1. Questions

. . . .

This chapter helps you navigate the first challenge you will face in your research process: How do you transform broad and vague "topics" of interest into a set of concrete and (for you, at least) fascinating *questions*? In the earliest phases of research, most people don't have specific questions in mind. They have *topics of interest*. You have already started recording some of your own in the introduction. The main challenge is not identifying potential topics of interest, but in moving from these generic topics to a specific set of questions. While seemingly straightforward, this surprisingly demanding process requires a mix of confidence and vulnerability.

A Topic Is Not a Question

Topics are wonderful things to have. They're useful at the beginning of any research project. A topic suggests a field or scope of inquiry. It empowers. It gives a sense of identity and purpose. I work on . . . the Harlem Renaissance, Soviet history, women's studies, experimental poetry, urban planning, environmental history. Having a topic makes one feel solid, self-aware, oriented.

Topics can be deceptive, however. They are immense and abstract categories. They organize universities, businesses, and research organizations — the Department of Topic X, the Institute for Topic Y. They show up on business cards: Professor of Topic Q. They shape how we think about the world.

But their use to the researcher is limited for one very obvious reason: a topic is not a question.

How do topics and questions differ? Let us count the ways (see table 1).

TABLE 1. DISTINGUISH BETWEEN A TOPIC AND A QUESTION

A TOPIC	A QUESTION
Is a noun, perhaps with a modifier	Is a sentence with a question mark at the end
May be broad or specific	May be broad or specific
Indicates an area of curiosity	Indicates an area of curiosity, and some sense of how you will satisfy that curiosity
Raises innumerable questions, but often ones that pull in a thousand different directions	Raises more specific, related questions
Has no answer	Has an answer—and sometimes several

You can see already how topics can even be *obstacles* to the research process. When a researcher tells you what topic they're interested in, more often than not they leave you wondering which of the many possible pathways and potential questions about that topic they intend to follow, or why the topic matters to them. Simply put, when we speak about topics, we could be speaking about *anything* (and thus *nothing*) at all.

Harlem Renaissance *what*? Soviet economic history *how*? Environmental history *where*? When someone tells you what their topic is, you actually still know very little about what drives them as a researcher, much less what direction their research takes. A study of the Harlem Renaissance might turn out to be about urban migration. But it could just as readily be about poetry, intellectual history, or housing

markets. A researcher working on Soviet economic history might be interested in the history of steel production technology, labor relations during World War II, or perhaps the development of economic think tanks in Moscow. Likewise, research on environmental history might be interested in invasive species, hydroelectric dams, or fire-stick farming. There's simply no way to know. All of these avenues (and many more) are equally probable, yet some might be of *no interest* to the researcher — some of these potential avenues might even bore them to tears. A person working on environmental history might have more in common with a scholar of the Harlem Renaissance than with their "fellow" environmental historians. By themselves, topics are not very good guides for the research process. That's why they can be dangerous.

When you have a topic and are struggling to turn it into a project, the common advice you will hear is "Narrow it down."

We call this the *Narrow-Down-Your-Topic Trap*.

Its seemingly straightforward logic — a "narrow" topic is easier to work on than a "broad" topic — leads many researchers, especially inexperienced ones, into dead ends. A more discrete scope that reduces the volume of sources you need to analyze can, to be sure, answer the *when* and *where* questions. But a topic alone — even a "narrow" one — is insufficient, because it still leaves unanswered the *how* and *why* questions. Tell someone your "narrow" topic, and they may still have no clue what you're doing. Even a "narrow" topic cannot tell *you* what to do.

Simply put, *you cannot "narrow" your way out of Topic Land*.

Every researcher needs to figure out *what to do* and *how to do it*. And — assuming that you want to devote your time and energy to something worthwhile — the question that comes before *what* and *how* is *why*.

A brief example: a student sat down with Tom to discuss

potential paper topics for a history course. The topic of the paper, the student explained, would be Chinese geomancy, or feng shui. In feng shui, the landscape and the natural environment are understood to be energetically alive, with this energy having the capacity to affect—for better or for worse—the fortunes of the living, as well as the afterlives of the deceased. By building one's home or city in harmony with the logics and flows of these energetic forces, one can improve one's fortune. Neglecting or violating these logics can bring ruin.

Feng shui is a promising and potentially fascinating topic, to be sure, but Tom was still unclear about the student's concerns. What were the student's *questions* about the topic? What was at stake for them? *Why feng shui?*

The student was equipped with a "straight-A" vocabulary, and had clearly rehearsed prior to the meeting, using key terms and concepts from the course. Feng shui offered a way to examine "Chinese modernity," the student explained, to examine "knowledge production" during China's transition from "tradition" to "modernity." Everything about the presentation was polished.

Something was still missing, though.

OK, but why feng shui? *If the main motivation is to understand "Chinese modernity," your paper doesn't need to be on* feng shui. *You could just as easily have chosen to work on education reform, the development of chemistry, or perhaps the history of translation. There are an infinite number of ways to "get at" the issue of modernity.*

The student tried again, pulling out all the stops by using as many "smart-sounding" justifications as possible. There were "gaps in the literature," they explained, using an academic code word to mean "important areas in our map of knowledge that have yet to be filled in." Feng shui had the makings of a powerful "intervention" in the historiography, they suggested, using another word commonly heard in the academy. In other words, the student was trying to speak in

code with Tom, using terminology they assumed would resonate with an academic mentor.

It all still begged the question. To say that there is a "gap in the literature" is to assume that the topic in question is of unquestionable importance and needs to be addressed. *But important to whom, and why?* Besides, "gaps" in human knowledge are infinite. Why fill *this* particular gap?

The impasse cannot simply be blamed on the student being "inexperienced." Most researchers (even seasoned ones) instinctually try to justify their incipient research ideas using the vocabulary of "importance" or "significance"—as defined by an imaginary, *external* judge. But at the outset, external judges are not what we need. Instead, what every researcher needs in the earliest phase of a project is to answer a question that is profoundly personal: Out of the infinite number of potential topics of interest, why am I drawn to *this* one? If I had to guess, what is my connection with *this* topic? Why is it so magnetic to me?

There was a noticeable pause in the conversation, and the student's entire disposition shifted. The tone and volume of the voice softened. Even the posture relaxed. Suddenly, the conversation felt less like a performance, in which the student was trying to impress the professor. Instead, the exchange became more open, even vulnerable. The student allowed themself to share more fundamental concerns, to stop acting intelligent and just *be* intelligent.

My mom is a lawyer, the student continued. *She's highly educated and is the most rational person I know. She's not superstitious at all. But she also believes in* feng shui—*truly believes in it—and I just can't understand how.*

All of a sudden, the room was full of new questions. *What else might a "rational" person not believe in, do you think? Meditation? Yoga? Reflexology? Numerology? What about psychiatry, or perhaps economics? Who or what defines this "rational/irrational" boundary? Is this boundary the same in all parts of the world? How and when have views about*

rationality taken shape in history? Why? What might I find if I looked at primary sources from other time periods, or other cultures? What do I mean by "rational" anyway? Why am I using that word? Is it because "rationality" depends on logic, and I think feng shui *is illogical? Or is there another reason I think* feng shui *and rationality are incompatible?*

It was like getting away from the glare of the city lights — suddenly, the sky was filled with stars.

The questions went on, filling the student's notepad.

A few key aspects of the discussion led to this breakthrough. Here's how we'd phrase them for a researcher trying to move from a topic to questions:

1. **Make yourself vulnerable.** The student sounded unpolished (as they had initially worried) — *but that's a good thing*. The questions one generates during this early phase are not final products. Many things in our lives coach us against opening up. We want to appear mature and professional, and we hesitate to ask questions that might make us appear unpolished or naive. But at this stage, our questions don't need to be polished or even coherent. All they have to be is *honest, to the best of our knowledge.* Trust yourself.

2. **Keep the conversation affirmative and nonjudgmental.** Neither the researcher nor the Sounding Board said anything to denigrate the researcher's assumptions about rationality. At the brainstorming stage, it's easy to shut down lines of inquiry prematurely, with inhibitory thoughts or statements like *Your assumptions are wrong: there is nothing inherently irrational about non-Western practices!* Or perhaps by chiding oneself with high-level language like *My concept of rationality is clearly a "social construction."* Resist the temptation. Far better is simply to allow the questions to proliferate, no matter how seemingly unimportant, naive,

incoherent, scattered, or biased they might seem. Whether you're working alone or with someone else, the goal at this point is simply to generate questions. We'll discuss how to use them later on.

3. **Write down your ideas.** The researcher and Sounding Board wrote down all the questions as they spilled out. Ideas can come rapidly, but they can be forgotten rapidly too if not recorded. As we will emphasize again and again, during this early phase of research, thinking about things is not enough. You need to get things down in writing, to create traces of thought that you can later use for other purposes.

4. **Generate questions internally.** In the conversation described above, it was the student who was producing questions; the Sounding Board barely needed to chime in. The questions you should be aiming at now are those driven by your own knowledge, assumptions, and curiosities. At this point, don't try to think from the "outside in" by trying to generate questions you think might satisfy some imaginary judge.

This particular student was in a more fortunate position than most, having clearly done a great deal of self-reflection in advance of the meeting. They were already aware of why their topic mattered to them personally and simply had to overcome reluctance to share those reasons.

For most of us, the challenge is greater. We might be drawn to a particular topic without having any idea why. Or, perhaps more accurately, *some part of us knows why*, but the *rest* of us — the part of us that has to field questions like "Why does that interest you?" — still has absolutely no idea.

As we progress through the stages of Self-Centered Research, we'll discuss several ways to close the distance between these two parts of ourselves. You will learn how to bring together

- the *intuitive* part of you that knows, but cannot speak;
- the *executive* part of you that speaks, but does not know.

Questions lead us in specific directions—whether toward specific answers or to primary sources that we need to answer the questions or to the work of fellow scholars who are grappling with similar questions (i.e., secondary sources) or, more often than not, to more and better questions. Questions force a self-reckoning.

Questions have another virtue. Every question a person asks about the world is a piece of "self-evidence" about the researcher—evidence that helps the researcher reflect on their own intellectual, emotional, and personal motivations for asking the question in the first place. The goal here is to explain, rather than simply assert, one's interest in a topic.

Consider the following example:

Soviet history is fascinating.

Questions give much more self-evidence:

Given the Soviet Union's vociferous critique of capitalism, did it develop its own form of accounting practices? The USSR must have had accountants to keep track of economic data, and yet most accounting theory to that point had been developed in capitalist contexts — was that a problem for the Soviets?

Now you have more clues to answer the obvious question, *Why are you interested in that?* Your questions place you in the hot seat. They require you to ask probing questions about yourself, without falling back on vague and tautological responses like "The topic is interesting, which is why I'm interested in it!"

TRY THIS NOW: Search Yourself

The goal: *To use a list of primary-source search results to figure out the aspects of your topic that most interest you, and draft questions based on these interests.*

You already know how to search the internet. This exercise prompts you to use the results of an internet search to *search yourself*.

This exercise offers one way to get from a topic to questions.

Here's a quick summary of the steps of this exercise, before we dive into details about each:

1. Based on the "Try This Now" exercise you completed in the introduction, write down any and all of the research topics you are drawn to. Feel free to be as general as possible, and to include more than one.
2. Select one of the topics on your list and run a search using at least three (or more) of the web-based databases listed below. (You can find more on selfcenteredresearch.com.)
3. Click on a few of the search results that interest you—say, five to ten.
4. Don't read the search results in depth. Instead, your goal is to dedicate (a) perhaps 20 percent of your mental energy to scanning the list of search results (and perhaps the contents of a few) and (b) the remaining 80 percent of your mental energy to self-observation. You want to read yourself as you read the results.
5. In particular, pay close attention to how your mind and body are responding to different search results: Which ones seem to jump out at you? Which ones cause you to linger just a split second longer? Which ones quicken your pulse, even slightly?

6. Write down at least ten entries that attract you, without worrying about why they do.
7. Based on this list of ten entries, answer the three questions on page 31 about those entries, to generate self-evidence.
8. Sleep on it (take a break of at least twenty-four hours).
9. Return to the answers you wrote out and ask yourself: If I didn't know the person who wrote these answers, or flagged these search results as "interesting," what kinds of guesses would I make about this researcher? What story does this "self-evidence" seem to tell about the researcher, in terms of their concerns and interests?
10. Write down your thoughts on these questions, getting as much down on paper as possible.

Let's dive in a bit deeper.

Step 1 is straightforward enough.

Step 2: Select a database. We list a few good choices here, and you can find dozens more at whereresearch begins.com.

- WorldCat: www.worldcat.org
- HathiTrust: https://www.hathitrust.org
- Trove: https://trove.nla.gov.au
- Online Archive of California (OAC): www.oac.cdlib .org
- Archives Portal Europe: http://www.archivesportal europe.net
- Collaborative European Digital Archive Infrastructure (CENDARI): www.cendari.eu
- Consortium of European Research Libraries (CERL): https://www.cerl.org/resources/main

Don't worry about choosing the "right" database for your topic. For the purposes of this exercise, it really doesn't matter which one you choose. (You'll see why in a moment.) And don't worry about where the library is located, geographically. If you imagine you're unlikely to find anything in a New Jersey–based archive about, say, Armenian politics, or anything in a Kansas-based archive on Etruscan pottery, you might be surprised.

Familiarize yourself briefly with how the search engine works, and then run a basic query. Enter your search term—your topic, or some variation thereof—and see what comes up. If your search yields zero results, try a more generic search term, or perhaps a related but different one. If all else fails, go to a different site and try again. The database itself is not the vital part.

Step 3: Once you get a set of search results—any results—your work is simple. Just scroll through and scan the results to see what you find. Click on a few and read them. On most of these sites, you won't be able to view the original source, only the catalogue entry. But even if a site does offer full text results, try not to get caught up in any one source for too long at this point. This is not yet the time for close reading.

Instead—and this is key—while scrolling through your search results, try to imagine that you are strapped to an EKG machine that is recording the electrical pulses going through your system *as you read*. Which primary sources raise your heart rate, even slightly? Write them down. Which ones have no effect on you one way or another? Take note of them too (since, a bit later on, we will also be taking stock of things that *bore* you!).

The goal right now, as we said above, is to "read yourself" as you read other things. As you read through the

search results, only 20 percent of your cognitive energy should be dedicated to clicking on links, reading snippets of sources, and the like. The other 80 percent—and the critical part—should be dedicated to *paying attention to yourself as "you" pay attention to the sources*.

Why bother? How does this get a researcher any closer to discovering their research direction?

Well, consider this: every day our physical senses are so bombarded by stimuli that most sights, sounds, and smells go unnoticed. In fact, if we tried to pay attention to all of these stimuli all the time, our systems would get so overloaded that we would be incapable of carrying out even basic tasks. As a consequence, our bodies have evolved into refined filtration systems that decide what to ignore. Our bodies and minds have evolved into amazing not-seeing, not-feeling, not-smelling, not-hearing, and not-tasting machines.

Given how efficient we are at *ignoring* stimuli, it follows that when we *do* take notice of something—however small or insignificant—we should *take notice that we're noticing*. This form of self-evidence gives a potential clue about our underlying concerns and curiosities.

Put plainly, whenever your mind takes notice of something—*anything*—you can be certain that there is a question there, even if you are not sure what that question is.

Learn to pay attention to these clues, and then to uncover the questions whose presence they indicate, and you'll be able to move quickly and effectively from generic topics to precise and generative *questions*.

"Noticing what you are noticing" can be surprisingly difficult. You need to listen very closely to yourself, since the act of noticing something is rarely a dramatic affair. Epiphanies are not always loud. You might utter a semi-

audible *Hmm*. Moments of *Eureka!* can even be silent. You might simply grin or furrow your brow or linger on an image or a line of text just a little bit longer than normal. No one needs help to hear a sonic boom. Your job here is more akin to detecting the faintest of gravitational waves.

Step 4: Go back to your search results. Write down, circle, or asterisk the ones that seem to have *any* effect on you, however small. Write a list out by hand, copy and paste the titles of the sources into a text file, or click a checkbox to save those sources in a folder or email. However you choose to do it, take notes.

To repeat: take note of anything that jumps out at you, even if it seems completely unrelated to your topic.

Let's say you run a search on the Ottomans or New Jersey or China, and in addition to "relevant" materials pertaining to the empire, the state, or the country, your list also includes what appear to be fluke materials about Ottoman furniture, Jersey cows, or porcelain china. Do not dismiss these out of hand. Scan them too. If any of them make you pause or wonder about something, make a note just as you would for any other item. Don't worry if your list seems incoherent or inconsistent. Your only job at this stage is to listen to yourself, and to take note of everything that attracts you. The winnowing comes later.

Step 5: Once you have an initial list of at least ten items (don't simply copy and paste *everything*, although definitely err on the side of inclusion rather than exclusion), take thirty minutes or so to ask yourself three questions about each item, setting down your answers in writing:

- What does this make me think of?
- If I had to venture a guess, why did I notice this one?
- What questions come to mind for me when I look at this search result?

A few words per item will do. And keep in mind: at this stage, it is quite likely that you won't know why each item attracted your attention. Some of your answers to these questions might end up feeling tentative or silly. That's OK. Just remember: as in the case of the feng shui example, avoid the temptation to act smart or to use language designed to impress some imaginary, external judge. Your only audience is you, so allow yourself to be inarticulate, instinctual, and honest. *Why did this jump out at me?*

Step 6: Put your list away, and don't look at it for a full day. We mean it. Close this book and your computer, and set a timer for twenty-four hours.

Step 7: Now return to your list with fresh eyes. Imagine for a moment that someone you don't know wrote it. If this list was all you had to go on, what would you say this researcher is concerned with? If you didn't know their topic, what would you guess is their primary concern? Since you do know the topic, does the list of "noticings" tell the *same* story or a *slightly different* story or an *extremely different* one? Are their concerns intrinsic to the topic? If so, *which aspect* of the topic? Or is the topic merely a *case of* or the *vehicle for* a different question? Write out your thoughts on paper.

COMMONLY MADE MISTAKES
- Not writing things down
- Getting bogged down in individual sources too soon
- Excluding "fluke" search results that seem unrelated to the keywords you entered in the database or unrelated to your topic
- Feigning interest in a search result that seems "important," even if it doesn't really interest you
- Only registering interest in search results for which

you think you know *why* you're interested in them, instead of being more inclusive
- Trying to make a list of noticings that is coherent and fits together
- When speculating about why a search result jumped out at you, worrying about whether or not the reason is "important," based on some imagined external standard

TRY THIS NOW: Let Boredom Be Your Guide

The goal: *To become attentive to your active dislikes, identifying questions that you "should" (in theory) be interested in based on your topic of interest, but aren't. By understanding what you* don't *care about regarding your topic, you accelerate the process of figuring out what you* do *care about*.

In the exercise above, you took notice of all of the search results that appealed to you. But what about the search results that had a *negative* impact—that seemed *boring* to you? Quite likely, they also registered on your imaginary EKG readout, but not because they attracted you. Rather, they *repelled* you, and so it's unlikely that you included them in your list. After all, the most common reaction human beings have to boredom is *avoidance*. We try to dismiss or ignore things that bore us.

Don't. Boredom is a powerful teacher, and deserves our attention. Boredom is not the same thing as disinterest or lack of interest. It is not a passive experience. Boredom is an *active* sentiment, a *rejection* of something that, like excitement, provides you with more self-evidence through which you can understand your concerns and motivations more clearly. By taking note of your boredom—in precisely the same way you just did with your excitement—you will

gain clues about what your real research questions and problems might be.

Imagine a conversation between you and a well-meaning friend:

FRIEND: What are you working on?

YOU: Institutional sociology.

FRIEND: Ah, how interesting! I read an article the other day comparing the managerial structures of different companies, to see which ones created the most opportune conditions for workplace satisfaction and productivity.

YOU (TO YOURSELF): Wow, how painfully boring. That's not something I'm interested in studying at all.

Your friend rattles off more examples that, given your professed topic of interest, should in theory be of interest to you as well. They list the titles of books and summarize a few. The more you listen, the more confused you become. *Everything my friend is listing here is all so boring to me. Why? All these examples are clearly related to my topic, and so I guess I* should *be concerned with them. And yet I just don't care. What's wrong with me?*

A quiet terror begins to set in.

Maybe my topic is boring. Maybe I should switch topics. Or maybe this is just what research is like: a fleeting moment of excitement followed by the tedium of studying things you don't care about. Maybe I shouldn't do research!

Hold off on judging yourself (or your friend—they might actually be helping you!) and take a moment to reflect. Ask yourself: What about your chosen topic *bores* you? Among the potential questions or subtopics that derive quite naturally and obviously from your stated topic, which ones repel you, perhaps even unnerve you?

This might be the first time you've ever considered questions like these. After all, no one asks us what bores us. Everyone asks what *interests* or *excites* us. It's easy to see why answering questions about interests might lead us to learn something about ourselves that we might not know in a conscious way. But how would you explain why something *bores* you—especially something that seems like it should align with your topic of interest?

Here's what to do:

1. Go back to your search results, and scan them again.
2. Pay close attention to your EKG readout, focusing this time on the results that bore you. In the very same way that we spoke of not "outsmarting" yourself regarding your interests, you will need to be cautious during this process as well.
3. Choose a few "boring" results and write down answers to the same questions you answered before—this time for these different, *boring* search results:
   a. What does this make me think of?
   b. If I had to venture a guess, why did this one *not* jump out at me?
   c. What questions come to mind for me when I look at this search result?
4. Now, for each search result, write some version of this sentence: "I'm more interested in [something else] than [search result]."

Steps 3 and 4 produce two types of self-evidence that can give you detailed clues about the interior, unspoken, often unconscious mental makeup with which you are constantly making sense of the world.

Taking account of your boredom is part of your conversation with your research-self. Besides helping the process of elimination, steering you away from unprofitable lines

of inquiry, boredom can also help you to ask better questions and zero in on your Problem.

COMMONLY MADE MISTAKES
- Denying boredom, or feigning interest in something because you feel it's "on topic" and demands your interest because it's "important."
- Engaging in circular logic. Don't fall prey to explanations that go something like this: "The reason this thing bores me is because it's boring!" Boredom is not something that "happens" to you. Boredom, like inspiration, is a dynamic process that happens *between* you and whatever it is you're interacting with. The sensation of boredom is the *by-product* of reactions between the substance that makes you you, and the substances of the reality you're encountering.

TRY THIS NOW: Go Small or Go Home
The goal: *To generate specific, fact-focused questions about your topic* before *you've done in-depth research. These will lead to bigger questions later on.*

You are now in a great position to start moving from a topic to questions. You have a set of notes about two things:

1. What you noticed about sources on a topic, and your best guesses as to why you noticed those things
2. What, among the "logical" or "obvious" aspects of your proposed topic, bored you and why

Using all of this as inspiration, try the following—as always, *in writing*.

In a stream of consciousness, write out a minimum of twenty questions related to your topic. The key is to make your questions *as specific as possible*, using the following prompts:

- What facts do you wish to know about your topic?
- Which data or information about your topic might you need to satisfy your curiosity?
- What telling details about your topic do you imagine might exist?

Some questions might be prompted by your initial contact with the sources you used in the "Search Yourself" exercise, or in "Let Boredom Be Your Guide." Others might be new.

Try to avoid posing questions that strive to be profound or too big-picture. If you find yourself asking questions about the essential "meaning" or "significance" of your topic, chances are you are thinking too abstractly.

Remember, too that *question means question*—with a question mark—and not a statement or sentence fragment masquerading as a question. "The question of justice" is not a question.

Again, your goal here is *not* to justify the significance of your project to someone else. You need to start with questions about basic facts. After all, you are new to your topic, and what you *don't* know about it far outweighs what you do.

An example: let's say you are looking at a black-and-white photograph taken during the proceedings of the military tribunals held in Nuremberg, Germany, in the wake of World War II. You might well wonder about big-picture questions, like "What effect did the trials at Nuremberg have on post–World War II Europe?" or "What was the significance of these trials?" But when you're try-

ing to formulate a research project, it's the specific questions that will get you there faster:

Which nations were represented at the trial?
Who was sent as the delegate of each nation?
How were the delegates chosen?
What were their roles in the proceedings?
Did anyone refuse to go?
Who were the judges?
How were they appointed?
Who appointed them?
Was this the first postwar tribunal of its kind?
If not, where were the earlier ones?
Were members of the media permitted to attend?
Who took this photograph?
How was the photo distributed, when, and by whom?
In what building and what room did the proceedings take place?
Did the trial proceed during one contiguous period of time—over the course of a few days, weeks, months—or were there different parts of the trial, spread out over time?
Was there a deadline by which the trial had to conclude?
Who created a transcript of the proceedings?
Where was the transcript kept, or how was it distributed?
Who paid for all of this?
Who paid to transport judges, lawyers, and witnesses to the city?
Who paid for their lodging, or for their lost wages?
While standing trial, where, and for how long, were the accused parties incarcerated?

Notice that none of these questions is profound. They are small-scale and specific. Specificity is the goal at this point, for two reasons.

First, it is only through *small* questions like these that you can begin to form a picture in your mind (and in your notebook) about the core fundamentals of the topic you are researching. To try and answer "profound" questions at this point—about "meaning" and "significance"—is premature, since you don't yet have the facts, much less the opportunity, to analyze them. By contrast, the more facts you know about the physical space where the tribunal was held, or about the identities of the judges, lawyers, witnesses, onlookers, reporters, families, and others present, the greater command you begin to have of your subject matter. This, in turn, *prepares you for the process of asking "bigger" questions—"profound" questions—when the time is right.*

Second, lurking in one or more of those "small" questions may be an *unexpected* question that could, when you hear yourself ask it aloud, send your research off into an entirely new direction. For example: when asking a simple question like *Who paid for the lodging, food, and transportation costs of witnesses?* you might find yourself wanting to explore the history of international tribunals from a different perspective—not from the perspective of the courtroom drama itself, but, say, from the perspective of urban history, asking questions like *How did cities like Nuremberg, Tokyo, and Nanjing handle war crimes trials logistically? How did war-torn cities, whose infrastructures lay in ruins, handle transportation, housing, security, and more for such important events?*

Even from these few further questions, you can already see how you are moving toward a research project that might teach us something new and insightful about a grand topic like justice.

When you begin to ask (and then to answer) precise and seemingly mundane questions like these, you begin

to liberate yourself from the confines of vague and unproductive "topics," moving instead toward specific and coherent clusters of questions that will, over time, add up to something compelling, open-ended, and doable.

Asking precise factual questions is one key to escaping Topic Land.

COMMONLY MADE MISTAKES
- Asking vague, grand, abstract, or big-picture questions about "meaning" or "significance," instead of specific and precise factual questions
- Not asking actual questions (with a question mark), but instead writing statements or sentence fragments—topics masquerading as questions
- Not asking a question because you think you couldn't answer it, perhaps because you think that the data doesn't exist or is unattainable
- Asking too few questions, resulting in an inadequate quantity of self-evidence

SOUNDING BOARD:
Start Building Your Research Network
You've done quite a bit of work by this point, all on your own. You've been thinking through topics and questions, and have done three exercises to generate new questions based on a topical interest.

Now's a good time to use the questions you've generated to start a conversation about research with someone you know. Start building your research network—a community of people you can consult with and seek advice from during the research process. Make a list of teachers, colleagues, students, and fellow travelers you think would

be willing and available to discuss ideas with you on a periodic basis. Some researchers do a large portion of their research individually, but a reliable Sounding Board can be a catalyst.

Circle a couple of names on your list of potential Sounding Boards. Choose a few of the questions you've generated while reading this chapter, and make a meeting to discuss them. Keep things open-ended. You're not asking your Sounding Board to tell you which of your questions is "the best." Tell them you're not trying to settle on a research question just yet. You're in an exploratory stage. The goal is to get on their radar, and to start the process of communicating about your research ideas orally—since you've already done some writing.

You could send them your questions in advance, but strive to make it a casual conversation. Don't ask them "Are my questions *good*?" but "What do these questions make you think of?" and "What other questions do these questions make you ask?" Spend some time generating questions about a topic together.

And say *Thank you*. You may well be seeking them out again.

You Have Questions

You're now well on your way. You started with a general interest and identified an equally general "topic"—an object or focus of inquiry. You "searched yourself," generating a preliminary body of notes—self-evidence—based on an honest exploration of your attractions and repulsions. By writing about why certain things jumped out at you, and why others bored you, you've gained a clearer sense of your own standpoint and concerns, and you've used those exercises to generate specific and narrow questions. If your questions seem

scattered, fragmentary, and chaotic, that's OK; in fact, that means you're doing things right. (If you have only a few questions, however, you should take another pass at the preceding exercise.)

Most importantly, in formulating these possible research questions, you've set aside for the time being any concerns about whether or not your questions are Important, with a capital *I*. We'll get to what other people think in part 2. Your list of questions contains questions that *matter to you, even if you don't know why yet*. As a bonus, you also have an initial set of primary and secondary sources from your database searches.

You have begun the process of transforming a topic into *questions*.

In the next chapter, we will show you how to analyze these questions to determine how they all connect. And once you connect them, you will discover that, underlying many if not all of these narrow and scattered questions, there resides something deeper that drives your work: your *Problem*.

For now, close this book, and give yourself time to recharge. We'll see you soon.

2. What's Your Problem?

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Now that you have questions, the next step is to answer them, right?

Not exactly.

In this chapter, you will begin to find and use primary sources, and you may find the answers to at least some of your questions. But answering questions will not be the primary focus. *Educating* your questions will be the focus.

The questions you have generated thus far are, by and large, less developed than they could be and will become. This has nothing to do with your abilities as a researcher. Rather, it is a fundamental part of this stage of research: your questions are underdeveloped at this point because you have not yet had the chance to conduct research into your subject matter. It's to be expected.

Wait a minute! You might protest at this moment. *Before, you told me that I need to generate questions in order to do research. But now you're telling me that I need to do research in order to generate questions? That's impossible. It's an infinite loop. It's a trap!*

It's not a trap. But it is true that it takes a lot of research to arrive at the right questions. And then it takes *more* research to *answer* these questions and generate new ones. In this early stage of research, the goal is not, as many assume, the generation of answers. It is about the refinement of your existing questions and the generation of new (and better) ones.

The goal of this chapter is to help you identify and articulate the *problem* underlying your many research questions.

Accomplish this, and you will end up asking better questions, doing more significant research, and carrying it out more effectively.

Don't Jump to a Question (or You'll Miss Your Problem)

Over the course of generating, analyzing, refining, and adding to your questions, you may have wondered: *How do I know when I've found my Problem? Do I really have a "Problem," or have I merely compiled a random set of questions that don't really add up to anything?* After all, we're curious about many things, but we don't launch research projects to satisfy every curiosity. Nor should we.

Here's a simple way to distinguish a *problem* from a random set of curiosities: if it changes by the day, week, or month, chances are it's a passing curiosity. If it endures, it just might be a problem.

A problem is a nagging presence within you—one that disturbs, bothers, and unsettles you, but also attracts, compels, and keeps you coming back. It's something that generates questions in your mind—questions that, no matter how varied and unrelated they might seem to an outsider, you know to be somehow interrelated, even if you can't explain why. A problem is something that follows you around. It doesn't care if you are a historian of France, a sociologist of the Philippines, or a literary scholar of India—it calls out for you to try to solve it. Your job is to give that problem a name, to identify a *case* of that problem that you will be able to study (given your personal abilities and constraints), and to figure out how to study that *case* so that you might arrive at a broader solution.

Researching a problem requires asking questions, of course, but (again, to state the obvious) a question is not a problem.

You can think of plenty of questions that have answers, but whose answers do not solve any problem. Asking and answering such idle questions is a waste of time, so *you want to make sure that your questions are indeed problem-driven*. This is why it is so important not to jump to a question.

A problem has several functions for the researcher, among them the following:

- It motivates you to ask questions about your topic.
- It determines which questions you ask.
- It defines the what/why/when/how of your engagement with your topic.
- It guides the path of your inquiry.
- It shapes the story you tell when the time comes to share your research results.

Up to now, you have been generating "first-draft" questions based on an initial foray into sources. But you want to make sure that you are asking questions that do more than just satisfy a personal curiosity. The next steps in this chapter will help you figure out

- how to *improve the questions* you have already generated;
- how to *use sources to identify the problem* motivating your questions;
- how to use your Problem to *generate new and better questions*.

We all know not to "jump to a conclusion"— an action prompted by prejudicial or hasty thought. We've all seen it happen, and we've all done it ourselves — we arrive at an argument or thesis about a sure thing even though we haven't spent sufficient time thinking it over. And we end up being wrong.

What the early-stage researcher has to avoid is jumping to a *question*. You have generated many questions, and the risk

now is that you'll feel pressured to jump ahead and choose one prematurely.

What is your Research Question? You'll hear this demand from other people, and eventually from a little voice in your head that tries to trick you into thinking that your project must have only *one* Research Question, and that you must settle on it early.

The *Jump-to-a-Question Trap* can be as harmful as the Narrow-Down-Your-Topic Trap.

Jumping to a question is like constructing a home without examining the ground on which it will stand. Your architectural plan might be stunning, the plot of land spacious, and the vista marvelous, but if you build on sand you are going to have serious issues when those sands shift. By the time issues show up, renovations may be costly, and you might find it impossible to relocate.

Stress-Testing Your Questions

Now that you have done the work of producing a multitude of questions — small, factual questions, ideally — you will still need to stress-test, refine, and winnow them out, removing any dead ends, enhancing the rest, and adding additional questions that will better serve your research process.

Think of a question as if it were a car. Before jumping into this vehicle, and certainly before bringing others along, you would want assurance that its steering and brakes have been subjected to rigorous testing. You'd want to know that prototypes underwent crash tests, over and over, until the manufacturer felt certain that the structure of the vehicle was ideally suited to protect the driver and the passengers.

Here are two ways to stress-test your questions and improve their soundness. The first focuses on language; the second is subject-specific and focuses on sources. We recommend that you tackle them in that order.

Run a Diagnostic Test on Your Questions

The goal: *To ensure that the vocabulary, grammar, and phrasing of your questions are specific and unprejudiced so that they do not presume a certain outcome.*

Rewrite your research questions with particular attention to the following:

1. *Punctuation.* Do your questions actually end in a question mark? Or have you phrased them in more general, and vaguer, terms like "This is an examination of . . . ," "I plan to explore . . . ," or "My project is about the question of . . . "? If you find yourself articulating your questions as "I want to examine how" something happened, there is a fair chance that what you have are not really questions at all, but rather *topics disguised as questions*. Be more specific, and add a question mark.

2. *Adjectives and adverbs.* Do your questions rely on broad, generic, imprecise, or sweeping adjectives like "modern," "traditional," or "Western," or adverbs like "scientifically," "rationally," or "effectively"? Try to cut such adjectives and adverbs out entirely.

3. *Collective nouns.* Do your questions depend upon collective nouns like "Asians," "the French," "students," "women," or "North Americans"? If so, do your best to replace these nouns with more precise demographic categories: *women of what ages, students living where and when, North Americans of what background, socioeconomic status, race or ethnicity, or family status?* You do not need to take into account *all* possible demographic variables, but you do want to try to include all of those that might make a difference to your project.

4. *Verbs*. Do your questions contain verbs like "influence," "affect," "shape," or "impact" or passive constructions such as "was affected by," "responded to," or "reacted to"? In such cases, chances are high that you are building your questions in such a way that they rule out an entire set of possible answers and outcomes. Rephrase to avoid presumptions that could result in confirmation bias.

By the end of this process, your questions should meet these criteria:

• *They should be clear, precise, and jargon-free*. If your questions are too hard for a colleague or mentor to understand, this means that *you* (and not they) still don't get what your Problem really is. Your shorthand might be hiding your significant specifics. Likewise, if your Problem is hiding behind jargon—words designed to make your questions sound "smart" and "important"—replace it with language that is clear and *vulnerable*. You and your intended audience should be able to tell what your research is driving at, even if this means your language might be less articulate and refined—and less concise—than you might like.

• *They should be rooted in verifiable and falsifiable data*. Your research questions should have integrity. This means that they should be inspired by fact, rather than by speculation, prejudice, or opinion. What are the facts that motivated these questions? Are they verifiable? Where and how could these facts be checked? Have you checked them yourself?

• *They should be indifferent to the outcome*. The

best research questions are open, agnostic, unprejudiced. Put another way, a research question should not presume a certain answer. If yours does, rewrite it to eliminate that presumption.

- *They should be clear about the subject*. Your questions should not be reliant on broad categories of identity, such as "students," "women," "Europeans," "Brazilians," "Christians," and so on. Refer to the suggestions above, and be as specific as you can be about the *who* in your question.
- *They should be raw and undisciplined*. At least for now. Although we encourage you to make sure that each *individual* question in your list is as precise, detailed, and grounded as possible, remember that, collectively, your list of questions need not be overly polished or coherent at this point. If the questions seem random to you, *let them be random*. If they seem unrelated to one another, *let them be unrelated*.

COMMONLY MADE MISTAKES

- Asking *leading questions*, which are phrased so as to predetermine the answer. These questions are motivated by unproven assumptions, and result in confirmation bias. The result of leading questions is that you inevitably find what you are looking for. (See the example below of *How did X influence Y?*)
- Asking *advocacy questions*, which promote a certain ideology (taken-for-granted worldview) or course of action. These questions take a position and encourage others to adopt it, irrespective of the actual facts of the case or

which interpretations the evidence suggests are plausible. Example: "Why is 'feminism' a better analytical rubric than 'romance' for understanding Joan Didion's novels?"

- Forcing all your questions to "make sense" or "add up." Don't worry. That part will come soon.

Leading questions are so common and so prejudicial to the research process that it's worth looking at one extended example. Maybe you've seen a version of this question before:

How did X affect Y?
Consider this example:

How did the ruinous taxation policies of King Louis XVI during the 1780s erode popular support for the aristocracy and pave the way for the French Revolution?

Wow, that is one "educated" question! In order to pose it in the first place, one would already need to know a considerable amount about French history.

But take another look. See any issues? When we ask *How did X affect Y?* the implied answer is that X *did* affect Y, and all that remains are questions of *how* and *to what degree*. Building a question this way creates a major weakness. The researcher at this point has not yet established that such influence existed in the first place. The very phrasing of the question rules out the possibility that X *didn't* affect Y at all. If it turns out that it didn't, you'd have one heck of a short paper.

Let's say your hunch is still that X *did* affect Y. It might have. However, you couldn't know at this point, having not yet done the research. What you want to avoid is building your questions in such a way that you actually *need* this "influence" to exist in order for your questions to be

viable. Almost inevitably, you will end up discovering spe-cious "proof" of influence in primary source material, mis-leading both your readers and yourself.

If you detect shortcomings in your question, try to re-pair them. If your question is actually a topic masquerad-ing as a question, reword or restructure it. If you are rely-ing on abstract nouns, adjectives, or adverbs, substitute specific words. Articulate your question without using any sweeping words at all. And if your choice of verbs is com-mitting you to research outcomes too early, change them.

TRY THIS NOW:
Use Primary Sources to Educate Your Questions

The goal: *To learn how to run keyword searches designed to enhance or "educate" the questions you are asking about your topic. These searches uncover primary sources relevant to your research that themselves contain* new *keywords you were previously unaware of (thereby enabling you to run follow-up searches to reveal even more, and more use-ful, primary sources).*

Running language diagnostics like you did in the pre-vious exercise is a first step that will help you avoid com-mon errors when posing your questions. This next exercise requires you to delve back into your specific subject mat-ter and into primary sources.

Thus far, we've been keeping you somewhat at arm's length from primary sources. In chapter 1, we specifically encouraged you *not* to delve into them too deeply in the "Search Yourself" exercise. We now encourage you to dive in, but in ways you might not expect. Rather than trying to use primary sources to start answering the questions you've come up with, we want you to use them to develop,

refine, and expand those questions. You'll eventually start using primary sources to answer questions, but at this early stage, we suspect, your questions still need more refinement before you start investing large amounts of time and energy in answering them.

How do we use primary sources to strengthen and "educate" our questions? The answer is simple: primary sources alert you the existence of *other* primary sources, exposure to which helps you ask more mature questions about your subject. By contrast, researchers who "jump to questions," and then dive headlong into answering those questions using primary sources, run the risk of confining themselves to a kind of intellectual and empirical bubble.

Let's imagine that you're interested in one of the following topics:

- turn-of-the-twentieth-century African American literature
- the history of artificial intelligence
- food culture in twentieth-century Hong Kong

Let's further imagine that you've already done the hard work of transforming your initial topic into a set of specific questions, and that you're now collecting and exploring sources. You start by running searches in a digital repository of historic newspapers containing hundreds of fully text-searchable periodicals from across the world.

But then you hit a stumbling block. Practically all the search results for "*food AND Hong Kong*" come from the 1950s onward. Or a search for "*African American AND literature*" yields many articles and reports from the 1980s onward, but almost nothing from earlier periods. A search for "artificial intelligence" returns lots of materials from the 1980s onward, but very little before.

What is going on? Common sense tells you that there

were African American writers circa 1900, that food culture in Hong Kong predates the 1950s, and that research into AI began before the 1980s. Why is your search failing?

In this case, the cause is simple: the keywords you're using are anachronistic. That is to say, these are terms that people use *here* (in your hypothetical location) and *now* to describe the identities, places, and subjects you are trying to find primary sources about. But these are *not* the words that people necessarily used in the past, or in other places. "Artificial intelligence" is a term *we use today* to describe a branch of computer science. *It isn't necessarily the one used by the scientists who gave rise to this field*. They more often used terms like "Systems Thinking," "Machine Intelligence," and a number of others. As a place-name, "Hong Kong" has been in usage for a very long time, and yet *as an English-language spelling*, it has changed a great deal (decades ago, you would have been much more likely to see it spelled "Hong-Kong," with a hyphen, or "Hongkong," as one word). Likewise, the term "African American" was popularized only in the 1980s, prior to which one would have encountered terms like "Afro-American," "Negro," "Colored," and other appellations, many of which are deeply offensive today.

In this earliest stage of finding primary sources, then, your main goal is actually *not* to start answering your questions, but to use the primary sources you do find to reveal *new* keywords that you did not know existed—keywords that you can then feed back into the search process, in order to uncover *more and better primary sources*, *more and better keywords*, and most importantly of all, *more and better questions*.

This may seem like a daunting recommendation. After all, even if your search terms are "imperfect," they may return thousands—even tens of thousands—of results.

Should you *really* be expected to read, notate, and cite even *more* sources?

Not necessarily. And fear not—we'll get to source management. Right now, your goal is to identify omissions in your search inputs, so as to eliminate false negatives in those searches' outputs. Big picture, you're improving your grasp of your topic by eliminating blind spots.

Whenever you do a keyword search, ask yourself: *Are there other search terms I should be using? Might there be different spellings of the search terms I already have?* You need to be as confident as possible that the search results you are getting are broadly representative and reflective of available primary sources, and not the by-product of narrow or unrefined searches. If your search results all cluster within a narrow time period (as in our example) or were produced in a very small number of places or were written by a very small number of people, chances are that something in your search process is the cause. Phrased differently: Hong Kong existed before the 1950s, as did African American writers before the 1980s, and so the peculiar clustering of your results has nothing to do with "reality" at large, and everything to do with the *way you searched*. And if you didn't stop to refine your search, instead racing ahead to read, notate, and cite all of the materials you did find, your overall research project would be woefully incomplete.

Here are some techniques to help you use primary sources to refine your keyword searches.

The Art and Science of Keyword Search: A Few Tricks

Improving a keyword search might sound like a rather straightforward process, but there is a devilish paradox at play: most of the primary sources you discover that con-

tain "present-day keywords" (e.g., "artificial intelligence," "African American," "Hong Kong," . . .) will *not contain the other keywords that you need to find* ("Hong-kong," "Hongkong," "Afro-American," . . .). In most searches, the situation is all-or-nothing. Either the keyword you used in your search is present in the primary source, and thus the source appears in your search results; or the keyword is not there, and it simply doesn't. Here's how to get around that impasse.

Take Advantage of Category Searches

In certain databases, you might be fortunate to come across materials that are accompanied by *metadata* (data about data), crafted by librarians and archivists whose goal it is to make sources more discoverable to researchers like you. In such cases, you might find a primary source that contains the term "artificial intelligence," and then discover that it has also been "tagged" with this same keyword in the database. By clicking on this tag, you gain access to all of the *other* primary sources in that database that were categorized that way—including those that may not contain the term "artificial intelligence" at all! This is one way to get from a source that contains *only* the keywords you used in your search to another source that contains *none* of the keywords you used.

Here's what to do: after you run your search, and receive your results, sort the results chronologically, and then explore only the results that come *before* the 1980s—the time when, in your preliminary search, they seemed to disappear. As you scan through these titles, take notice: What words show up in the title? If you are able to read the work online, scan the table of contents, the preface, the introduction, and the index. What words, terms, and

vocabulary are used? Are there any words or phrases you notice that might, if you were to run them through your database search, yield other hits that your first keyword search did not? *These are your new keywords. Write them down.*

A caveat: metadata, too, is the product of context and should not be taken as definitive. Categories are cultural constructs, including those created by librarians and archivists. The categories in the metadata they create are shaped by stances and worldviews and protocols, and thus should never be taken as the "final word" on any subject. Always assume there is more, and that no one can do your work for you.

Locate Self-Reflexive Sources

In some cases, you might be fortunate to find a primary source, like a historical dictionary, that explicitly addresses the shifting nomenclature surrounding the very topic the primary source is about, outlining for you the varied ways a given idea, place, community, practice, or the like has been named and renamed across time and space. Moments like this are joyous, since they open countless doors that the researcher need only walk through!

Even in such cases, however, remember that a primary source still is subject to its own limitations. No source will document all of the variants in terminology that might be useful to your searches. No source (as we explain below) should be taken as the final word on the subject. It still falls to you to determine whether or not the primary source in question is empirically accurate. Every source possesses its own stances, worldviews, and perspectives. But for your current purpose of finding more generative keywords, the source can be useful to you whether or not its data or conclusions are accurate, so you can defer judg-

ment on those questions for the time being. The goal for now is to determine if this source will lead you in the direction of further primary sources that you wouldn't have been able to find otherwise.

Keep Track of Your Keywords and Searches

As you discover and try out more and more keywords—and even a smaller-scale project can produce hundreds—it's easy to lose track of them and get overwhelmed. The other fundamental aspect of this process involves, alas, the unglamorous world of record-keeping.

Did I try this keyword before? I can't remember. Did I try this keyword in this database? Not sure. When was the last time I ran this keyword in this database? No idea.

The dangers of missing something are real, since databases are continually updated and expanded, and since some projects can take many months—or years—to complete. You can imagine how many hours you might end up wasting repeating searches you've already done.

Fortunately, there's a simple solution: track your searches using a table. Here's how, in three steps:

1. In the rows on the left side, enter the keywords you plan to use.
2. In the column headers, enter all of the electronic databases or library catalogues you plan to explore.
3. Inside each cell, keep track of when you ran a particular search. Enter the date of your search, and perhaps also a brief note on the number of results you found.

The result is a huge time-saver, and better research results: you will always know, at a glance, which searches you have run, or still need to.

TABLE 2. TRACK YOUR KEYWORD SEARCHES

	Database 1	Database 2	⋮	⋮	Add columns as needed.
Keyword 1	☐	☑ 9/30/20			
Keyword 2	☑ 9/27/20	☐			
…					
…					
…					
…					
Add rows as needed.					

For more keyword search tips, tracking sheets, and a downloadable version of table 2, visit whereresearchbegins.com.

As you continue to use primary sources to further "educate" your questions, two other helpful things will inevitably happen: you will end up *answering* some of your questions along the way, and you will find that some are not actually worth answering. In other words, you will discover that some of your initial questions can be scrapped. This is precisely what you want to happen.

The process can feel miraculous. As you stress-test your questions, you learn more about your subject matter. And as you learn more, your *instincts* regarding your subject matter improve. In "educating your questions" you are educating your instincts. When an experienced mechanic tells you, "Sounds like something's wrong with your transmission," you listen closely because their ability to detect anomalies is highly refined. To the average car owner, any

loud noise might prompt us to ask the generic question "What's wrong?" Educating your questions will help you to hone in on "real" questions, and you'll happily discard the ones born of naivete.

TRY THIS NOW: Make Your Assumptions Visible
The goal: *To become aware of the assumptions you bring to your research project and use them to identify the problem that motivates your research questions.*

Now that you've analyzed your questions using the two techniques described above, there is still one more thing you need to do: identify the assumptions that underlie your questions, make them visible, and make peace with them.

You are not a blank slate. You arrived at your topic and your questions with a whole mess of assumptions. This is a natural thing—a good thing, in fact. After all, these are the reasons why you *thought* the topic is interesting and why you *think* your question is the right one for you. Everyone brings their own baggage to a research project.

Welcome to Baggage Claim.

Some teachers take it as their mission to "shatter" all your "illusions" about the world.

You believe the Vikings were a horde of marauding savages? Behold as I tear the veil of ignorance from your eyes!

You believe that Japanese society is homogenous? Watch as I reduce your prejudicial views to dust!

Dispelling misconceptions can be useful in many pedagogical and research settings. Yet the dispelling process, however well meaning, can have an inhibitory effect. Watching a fellow researcher or student get "disabused" can make others want to keep quiet to avoid being em-

barrassed themselves. For the researcher, the "disabuse" model can also lead to the unproductive belief that assumptions are the *enemy*—that they are shameful things to keep hidden, obstacles to be overcome, or evidence of incompetence.

Self-Centered Research is premised upon a very different approach to assumptions, as follows:

1. Assumptions should be made visible, and thus vulnerable.
2. Assumptions should not, however, be stigmatized, silenced, or driven underground, since this, counterintuitively, encourages *holding on to them more tightly*.
3. Assumptions are *fuel* to be consumed. Using them, you can achieve two goals at once: you can move in a new direction, and you can exhaust your assumptions in the process (meaning that you will eventually need *new* fuel).

Your assumptions about the world—even the most naive or negative—*serve you* at this point in the research process. To set out on a research quest with no assumptions at all would be like trying to sail on a windless day. Assumptions are the wind in your sails, and you need to channel them to keep your voyage on course.

Before evaluating your assumptions—which you will do shortly—thank them for helping you take note of things. They are the reason that any search results jumped out at you at all. They are what helped you notice a detail in a primary source. *It was the gaps between your assumptions and the world as it really is that gave rise to all those specific research questions*. Your assumptions shape your *expectations* about reality. And when those expectations are *not* met, it's time to pay attention.

So let's get to work on making your assumptions visible, and vulnerable. Here's what to do:

1. Review your most recent set of questions and ask yourself: For each of these questions, what has to be true *in advance* in order for me to ask this question in the first place?
2. List the small questions/things you noticed, and write down the assumptions you may hold that helped you notice each in the first place.
3. Make a list of the assumptions you bring to this particular question, and sort them into the following categories:
   a. Assumptions you want to work with, for now
   b. Assumptions you want to discard right away
   c. Assumptions you are unsure or ambivalent about
4. Write two lines to justify your choice for putting each assumption in a particular category.
5. Now go back to all of the questions in your list whose underlying assumptions fall into category A. Since these are built on assumptions that you, having reflected on them, feel safe in maintaining, then these questions are good as they are.
6. What about questions whose underlying assumptions fall into category B? Although you might be tempted to, *do not throw them away just yet!* If you find them to be based on weak, prejudicial, or unfounded assumptions, try to rephrase them so that they aren't. Can they be rebuilt as more grounded, open-ended questions? Try to improve them before you discard them.
7. As for questions built on category C assumptions, these fall somewhere in between. Most likely you would want to keep them in your list, but perhaps

flag them, as reminders to yourself that you want to keep an eye on them, and revisit them as your research deepens.

To keep things organized, try creating a chart like the one in table 3 for each question, in which you identify and analyze underlying assumptions and revise the question as needed.

TABLE **3.** MAKE YOUR ASSUMPTIONS VISIBLE

RESEARCH QUESTION:		
Assumption (one-sentence description)	Category (A / B / C)	Why did I put this assumption in this category? (two-sentence explanation)
Revised research question:		

Here's an example: Imagine that one of the things you noticed and wrote down was a short quotation that jumped out at you in a letter written between two friends in the year 1944, during World War II. Perhaps there was a particular passage or sentence that jumped out at you—perhaps one of the friends cracked a joke about the war, and it stuck with you.

In this exercise, it's your goal to brainstorm *why* the passage or sentence jumped out at you, by contemplating what assumptions you may hold that this quote contradicted. Feel free to speculate. You are not expected to

"know yourself completely" right away—that takes time. Perhaps you think that people living during World War II would never have dreamed about making jokes about a conflict that, by the year 1944, had already taken the lives of millions and destroyed the lives of countless more. Or perhaps you assume that, during wartime in general, people are allergic to humor itself, and prefer to carry themselves in a somber manner befitting the gravity of their situation. Or perhaps you assume that there are some episodes and experiences in history that are so horrific—the Holocaust, the Armenian Genocide, the slave trade—that no one would dream of speaking about them comically.

Write down every possible reason why you might think what you think, even if you're uncertain, without judging them as good or bad. The point here is not to "expose" your assumptions in a negative way. Rather, the goal is to bring to the surface those parts of your thinking that remain invisible yet influence how you think.

COMMONLY MADE MISTAKES
- Not identifying or divulging the assumptions motivating your research questions—for any reason, including embarrassment or self-consciousness. Remember: you're admitting these assumptions to yourself so that you can improve your own thinking. There is no external judgment here.
- Not attempting to revise or restructure a research question based on category B assumptions.
- Dismissing or throwing out category C assumptions, instead of examining them as a type of self-evidence. Remember: the gaps between your assumptions and the world as it really is can generate useful research questions.

Identify the Problem That Connects Your Questions
The goal: *To identify the problem underlying your multiple draft research questions.*

Now you're ready to take a key step. In chapter 1 you "searched yourself" to find questions within a topic. Now you'll search yourself again, but this time with more self-evidence. By now, you have completed several exercises to produce a large number of questions about facts related to your project. What you now want to figure out is, What is the problem that connects your questions?

Try to think flexibly but rigorously. What relationships can you find between the different questions and fragments you have created and gathered thus far? What motivates your search for these particular facts? You could have asked any questions about this topic—why *these*? Which questions are the most compelling to you (and which seem less important)? Figure this out, and you'll have accomplished a major breakthrough: you'll have identified the underlying pattern that connects all (or most) of your questions in a coherent whole. In other words, you will have found your Problem.

Try this procedure:

1. Lay all of your questions out in front of you.
2. Do not try to answer all those questions for now. Instead, ask yourself: What are the shared concerns that connect these questions?
3. Step outside yourself. If you were someone else looking at these questions, what might you speculate are the deeper questions that connect these small questions?
4. Write down those questions.
5. If necessary, prioritize your questions by degree of specificity or generality, as medium-level or high-

level questions. These questions should be more general than the specific factual questions you generated earlier.

The higher-level questions might not all add up. Don't force them to. But be creative, and spend some time on this. What are the parent categories that connect two or more of your questions? The connective tissue might not be obvious immediately. Finding it might require thinking counterintuitively.

COMMONLY MADE MISTAKES

- Trying to answer your multiple questions, instead of focusing on identifying the shared concern that underlies them.
- Not thinking beyond the particular topic or case, and ignoring a more fundamental concern.

SOUNDING BOARD: Get Leads on Primary Sources
When you are searching for your Problem, or verifying that the problem you've been working with is the right one for you, it might still be too early to talk to a Sounding Board about your assumptions. As we mentioned above, the tendency of experts and authorities to "disabuse" us of "bad" assumptions is so prevalent that you will probably want to delay that conversation for the time being.

What your Sounding Board *can* help you with at this stage is finding primary sources that you can use to educate your questions. Above, we've provided a few examples of databases you can use for the exercises in this chapter. Describe those exercises to your Sounding Board, and ask for suggestions of other databases or archive catalogues or repositories of primary sources you might use.

You Have a Problem (in a Good Way)

You have now taken a close look at your many factual questions and grouped them under parent categories by shared concern. You have formulated higher-level questions motivated by these concerns. The key concern that overshadows all others might have emerged in a flash or intuition. Or perhaps you're still trying to decide which of them is the most important to you. If you feel like you don't yet have enough self-evidence, you can of course repeat the exercises in this chapter. But even if you think you do, you might still wonder: How do I know when I've truly discovered my Problem?

A problem is never a fleeting thing. Rather, it is something that is sustained and enduring. To you, it can't be easily dismissed or ignored. Frida Kahlo painted surrealistic self-portraits because she was driven by a *problem*. In the world of music, John Coltrane worked on *A Love Supreme*, and Billie Holiday sang "Strange Fruit" because they were driven by problems. Bob Dylan entered a "blue period" because of a problem. Researchers are just the same.

Problems are good things. They are good to have, good to worry about, good to mull over. The problems we carry around with us can be thought of as the productive frictions that happen as we move through, and rub up against, existence itself.

Ultimately, however, the final decision can only come from you. Only you can know whether or not the cluster of fascinating questions you've generated thus far add up to a problem, or just a highly sophisticated and interesting set of curiosities.

You may well have multiple problems, but for now let's just tackle one problem at a time. We'll discuss what to do with the others in the final chapter.