Popper, Kuhn, Lakatos, and Feyerabend

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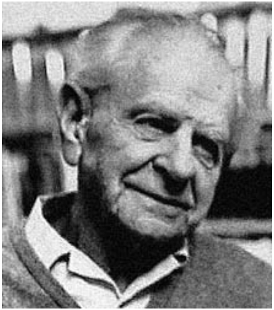
145 Philosophy of Science

Sir Karl Popper (1902-1994)



- born in Vienna, educated at U of Vienna
- 1928 PhD, 1930-1936 secondary school teacher
- 1934 *Logik der Forschung* (translated 1959)
- 1937 emigration to NZ, lecturer at Canterbury U College of NZ
- 1946 emigrated to UK, position at LSE
- 1963 *Conjectures and Refutations*
- popular in science; 'Popperazzi'

Popper's theory of science in a nutshell

- problem of induction $\Rightarrow$ forget about induction altogether
- theory of deductive method of testing instead
- 'deductivism' (as opposed to inductivism)
- explicitly acknowledges Duhem as forerunner of deductivism
- rejected log positivist demarcation criterion of verifiability (or confirmability)

- instead proposed as demarcation criterion ‘falsifiability’
- there’s no logic of scientific discovery, but of ‘scientific justification’
- scientific progress results from the continued cycles of conjectures and refutations
- can never be completely sure that a theory is true; nor can we reasonably increase our confidence in the truth of a theory when it is ‘corroborated’
- intended as descriptive and prescriptive

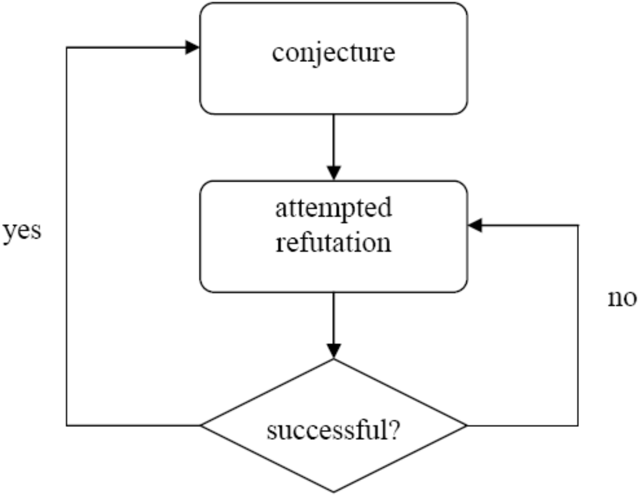
Falsification

Thesis (Falsification)

A hypothesis is scientific *iff it has the potential to be refuted by some possible observation.*

- Simplistic slogan: the easier a hypothesis can be refuted, the better, i.e. the more scientific it is!
- asymmetry between verification and falsification:
  - a observation contradicting prediction leads to definite falsification (and rejection) of thy under scrutiny
  - b no amount of corroboration ever verifies a theory; confirmation is a myth
- we should entertain a tentative attitude toward scientific theories
- science is search for truth, but we can never know whether we attained it!

Scientific change: conjectures and refutations



Problems with falsificationism

- holism about testing: no isolated hypothesis can be falsified individually
- Popper was aware of fact that logic itself does not force a scientist to reject a particular hypothesis in the face of recalcitrant data
- but good scientist would never do that
- falsification process is based on a decision regarding the observation report that can be challenged
- Popper insisted that making these decisions about observations is different from making decision about the theories themselves
- any hypothesis can be retained despite apparent falsification if people are only willing to make certain decisions

$\Rightarrow$ scientific theories can be immunized against falsification

Question: can Popper really justify that science is rational and objectively progressive when it ultimately depends on purely conventional and arbitrary decisions?

More trouble...

- What if a hypothesis does not forbid any particular observation and is thus taking no risks?
- example: coin toss
- Popper: logically speaking, these hypotheses are unscientific, but ‘in practice’ falsifiable

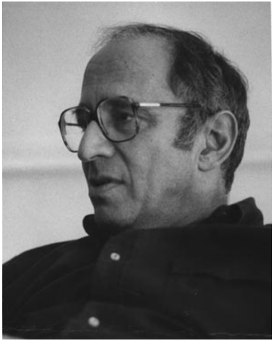
$\Rightarrow$ falsification can occur ‘in practice’ without its being backed up by deductive relation between observation and theory

- Even assuming falsificationism, how can we rationally distinguish between a highly ‘corroborated’ theory and a new theory?
- But if corroboration is different from confirmation in that it is only ‘backward-looking’, how can it be rationally justified?
- Salmon (1981): if there’s no confirmation, then no policy is more rational than choosing the untested theory (really, it’s a tie)

"[I]t's a mistake to try to work out whether theories like Marxism or Freudianism are themselves 'scientific' or not, as Popper did. A big idea like Marxism or Freudianism will have scientific and unscientific versions... Scientific versions of Marxism and Freudianism are produced when the main principles are connected with other ideas in a way that exposes these principles to testing. To scientifically handle the basic principles of Marxism is to try to work out what difference it would make to things we can observe if the Marxist principles were true."

Peter Godfrey-Smith, p. 71 (emphases in original)

Thomas S Kuhn (1922-1996)



- born in Cincinnati OH, educated at Harvard (physics)
- 1949 PhD, taught at Harvard, Berkeley, Princeton, MIT
- 1962 *The Structure of Scientific Revolutions*
- 1977 *The Essential Tension: Selected Studies in Scientific Tradition and Change*

What is a paradigm?

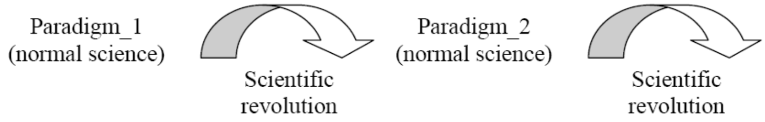
Characterization (Broad sense)

A paradigm (in the broad sense) is a whole way of doing science in a particular field that includes an entire package of ideas and claims about the world, as well as of methods for gathering and analyzing data, of pursue the theoretical elaboration of the field.

Characterization (Narrow sense)

A paradigm in the narrow sense is a key part of a paradigm in the broader sense, a specific achievement, typically in the form of a exemplary problem and an exemplary solution.

Scientific change: normal science and revolutions



Contrast with Popper

1. Normal science:
    - Popper: science permanently open to criticism and revision
    - Kuhn: no, periods where a lot of background is held constant
2. Scientific change:
    - Popper: smaller and bigger conjectures and less or more dramatic refutations, but essentially the same
    - Kuhn: change within normal science v. revolutionary change
    - normal and revolutionary (or ‘crisis’) science must be described very differently
    - in normal science: conventions about standards for justification of arguments
    - in revolutions: no such thing
    - progress: obvious in normal science, problematic in revolutionary science
3. role of history of science: Popper: none; Kuhn: important

“1. Kuhn’s constant emphasis on the arbitrary, personal nature of factors often influencing scientific decisions, the rigidity of scientific indoctrination of students, the ‘conceptual boxes’ that nature gets forced into by scientists..., and

“2. Kuhn’s suggestion that these features are actually the *key to science’s success*—without them, there is no way for scientific research to proceed as effectively as it does.”
Godfrey-Smith, p. 79

Question: how can it be beneficial for science to involve decisions which are grounded in such personal and biased inclinations?... really: in anything other than data??

Normal science

- pre-normal science: before establishment of paradigm, not well ordered, not effective
- establishment of a paradigm
- examples of paradigms: Newton's, Einstein's, Skinner's behaviorism, modern molecular genetics, etc
- one paradigm per field at any given time (usually)
- characteristic of normal science: absence of debate over fundamental tenets ('consensus-forging' role of paradigms)

  Kuhn: Yea   Popper: Nay
- 'puzzle-solving' in normal science, extending and refining the paradigm

Anomaly and crisis

- “only a poor workman blames his tools”
- anomalies: data irreconcilable with paradigm or puzzle that has resisted resolution
- disposal of entire paradigm only if two necessary conditions are met
    1. critical mass of anomalies is reached
    2. a rival paradigm has emerged
- crisis science when first condition has been met, but not second
- for Popper, every little anomaly should count as a refutation
- Kuhn: willingness to reject hypotheses can go too far

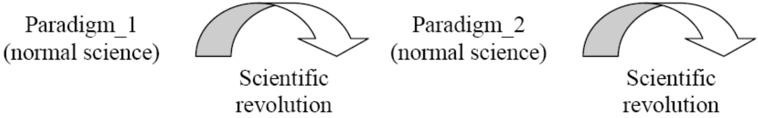
Difficulties so far

- some have challenged Kuhn's insistence on there only being one paradigm in a given field at a given time (will come back to this maybe in a week)
- Kuhn seems to have exaggerated the degree of commitment of normal scientists to their paradigm

Next: revolutions

'The storm of the Bastille' on 14 July 1789, by Jean-Pierre L L Houel (1735-1813)

Scientific revolutions



in scientific revolutions, rules and standards break down and are replaced, even the scientific language

Two issues:

1. How do revolutions occur?
2. What's the relationship between pre-revolutionary and post-revolutionary science?

How do revolutions occur?

Recall the necessary conditions for large-scale scientific change:

1. critical mass of anomalies is reached
2. a rival paradigm has emerged

⇒ crisis alone will not lead to a paradigm being ‘falsified’

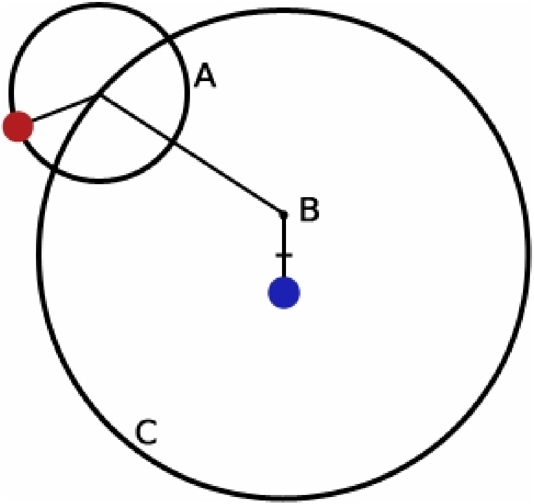
⇒ emergence of new paradigm alone does not suffice to persuade scientists to change camps

Challenge: are crises really necessary preludes to revolutions? E.g. in case of appearance of genetics around 1900?

Example: Ptolemaic astronomy

Schema huius præmissæ diuisionis Sphærarum.

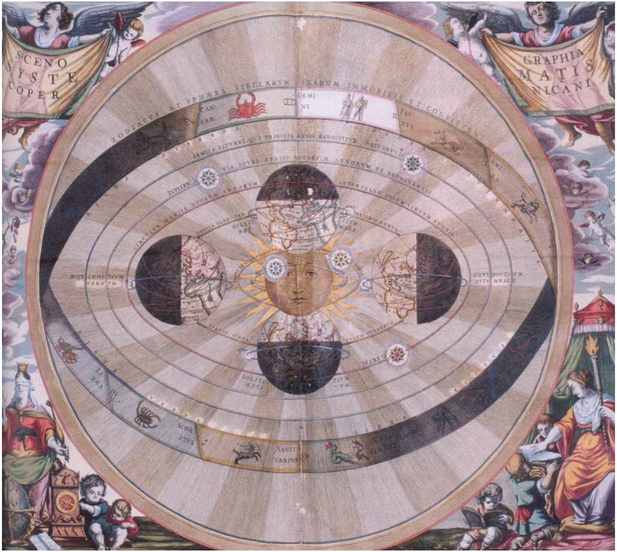
Deferents, eccentrics, epicycles, equants



Problems for Ptolemy

- predictively unreliable
- more and more ‘bells and whistles’ as model is adjusted to fit new data
- Copernicus: equants are particularly unpleasing
- not replaced for long period despite anomalies because there was no credible competitor

Copernican astronomy



- advantages of Copernican theory: qualitative features of planetary motions (such as retrograde motions, bounded elongation of inferior planets) and variations in brightness of planets can be neatly explained
- objections against heliocentrism: unobserved stellar parallax, terrestrial physics, theological and psychological arguments
- also had trouble with predictions and accounting for data (Copernicus noted, not without pride, that his theory accounted for the data just as well as did Ptolemy's theory)
- despite popular belief, Copernican theory was thus not much simpler than its Ptolemaic opponent—it also needed eccentrics and epicycles
- only became much simpler once ellipses were introduced as planetary orbits

The changing standards

- standards, particularly their explication and specification, are part of paradigm
- if paradigms are replaced, so are some of the standards
- shift from paradigm to another is like conversion, ‘gestalt switch’
- There are rock-bottom standards: theories must be predictively accurate, consistent with well-established theories in neighbouring fields, able to unify disparate phenomena, fruitful for future research
- diversity and variation within and without paradigm is strength of science

In short

Science is a social mechanism that combines capacity for sustained, cooperative work with capacity to break down and reconstitute itself.

Gains and ‘Kuhn losses’

- revolutions are non-cumulative episodes in the history of science
- there are gains, particularly concerning problem-solving power
- but there are also some losses (‘Kuhn losses’): questions that the old paradigm answered may appear puzzling in the new paradigm, or disappear altogether
- example of Kuhn loss: Descartes’ vortex theory had explanation of why all planets revolve around sun in same direction while Newton’s theory of gravitation did not; “Lavoisier’s chemical theory inhibited chemists from asking why the metals were so much alike, a question that phlogistic chemistry had both asked and answered” (*SSR*, p. 148)
- question: is there an unbiased way of comparing gains and losses resulting from a revolution?

Incommensurability

Definition (Incommensurability)

Two entities are incommensurable just in case they are "not comparable by use of a common standard or measure".

(Godfrey-Smith, p. 91)

This is a standard definition; Kuhn's notion is slightly, but importantly, different.

1. linguistic incommensurability: holistic view about meaning of scientific language implies that languages used in different camps are different
    - examples: 'mass' in Newtonian mechanics and in special relativity; 'planet' (intension and extension) in various astronomical theories (and recent redefinition)
    - problems: hard to come up with real examples from history of science; scientists often 'bilingual'

2. methodological incommensurability: standards of what qualifies as good evidence, good argument, etc depend on paradigm
    - example: role of causal explanation
    - Newtonian theory of gravitation involving instantaneous action at distance hard to supplement with mechanistic explanation
    - Does this mean Newtonian theory is to blame, or should demand for causal explanation be dropped?

Hoyningen: two misunderstandings of incommensurability

Paul Hoyningen-Huene

Kuhn on Hoyningen (in 1988): "I first met Paul Hoyningen in mid-August 1984... I rapidly discovered that Hoyningen knew my work better than I and understood it nearly as well... Our discussions often grew passionate, and it was not always Hoyningen who changed his interpretation of what I had meant."

1. First misunderstanding: Incommensurability implies complete incomparability
    - theories can be compared in their empirical potentials
    - "Though the incommensurable concepts may be central to both theories, many [...] predictions may be formulated entirely commensurably" (Hoyningen, p. 220)
    - example: position of celestial bodies predicted by Ptolemaic and Copernican astronomy
2. Second misunderstanding: Incommensurability implies full discontinuity
    - at least part of achievement of a paradigm is permanent
    - "[after a revolution] much of [the scientist's] language and most of his laboratory instruments are the same as they were before. As a result, postrevolutionary science invariably includes many of the same manipulations, performed with the same instruments and described in the same terms as its revolutionary predecessor." (*SSR*, p. 130)

Scientific progress according to Kuhn

Main idea: scientific progress as increased problem-solving capacity

- cumulative only in periods of normal science, non-cumulative through revolutionary phases
- appearance of progress from standpoint of victorious paradigm
- objective sense in which revolutions increase problem-solving power, i.e. number and precision of solutions to problems tend to grow over time
- this is guaranteed because new theory is required to be able both to solve most problems the old theory solved, and to cope with the anomalies incurred by the old theory
- this constitutes objective scientific progress for Kuhn, and allows him to reject charges of relativism (according to which successive theories don't differ in quality)

Hoyningen: three qualifications

1. First qualification: ‘Losses’ in revolutions
2. Second qualification: devaluation of the loser’s perspective
3. Third qualification: no ‘drawing closer to the truth’
    - development of science isn’t a process towards a fixed goal set in advance, but development in which articulation and specialization of knowledge increases
    - historical argument: no ontological convergence in lineages (Aristotelian physics $\Rightarrow$ Newtonian mechanics $\Rightarrow$ Einsteinian relativity)
    - epistemological argument: meaningless to talk of what there really is, beyond all theory

Scientific progress is not progressive approximation to truth, but instrumental improvement of scientific knowledge.

Imre Lakatos (1922-1974)

- main idea: ‘research programmes’ rather than paradigms
- more than one research programme per field at any given time
- large-scale dynamics governed by competition between research programmes
- tries to reconcile Kuhn’s forceful historical arguments with Popper’s attempts to rationally account for all scientific reasoning

$\Rightarrow$ rational reconstructions of historical episodes; almost beyond recognition...

Methodology of scientific research programmes

- research programmes (RP) have two elements: hard core (set of basic ideas and methods such as Newton's laws) and a protective belt (less fundamental ideas, links to applications such as views about matter, structure of the universe, mathematical tools)
- competition bw RPs is what ensures the rationality of and progress in science
- RPs are justifiably alive beyond the first signs of falsification and anomaly
- two types of change: (1) within individual RP; (2) at level of collection of RPs

Scientific change

1. change within RPs:
    - Rule 1: changes should only be made to protective belt, not to hard core
    - Rule 2: changes to protective belt should be progressive (i.e. expansion of application to ever larger set of cases, more precise treatment, more predictive power)
    - if changes are no longer progressive, and more and more anomalies occur, RP becomes ‘degenerate’
2. large-scale change:
    - acceptable to protect degenerating RP for a while, to give it a chance of recovering
    - Lakatos couldn’t give rules for when it becomes irrational to defend degenerating RP
    - Feyerabend: that’s why Lakatos’s account of rational theory choice is ultimately empty

Godfrey-Smith: some sciences might be best described by mixture of Kuhnian paradigms and Lakatos' competing RPs

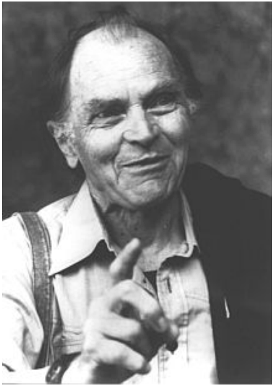
Example: evolutionary biology

- single over-arching paradigm of 'synthetic theory' (= combination of Darwinism and genetics
- several competing RPs of how to articulate paradigm, i.e. how to understand change and variation
- e.g. levels debate: natural selection operative at level of genes, genetic variations within populations, group selection etc; exact contribution of each level

Larry Laudan, *Progress and Its Problems* (1977)

- 'research traditions' (RTs, rather than RPs): thys within RTs more loosely related, variation also in hard core
- distinction between acceptance (like belief) and pursuit of theory
- pursuit: decision to work with idea, explore, refine, articulate it
- it can be rational to pursue an idea without accepting it
- rational to pursue RT with highest rate of progress
- rational to accept RT with highest level of problem-solving power
- Difficulties:
  - would come out as irrational to pursue RT with currently low rate of progress, but with high hopes of future high rate
  - what is best distribution of workers across range of RTs?

Paul Feyerabend (1924-1994)



- born and educated in Vienna
- teaching positions at Berkeley, London, Zürich
- *Against Method* (1975)
- often incorrectly called ‘worst enemy of science’
- ‘the wild man’ of C20 phil of science
- epistemological anarchism: opposition to all systems of rules and constraints of rationality in science
- incommensurability, theory-ladenness of observation (cf. Ch. 10)
- science as highly creative enterprise

The Master Argument in *Against Method*

- problem for heliocentrism: highly counterintuitive (contrasensual) 'implications' for terrestrial physics
- tower thought experiment
- Galileo: shipmast thought experiment ⇒ composite motion, notion of 'circular inertia'

⇒ Galileo created new observational description of the world with apparent motions compatible with heliocentrism

⇒ example of how science often challenges rather than follows observations

- very basic empiricist principle seems to point away from scientific theory we now consider more meritorious
- Feyerabend's radical extrapolation of this historic episode: "it is advisable to let one's inclination go against reason in any circumstances, for science may profit from it" (*Against Method*, 156)

Feyerabendian general principles (!?)

- Principle of tenacity: hold onto attractive ideas despite initial difficulties and anomalies
- Principle of proliferation: propose new ideas and theories
- Motivation: science benefits from diversity of ideas and approaches
- problem: no mechanism for eliminating and rejecting ideas and theories
- Feyerabend stresses analogy of science to art, thereby compromising the practical aspects of science

Recurring theme in C20 philosophy of science

One-level theory of scientific change

- Popper, Quine, (Feyerabend)
- unified account
- no way to give non-question-begging distinction between two kinds of change ('guided by facts' v. 'merely pragmatic')

Two-level theory of scientific change

- Carnap, Kuhn, Lakatos, Laudan
- one-levellers ask for overly stringent conception of distinction
- historical examples suggest utility of two-level account