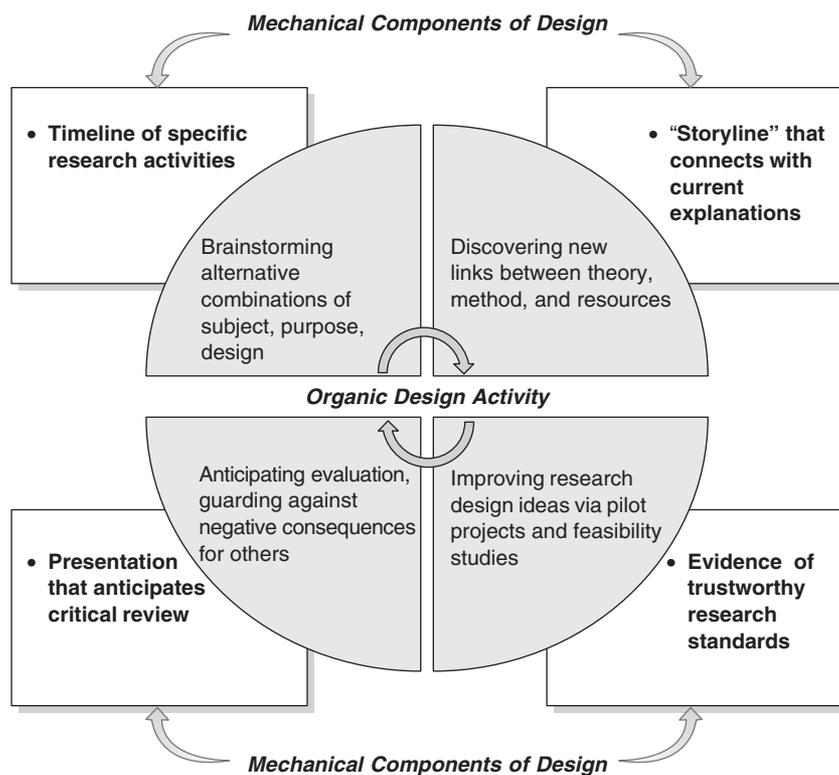
Figure 5.2   Organic Activities That Lead to Logical (Mechanical) Outputs

Fourth Category of Design Activities—Redesign

We all know that projects often do not progress as planned. While research design significantly increases the odds that a desired conclusion is reached, surprises are almost inevitable. Some of these outcomes can be turned into positive contributions to a research project, given an organic appreciation of opportunity. However, it is not a good idea to be a perpetual optimist. Two additional design activities are therefore important: initially anticipating the need for flexibility and actually taking advantage of this capacity when the demands of a project underway tend to promote tunnel vision.

M. *Including flexibility in design.* Good designs prepare for both welcome and unwelcome outcomes by being flexible. The specifics you can anticipate and plan for may not occur, but that is much less important than the more prepared state of mind generated by considering contingencies. It is hard to be specific, given the vast array of projects that interest scholars, but as an example, I've tried to have backups in mind in case my informants are unwilling or unable to remain involved in a project.

N. *Reconsidering activities and relationships among activities as required.* I've added new methods of analysis when my original ideas did not work out as planned. Additional insight has also been achieved by adding a colleague after a project was underway. I am sure you can imagine other things that might improve your project, but remember that you may not feel so flexible in the midst of a project. I have felt like a rabbit in the head-lights more than once: unable to choose a good direction when faced by confusing or disappointing results and a tight timeline for completion. In addition, remember that an audit trail is an important part of trustworthy research. If you do not document as you go along, you may find it hard to remember and document changes in design at the end of a project.

The process of redesign, which is almost inevitable for interesting/significant projects, shifts attention in various ways. Some alterations involve relatively minor additions to and subtractions from activities and do not change research objectives. More radical redesign requires going back to reconsider activities from early phases of the project. The bounce can be considerable, and may generate considerable doubt about purpose as well as activity.

My thoughts about the nature of redesign have been influenced by Henry Mintzberg, who made an important contribution to the field of strategic management when he pointed out that intended strategies metamorphose over time as organizations attempt to carry them out.[7] Organizations gradually discover what tends to work, and what is more difficult. That information and more reflection often alter what strategists then try to do. The overall result is that realized strategy (which I directly compare to research design) often deviates in direction from what was initially intended, as illustrated in Figure 5.3.

Before concluding this discussion of research design, I want to reassert that the steps in project development just described are relevant to projects that are externally defined. As acknowledged from the beginning of this book, scholars often find themselves responding to a topic area suggested by advisors, agreeing to work on a project defined by a potential coauthor, seeking funding on a topic announced by government agencies, and so on. Table 5.2 provides some ideas in each of the areas just discussed that might help you respond successfully to these potential opportunities.
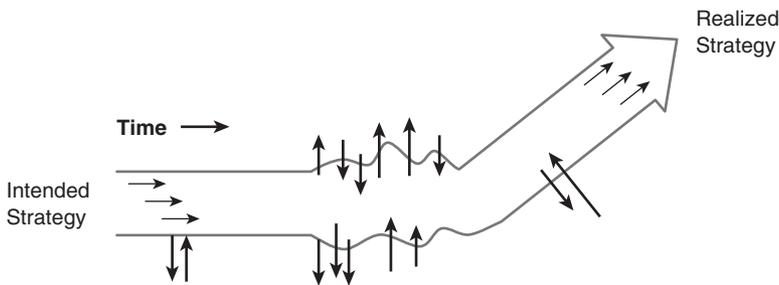
Figure 5.3   Research Direction Often Changes Over Time

SOURCE: Visualization based on Mintzberg, H., and Waters, J. A. (1985). Of strategies, deliberate and emergent. *Strategic Management Journal, 6,* 257–272.

Table 5.2   Additional Ideas for Developing Projects in a New But Externally Defined Domain

Initiating the Project		
A.	Testing vocabulary and short descriptions	Use vocabulary primarily from the domain given, but seek a few additions from your experience as well as other academic conversations that might be considered interesting/engaging, etc. by external evaluators.
B.	Seeking interesting/ interested conversations	Ask for advice from those familiar with the area and known within it, including advisers and evaluators, then add insights from your own experience.
C.	Assessing opportunity	Make adjustments in the assignment that increase the fit with your career interests, if possible (see Chapter 1).
Attempting to Be Logical		
D.	Connecting with specific literatures	Focus on identifying references within the designated arena, but continue to search for additional literature from which you might bring a few interesting (engaging) ideas, methods, etc. for project design.
E.	First "maps" of the phenomenon	Remember, "it's not about me/it's about you." Your definition of the project must communicate with scholars in the new conversational area.
F.	Early experimentation	Seek direct experience, if possible, to increase your understanding of a new area.
G.	Increasing clarity about my contribution	Find the will to "pull the plug" if you cannot find a convincing contribution to an externally designated project. However, it is a good idea to ask for advice before doing so. You cannot expect to be as insightful as those who are already working in your newly assigned area.
H.	Managing commitment	Create commitment as part of the project design process. Look at ideas in Chapter 1 and use your brainstorming ability. Academic life is a pleasure precisely because it offers a rich landscape of possibilities.
Formalizing Design		
I.	Clarifying purpose and linking it to a sequence of activities	Pay attention to biographical information about known evaluators, including information about projects they have advised. A delicate decision involves whether or not you cite work by these gatekeepers. On the one hand, being obvious can be interpreted negatively; on the other hand, if you do not have a positive response to previous research, are you in the right domain?

(Continued)

Table 5.2 (Continued)

J. Choosing a theoretical framework, specifying methods to supplement current explanation, and gathering needed resources	If necessary, arrange for additional training to carry out research activities that those in the area will find trustworthy. Even if you are relying on the expertise of coauthors and advisers, it is important to understand these requirements. Early investment and systematic attention are very likely to save time later in the project.
K. Anticipating evaluation of outcomes and guarding against negative consequences	Externally defined projects can be more successful than individually designed projects, because they typically require attention to outcomes from the outset. You must convince an external audience that you are a good risk, someone who is able to deliver valuable outcomes. Convince yourself as well.
L. Improving design via feasibility studies and pilot projects	Gain expertise as quickly as possible. More than one pilot may be important in an unfamiliar area to help you increase your authority.
Redesign	
M. Including flexibility in design	Establish your own calendar of due-dates ahead of imposed deadlines so that improvements can be made before evaluation.
N. Reconsidering activities and relationships among activities as required	Be flexible! It can be particularly hard to find solutions to problems or recognize opportunities to improve your project mid-stream when working on an externally defined project.

Workshop Question: *I'm unhappy with your advice to cite advisors and evaluators! It seems too obvious. Do I have to do that to increase my chances for success?*

I once found this practice completely at odds with my sherry-sipping view of academic life, even though it seemed to be common practice in many areas of inquiry. I now see it in a more nuanced way, though it still makes me pause.

On the one hand, it is easy to imagine that if I were in face-to-face conversation with an evaluator, I might naturally say, "You've been interested in subject X, and I am interested in something quite similar." Advisers and evaluators—people like dissertation committee members, journal editors and board members, and those responsible for advising funding bodies—should be experts in order to gain these

positions. If I don't recognize and admit to their expertise, then it is worth reexam-
ining the fit between my identity/career goals and the situation within which I find
myself chafing.

On the other hand, I have to admit that too many academics are egotistical to a fault
(and who among us can be sure we are free from this human disease?). I have suspected
that ego affected decisions that I wanted to turn my way, but did not. I have seen col-
leagues reward those who praise them and avoid choosing those who do not. The gener-
ous interpretation is that it was easiest to understand projects that were clearly framed in
the evaluator's territory. The often more realistic assessment is that academic endeavors,
like all others, are subject to power and self-interest.

You have context-specific knowledge about yourself and others, and have to decide
how to respond to this situation. However, I advise the following: (1) keep your work in
the general domain that interests you, (2) limit paranoia (because it is impossible to know
all the factors that affect evaluation), (3) anticipate that your own reputation will gradu-
ally limit the need and impulse for "political citation," and (4) remember at that point to
reassure those around you that you do not need gratuitous referencing! The overall point
is that scholars are professionals responsible for their actions.

❖   FUNDING AS A STIMULUS FOR GOOD DESIGN

Although Table 5.2 can be used to develop an application for funding, I want to give this
important aspect of academic life additional attention. As I became more adept at
research design and redesign, I discovered I was in a better position to seek external
funding for my work, and found that it definitely reinforces attention to the mechanical.
My rule is not to seek funding unless I anticipate that the process will have a positive
impact on my agenda, whether or not the application is accepted. Within these bound-
aries, I am now quite positive about making funding applications because the require-
ments of funding agencies can contribute to good design. I therefore recommend that
you, too, recast your research ideas as a proposal.

Funding organizations tend to emphasize several issues that you have probably con-
sidered, but perhaps not in sufficient depth. The translation only makes sense, however,
if you find a funding source with requirements that fit your needs as a designer, and the
requirements that you anticipate meeting for ultimate publication. You are likely to find a
surprising amount of commonality, as shown in Figure 5.4.

This figure deliberately broadens the overlap of a typical Venn diagram. As a test
of whether you can find this amount of overlap in your own area of inquiry, consider
the two criteria for funding used by the National Science Foundation (NSF).[8] NSF is an
important source of funding across the sciences in the United States and is representa-
tive of the many sources of support for academic work around the world that fund
research. The criteria they use are shown in Table 5.3.[9]
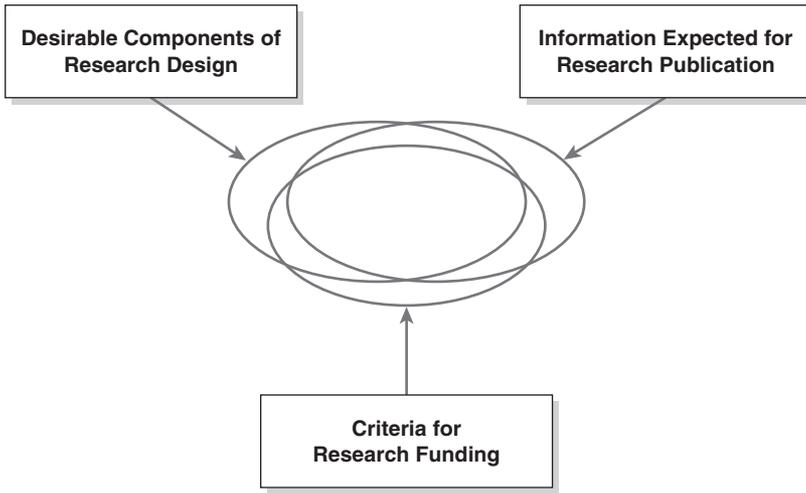
Figure 5.4   Overlapping Requirements for Design, Funding, and Publication

Table 5.3   Criteria for Funding From the National Science Foundation

1. What is the intellectual merit of the proposed activity?	• How important is the proposed activity to advancing knowledge and understanding within its own field or across different fields? • How well qualified is the proposer (individual or team) to conduct the project? (If appropriate, the reviewer will comment on the quality of prior work.) • To what extent does the proposed activity suggest and explore creative and original concepts? • How well conceived and organized is the proposed activity? Is there sufficient access to resources?
2. What are the broader impacts of the proposed activity?	• How well does the proposed activity advance discovery and understanding while promoting teaching, training, and learning? • How well does the proposed activity broaden the participation of underrepresented groups (e.g., gender, ethnicity, disability, geographic, etc.)? • To what extent will it enhance the infrastructure for research and education, such as facilities, instrumentation, networks, and partnerships? • Will the results be disseminated broadly to enhance scientific and technical understanding? • What will be the benefits of the proposed activity to society?

SOURCE: Reformatted from National Science Foundation. (2004). *Grant Proposal Guide* (NSF 04-23), p. 39. Effective September 1, 2004. Accessible at http://www.nsf.gov/publications/pub_summ.jsp?ods_key=gpg (last accessed March 30, 2007).

Obviously, you have to present a good idea to get NSF funding—an arresting propo-sition that makes it plausible your project will be chosen by well-qualified and critical reviewers from a large group of contenders. That comes from the organic side of design-ing, which I hope my alphabetical guide has helped you imagine. The unique require-ments for project proposals adds to this list, beginning with the request to be very explicit about resources.

O. *Explicitly listing available resources, including access to data.* It does not make sense to start a scholarly project without minimum resources, but an interesting test of your research design is to imagine the consequences of generous funding. Attention to the pos-sibilities of further funding encourages you to work at capacity, and consider expanding that capacity. Proposals also require that you combine new resources with what you already have available. Many people find overlooked assets in this review.

A particularly important resource, one that often figures in decisions for or against funding, involves access to data. Reassuring a funding source can be difficult for various reasons, including inquiries that require large amounts of data, confidential information, or socially sensitive reports. It is therefore important to arrange access to the data you hope for as soon as possible.

P. *Summarizing qualifications of personnel.* A too often neglected aspect of resource eval-uation and action involves your own qualifications. Careful thinking in anticipation of a funding review often suggests that a project would be enhanced by additional training or the inclusion of compensating skills from other people—surely it is worth considering these actions whether or not a funding application is actually made.

Q. *Relating current project to past work.* NSF provides funding opportunities for those early in their careers, so it may not be a problem if you have no previous work to report in support of your proposal. Enticed by additional resources, however, I will bet that you can write a few things about the experience that led you to the project you are now design-ing that can add to your authority.

Once you begin to establish a track record, you can be more loquacious about past experience and should be. You have to expect that some people in your audience will relate what you say now to what you have said in the past. Your job is to help them do that easily. My advice is to take time to paint the broader picture, even if you are making a sig-nificant departure from past work in a new project when you are preparing for job talks, promotion review, and other evaluations where this sensegiving can be very helpful.

R. *Considering opportunities for inclusion.* We know that it is our academic responsibility to attend to teaching and other conversation-enhancing activities, and that encouraging diversity is intrinsic to academic ideals. Furthering these goals is not always connected to research activities, yet it can be both efficient and effective. The time to consider the pos-sibilities is in the early stages of research design.

S. *Anticipating broad dissemination of research findings.* NSF insists that researchers think in detail about how their work will be disseminated—again, before it begins—including consideration of how society will benefit from the research. Once more, these are issues that are not considered early enough in most projects. Anticipating the outcome of review tends to improve research design.

T. *Supplying preliminary evidence.* Successful proposals typically convince reviewers with evidence. Considering how supporting evidence can be collected, and then doing it, is another potential benefit from writing a proposal.

Attending to all five of these concerns (resources, researcher qualifications, inclusion, dissemination, and supporting evidence) tends to be rather artificial as a proposal is first drafted. However, if funds are achieved, these promises have to be fulfilled. In the process, you will advance your own work, meet the needs of your institution, and support the academic enterprise as a whole. The accomplishment of these objectives also tends to increase your authority, which is a worthwhile though clearly more self-serving objective.

I've almost run through the alphabet of design suggestions, but Table 5.4 provides more detail about NSF proposal requirements, that adds a few last ideas. Once again, each of these requirements tend to have a positive effect on research design. My experience as a researcher and a facilitator of others' research convinces me that the brevity required on an NSF proposal is particularly important.

U. *Succinctly stating purpose and plan.* I agree with the writer who said, "I'm sorry to write you such a long letter. I didn't have time to write a short one."[10] Having produced pages and pages of text can lure hardworking scholars into thinking they are ready to begin a research project, but if the description cannot be reduced to a compelling short statement, the job is not yet done. Three headings carry that load in an NSF summary: objectives, intellectual merit, and broader impacts.

Note that these are expected to be repeated and elaborated in the 15-page project description that includes a "general plan of work." The details will depend upon your area of scholarship, and thus NSF provides no specific rules. For example, a timeline is not required, though I typically include one as a way of briefly specifying and relating "activities to be undertaken."

Methods have to be described, but the level of detail again is up to the proposal writer, who should consider the standards that reviewers from related fields are likely to apply. In many areas, discussion about methods and analysis is very important. As always, looking at similar successful projects is the best advice.

V. *Visualizing key arguments.* Visual materials are welcome in an NSF proposal, but only within the 15-page limit. I have worked with many people who thought their research design was enhanced by visual aids, perhaps generated for verbal presentations. When writing a proposal, they have the difficult task of deciding which of these graphics is central to their design. Often, that is like cutting the underbrush in an overgrown

Table 5.4   NSF Proposal Requirements

Project summary	The proposal must contain a summary of the proposed activity suitable for publication, not more than one page in length. It should not be an abstract of the proposal, but rather a self-contained description of the activity that would result if the proposal were funded. The proposal should be written in the third person and include a statement of objectives and methods to be employed. It must clearly address [the following] in separate statements (within the one-page summary) (1) the intellectual merit of the proposed activity; and (2) the broader impacts resulting from the proposed activity . . . [as described in the criteria above]. It should be informative to other persons working in the same or related fields and, insofar as possible, understandable to a scientifically or technically literate lay reader.
Project description	The Project Description should provide a clear statement of the work to be undertaken and must include [the following]: objectives for the period of the proposed work and expected significance; relations to longer-term goals of the PI's [project investigator's] project; and relation to the present state of knowledge in the field. The Project Description should outline • [T]he general plan of work, including the broad design of activities to be undertaken . . . • [W]here appropriate, provide a clear description of experimental methods and procedures . . . • [P]lans for preservation, documentation, and sharing of data, samples, physical collections, curriculum materials . . . • [O]ther related research and educational products . . . • [And] must describe, as an integral part of the narrative, the broader impacts resulting from the proposed activities, addressing [points listed under Criteria 2 in Table 5.3].
Page limitations	Brevity will assist reviewers and the foundation staff in dealing effectively with proposals. Therefore, the Project Description may not exceed 15 pages. Visual materials, including charts, graphs, maps, photographs, and other pictorial presentations are included in the 15-page limitation.

SOURCE: National Science Foundation (n.d.), http://www.nsf.gov/policies/reuse.jsp (accessed December 10, 2007; bullets added to the original).

garden—it is not possible to see the shape of the whole until serious pruning takes place. (If shape does not emerge, at least the ground is ready for more systematic planting.)

The preceding points suggest why I often ask students to write a funding proposal when reporting on their research design. I hope you find it fits your own research needs, and that you will carry out the following exercise.

EXERCISE 13

Prepare a Funding Request

1. Identify a funding program that might support your research, paying particular attention to opportunities that fit your subject and your demographic characteristics.

2. If possible, collect several examples of proposals funded by your targeted source(s) from the research office of your university, from your colleagues, or from the organization itself.

3. Prepare a description of the research you are now designing using the format required.

4. With the help of others, evaluate your chances for success.

The criteria of the agency are the primary guidelines for evaluation in this exercise, but also make your own assessment of how satisfactory your document is as an overall guide for putting your project into action. In addition, consider if the categories you used to attract funding are similar to those typically covered by publications in your field. If not, the funding agency you chose may not be right for you.

❖   THE VALUE OF PLAIN LANGUAGE

Words are the primary vehicle of research design, and one of the realities of scholarly work is how much time is spent rethinking and rewriting. Attention to language becomes particularly important as your design changes and the overarching sense of the research project has to be reestablished. This topic deserves book-length treatment and quite a few such books are available. I have recently discovered succinct but helpful advice from Martin Cutts, director of the Plain Language Commission in the United Kingdom. He cofounded the "Plain English Campaign" over 25 years ago by shredding government documents in Parliament Square. It was a high-profile and worthy action that had a positive impact on government writing around the world, though more could obviously be accomplished. His book, *The Oxford Guide to Plain English*, provides details of the plain language agenda.[11] The AskOxford website offers this summary:

Plain English refers to . . . the writing and setting out of essential information in a way that gives a co-operative, motivated person a good chance of understanding the document at first reading, and in the same sense that the writer meant it to be understood. This means pitching the language at a level of sophistication that suits the readers and using appropriate structure and layout to help them navigate

through the document. It does not mean always using simple words at the expense of the most accurate words or writing whole documents in kindergarten language.[12]

The tips offered include many that are relevant to designers. I have clustered eight into three categories worth separate passes through your design document.

W. *Focusing attention on key issues.*

- Organize your material in a way that helps readers. . . . *grasp* the important information early and . . . *navigate* through the document easily.
- Use vertical lists to break up complicated text.

X. *Simplifying language.*

- Over the whole document, make average sentence length 15 to 20 words.[13]
- Use only as many words as you really need.
- Avoid fusty [stale, dated] first sentences and formula finishes.
- Put accurate punctuation at the heart of your writing.

Y. *"Activating" the message.*

- Prefer the active voice unless there's a good reason for using the passive.
- Use the clearest, crispest, liveliest verb to express your thoughts.[14]

As always, these suggestions are relatively easy to say and much more difficult to carry out. Those who make significant progress have a better chance of thinking through their own design needs, obtaining external funding, and ultimately achieving publication. That is a worthy agenda for a last exercise.

EXERCISE 14

Clarify Your Design

1. Systematically focus, simplify, and use active voice to improve your research proposal.

2. Ask others for advice for further improving this summary.

3. If you and others feel your design has improved, make a list of the plain language guidelines you need to pay most attention to and post it over your computer.

4. Continue to refer to and update your research design as your scholarly work progresses, paying particular attention to whether these decisions affect the overall clarity of your design.

I hope that you have the energy to carry out this effort. It is more about good thinking than about good writing. Too often, I feel that my procedures are crystal clear, but find that others do not have the same feeling. Closing the gap can be a real struggle, but makes improvements that I did not realize I needed.

❖   CONCLUSION

This chapter began with a metaphor linking small plane crashes to early rejection of a manuscript sent for publication. I believe there is a lot you can do to avoid a crash, and hope that the next chapters addressing important aspects of research design will be especially helpful. However, while there is a great deal you can do to merit review, it is not possible to assure publication or other outcomes that academics desire. Reviewers and editors are rarely quixotic, in my experience, though occasionally they seem so. Rather, editors have to think about important issues that authors cannot know beforehand, including the balance among articles that will contribute to overall conversation. It is not worth trying to second-guess issues that you do not have the information to predict.

This is better advice:

—❖❖—

Do not forget what excites you! Too much attention to design details can gradually obscure initial motivation.

—❖❖—

In short, a primary reward of scholarship is enjoying the process. I will resist labeling that observation with the letter Z.

❖   NOTES

1.   Herbert Alexander Simon won the 1978 Nobel Prize in economics. His 52-year interdisciplinary career at Carnegie Mellon is well worth emulating, even by those of us who recognize we will not achieve the same public regard. Discussion of explanatory and design sciences can be found in Simon, H. (1996). *The sciences of the artificial*, 3rd Ed. Cambridge, MA: MIT Press (first published in 1969).

2.   My observations on design are influenced by "Management as a design science, mindful of art and surprise: A conversation between Anne Huff, David Tranfield, and Joan van Aken" in *Journal of Management Inquiry, 15*(4), 413–424.

3.   Campbell, J. P., Daft, R. L., & Hulin, C. L. (1982). *What to study: Generating and developing research questions* (p. 110). Beverly Hills, CA: Sage Publications.

4. Ibid.

5. Simon, H. (1982). Theories of bounded rationality. In H. Simon (Ed.), *Models of bounded rationality. Behavioral economics and business organization* (Vol. 2, pp. 408–423). Cambridge, MA: MIT Press.

6. More advice along these lines can be found in Huff, A. (1998). *Writing for scholarly publication.* Thousand Oaks, CA: Sage Publications.

7. Mintzberg, H., & Waters, J. A. (1985). Of strategies, deliberate and emergent. *Strategic Management Journal, 6,* 257–272.

8. Chapter 12 discusses the importance of asking for permission when graphic material or long passages are cited from other sources. It also suggests "creative commons" arrangements. One example can be found on the NSF website, which says, "Most text appearing on NSF web pages was either prepared by employees of the United States Government as part of their official duties and therefore not subject to copyright or prepared under contracts that gave the Foundation the right to place the text into the public domain. The same is true of most publications available for downloading from this web site. You may freely copy that material and, at your discretion, credit NSF with a 'Courtesy: National Science Foundation' notation." I am glad to provide that reference. The quote is from http://www.nsf.gov/policies/reuse.jsp, accessed December 10, 2007.

9. Ibid.

10. I had thought George Bernard Shaw was the author of this judicious observation about the inverse relationship between time and length of communication. A little research on the Internet shows that it is often attributed to Mark Twain, but also to Pascal, Voltaire, Proust, and others! Search on Google Scholar did yield a delightful paper that advocates taking the time required for scholarship: "Quick, Quick, Slow! The case for Slow Research," by Ray Poynter and Quentin Ashby (n.d.). Available at http://www.ukopinion.com/news/papers/paper_20.asp (accessed March 11, 2008).

11. Cutts, M. (2004). *Oxford guide to plain English,* 2nd Ed. Oxford, UK: Oxford University Press.

12. See http://www.askoxford.com/betterwriting/plainenglish (last accessed March 30, 2007).

13. My advice is to use many sentences that are shorter than 10–15 words to energize your message.

14. Bulleted information from http://www.askoxford.com/betterwriting/plainenglish (last accessed March 30, 2007).