1 What Research Is and How Researchers Think about It

You do research every time you ask a question and look for facts to answer it, whether the question is as simple as finding a plumber or as profound as discovering the origin of life. When only you care about the answer or when others need just a quick report of it, you probably won't write it out. But you must report your research in writing when others will accept your claims only after they study how you reached them. In fact, reports of research tell us most of what we can reliably believe about our world—that once there were dinosaurs, that germs cause disease, even that the earth is round.

You may think your report will add little to the world's knowledge. Maybe so. But done well, it will add a lot to yours and to your ability to do the next report. You may also think that your future lies not in scholarly research but in business or a profession. But research is as important outside the academy as in, and in most ways it is the same. So as you practice the craft of academic research now, you prepare yourself to do research that one day will be important at least to those you work with, perhaps to us all.

As you learn to do your own research, you also learn to use—and judge—that of others. In every profession, researchers must read and evaluate reports before they make a decision, a job you'll do better only after you've learned how others will judge yours. This book focuses on research in the academic world, but every day we read or hear about research that can affect our lives. Before we believe those reports, though,

we must think about them critically to determine whether they are based on evidence and reasoning that we can trust.

To be sure, we can *reach* good conclusions in ways other than through reasons and evidence: we can rely on tradition and authority or on intuition, spiritual insight, even on our most visceral emotions. But when we try to *explain* to others not just why we believe our claims but why they should too, we must do more than just state an opinion and describe our feelings.

That is how a *research* report differs from other kinds of persuasive writing: it must rest on shared facts that readers accept as truths independent of your feelings and beliefs. They must be able to follow your reasoning from evidence that they accept to the claim you draw from it. Your success as a researcher thus depends not just on how well you gather and analyze data but on how clearly you report your reasoning so that your readers can test and judge it before making your claims part of their knowledge and understanding.

1.1 How Researchers Think about Their Aims

All researchers gather facts and information, what we're calling *data*. But depending on their aims and experience, they use those data in different ways. Some researchers gather data on a topic—*stories about the Battle of the Alamo*, for example—just to satisfy a personal interest (or a teacher's assignment).

Most researchers, however, want us to know more than just facts. So they don't look for just any data on a topic; they look for specific data that they can use as evidence to test and support an answer to a question that their topic inspired them to ask, such as *why has the Alamo story become a national legend?*

Experienced researchers, however, know that they must do more than convince us that their answer is sound. They must also show us why their question was worth asking, how its answer helps us understand some bigger issue in a new way. *If we can figure out why the Alamo story has become a national legend, we might then answer a larger question: how have regional myths shaped our national character?*

You can judge how closely your thinking tracks that of an experienced researcher by describing your project in a sentence like this:

1. I am working on the topic X (*stories about the Battle of the Alamo*)
2. because I want to find out Y (*why its story became a national legend*)
3. so that I can help others understand Z (*how such regional myths have shaped our national character*).

That sentence is worth a close look, because it describes not just the progress of your research but your personal growth as a researcher.

1. "I am working on the topic . . ." Researchers often begin with a simple topic like *the Battle of the Alamo*, perhaps because it was assigned, because something about it puzzles them, or because it merely sparks an interest. But inexperienced researchers too often stop there, leaving themselves with nothing but a topic to guide their work. They mound up hundreds of notes but have no way to decide what data to keep and what to discard. When it comes time to write, they dump everything into a report that reads like a grab bag of random facts. If those facts are new to readers who happen to be interested in the topic, they might read the report. But even those readers will want to know *what those facts add up to.*
2. ". . . because I want to find out how or why . . ." More experienced researchers usually begin not with just a topic but with a research question, such as *Why has the story of the Alamo become a national legend?* And they know that readers will think their facts add up to something only when those facts serve as evidence to support its answer. Indeed, only with a question can a researcher know which facts to look for and which to keep—not just those that support an answer but also those that test or even discredit it. When he thinks he has enough evidence to support his answer and can respond to data that seem to contradict it, he writes a report first to test his own thinking, then to share his answer with others so that they can test it too.
3. ". . . so that I can help others understand . . ." The most successful researchers, however, realize that readers want to know not only that an answer is sound but why the question was worth asking. So they anticipate that readers will ask a question of their own: *So what? Why should I care why the Alamo story has become a legend?* That *So what?* can vex even the most experienced researcher, but every researcher must try to answer it before it's asked: *If we can find that out, we might better understand the bigger question of how such stories shape our national character.*

But a shrewd researcher doesn't stop there. She anticipates her readers' asking *So what?* again by looking for another, still larger answer: *And if we can understand what has shaped our national character, we might understand better who we Americans think we are. And before you ask, when we know that, we might better understand why others in the world judge us as they do.* The most successful researchers know that readers care about a

question only when they think that its answer might encourage them to say not *So what?* but *That's worth knowing!*

In short, not all questions are equally good. We might ask how many cats slept in the Alamo the night before the battle, but so what if we find out? It is hard to see how an answer would help us think about any larger issue worth understanding, so it's a question that's probably not worth asking (though as we'll see, we could be wrong about that).

1.2 Three Kinds of Questions That Researchers Ask

Experienced researchers also know that different readers expect them to ask and answer different kinds of questions. The most common questions in academic work are *conceptual*. The ones most common in the professions are *practical*.

1.2.1 Conceptual Questions: *What Should We Think?*

A question is conceptual when your answer to *So what?* doesn't tell readers what to *do* but helps them *understand* some issue:

1. I am working on the topic X
   2. because I want to find out how/why/whether Y (*So what if you do?*)
      3. so that I can help others *understand* how/why/whether Z.

If you were explaining your research, the conversation might go like this:

I'm working on the topic of risk evaluation.

Why?

Because I want to find out how ordinary people evaluate the risk that they will be hurt by terrorism.

So what if you do?

Once I do, we might better understand the bigger question of how emotional and rational factors interact to influence the way ordinary thinkers think about risk.

Researchers in the humanities and the social and natural sciences work mostly on conceptual questions, such as *How did Shakespeare's political environment influence his plays? What caused the extinction of most large North American mammals? What are comets made of?* The answers to those questions don't tell us how to change the world, but they do help us understand it better.

To be sure, the answer to a conceptual question often turns out to be unexpectedly relevant to solving a practical problem. And before we can solve any important practical problem, we usually must do con-

ceptual research to understand it better. But in most of the academic world, the primary aim of most researchers is only to improve our understanding.

1.2.2 Practical Questions: *What Should We Do?*

You pose a different kind of question—call it a *practical* one—when your answer to *So what?* tells readers what to *do* to change or fix some troublesome or at least improvable situation:

1. I am working on the topic X
2. because I want to find out Y (*So what if you do?*)
3. so that I can tell readers *what to do* to fix/improve Z.

You would explain your work on a practical question like this:

I'm working on the topic of communicating risk effectively.

Why?

Because I want to find out what psychological factors cause ordinary Americans to exaggerate their personal risk from a terrorist attack.

So what if you do?

Then I can tell the government how to counteract those factors when they communicate with the public about the real risk of terrorism.

Practical questions are most common outside the academic world, especially in business. In academic fields such as health care and engineering, researchers sometimes ask practical questions, but more often they ask a third kind of question that's neither purely practical nor purely conceptual: call it an *applied research* question.

1.2.3 Applied Questions: *What Must We Understand Before We Know What to Do?*

Often we know we must do *something* to solve a practical problem, but before we can know what that is, we must do research to understand the problem better. We can call that kind of research *applied*. With this middle kind of question, the third step raises a question whose answer is not the solution to a practical problem but only a step toward it.

I want to find out how Americans have changed their daily lives in response to the terrorist attacks on 9/11.

So what if you do?

Then we can understand the psychological factors that cause ordinary Americans to exaggerate their personal risk from a terrorist attack.

So what if you do?

Then we can understand how to reduce the effects of those psychological factors.

So what if you do?

Then perhaps the government can use that information to communicate more effectively the real risk from terrorism.

Applied questions are common in academic fields such as business, engineering, and medicine and in companies and government agencies that do research to understand what must be known before they can solve a problem.

1.2.4 Choosing the Right Kind of Question

Some new researchers dislike purely conceptual research questions because they think they're too "theoretical" or irrelevant to the "real" world. So they try to cobble an implausible practical use onto a conceptual answer: *When we know how race shaped the political impact of the Alamo stories, we can understand how racism has been used to foster patriotism and thereby eliminate racist appeals to patriotism in relation to conflicts in the Middle East.*

That impulse is understandable. But unless you've been assigned an applied or practical problem, resist it. You are unlikely to solve any significant practical problem in a class paper, and in any case, most of the academic world sees its mission not as fixing the problems of the world directly but as understanding them better (which may or may not help fix them).

1.2.5 The Special Challenge of Conceptual Questions: Answering *So What?*

With most practical questions, we don't have to answer *So what?* because the benefit is usually obvious. Even most applied questions imply the practical benefits of their answers: few readers would question why a researcher is trying to understand what causes Alzheimer's. With conceptual questions, however, the answer to *So what?* is often not obvious at all, even to an experienced researcher: *So what if Shakespeare had Lady Macbeth die offstage rather than on? So what if some cultures use masks in their religious rituals and others don't? Why is it important to know that?*

For a research paper in an introductory course, your instructor may be satisfied with any plausible answer to *So what?* So if early in your research career you find yourself struggling with that question, don't take it as a sign of failure, much less as evidence that you're not ready to do the work. In fact, you might not discover the answer to *So what?* until

you've drafted your report, maybe not even until you've finished it. And even then, maybe the answer will matter only to you.

But if your project is a thesis or dissertation, it's not just an advisor that you have to satisfy. Your answer must also satisfy those in your field (represented by your advisor), who will judge your work not just by the quality of your answer but by the significance of your question. Experienced researchers know that some readers, perhaps many, will read their report and think, *I don't agree.* They accept that as an inevitable part of sharing research on significant issues. What they can't accept is *I don't care.*

So as hard as it will be, the more often you imagine others asking *So what?* and the more often you try to answer it, if only to your own satisfaction, the more confident you can be that eventually you'll learn to succeed at every experienced researcher's toughest task—to convince your readers that your report is worth their time. (In chapter 10 we discuss how to write an introduction that motivates your readers at least to start reading your report.)

2 Moving from a Topic to a Question to a Working Hypothesis

A research project is more than collecting data. You start it before you log on to the Internet or head for the library, and you continue it long after you have all the data you think you need. In that process, you face countless specific tasks, but they all aim at just five general goals. You must do the following:

- Ask a question worth answering.
- Find an answer that you can support with good reasons.
- Find reliable evidence to support your reasons.
- Draft a report that makes a good case for your answer.
- Revise that draft until readers will think you met the first four goals.

You might even post those five goals in your workspace.

Research projects would be easy if you could march straight through those steps. But as you've discovered (or soon will), research and its reporting are never straightforward. As you do one task, you'll have to look

ahead to others or revisit an earlier one. You'll change topics as you read, search for more data as you draft, perhaps even discover a new question as you revise. Research is looping, messy, and unpredictable. But it's manageable if you have a plan, even when you know you'll depart from it.

2.1 Find a Question in Your Topic

Researchers begin projects in different ways. Many experienced ones begin with a question that others in their field want to answer: *What caused the extinction of most large North American mammals?* Others begin with just a vague intellectual itch that they have to scratch. They might not know what puzzles them about giant sloths and mastodons, but they're willing to spend time finding out whether they can translate their itch into a question worth answering.

They know, moreover, that the best research question is not one whose answer others want to know just for its own sake; it is one that helps them understand some larger issue (*So what?* again). For example, if we knew why North American sloths disappeared, we might be able to answer a larger question that puzzles many historical anthropologists: *Did early Native Americans live in harmony with nature, as some believe, or did they hunt its largest creatures to extinction? (And if we knew that, then we might also understand . . .)*

Then there are those questions that just pop into a researcher's mind with no hint of where they'll lead, sometimes about matters so seemingly trivial that only the researcher thinks they're worth answering: *Why does a coffee spill dry up in the form of a ring?* Such a question might lead nowhere, but you can't know that until you see its answer. In fact, the scientist puzzled by coffee rings made discoveries about the behavior of fluids that others in his field thought important—and that paint manufacturers found valuable. So who knows where you might go with a question like *How many cats slept in the Alamo the night before the battle?* You can't know until you get there.

In fact, a researcher's most valuable ability is the knack of being puzzled by ordinary things: like the shape of coffee rings; or why Shakespeare has Lady Macbeth die offstage rather than on; or why your eyebrows don't grow as long as the hair on your head. Cultivate the ability to see what's odd in the commonplace and you'll never lack for research projects, as either a student or a professional.

If you have a topic, skip to 2.1.3 to find questions in it. If you already have a question or two, skip to 2.1.4 to test them by the criteria listed there. If you're still looking for a topic, here's a plan to help you search for one.

2.1.1 Search Your Interests

If you can pick any topic appropriate to your field, ask these questions:

- What topics do you already know something about? You can learn more.
- What would you like to know more about? A place? A person? A time? An object? An idea? A process?
- Can you find a discussion list on the Internet about issues that interest you?
- Have you taken positions on any issues in your field in debates with others but found that you couldn't back up your views with good reasons and evidence?
- What issues do people outside your field misunderstand?
- What topic is your instructor working on? Would she like you to explore a part of it? Don't be too shy to ask.
- Does your library have rich resources in some field? Ask your instructor or a librarian.
- What other courses will you take in your field or out of it? Find a textbook and skim it for study questions.
- If you have a job in mind, what kind of research report might help you get it? Employers often ask for samples of an applicant's work.

You can also consult print sources for ideas:

- Skim the topics in specialized indexes in your field such as *Philosopher's Index*, *Geographical Abstracts*, *Women's Studies Abstracts*, and so on (in the bibliography, see items in category 2 in your field).
- Skim a journal that reviews the year's work in your field (in the bibliography, see items in category 2 in your field).

Academic research is meant to be shared, but the understanding it brings may also be valuable to you in the future. So think ahead: look for a project that might help you a year from now. Keep in mind, though, that you may be in for a long relationship with your topic, so be sure it interests you enough to get you through the inevitable rocky stretches.

2.1.2 Make Your Topic Manageable

If you pick a topic whose name sounds like an encyclopedia entry—*bridges*, *birds*, *masks*—you'll find so many sources that you could spend a lifetime reading them. You must carve out of your topic a manageable piece. You can start before you head to the library by limiting your topic to reflect a special interest in it: What is it about, say, masks that made you choose them? What particular aspect of them interests or puzzles you? Think about your topic in a context that you know something about, then add words and phrases to reflect that knowledge:

masks in religious ceremonies
masks as symbols in Hopi religious ceremonies
mudhead masks as symbols of sky spirits in Hopi fertility ceremonies

You might not be able to focus your topic until after you start reading about it. That takes time, so start early (you can do much of this preliminary work online):

- Begin with an overview of your topic in a general encyclopedia (in the bibliography, see items in category 2 in the general sources); then read about it in a specialized one (see items in category 2 in your field).
- Skim a survey of your topic (encyclopedia entries usually cite a few).
- Skim subheads under your topic in an annual bibliography in your field (in the bibliography, see items in category 4 in your field). That will also give you a start on a reading list.
- Search the Internet for the topic (but evaluate the reliability of what you find; see 3.4.3).

Especially useful are topics that spark debate: *Fisher claims that Halloween masks reveal children's archetypal fears, but do they?* Even if you can't resolve the debate, you can learn how such debates are conducted (for more on this, see 3.1.2).

2.1.3 Question Your Topic

Do this not just once, early on, but throughout your project. Ask questions as you read, especially *how* and *why* (see also 4.1.1–4.1.2). Try the following kinds of questions (the categories are loose and overlap, so don't worry about keeping them distinct).

1. Ask how the topic fits into a larger context (historical, social, cultural, geographic, functional, economic, and so on):
   - How does your topic fit into a larger story? *What came before masks? How did masks come into being? Why? What changes have they caused in other parts of their social or geographic setting? How and why did that happen? Why have masks become a part of Halloween? How and why have masks helped make Halloween the biggest American holiday after Christmas?*
   - How is your topic a functioning part of a larger system? *How do masks reflect the values of specific societies and cultures? What roles do masks play in Hopi dances? In scary movies? In masquerade parties? For what purposes are masks used other than disguise? How has the booming market for kachina masks influenced traditional designs?*
   - How does your topic compare to and contrast with other topics like it? *How do masks in Native American ceremonies differ from those in Africa?*

What do Halloween masks have to do with Mardi Gras masks? How are masks and cosmetic surgery alike?

2. Ask questions about the nature of the thing itself, as an independent entity:
   - How has your topic changed through time? Why? What is its future? *How have Halloween masks changed? Why? How have Native American masks changed? Why?*
   - How do the parts of your topic fit together as a system? *What parts of a mask are most significant in Hopi ceremonies? Why? Why do some masks cover only the eyes? Why do so few masks cover just the bottom half of the face?*
   - How many different categories of your topic are there? *What are the different kinds of Halloween masks? What are the different qualities of masks? What are the different functions of Halloween masks?*

3. Turn positive questions into a negative ones: *Why have masks not become a part of Christmas? How do Native American masks not differ from those in Africa? What parts of masks are typically not significant in religious ceremonies?*

4. Ask speculative questions: *Why are masks common in African religions but not in Western ones? Why are children more comfortable wearing Halloween masks than are most adults? Why don't hunters in camouflage wear masks?*

5. Ask *What if?* questions: how would things be different if your topic never existed, disappeared, or were put into a new context? *What if no one ever wore masks except for safety reasons? What if everyone wore masks in public? What if movies and TV were like Greek plays and all the actors wore masks? What if it were customary to wear masks on blind dates? In marriage ceremonies? At funerals?*

6. Ask questions that reflect disagreements with a source: if a source makes a claim you think is weakly supported or even wrong, make that disagreement a question (see also 4.1.2). *Martinez claims that carnival masks uniquely allow wearers to escape social norms. But I think religious masks also allow wearers to escape from the material realm to the spiritual. Is there a larger pattern of all masks creating a sense of alternative forms of social or spiritual life?*

7. Ask questions that build on agreement: if a source offers a claim you think is persuasive, ask questions that extend its reach (see also 4.1.1). *Elias shows that masked balls became popular in eighteenth-century London*

in response to anxiety about social mobility. Is the same anxiety responsible for similar developments in other European capitals? You can also ask a question that supports the same claim with additional evidence. *Elias supports his claim about masked balls entirely with published sources. Is it also supported by evidence from unpublished sources such as letters and diaries?*

8. Ask questions analogous to those that others have asked about similar topics. *Smith analyzed the Battle of Gettysburg from an economic point of view. What would an economic analysis of the Battle of the Alamo turn up?*

9. Look for questions that other researchers pose but don't answer. Many journal articles end with a paragraph or two about open questions, ideas for more research, and so on. You might not be able to do all the research they suggest, but you might carve out a piece of it.

10. Find an Internet discussion list on your topic, then "lurk," just reading the exchanges to understand the kinds of questions those on the list discuss. If you can't find a list using a search engine, ask a teacher or visit the website of professional organizations in your field. Look for questions that spark your interest. You can even ask a question of the list, so long as it is very specific and narrowly focused, but wait until you see whether questions from students are welcomed.

2.1.4 Evaluate Your Questions

Not all answers are equally useful, so evaluate your questions and scrap those that are unlikely to yield interesting answers. Reconsider when the following is true.

1. You can answer the question too easily.
   - You can look it up: *What masks are used in Navajo dances?*
   - You can summarize a source: *What does Fisher say about masks and fears?*

2. You can't find good evidence to support the answer.
   - No relevant facts exist: *Are Mayan masks modeled on space aliens?*
   - The question is based on preference or taste: *Are Balinese or Mayan masks more beautiful?*
   - You must read too many sources: *How are masks made?* You don't want to plow through countless reports to find the best evidence (this usually results from a question that's too broad).
   - You can't get the sources that your readers think are crucial. In even moderately advanced projects, you'll be expected to work with the best sources available; for a thesis and dissertation, they're essential. If you can't obtain those sources, find another question.

3. You can't plausibly disprove the answer.
   - The answer seems self-evident because the evidence overwhelmingly favors one answer. *How important are masks in Inuit culture?* The answer is obvious: *Very.* If you can't imagine disproving a claim, then proving it is pointless. (On the other hand, world-class reputations have been won by those who questioned a claim that seemed self-evidently true—for instance, that the sun circled the earth—and dared to disprove it.)

Don't reject a question because you think someone must already have asked it. Until you know, pursue its answer as if you asked first. Even if someone has answered it, you might come up with a better answer or at least one with a new slant. In fact, in the humanities and social sciences the best questions usually have more than one good answer. You can also organize your project around comparing and contrasting competing answers and supporting the best one (see 6.2.5).

The point is to find a question that you want to answer. Too many students, both graduate and undergraduate, think that the aim of education is to memorize settled answers to someone else's questions. It is not. It is to learn to find your own answers to your own questions. To do that, you must learn to wonder about things, to let them puzzle you—particularly things that seem most commonplace.

2.2 Propose Some Working Answers

Before you get deep into your project, try one more step. It is one that some beginners resist but that experienced researchers usually attempt. Once you have a question, imagine some plausible answers, no matter how sketchy or speculative. At this stage, don't worry whether they're right. That comes later.

For example, suppose you ask, *Why do some religions use masks in ceremonies while others don't?* You might speculate,

> Maybe cultures with many spirits need masks to distinguish them.
> Maybe masks are common in cultures that mix religion and medicine.
> Maybe religions originating in the Middle East were influenced by the Jewish prohibition against idolatry.

Even a general answer can suggest something worth studying:

> Maybe it has to do with the role of masks in nonreligious areas of a culture.

Try to imagine at least one plausible answer, no matter how tentative or speculative. If after lots of research you can't confirm it, you can organize your report around why that answer seemed reasonable at the time but turned out to be wrong and so isn't worth the time of other research-

ers. That in itself can be a valuable contribution to the conversation on your topic. (See 10.1.1–10.1.2 for how to use an apparently good idea that turns out to be wrong.)

In fact, look for two or three plausible answers. Even if you prefer one, you can improve it by testing it against the others, and in any event, you can't show that an answer is right if you can't also show why others are wrong. Even early in the project, write out your answers as clearly and as fully as you can. It is too easy to think that you have a clear idea when you don't. Putting a foggy idea into words is the best way to clarify it, or to discover that you can't.

2.2.1 Decide on a Working Hypothesis

If one answer seems promising, call it your *working hypothesis* and use it to guide your research. You can, of course, look for evidence with no more than a question to guide you, because any question limits the number of plausible answers. But even the most tentative working hypothesis helps you to think ahead, especially about the *kind* of evidence that you'll need to support it. Will you need numbers? quotations? observations? images? historical facts? More important, what kind of evidence would *disprove* your hypothesis? Answer those questions and you know the kind of data to watch for and to keep. In fact, until you have a hypothesis, you can't know whether any data you collect are relevant to any question worth asking.

If you can't imagine any working hypothesis, reconsider your question. Review your list of exploratory questions to find one that you can answer; if you skipped that step, go back to 2.1.3. You may even decide to start over with a new topic. That costs time in the short run, but it may save you from a failed project. If you're working on a thesis or dissertation, you can wait longer to firm up a hypothesis while you read and ponder, but don't get deeply into your project without at least the glimmer of a possible answer.

Under no circumstances should you put off thinking about a hypothesis until you begin drafting your report or, worse, until you've almost finished it. You might not settle on the best answer to your question until you've written your last page: writing, even revising, is itself an act of discovery. Just don't wait until that last page to start thinking about *some* answer.

2.2.2 Beware the Risks in a Working Hypothesis

It is a bad idea to settle on a final answer too soon. But many new researchers and some experienced ones are afraid to consider *any* working hypothesis early in their project, even one they hold lightly, because they fear it might bias their thinking. There is some risk of that, but a work-

ing hypothesis need not close your mind to a better one. Even the most objective scientist devises an experiment to test for just a few predicted outcomes, often just one. In fact, researchers who don't state a hypothesis usually have one in mind but don't want to seem publicly committed to it, lest it turn out wrong.

A working hypothesis is a risk only if it blinds you to a better one or if you can't give it up when the evidence says you should. So as in all relationships, don't fall too hard for your first hypothesis: the more you like it, the less easily you'll see its flaws. Despite that risk, it's better to start with a flawed hypothesis than with none at all.

2.2.3 If You Can't Find an Answer, Argue for Your Question

We have focused on questions so much that you might think that your project fails if you can't answer yours. In fact, much important research explains why a question no one has asked should be, even though the researcher can't answer it: *Do turtles dream? Why is yawning contagious but being sleepy isn't? Or is it?* Such reports focus on why the question is important and what a good answer might look like. Or you may find that someone has answered your question, but incompletely or even, if you're lucky, incorrectly. If you can't find the right answer, you help readers by showing that a widely accepted one is wrong. (See 10.1.2 for how to use this plan in your introduction.)

Only when you ask question after question will you develop the critical imagination you'll need in any profession you pursue. In fact, as experienced researchers know, most issues have few, if any, final answers, because there are no final questions. They know that it's as important to ask a new question as it is to answer an old one, and that one day their new question will become old and yield to a newer researcher's still newer one.

Your job is to become that newer researcher.

2.3 Build a Storyboard to Plan and Guide Your Work

For a short paper, you might not need a detailed plan—a sketch of an outline might do. But for a long project, you'll usually need more, especially for one as long as a thesis or dissertation. The first plan that comes to mind is usually an outline, with its *I*'s and *II*'s and *A*'s and *B*'s and so on (see 23.4.2.2). If you prefer an outline, use one, especially if your project is relatively short. The problem is that an outline can force you to specify too much too soon and so lock up a final form before you've done your best thinking.

To avoid that risk, many researchers, including those outside the aca-

demic world, plan long reports on a *storyboard*. A storyboard is like an outline spread over several pages, with lots of space for adding data and ideas as you go. It is more flexible than an outline: it can help you plan your search for evidence, organize your argument, write a first draft, and test a final one. As opposed to lines in an outline, you can physically move storyboard pages around without having to print a new plan every time you try out a new organization. You can spread its pages across a wall, group related pages, and put minor sections below major ones to create a "picture" of your project that shows you at a glance the design of the whole and your progress through it.

2.3.1 State Your Question and Working Hypotheses

To start a storyboard, state at the top of its first page your question and working hypothesis as exactly as you can. Then add plausible alternatives to help you see more clearly its limits and strengths. Add new hypotheses as you think of them, and cross off those you prove wrong. But save them, because you might be able to use one of them in your introduction (see 10.1.1).

2.3.2 State Your Reasons

Put at the top of separate pages each reason that might support your best hypothesis, even if you have only one or two (for more on reasons, see 5.4.2). Imagine explaining your project to a friend. You say, *I want to show that Alamo stories helped develop a unique Texan identity*, and your friend asks, *Why do you think so?* Your reasons are the general statements that you offer to support your answer: *Well, first, the stories distorted facts to emphasize what became central to Texan identity; second, the stories were first used to show that Texas (and the Wild West) was a new kind of frontier; third,* . . . and so on.

If you can think of only one or two reasons (you'll usually need more), put placeholders at the tops of pages: *Reason 3: Something about Alamo stories making Texans feel special.* If you know only *how* you want a reason to support your answer, state that: *Reason 4: Something to show that Alamo stories were more than just myth.* Each reason, of course, needs support, so for each reason, ask *Why do I think that? What evidence will I need to prove it?* That will help you focus your search for evidence (see 2.3.3 and 5.4.2).

If you're new to your topic or early in your project, your reasons may be only educated guesses that you'll change; if you don't, you might not be self-critical enough. But a list of reasons, no matter how speculative, is the best framework to guide your research and focus your thinking, and certainly better than no reasons at all.

2.3.3 Sketch in the Kind of Evidence You Should Look For

Every field prefers its own kinds of evidence—numbers, quotations, observations, historical facts, images, and so on. So for each reason, sketch the kind of evidence that you think you'll need to support it. Even imagine what the most convincing evidence would look like. If you can't imagine the kind of evidence you'll need, leave that part of the page blank, then read secondary sources to find out the kind of evidence that researchers in your field favor (see 3.1.2).

2.3.4 Look at the Whole

Lay the pages on a table or tape them on a wall. Then step back and look at their order. When you plan a first draft, you must put its parts in some order, so you might as well think about one now. Can you see a logic in your order? cause and effect? narrative time? relative importance? complexity? length? (See 6.2.5 for more principles of order.) Try out different orders. This storyboard isn't your final plan; it's only a tool to guide your thinking and organize what you find.

When you fill a page, try drafting that section, because writing out your ideas can improve your thinking at every stage of your project.

Someday you may have the leisure to amble through sources, reading just what interests you. Such random browsing has opened up important lines of research. But if your report is due in a month or so, you can't wait for lightning to strike; you need a plan. A storyboard is a simple and reliable device to help you create one.

2.4 Organize a Writing Support Group

A down side of scholarly research is its isolation. Except for group projects, you'll read, think, and write mostly alone. But it doesn't have to be that way, at least not entirely. Look for someone other than your instructor or advisor who will talk with you about your progress, review your drafts, even pester you about how much you have written. That might be a generous friend, but look first for another writer so that you can comment on each other's ideas and drafts.

Better yet is a writing group of four or five people working on their own projects who meet regularly to discuss one another's work. Early on, start each meeting with a summary of each person's project in that three-part sentence: *I'm working on the topic X, because I want to find out Y, so that I (and you) can better understand Z.* As your projects develop, start with an "elevator story," a short summary of your research that you might give someone in the elevator on the way to the meeting. It should include that three-part sentence, a working hypothesis, and the major reasons supporting it (see 13.4).

In later stages, the group shares outlines and drafts so that they can serve as surrogate readers to anticipate how your final readers will respond. If your group has a problem with your draft, so will your final readers. They can even help you brainstorm when you bog down. But for most writers, a writing group is most valuable for the discipline it imposes. It is easier to meet a schedule when you know you must report your progress to others.

Writing groups are standard practice for those preparing theses or dissertations. But the rules may differ for a class paper. Some teachers think that a group or writing partner might provide more help than is appropriate, so be clear with your instructor about what your group will do. If you don't, she may decide the assistance you have received is inappropriate (see 7.10).