Thesis / Dissertation Topic (research question)

There are three approaches to **novelty** (by analogy to generating theory). The first is to aim for a **specific outcome** nobody has achieved before. In research projects, there is a clear goal, but a risk that another person or group working on the same problem might beat you to it.
The second approach is to pick a **specific subject** you want to investigate, and identify a question or problem that has not been addressed before on that subject. This could be a specific group of people, a specific situation, or a new class of data materials.
The third is to apply a **new methodology**. There could be a subject which has been widely studied, but never before using a particular approach or methodology. The methodology as such does not need to be new, just new to the problem you are addressing.

You may have found an interesting problem to work on. At some point you will need to put it in context with existing research. This is because

- You want to make sure nobody has already done it
- You can learn from the methodologies used by other researchers
- You need to justify why your research matters

So sooner or later you need to do some reading to find out what other people have done and so far as possible what people are doing concerning your chosen problem. You need to track and consider recent and relevant publications systematically. If you discover that another scholar has completed some research on a topic similar to your own, don't despair. Carefully read that material to gain an understanding of what it is that the earlier research has accomplished and consider ways in which your project could further develop the topic or could approach the topic from a different perspective. You are likely to find that the former research is not the same as what you are considering. You might try to find out about any current work being pursued in relation to your chosen problem, for instance from conference agendas and papers. If you should discover that another scholar happens to be working on the same topic you've chosen, you might consider contacting that person to get a better idea of whether and how your ideas overlap. Be careful, however, not to give away too much of your own thinking on the topic as you conduct such a discussion.

The ideas do not need to be completely unique: you could start with one idea, then consider multiple variations on your preferred topic. Take some time to think of as many different variations as you can. Also, you might think of several possible topics that might be interesting enough. This takes away the pressure to choose the perfect topic immediately, and will open up potential avenues of exploration you may otherwise not have considered.
Ask yourself, what is the simplest first step that would need to be taken? Figure out if it is possible and manageable.
Testing possible topics may involve:

- Doing simplified mini-investigations
- Checking that you will have sufficient access to equipment, materials, people or information
- Reading relevant literature
- Asking relevant experts

For example, if you have a research idea based on the assumption that you will have access to a database, then it makes sense to check (before you commit to the project) that you will actually have access. If the material or the database is in a remote location from your university, will your whole project depend on being able to get there and staying for as long as necessary?

If you hit upon an idea which seems viable, you may not be familiar with the methodology that is needed. In that case you should ask for an expert opinion from someone with experience, who can tell you what you need to consider.

How to Choose Topic Criteria

A thesis/dissertation is an idea or theory that is expressed as a complex argument for which evidence is gathered and discussed logically. One of the most important concerns in choosing a topic is that the topic speaks to an area of current or future demand. A good topic is an idea that is in need of development, testing, or in some cases falsification/refutation. Your topic should be of interest to you for months or years, but also to your advisor, and the research community. Try not to pick a topic that is too exotic or overly specialized, as it will be hard to generate interest in your results. Your institution may advise you about 'acceptable' or desirable topic areas. Marketability is subject to change, so don't feel compelled to choose a topic that reflects a current craze in your field. When searching for a topic, remember that your project should attempt to solve a real and complex problem, and for that purpose should contain solid theoretical substance as well as empirical results. It should be connected in a meaningful way to existing research, and you will need to demonstrate this.

Choose an adequately narrow, well-defined topic that branches out in a new direction. For the right degree of precision in defining a topic, you will get ideas from the advice about research questions. The size of your topic is important; you want it to be manageable but not so narrow that you will be limited while researching. The data you collect (or research materials) should be extensive enough to enable you to work on them in a variety of ways. If you should happen to get through faster than you had anticipated, you might do well to have some reserve materials that you could use at a later stage to extend the scope of your project work.

Topic Generation

It can be helpful to choose a broad subject area as a preparatory stage. As you learn more about that subject area, you will narrow down toward a more specific subject or topic. As you search for your topic, you may be able to use the materials and ideas from earlier coursework and earlier research projects. It may be helpful to discuss your ideas with others who are at a similar stage, and with people who are more advanced. Make an outline to group your ideas and locate the crux of argument or problem on which you would like to focus. It can be helpful in some cases to think of as many researchable questions regarding your chosen problem as you can, to write these down (they can cover several pages), and then try to find some order or relationship between them (topic map!). Drafting a written proposal can help you define problems, outline possible solutions, and identify evaluation criteria.

Maintaining a journal can be helpful in tracking your ideas for topics. You can keep notes in the journal as you research possible topics and write down questions to which you would like to find answers. It is very important to have an awareness of ongoing and directly related research. Be aware that new work is constantly being published, and you will need to track this regularly. You might consider setting up interviews (in person, by telephone/Skype or via e-mail) with leading figures who have researched and written material relevant to your chosen problem. When you record such an interview, and gain the interviewee's approval, you could even cite it in your work. Or you may just use the contact to grow aware of aspects of the problem you had not considered, and whether the leading figure knows of any relevant work being done on the problem at present.