FINDING A THESIS TOPIC By Peter J.S. Franks

FINDING A THESIS TOPIC is hard. It may be the hardest thing you do during your graduate degree. But there are commonalities to thesis topics—and the approaches to finding them—that might help you focus your efforts during your thesis-topic quest. Here I offer my advice and experience to help you find your way, and perhaps shorten your journey.

A thesis topic is usually designed to fill an identified knowledge gap. The topic is further honed as a question or series of questions that motivate the specific work of the thesis (e.g., experimental design, data analyses, instrument development). But not all topics and questions can become your thesis topic: you must work within the pragmatic constraints of funding, advising, collaborating, data, infrastructure, and time.

Here I'm going to give you some context, considerations, guidance, and advice for identifying your thesis topic. In the course of this discussion, I will explore approaches to identifying the scientific content of your thesis, as well as the equally important considerations of the type of thesis you might do, and offer some pragmatic considerations that will constrain the scope of your science.

At the very least, I hope that I will convince you that you are not alone in struggling with finding a thesis topic. I certainly did, and all my students have. It's a complex dance that you have never done, and you don't know the steps. I will try to get you pointed in fruitful directions, give you things to think about as you're learning the dance, and offer some thoughts based on my experiences, and those of my students. I will also say that once you have successfully identified a thesis topic, it becomes exponentially easier to do it again. The struggles you experience are

important investments in your subsequent ability to identify problems, reduce them to tractable questions, and design approaches to answer them. This will benefit you no matter what you do after you earn your PhD.

FINDING A THESIS TOPIC

I like to think of scientific knowledge as a gigantic brick wall—the Great Wall of Scientific Knowledge, if you will. As we think about coming up with a thesis topic, we typically think about building an entire wall—an edifice based on the scientific breakthroughs that will come from our research. I've found that this vision can be utterly daunting and counterproductive. How could we possibly envision changing the world with our science, when we don't even know how to do science? Viewed this way, coming up with a thesis topic is an insurmountable task.

Instead, I suggest that my students envision their contribution to the Great Wall of Scientific Knowledge as one small brick. One brick is easier to accomplish and less stressful to envision than an entire wall. And while a single brick might seem insignificant relative to the vastness of the wall, it is important to recognize that the wall only grows through these incremental contributions; every brick is important. The collective, relatively small contributions of many people over many decades are what keep the Great Wall of Scientific Knowledge expanding.

But how do you figure out what your brick—your incremental contribution— will be? Based on my own experience, and those of the hundreds of students I've interacted with, I have found that coming up with a thesis topic is the single most difficult part of doing a thesis. Forming your brick is hard.

The Scope of a Thesis

If you are just starting to come up with ideas for a thesis topic, I will tell you right now that your topic is too big. WAY too big. Your ideas are excellent, I'm sure. But it is simply not possible to fully explore them all during a five- or six-year thesis. Or even 10 theses. This happens to us all, I assure you.

Finding a thesis topic is basically a process of taking a broad question or idea and focusing it down. Often, it gets focused to the point that, from your vantage point of beginning graduate school, it seems ridiculously small and irrelevant. Even so, your thesis committee will probably tell you that it's still too big, and to just focus on the first one or two of the five ideas you presented in your qualifying exam. And ultimately, your thesis— which will be excellent, by the way—may turn out to be three chapters on only the first of your five ideas.

I've found that my students are often petrified at the notion of a *THESIS*. I certainly was when I was a student. The *THESIS* is a huge, nebulous thing that takes years and years, and must change the world. Those haggard fifth- and sixth-year graduate students who walk the halls and know everything—how could we ever possibly be as smart as they are?

The trick is to not think about the *THESIS*. Instead, think about a paper. Or part of a paper. Think of something you could do this week, that might contribute to a paper. Do a calculation, experiment, or make a figure. Read a paper, go to talks, and muse on ideas. Talk to people. By focusing on (and accomplishing) small, tractable tasks, a paper will gradually emerge—then other papers— and eventually you'll have a thesis. The *THESIS* is pretty hard to envision at the

start, but the topic will define itself as you go along. Don't stress about the *THESIS*. Instead, direct your energies to learning, and trying small-scale things. Your brick in the Great Wall of Science will begin by working with small lumps of clay.

The Science

Fundamentally, a thesis should fill a knowledge gap. The thesis topic, therefore, should be chosen to explicitly address a specific area that we do not yet understand.

This sounds pretty easy, right? Find something that hasn't been done, and do it. How hard could that be?

Very hard, as it turns out. First, you have to know what's been done. You can't identify a knowledge gap without knowing the state of the art of the field. There are numerous practical solutions to this: read, listen, and talk. You must read the literature and know the field. Read the old papers—I guarantee that there were scientists out there who were addressing the same questions many decades ago that we are still asking today. We just have better toys than they did; these toys allow us to answer questions in ways that were utterly intractable even two decades ago. Applying a new toy to an old question is often a fruitful approach to finding a thesis topic.

ture that I've found useful are (1) looking for "It's well known that…" statements, and (2) looking for papers that you wish you'd written. I know quite a number of people who have built theses on a deep exploration of "It's well known that…" statements. If you come across something in a paper that is asserted as being well known, it's worth putting on your skeptical hat and asking whether it is actually true. Some of the nicest papers I've come across were difficult to publish because the reviewers thought that the topic was already accepted lore in the field. It turns out that, just because something seems obvious upon reading, doesn't mean that it's actually been published before. Indeed, if I'm reading something and think, "Everybody knows that," I'm usually in for a learning experience. Don't accept dogma: delve into it. This will require a serious, extended dive into the literature to convince your skeptics that the particular issue is not, in fact, well known.

The second technique I suggest to students is to find papers that they wish they'd written. This helps me (as advisor) to get a better feel for what the students are really interested in, and where they are in their literature searching. As I said before, every paper has its limitations; finding a paper that you think is just about perfect, and then thoroughly taking

Reading the literature will provide the broader scientific context for identifying a knowledge gap. You can't know what's novel until you know what's been done. But reading is not the only source of such context: you should attend lots of seminars and conferences and talk to people familiar with the topic (hopefully, your advisor among them). Tapping into the community will help to give you confidence that you've found something new and interesting. Other researchers will suggest papers (often their own) and people they're aware of who can expand the scope of your reading and understanding. They might tell you that your idea has already been done; always take such statements with a grain of salt—somebody might have done something similar, but you might have a completely different approach or tools for addressing the problem. Read the papers carefully, but keep asking yourself whether the authors actually did what you were told they did—or what they say they did. I know from much personal experience that my recollection of what was in a paper (including my own papers) is often quite different than what was actually in a paper. Don't believe everything you read—or hear.

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So, read old papers. Read recent papers. But read them critically. If you read any paper critically enough, you'll find that it had limitations—too few samples, a lack of modern instrumentation or methods, incomplete analyses, etc. Once you've identified those limitations, you're well on your way to identifying a thesis topic.

Two techniques for reading the litera-

it apart, can lead to surprising insights about what was *not* done, and therefore what a useful research direction might be. I've often seen an entire thesis grow out of one or two sentences in a published paper; you have to be prepared to appreciate the importance of those sentences. You do this by knowing the literature and by trying lots of different things.

ing good questions is central to being a scientist. When I write reference letters for my students, one of the highest compliments I can give them is to say, "They ask excellent, synthetic questions." I love it when one of my students puts together disparate bits of seemingly unrelated information and asks a question that brings the bits together in an insightful

and synthetic way. These students have demonstrated the first of the several characteristics that I think will almost guarantee a successful thesis (the other characteristics include persistence, resilience, an ability to learn, and an ability to find balance in life). Academic achievement (e.g., passing tests) is not high on my list of necessary or important characteristics for success in graduate school. But the ability to ask good questions is fundamentally important; students should be asking questions—to themselves at the very least—all the time. Questions are a scientist's bread and butter.

Gaining Experience and the Importance of Failure

Reading the literature can certainly fill your early days as a graduate student. But if you don't know what you're looking for in the literature, it can be difficult to find a knowledge gap. Having a focused purpose to direct your reading can lead to much more efficient identification of knowledge gaps.

One of the best ways to find that focused purpose is to *do* things. The first two and a half years of graduate school are among the most important times in the life of a graduate student. You should be trying things—experiments, models, techniques, tools, analyses—anything you can. Many (most?) of these things will fail. But the point is not success. The point is to *learn*. And the best teacher is failure.

As the noted sage Yoda said in *The Last Jedi*, "Failure, the greatest teacher is."

In another wonderful film instance, *Meet the Robinsons*, the Robinson family literally celebrates failure. Some inspiring lines in the animated movie include, "…to fail at something doesn't mean you're a failure. It means you are trying." And, "From failure you learn. From success, not so much."

I agree with this philosophy. Failure is an essential element of success. I don't set my students up for failure; rather, I encourage activities that could lead to failure or success, but that will always lead to learning. Learning from, and building on,

failures leads to a constant refinement of your thesis topic—its subject, questions, approaches, and ultimately its results.

As you try, fail, try, fail, try, fail, you will constantly be learning what you like to do, what you're good at, how hard it is to get results that seemed trivial in the literature, and how much work it takes to make progress in science. But at some point, you may discover that the beautiful result in so-and-so's paper is actually an artifact of the technique they used. Or that you can get higher precision doing it differently. Or that there is structure in the data that nobody noticed before. Or that the parameters of the model that your advisor asked you to play with are wrong, and the published results are flawed. These sorts of things happen all the time: just because something is published does not guarantee that it was done properly. Your thesis might just lie in fixing these issues. Mine did (though I didn't know it until I was almost done), and my thesis topic defined itself as I gathered more and more data.

My students often ask me whether they should try something or not. I will almost never tell them "No," even if I think that it will never work. I avoid saying "No" for two reasons: (1) students will learn more by proving to themselves that something doesn't work than having me tell them, and (2) I could very easily be wrong, and a student will come up with a novel approach to a problem that had never occurred to me (or anyone else), and it might turn into a thesis.

Try/fail, try/fail will almost always lead to try/succeed. These are the delicious events that keep us engaged and inspired in our research. Being thoughtful about failure is an excellent path to discovery. The downside of failure is the emotional toll it can bring: failure is depressing. Learning to deal with failure is a critical part of graduate school (so is learning to deal with criticism). I will tell you right now: you WILL fail. Indeed, you SHOULD fail. Not all the time. But definitely more frequently at the beginning of graduate school than at the end. Probably the best way to deal with the emotional

pain of failure is to analyze it: figure out what you have learned from it, and use it to design the next step. Always recognize the lessons learned from failing; they are the keys to success.

The Evolution of a Thesis Topic

It is important to recognize that the topic that forms your thesis proposal may end up being entirely different from the topic that ultimately forms your thesis. In other words, a thesis topic is not an immutable *thing* that you come up with once, and you are done. Rather, over the course of your work you will discover that your initial idea was naïve (this happens to almost all of us), that an initially simple-seeming question was actually quite complex and required a full thesis to address, or that the scope of your initial topic was too broad and needed to be focused to something far more specific and tractable. Not many of us do a thesis that is exactly what we initially proposed.

The good news is that we get better at asking the right question, finding the right information, designing the right experiment, formulating the right model, or designing the right instrument. The process of refining a thesis topic is exponentially easier than finding a topic in the first place. And that's because you get better at it. You and your thesis topic co-evolve: you gain knowledge and experience, while your thesis topic gains focus and refinement. This is expected—we all go through it. So, don't get hung up on your initial thesis (proposal) topic being "right"; it just needs to be sufficient to get you to the next state of thesis-topic evolution. It needs to demonstrate that you can identify a knowledge gap, ask answerable questions, and design approaches to answer those questions. Hopefully, your advisor and thesis committee will recognize that things will change by the time you write your thesis chapters.

TYPES OF THESES

In my experience, students' thesis topics generally fall into four broad categories that I have named "vehicle," "instrument,"

"system," and "dynamics." Knowing which category your thesis might fall into can help you focus your efforts in defining your thesis topic.

Vehicle

Some students are adamant that they want to focus on some very particular *thing* for their thesis. "Vehicle" is a catchall word for the *thing* that someone wants to study. One student I interacted with just had to have penguins as a thesis subject. Another had to study earthquakes. And another, dissolved organic carbon. It didn't really matter what they did with those things—they were just the vehicle that the thesis would be constructed around. On the one hand, it's wonderful to see students who are so passionate and focused on something—it has the potential of shortening the journey to a full-fledged thesis topic. On the other hand, if there's no one around who studies penguins or earthquakes or dissolved organic carbon, the student will have a difficult time in finding an advisor and executing a thesis. There might not be resources available to support a study of this particular vehicle. And, as one of my former students pointed out, a narrow focus on a vehicle may prevent you from seeing the possibilities in research outside your vehicle. Almost any subject can be interesting! Mostly, I encourage you to be aware of and anticipate potential pragmatic issues related to a vehicle thesis; when vehicles and pragmatic constraints align, wonderful theses can emerge.

Instrument

I've encountered students who don't really care what they study as long as they get to use a particular instrument. One friend in grad school wanted to use a gas chromatograph; all that was needed was something to run through it. I've had students who wanted to use acoustics but didn't really care what the sound was bouncing off. Others wanted to use plankton ecosystem models but didn't have a particular question in mind. Others insisted on scuba diving as part of their theses. A focus on an instrument or approach can be fruitful—if you have access to the instrument and can use it, you may relatively quickly gather data that will lead you to a thesis question. For some people, designing an instrument (or model or method) will be their main thesis product. It is important to recognize, however, that for most theses the instrument is a tool for facilitating the generation of questions: it is the questions that drive the thesis.

System

In the context of thesis topics, I use "system" to describe anything from an ecosystem (e.g., kelp forest or deep sea) to a geographic area (e.g., polar systems). A student may be passionate about Arctic research but agnostic about what to actually study in the Arctic. We often receive applications from prospective graduate students who want to study coral reefs but don't explain why. And studying climate change is a popular topic, but, by itself, is intractably huge and unfocused. Knowing the system you want to study can be a singular advantage—as with the vehicle and instrument theses, the programs you apply to and the potential advisor pool are immediately reduced. A drawback is that the system does not obviously define a question or an approach, which leaves a lot of room for floundering when trying to focus in on a thesis topic.

Dynamics

The fourth category of thesis topic is "dynamics"—investigating how a system is constructed, how the parts interact, and how the system responds to perturbations. The advantage of dynamics-focused theses is that they can subsume the vehicle, instrument, and system theses. If you want to find out how phytoplankton metabolomes respond to climate forcing, for example, then the vehicles, instruments, and systems are quickly defined. One particular benefit of focusing on dynamics is that similar questions and approaches can be applied to a broad range of systems, accessing a diverse suite of vehicles and instruments. The student can take advantage of the available resources (including people) in focusing dynamics questions on systems that are being explored by local researchers, using tools that are well understood.

There is no one right type of thesis topic. Your choice will depend on you, your advisor, your program, and the resources available to you. If everyone in your research group uses a particular tool (instrument), or studies a particular organism (vehicle), the odds are good that you will, too. In general, though (and I will reveal my bias here), I have found that my students (and I) have had sustained success taking the dynamics approach to identifying a thesis topic. I have advised students working on vehicles from bacteria to blue whales, using instruments from bottles on sticks to planar laser imaging fluorometers, and systems from runoff at beaches to fronts and eddies in the California Current. But all my students have explored the dynamics of organisms in the context of environmental and ecosystem forcing. Often, the same approaches have been applied to all these systems, sampling a range of vehicles, using a suite of tools. The beauty of the dynamics approach is that it is portable—it transcends vehicles, instruments, and systems.

THE THESIS YOU WANT TO DO VS. THE THESIS YOU CAN DO: PRAGMATICS

Beginning graduate students often have broad visions of their thesis topics. The vision may include the vehicles, instruments, systems, or dynamics that they would like to study. At one point in my own search for a thesis topic I wanted to develop the governing equations for planktonic ecosystems (recognizing that this might take longer than just my thesis). Later, inspired by work done around the British Isles, I wanted to study plankton at tidal fronts (they didn't occur in my study area). I have frequent discussions with my own first-year students about what they would like to study for their theses. And I've always received

thoughtful, interesting answers. "I'd like to study basin-scale effects of climate change." "I'd like to use acoustics to study organism distributions in relation to the oxygen minimum zone." These big, often relatively unformed ideas are crucial to getting started on finding a thesis topic.

Every thesis topic is a result of desire (the thesis you *want* to do) meeting pragmatics (the thesis you *can* do). A significant part of the process of identifying a thesis topic is figuring out what is possible. There will always be constraints.

institutions (e.g., working in a group at another university to learn new techniques), or consider revising your proposed thesis topic.

Is there any funding for your project? Is there a prospect of obtaining funding? What is the process and timescale for obtaining funding? If you are working on something that is not presently funded in your advisor's group, there may be severe constraints on what new things you can do. Buying new instruments,

perspectives. Basing a thesis on existing data may also shorten the time to PhD.

Are there potential political issues or personality clashes associated with working with people outside your advisor's group? Be sensitive to these. You may have no idea how these issues arose, but it can be unwise to launch into politically fraught relationships. Hopefully, your advisor will be up front with you about potential issues before they escalate. Talk with others in your group, and in other groups.

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Can your advisor advise you on this topic? Read your advisor's papers and find out what they have worked on. Talk with your advisor and others in the lab to find out what they're working on. Brainstorm with your advisor about the topic—what reaction do you get? Is it encouraging? Neutral? Negative? For one of my proposed thesis topics, neither my potential thesis advisor nor anyone else in the department said they felt capable of advising on that subject. You need to find a topic that your advisor will be invested in, knowledgeable about, and willing to support intellectually and financially.

Are there others around whose expertise complements your advisor's limitations? Perhaps other scientists in the department have the necessary expertise that your advisor lacks. Is your advisor comfortable with you working with them? Do they have time to spend with a student? Are they going to be good to work with? Might they be interested in co-advising you? If there is no one available to complement your advisor's expertise in areas that are central to your research, you might look for support at other

reagents, parts, maintaining cultures, getting access to computing resources can all take money that may not exist. You would be wise to tailor your thesis topic to things that can be relatively easily and immediately supported (financially, intellectually, and in terms of infrastructure).

Do the instruments/data/techniques/models/etc. exist in your group? Will you be able to gather the sorts of data you'll need to answer your questions? Are the tools available in other research groups, and can you use them? Does anyone exist who can help you troubleshoot? If it will be a struggle to gather data, perhaps it's time to rethink your thesis topic.

Will you be able to gather the types of data you need to answer your questions? Do the right instruments even exist? If archived data exist, will you have access to them? Can you get ship time to collect data at sea where and when you want? An approach that often works is to join a larger data-gathering effort in the group, or participate in a multi-group collaboration. You may also benefit from analyzing existing data in new ways and with new

If your proposed topic involves wading into a political quagmire, I would suggest that you re-evaluate your approach.

Are other people working on the same question? Is there potential that they may scoop you, or make important data inaccessible to you? If you are working in a larger group, all focused on a particular project, it is important to create written data-sharing, data-usage, and authorship agreements to protect students during their thesis research.

Will your investigations rely on a tool/method that someone else is currently developing? I strongly discourage students from basing their thesis research on someone else's new or in-development tool or technique. Developing these tools and techniques always takes longer than someone thinks, and they are always much more difficult to calibrate than anyone anticipated. It can be fatal for a student to base a thesis topic on a tool that has not been thoroughly tested and calibrated. That being said, if the thesis topic is to develop/test/calibrate a new tool, then you may be well on your way.

While these pragmatic considerations may seem tedious, they are real and important. Thinking hard about the realities of carrying out a particular thesis project may suddenly narrow the scope of your initial grand idea. Typically, it will be narrowed to something more practical and achievable. Considering pragmatics in the face of desires can be an important step in identifying a thesis topic—even before your specific questions have been formed.

CHARACTERISTICS THAT HELP FIND A THESIS TOPIC

I have known a number of students who came into graduate school knowing exactly what they wanted to do. A few of them did just that: their desires aligned with the resources available (advisors, instruments, funding, etc.). However, I have found that intellectual flexibility and a willingness to compromise are strong determinants of a student's success in finding a thesis topic. Most of us don't do the thesis we want—we do the thesis we can.

One characteristic that I've observed among successful students is that they can take an idea/data/technique/model/ etc. and make it their own. The same data set given to three different students will produce three different theses; each student interprets the data through their own lens of knowledge, context, interest, ability, and experience. Many of my students have found thesis topics by analyzing data that others have already published on but noticed things in the data that had been overlooked. Sometimes the approach is as simple as plotting the data in a way that should support the prevailing hypothesis—and it doesn't. Then the fun begins. This is one of the hallmarks of PhD students: they can engage scientifically with a problem no matter the vehicle, instrument, system, or dynamic. Science is a process for finding answers; a significant part of graduate school is learning how to do science. Finding a thesis topic is essentially finding something to study scientifically.

Different programs will favor different student types, with different constraints on finding thesis topics. European PhD programs are often only three years long—students must be given thesis topics at the outset, otherwise they will never finish on time. This approach definitely shortens the process. But it also makes it harder for students to make their theses their own: the try/fail period is curtailed or eliminated; students must rely on their advisor's experience on the topic. In North America, it's more common for students to identify their own topics. This naturally takes longer than being given a topic, but there's enormous scope for learning during the try/fail stage. Some programs are designed to accommodate flexibility of students' interests—students can change advisors and topics as they learn more about their fields, and their interests become more focused. This is not true of all programs or situations: sometimes, a student who wants to change topics may have to leave the program. Find these things out before you enter a graduate program.

Faced with such a dilemma, it's worth asking yourself whether you're willing to work on a topic that might not interest you as much as you had initially thought. But recognize that sometimes the topic of the PhD is less important than the degree itself. Your PhD does not have to define you or your science for the rest of your life. Doing a good job on your PhD will open doors that may allow you to pursue your interests more fully in the future. The PhD is a stepping stone to the rest of your life—which will hopefully be much longer than the time you spent doing your PhD.

In that sense, I suggest that you worry less about the specifics of your thesis topic and more about what you will bring to, and gain from, its execution. What tools will you acquire during your research that can be applied to other topics, or in other venues? What ways of thinking will you be able to generalize to other systems? My own thesis was on red tides and harmful algal blooms—a field that I am only

peripherally involved in 30 years later. An alternate interpretation is that my thesis was about physical-biological interactions—red tides were just the vehicle for the study. In that sense, I have spent my career pursuing my thesis research, but applying it to different vehicles and systems, and using new instruments for my investigations.

So, I encourage flexibility in your search for your thesis topic. It may not end up being exactly what you wanted or envisioned. It will almost certainly be smaller and more focused than you anticipate. You may have to compromise. But you should work to make it yours, whatever you do. Put your idiosyncratic imprimatur on your work, whatever it is. Be open to learning. Be open to recognizing that failure might be your guide to a different, untrodden path. Read. Try. Talk. Ask. Listen. Be curious and creative, hard-working, persistent, resilient, and flexible. Be skeptical of what you read and what you're told. You'll find a thesis topic. And you'll do some wonderful research. I'll look forward to reading about it. ⬡

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