2. Philosophy of Science: Experiments and Falsification
Science is all about...

Trying to falsify hypotheses (Popper)

Paradigm shifts and scientific revolutions (Kuhn)

"Anything goes"? (Feyerabend)

Scientists try to explain things, but what makes a good explanation?

What does all this imply for writing a PhD thesis?
What science is not

The oldest story of what scientists do:

Passively observe the world

Notice regularities

Come up with universal laws that explain the regularities

Use these laws to predict future events

This story doesn't work because induction isn't a reliable route to real-world knowledge, and there's no such thing as passive observation.
Falsificationism: Karl Popper

Popper understood the problem of induction.

Could see that the observe-and-induce model wasn't a good account of how science worked.

Noticed an important asymmetry in scientific law discovery:

Lots of positive observations prove nothing, but one negative observation can disprove a theory.
Falsificationism: Karl Popper

Popper therefore claimed that science was all about trying to falsify hypotheses.

This idea has three major consequences:

1. Experiments should be designed so that they can test and potentially reject hypotheses.
2. Scientific knowledge is the list of hypotheses that have not yet been falsified: we're not 100% confident of anything.
3. To count as a scientific proposition, something has to be potentially falsifiable.
Popper was also interested in how science, over time, appears to make progress in approaching the truth (i.e., how are modern theories better than old theories?).

He described an iterative process:
1. Identifying a problem
2. Proposing tentative hypotheses
3. Error elimination through rejecting hypotheses
4. Return to step 1 with a better take on what the problem is.

There is an evolutionary character to this part of Popper's account.
Problems for falsificationism

Is this what scientists really do? Try as hard as they can to falsify their most cherished hypotheses and then throw them away immediately? In practice scientists hold on to ideas for a long time despite empirical grounds for rejection.

Popper is clear on how hypotheses can be rejected but doesn't say much on where hypotheses come from in the first place. Why test this and not that idea?

Are we content with an account of knowledge that allows any proposition not yet falsified a potentially equal status? Are we confident that the Popperian process will lead us closer to truth?
Good points to take from Popper

Falsifiability is important: if an idea cannot be tested through experiment (e.g., psychoanalysis, religion) it is outside the scientific realm. This helps us to define what science is.

Absolute certainty is something we probably have to give up on. The argument against induction shows that we cannot have mathematical precision. The history of science shows that even the most elegant theories get modified or replaced.

Confidence in our hypotheses and theories is something that builds up over time as they stand up to repeated testing.
Scientific revolutions: Thomas Kuhn

Kuhn began with the observation that real scientists don't do what Popper says they should do.

They don't drop their favourite theories when new empirical evidence suggests those theories are wrong.

Most science is done within a particular "paradigm" and scientists are resistant to changing paradigms.
Kuhn described three stages of science:

1. Pre-science: sorting out an initial perspective on a new problem.
2. Normal science: doing experimental work that usually supports the dominant perspective. Inconsistent results are often treated as mistakes.
3. Revolutionary science: after enough results come up that are difficult to explain, some creative individual initiates a "paradigm shift" that results in a whole new way of seeing the problem.
Scientific revolutions: Thomas Kuhn

Examples of paradigm shifts: Ptolemaic to Copernican astronomy, the move from Newtonian to Einsteinian physics, the "cognitive revolution" in psychology.

Kuhn said that opposing paradigms are *incommensurable*, i.e., they're fundamentally different ways of viewing the world -- they can't both be entertained at the same time.
Problems for the Kuhnian view

People doing normal science sound a bit like they're getting nowhere, i.e., they're just supporting the orthodox view.

If opposing paradigms are incommensurable, how are we supposed to rationally choose between them?

Doesn't this make scientific paradigms sound a bit like changing fashions in clothes or music? The new paradigm is "cool", the old one is boring? How can this square with the idea of scientific progress, of closer approximations to the truth?
Some points to take from Kuhn

Science does have movements, fashions, and paradigms. Your work will be better if you're clear on which ones you belong to.

A minority of us will be involved in starting a brand-new paradigm: an exciting prospect! But how can we tell the difference between controversial but fruitful ideas and ideas that are just crazy?
Like Kuhn, Feyerabend began with the observation that real scientists weren't doing what Popper said they should do.

Feyerabend studied various scientific revolutions from history.

He found that not only were scientists not following the Popperian method, they weren't following any method.
Feyerabend argued that it was fruitless to try to formalize "the scientific method" as a well-defined procedure: he would not be happy with the goal of these lectures.

He noted that scientists try all sorts of creative and sometimes irrational strategies in their efforts to make discoveries and to see one hypothesis win out over another.

Feyerabend famously said that "anything goes" was an appropriate summary of the history of science.
Problems with Feyerabend's views

Feyerabend's ideas raise real problems for the "demarcation problem", i.e., the question of what counts as science and what doesn't.

If there's no special method that unifies different scientific efforts, then who says astrology or voodoo can't count as sciences?

Feyerabend believed that science did not deserve any special status: it was just another human project among many.
A fictional example

Think of an Agatha-Christie-style murder mystery as a scientific problem. We have a dead body, an English country house, and some suspects.

What recommendations would each of our philosophers of science make for solving the mystery?
A fictional example

**Popper:** a good scientist will treat everyone as a suspect. The evidence won't prove that someone is guilty, but the right piece of evidence will exonerate them. Rule out one suspect after another, and whoever is left must be guilty, although you're not 100% certain of it.

**Kuhn:** normal detectives will assume that the butler did it, and collect evidence to prove that idea. Revolutionary investigators will propose the radical paradigm of suicide.

**Feyerabend:** you're on your own! There are no rules; you figure it out.
Prediction versus explanation

Good theories are those that predict and explain the world.

Prediction is relatively easy to test: either a theory allows you to predict future events or it doesn't.

But the world is a complicated enough place that we don't always expect our theories to make precise predictions, e.g., weather.

Do you agree: is successful prediction an essential characteristic of scientific theorizing, or just a desirable one?
Scientific explanations

Sometimes it seems the best we can do is try to explain something after the fact (e.g., could not have predicted the global banking crisis, but can explain what happened).

But what makes for a good scientific explanation?

The traditional account is the "deductive-nomological" model of Hempel and Oppenheim.
Deductive-nomological explanation

I drop a stone and it falls to the ground. What's the best explanation for this?

1. The stone has mass.
2. The earth has mass.
3. All objects with mass will attract each other in proportion to the inverse-square of the distance between them.
4. Releasing my grip on the stone removed the force that was holding it up.
5. Therefore: the stone fell towards the earth.

This is a deductive argument, with at least one of the steps (3) taking a nomological or law-like form.
More pragmatic models of explanation

What counts as an explanation in ordinary discourse? Think of explaining how to find your house, how to open a tricky lock, or why you did something?

Much scientific explanation is mechanistic in character: "the hip bone is connected to the leg bone", etc.

An important form of explanation in biology is functional explanation: *what's it for?*

Historical explanations are also present in biology and obviously the social sciences: "The Napoleonic wars happened because..."
Good explanations?

In summary: useful explanations can take many different forms.

The D-N model is generally not taken seriously any more, although it's still the implicit model of scientific explanation we all learn at school.

Problems include: not taking causation seriously, not covering various forms of explanation from ordinary language, and not accounting for the appeal of simplicity.

A more promising view of scientific explanation is to link it explicitly to the practice of model-building: more on this next lecture.
Implications for writing a PhD thesis

From Popper: if you're going to make a contribution to knowledge, you typically need to put a hypothesis to some kind of test.

Be clear about exactly what that hypothesis is, and about how experimental results could either support or falsify the hypothesis. Conducting "business as usual" won't lead to new knowledge.

Going too far with Popper, though, you might see your job as trying to refute as many hypotheses as possible. If your PhD work attempts to refute solid theories without any special reason for expecting them to fail, you won't do well.
Implications for writing a PhD thesis

From Kuhn: think about what paradigm your work sits within. Use this to highlight the assumptions that underlie what you're doing.

Just possibly you may find yourself doing "revolutionary science", i.e., proposing a controversial new way of looking at the world.

Beware the temptation to imagine yourself as the creative revolutionary though: there are more kooks out there than paradigm-builders.

From Feyerabend: there is a sense in which the rules on how to do good science are always up for debate.
Suggested reading


