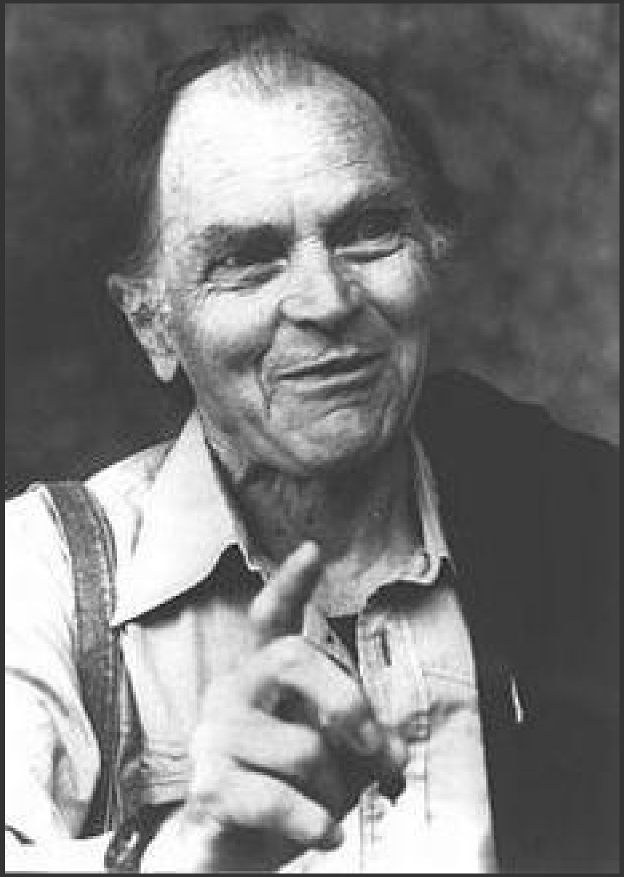
Against method: Paul Feyerabend

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.

Against method: Paul Feyerabend

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 this lecture.

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.

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.

Scientists as model makers

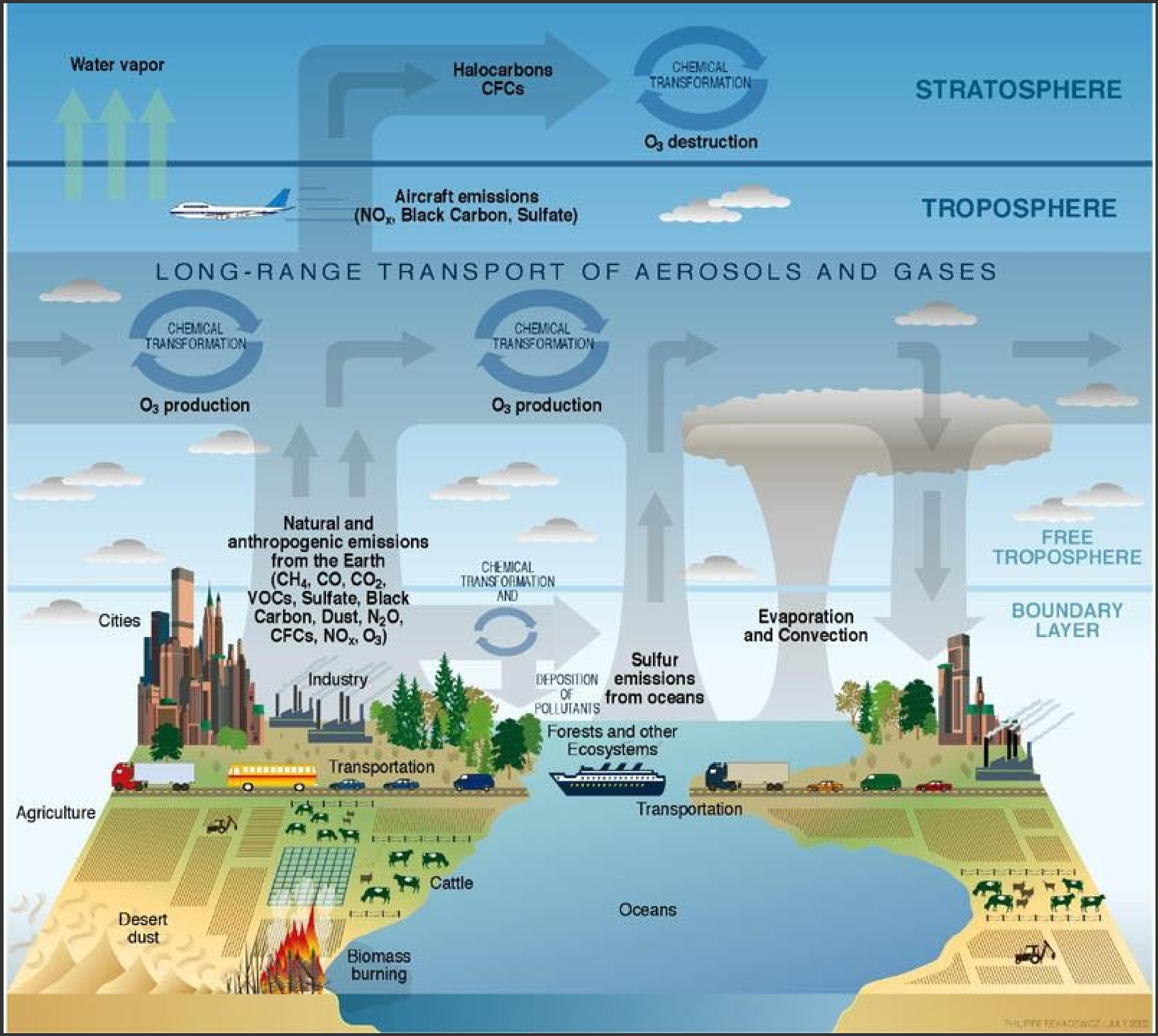
Why don't scientists spend all their time in the field or in the laboratory?

Much of the output of scientists is in the form of verbal, mathematical, and computational models.

This is very different from collecting data.

It's also different from hypothesis testing in Popper's sense.

So why do we do it?


Water vapor
Halocarbons
CFCs
CHEMICAL TRANSFORMATION
O_3 destruction
STRATOSPHERE
Aircraft emissions
(NO_x, Black Carbon, Sulfate)
TROPOSPHERE
LONG-RANGE TRANSPORT OF AEROSOLS AND GASES
CHEMICAL TRANSFORMATION
O_3 production
CHEMICAL TRANSFORMATION
O_3 production
Natural and
anthropogenic emissions
from the Earth
(CH_4, CO, CO_2,
VOCs, Sulfate, Black
Carbon, Dust, N_2O,
CFCs, NO_x, O_3)
CHEMICAL TRANSFORMATION AND
DEPOSITION OF POLLUTANTS
Sulfur
emissions
from oceans
Evaporation
and Convection
FREE
TROPOSPHERE
BOUNDARY
LAYER
Cities
Industry
Forests and other
Ecosystems
Transportation
Transportation
Agriculture
Cattle
Oceans
Desert
dust
Biomass
burning

What's a model?

A simplified abstract account of real objects, processes or phenomena.

Consider:

1. A written description of the theory of evolution.
2. A set of differential equations describing the rate at which gene frequencies change over time.
3. A genetic algorithm implemented as a computer program.

Science's way to deliver understanding and prediction.

Models and theories provide structure

Models / theories provide a framework that holds together individual hypotheses.

Models / theories are inter-connected sets of propositions (or statements, or hypotheses).

Inter-relations *between* complementary models and theories are the basis for viewing science as a unified body of knowledge.

An example from economics

1. Some people have widgets that they don't need.
2. Others have no widgets but want one.
3. A currency of some sort allows these people to trade.
4. Everybody has a reserve price or a limit price based on how much use they would get out of a widget.
5. Everybody is rational.

What follows?

This is (more or less) a model of market theory.

Questioning the theory means questioning one or more of these statements. They form an inter-connected story.

Falsifying a model?

Kuhn and Feyerabend both said that Popper's account of scientists trying hard to falsify hypotheses and then discarding them did not match what really happens.

One of the problems is that no single hypothesis gets dropped when we find contradictory evidence: we can always adjust one of the secondary hypotheses that went along with it.

This points to scientific statements being tangled up in networks. These networks constitute models.

The Quine-Duhem thesis

"Our statements about the external world face the tribunal of sense experience not individually but only as a corporate body" (Quine, 1951).

In other words, you can't reject just one hypothesis. They stand or fall in groups.

Doing science is the pragmatic process of deciding which groups of hypotheses (i.e., models) need to be modified or dropped.

Quine's attack on empiricism

In the early 20th century the dominant philosophy of science was "logical positivism": knowledge was either analytic truth or to be justified by being reduced to statements of sense experience.

Willard Van Orman Quine's criticisms of logical positivism led to a view of science as pragmatic model-building with no bedrock of analytic truth to support it.

A web of connected propositions?

Quine's view of science suggests a network of inter-connected statements.

The network changes and is revised over time as we make new discoveries.

At the core are heavily inter-connected claims that would require major revision of our knowledge if they turned out to be wrong (e.g., the existence of atoms).

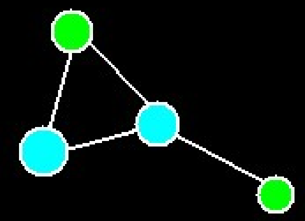
At the periphery are newer, less certain ideas.

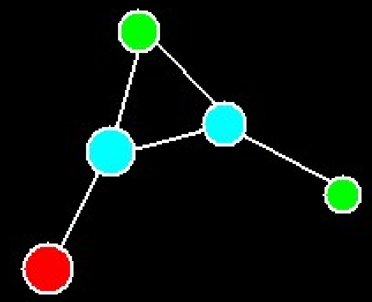
A web of connected propositions?

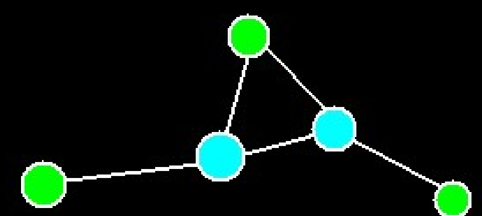
The diagrams that follow sketch the development of scientific knowledge in the Quinean picture.

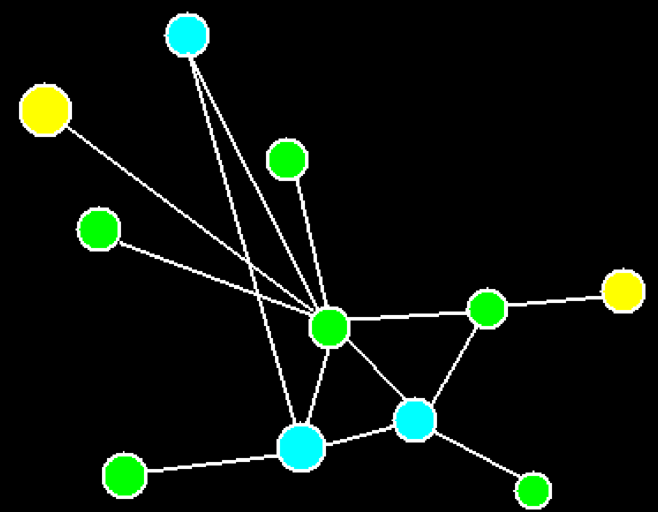
The circles represent hypotheses and the lines are the links between them.

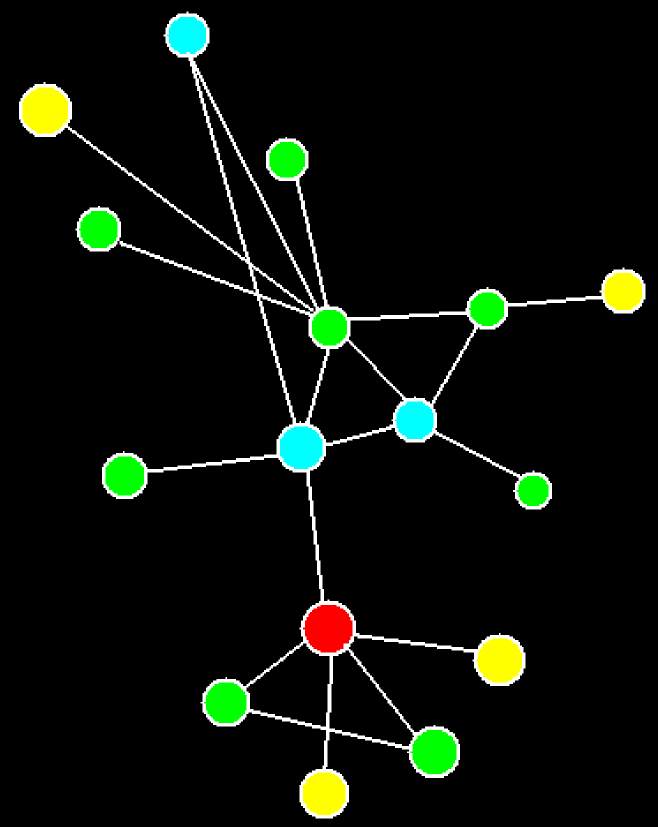
The circles are colour-coded: blue for true, through green and yellow, to red for false. Note that in the real world though we don't know the status of our hypotheses.

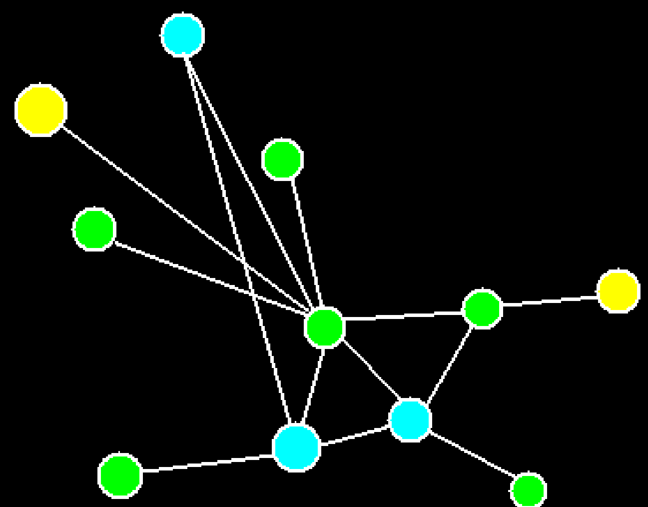


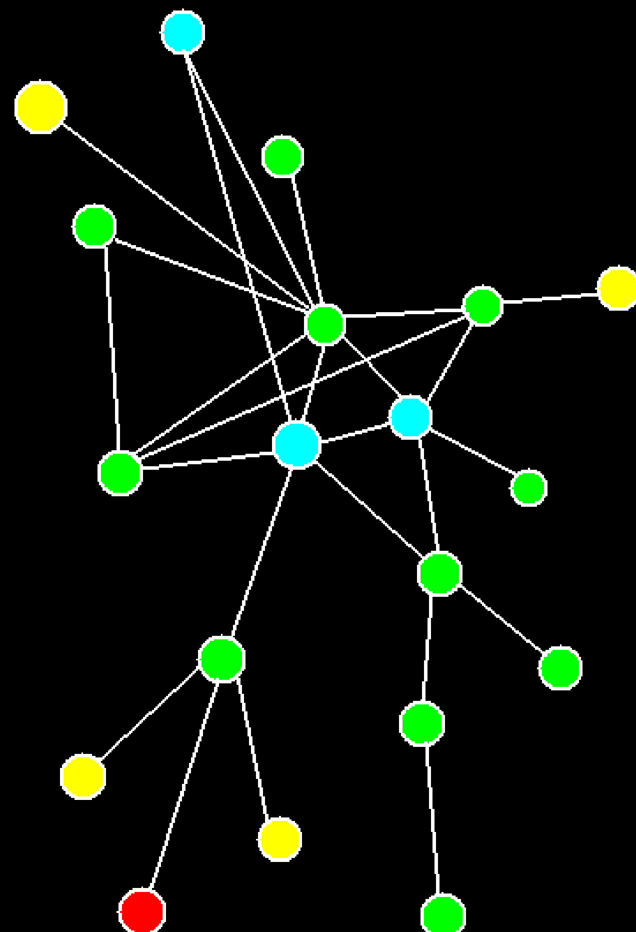


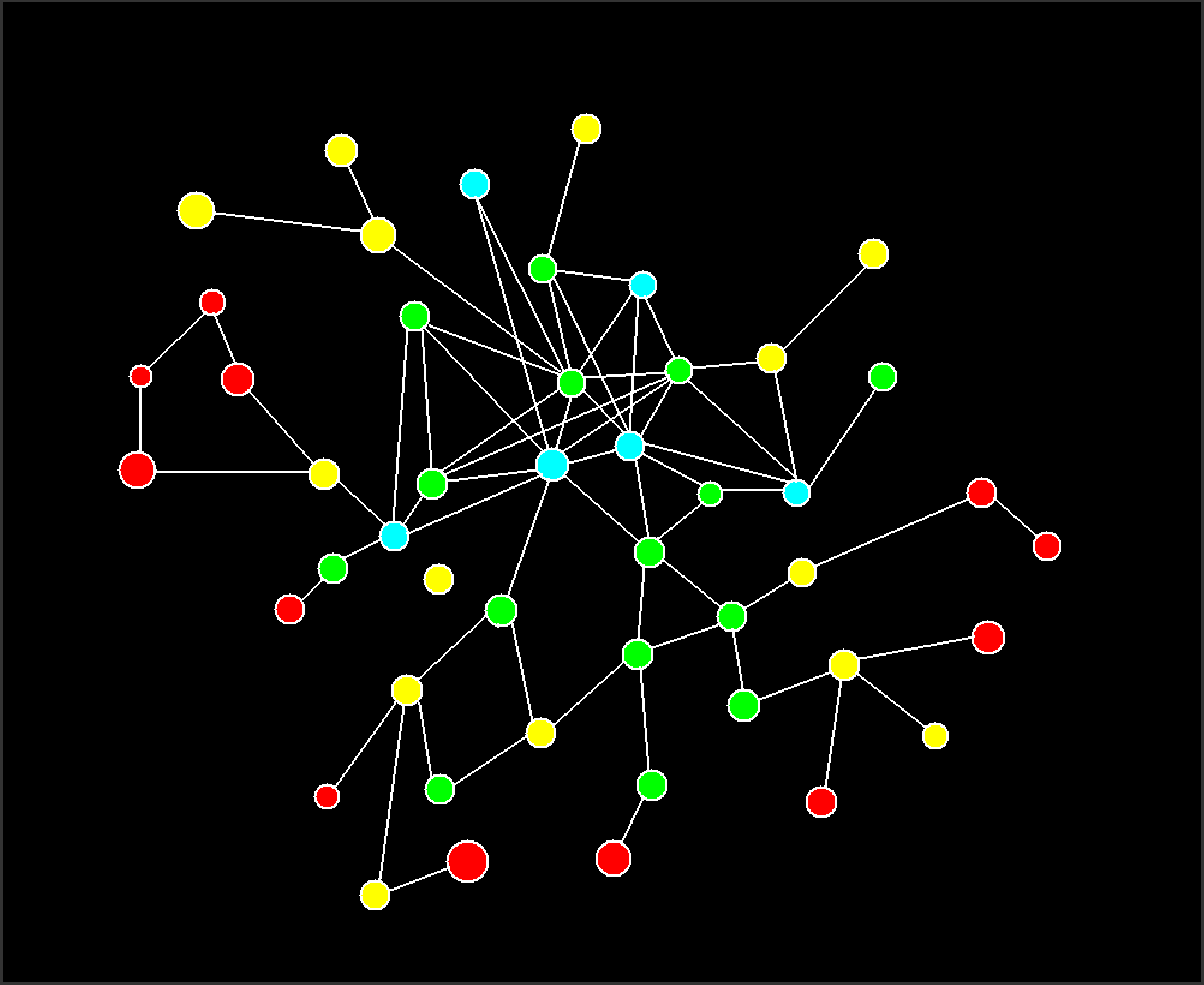












What Quine's view means for you

Writing a good literature review is not done out of some misguided respect for your elders. It's essential to position yourself in the network of knowledge by making connections to what has gone before.

It's very difficult to be a lone point out on your own in the darkness.

For your topic to be fruitful ground for scientific progress, you generally need to find your way to the *edge* of the network, where the uncertain statements are.

Making sense of previous views

Popper: hypotheses are not falsified alone, but groups of hypotheses (parts of the network) are adjusted and discarded.

Kuhn: a scientific revolution is a wholesale change in the network, necessitated by the revision of some core concept.

Feyerabend: nothing we know is totally safe from revision.

Matt Might's graphical PhD sketch: you need to get to the boundary of knowledge to do useful research, but the expansion of that knowledge is not a smooth process.

In conclusion: what is science ***for*** and are we doing it right?

The primary aim of science is to increase our storehouse of reliable knowledge.

Driven in part by basic curiosity about the world around us.

But science is also meant to improve a nation's economy, the quality of life of its people, etc.

History suggests scientific research has been an excellent investment for the taxpayer.

All going smoothly?

Since WW2, the number of scientists and their annual published output has increased forty-fold.

This has led to a struggle for key resources: employment, scientific visibility as reflected in citation counts, and research funding.

Rise in efforts to measure progress in science, so we can allocate those resources sensibly.

Hard to measure knowledge accumulation, so we use proxies such as funding obtained, papers published, journal impact factors, citation counts, etc.

Are we measuring the right things?

But a proxies are imperfect (e.g., more papers being published does not always mean that more knowledge has been obtained).

Important that science critically examine the effectiveness of its own mechanisms for accumulating reliable knowledge.

Are our current institutions and practices optimized for this goal, or are some of them the result of historical accidents?

Some processes to consider

The commercial journal publication model.

Anonymous peer review as the gold standard of assessing our work.

The effects of a publication bias towards positive results.

The competitive funding allocation process.

Commercial journal publication

The earliest scientists shared their results by writing letters.

The first scientific journal, Philosophical Transactions of the Royal Society, was founded in 1665 by Henry Oldenburg.

Commercial publishers provided a valuable service in an era when scientists did not have the facilities to publish their own work.

Commercial journal publication

We now have the internet, however. Distributing documents online costs almost nothing.

Many fields are still locked into commercial publishing arrangements. Research is done in universities, paid for by taxpayers, and freely given to publishing companies... who then sell access back to universities at very high prices.

Does this make sense?

Open access journals have started to appear. This looks like progress, although some charge high fees to authors.

Anonymous peer review

Oldenburg invented peer review as well as inventing the journal.

Too many papers were being sent to PTRS for Oldenburg to assess himself, so he sought help from colleagues in deciding what was good enough to be published in the journal.

Now, journal editors send out a manuscript to multiple reviewers and then decide to recommend acceptance, rejection, or revise & resubmit.

Traditionally anonymous so that reviewers can speak freely without worrying about negative effects on their careers.

Anonymous peer review

It sounds like a good idea.

One problem: most studies of peer review show that it doesn't work (reviewers disagree wildly about quality).

It can also be very slow.

A problem of incentives: why should I spend time reviewing papers when I would get more rewards for writing my own?

Alternative systems? Publish all, let online reviewing sort it out?

Publication bias

Journals are biased towards publishing positive results, i.e., results in which the authors have discovered an effect.

Understandable? There's not much of a story in a negative result.

However, this leads to the "file drawer effect". Only those studies that achieved statistical significance get submitted for publication, and negative results languish in file drawers.

Even when there is no effect, statistical significance in some fields is achieved 5% of the time.

Publication bias

The cumulative effect of this process can seriously distort the consensus on the truth in any given field.

Ioannidis, JPA (2005). Why Most Published Research Findings Are False. PLoS Med 2(8): e124.

What could we do differently? Journals for negative results? Require registration of planned studies before results were collected?

Competitive funding allocation

Most research funding is handed out by government agencies, e.g., the National Science Foundation in the US or the various Research Councils in the UK.

This funding is allocated through a competitive bidding process.

Funding agencies don't want to have to decide which ideas are most promising, and so the burden of making a case for support falls on the scientists who want the money.

Other scientists then spend time reviewing the proposals and deciding which ones get funded.

Competitive funding allocation

This too sounds like a reasonable idea at first.

One problem is that there is room for inefficiency as excessive academic time is spent on proposal writing.

Success rates for the EPSRC are now down below 20%.

That is a lot of unfunded proposals that occupied months of people's time.

The problem is that there is almost no limit to how much effort someone might put into writing and revising a proposal.

Interesting read:

Medawar, P. (1963) Is the scientific paper a fraud?, The Listener, 12 Sep 1963, 377-378.

A talk given by the biologist Sir Peter Medawar, originally for a BBC radio program.

Further suggested reading

Chalmers, A. F. (1982). *What is this thing called science?* (2nd ed.) Open University Press.

Godfrey-Smith, P. (2006). The strategy of model-based science. *Biology and Philosophy, 21*(5), pp. 725-740.

Kuhn, T. S. (1962). *The structure of scientific revolutions.* University of Chicago Press.

Okasha, S. (2002). *Philosophy of science: a very short introduction.* Oxford Paperbacks.

Popper, K. R. (1959). *The logic of scientific discovery.* Hutchinson.

Quine, W. V. O. (1951). Two dogmas of empiricism. *The Philosophical Review, 60*, pp. 20-43.