Guidance for Topic Selection

This guide is focussed on those students doing a taught Masters. It focuses on two separate issues: 1) What is a suitable 'research' topic for a dissertation and 2) scoping a research topic – making it neither too big nor too small. The Internet of Things is taken as an example throughout, but you could replace it with any topic in computing and information systems with the same effect.

What is 'Research'?
Research has many meanings. For this subject, we mean specifically scientific research. Science in computing has various approaches, across information systems, formal computer science, software engineering and empirical computer science (e.g. visualisation, information retrieval, data mining).

We are aiming for you to do a small piece of scientific research. In scientific research, you should aim to ask a question about the world that you can answer with a 'yes' or a 'no', and perform an impartial test to arrive at that answer. In scientific research, understanding the right impartial test to use is essential – make sure that you know what the common methods are in your area of interest is a key issue.

Where knowledge is highly uncertain or an area is novel the main contribution of a piece of research is to arrive at an initial understanding. From this point traditional scientific questions can be asked. This is a special form of research that is often called formative research, and will bring together knowledge of the world in a way that explains how it currently works. It is necessary where the factors that influence behaviour or performance in an area are not known, and primarily is found in how people or nature influence the performance of systems. Formative research is common in information systems and infrastructure engineering. An example of formative research might be "identifying the three biggest problems in using the Internet of Things to reduce domestic energy use".

You cannot do journalistic research, which intends to write opinion or insight pieces that synthesise speculation or opinion on a topic. So, for example "what is the future of the Internet of Things", "how will the Internet of Things improve business competitiveness", or "problems with algorithms for the Internet of Things" are journalistic research. These topics will involve speculating on a future benefit or current problem with a technology. Finding concrete evidence to prove that a future will certainly happen is almost impossible. This research will not itself produce new knowledge of how computing systems function today. The research will be written in a way that is descriptive and does not have a question that can be answered with a 'yes' or a 'no'. Look at Lecture 1's slides again with this knowledge.

You cannot do merely personal research. Personal research means that an issue is new to *you*, and you discover new information that others already know. This is practical and useful work, but it is not scientific research. New knowledge is not created, and there is not a way to falsify (prove untrue) or validate (prove true) the knowledge you find. For example, "programming the internet of things in Python" requires practical knowledge you may not have, but doesn't necessarily create new knowledge.

You should avoid deep science research. Deep science research requires an extensive knowledge of an area and its methods. You will be unlikely to become expert enough in an area to do complex work. For example, “using reverse NAT MAC address lookup to enhance security robustness in IOT web search” will involve a lot of different expertise that you may not have. Gaining a lot of new knowledge on a variety of topics is hard, and this may not be achievable in the time you have available. This leads onto the issue of *scope* – choosing a topic that can be done in the time available to you.

Scoping Research

You need to be careful in the amount of time you have available for your project. If you have only 25 credits, you have a nominal 400 hours available for your research project. This is only a small amount of time to plan, execute and write-up the work.

You will need some critical material for most research work: 1) the knowledge of what is currently known and not known on a topic to arrive at a question to answer; 2) some data to experiment with (for numerical areas such as data mining), or the means to create data through an experiment; 3) a method of analysis to test data and arrive at a ‘yes’ or ‘no’ answer. If your emphasis is on building a new mechanism or software, you will add a fourth requirement, 4) the time and knowledge to make the new thing.

It is particularly unwise to try all these steps in an area you do not know. If you are short of time (e.g. 25 credits), choosing to modestly extend an existing method to a new area or improve the method, and arrive at a small contribution to knowledge is very wise.

Some tips for reducing a project’s size are to work with readily-accessible data or materials and reproducing an experiment, but adding one new factor, or changing an existing one. On the other hand, if you have good knowledge of an area and more time, you might take on a small known issue and try to improve the world. Discuss these issues with your potential supervisor (if you have a project you are starting).

You need to study the existing literature, which should happen across this subject, to identify what is already known or not known. Any initial topic will need to be refined as you progress through the subject, but you need an initial place to start. Let’s take the issue of the Internet of Things as a starting example.

Don’t be too vague or general. “Improving the internet of things” is too vague, and big, and cannot be answered with a yes or a no. This is a not specific enough for a piece of scientific research.

Consider your skills. If you want to improve IoT and you know that sensor accuracy is an issue, then that’s more specific than our general idea. Perhaps you might start from there “improving sensor accuracy for the internet of things” is better, but it might require either software or hardware skills. Which do you have? Consider your skills, and think about the problem in a way that you can do. So, if you are a computer scientist, you might ask “how can software better handle sensor inaccuracy in the internet of things”? That’s getting closer, but not quite at the level of detail for a yes or a no…

Use ideas from other areas. This might need a bit of reading. Is there an existing approach that is being used somewhere else that might help? Has it been used before? If not, maybe it's a solution! 'Noise' is a known idea in signal processing that might help in the IoT; perhaps that would help. "Can noise identification help reduce system errors due to sensor inaccuracy in the internet of things is better" (and notice you almost straight away have a potential yes/no answer!).

Even more particularly, **think about a specific method** – I've (personally) done some work on identifying anomalous data when using GPS to track location. Maybe that method might help when our sensors are unreliable? "Can GPS anomaly techniques help identify faulty sensor readings in basic IoT sensors" is close to a MSc topic, and might still be too big, but you should be able to find that out and get it right in 2-3 weeks of reading.

For formative research, the path is similar. You again need to know the area, you need to see what has been done in similar areas, and you need to think about a specific method. It might be, for example, that you are interested in problems in using IoT sensors to reduce energy usage in homes (see above), but there are multiple angles (e.g. family use, power generator problems, power retailers, etc.). You can't cover all, but some won't have been done. Similarly, information systems or infrastructure engineering use different methods for different problems, and it might be that someone else has used a method you don't know, but not a method you do. Will you find the same thing?