Choosing a research topic and formulating a research question

# NOTE: This paper was prepared originally by Peter Neal; it has been edited to enhance it, and to suit this iteration of GSOE9011. Peter’s contribution is acknowledged, with thanks.

**Introduction**

Sometimes you will be given a specific question to answer in your research but other times you will simply be presented with a topic or have to choose a topic yourself. Thabane *et al.* (1) suggests there are six risks of not carefully framing your research question:

1. It can cause errors in the design of your project,
2. It can create confusion and make it harder to develop a clear methodology,
3. It makes it harder to interpret the results of your study,
4. It makes it harder to determine whether your theory follows from the results,
5. It can jeopardise publication of your results, and
6. It makes it harder for others to cite your research and include it in systematic reviews of your topic.

Therefore, it is very important to take some time to isolate your research topic and then frame your research question.

**Background**

In GSOE9011 you have been given the choice of topic from among 20 engineering research challenges (ERCs). People interesting in proposing research under a particular challenge will be formed into teams of, typically, 4-6 members.

This limited set of topics is analogous to the scope of some of the funding schemes offered by government, non-government and industry funding bodies (e.g. [ARC](http://www.science.gov.au/scienceGov/ScienceAndResearchPriorities/Pages/default.aspx) and [ACARP](http://www.acarp.com.au/Media/ACARPResearchPrioritiesNewsletter.pdf) research priorities). These organisations publish a set of funding priorities (usually once a year) which set out the sorts of topics which they would like researchers to investigate. These topics reflect strategic issues, current events/crises or ongoing challenges that the funding body has prioritised. Researchers interested in receiving funding from these organisations must demonstrate in their proposals how their project will impact one or more of these priority areas.

**Choosing your topic**

In Engineering, your research topic is typically given to you by a supervisor (at work or at university), especially during your formal study, or early in your research or professional career, or you may get to choose it yourself, as happens typically with career advancement.

In the university context, each academic will be interested in different topics and so choice of topic partly comes down to the choice of supervisor. You can find out information about the academics supervising projects in your specialty through the [UNSW Find a Researcher page](https://research.unsw.edu.au/researcher), or by scanning School web pages.

However, if you get to pick your own topic, start by doing some general reading in the research to understand what the current challenges are in that field (2, 3). One way to do this would be looking at the contents of industry or professional society magazines (e.g. The Chemical Engineer, SPE Journal). From these challenges, pick one that interests you and start doing more detailed reading on that topic. The UNSW Library is a great place to begin your research.

You will be spending a good portion of your time working on your chosen topic and motivation can flag over the months or years. Therefore, in selecting a research topic it is wise to choose something that is **relevant or interesting** to you (4). At the same time, remember that, early in your research career, the project may be more about developing generic research skills, and certain techniques, than about the specific context of the research.

The other important criteria for a research topic are that it is **creative or novel** (4, 5) and that its scope fits the available resources (e.g. cost, time, material resources). By its nature, research is about discovering new things or understanding things in new ways; therefore, in choosing your research topic you should be looking for an area where you can make a novel contribution. Your topic should be centred on a gap in our knowledge of the research area.

For example, it would be hard to get funding for a project that is simply about measuring the pH of soil – this is a mature technology and can be found in textbooks or Wikipedia. However, there may be funding available for developing a system for the remote monitoring of soil pH as an early warning system for the detection of leaks from a tailings dam and simulating/measuring the performance of the system under different conditions. In this example, the basic technology is mature, but the gap is in the application of the technology to a new context.

Regarding the **scope** of your project (6), you should ensure that the question you wish to investigate can be investigated with the time, equipment, consumables and personnel you have available. You can limit the scope of your project by focusing it to a particular context. Judging this may be difficult, but you might seek input from experts to gauge feasibility with respect to time, or simply and perhaps sufficiently, give enough time for discussion within your team. **This is a particularly important point for your team to consider.** Teams in GSOE9011 regularly try to propose a lifetime of work, a research question so large and vague that it may never be answered. Three keys words here: constraint; restraint; focus.

**Framing your research question**

Once you have your research topic, you can start thinking about formulating your research question. Using the example above, you might be tempted to simply restate the research topic as a question, such as “How can we measure soil pH near tailings dams?” However, this question is something that a trained technician or sales engineer could answer – it is not a research project. Spend some time brainstorming or mind-mapping different questions about by your topic (7).

This raises an important point about your research question; it must be something that **can’t simply be answered by retrieving existing data** (2, 5, 6, 8). Your research question must be directed at a gap or gaps in our knowledge of the topic. Further a proper research question should require the **development and testing of theories** that can predict or explain natural phenomena or the performance of systems (8).

Think about a standard lab report. A question that simply asks for data only requires the results section and not the discussion or conclusions. Yet, it is the discussion and synthesis of your results into conclusions that propose theories with explanatory power that distinguishes your project as research. Therefore, in framing your research question, think about what you hope to be able to demonstrate about the phenomena or performance of the systems you are studying.

In framing your research question, consider the following questions (3, 6, 7):

1. What – What is your topic?
2. Why – Why is this topic important? Think about how researching this topic will have an impact at the level of community/state/nation/industry/world.

Then there are the **context questions that can tighten the scope of your project** (6).

1. Where – Is your project connected to a particular geographical context? For example, the development of natural gas pipeline networks in the North Sea.
2. When – Is your project about a particular time period? For example, development of natural gas pipeline networks in the North Sea during the 1960s and 1970s.
3. Which – Is your project focused on a particular species or technology or material? In the pipeline example, we're looking at pipelines (not ships) and at natural gas (not oil).
4. How – Is your project evaluating/using a particular methodology, technique or technology? For example, X-ray CT vs SEM.

**Finally, you should (must) ensure that everything in your question is well defined and not vague or unclear** (2).For example, rather than “How can the performance of this scramjet be increased?” instead ask “How can the power output of this scramjet be increased?” This is perhaps the most problematic and important task in this course. Developing a well-framed research question is not easy, but your team must work hard and together to achieve it.

You may need to provide a brief paragraph of text to clarify the question, and you are welcome to do so, but a good research question can stand alone.

In addition to the above suggestions, there are some formal processes for framing and evaluating research questions, such as PICOT (9, 10) and FINER (1, 11) that you might find useful.

**Conclusion**

Choosing an interesting and novel topic is a big help in providing a stimulating research project and enabling you to make a significant contribution to your field. By taking time to think though the selection of your research topic and carefully framing your research question, you will ensure that your topic is relevant to the community, industry and/or funding bodies, as well as simplifying the execution, interpretation and communication your research.

**References**

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**Further reading**

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**Marking rubrics**

| **Criteria (and weighting)** | **Description** | **Marking** |
| --- | --- | --- |
| **Question (10%)** | Is the Research Question actually posed in the form of a question? | No (0) Yes (0.5) |
| **Scope (10%)** | Can the Research Question be answered reasonably through a 12-month industry or university research project? | No (0) Yes (0.5) |
| **Gap (40%)** | Why is this topic important? Does the Research Question target a gap or gaps in our knowledge of the topic that can't simply be answered by retrieving existing data? | Unsatisfactory (0) Satisfactory (1) Outstanding (2) |
| **Hypothesis (40%)** | Does the Research Question require the development and testing of theories that can predict or explain natural phenomena or the performance of systems? | Unsatisfactory (0) Satisfactory (1) Outstanding (2) |