Theories and Methods: Literature, Science and Medicine
Event 4: Philosophy and Sociology of Science for Literature and History Students

Session 1: Introduction to the Philosophy of Science

Hasok Chang
Dept of History and Philosophy of Science
University of Cambridge
hc372@cam.ac.uk

The main question for this session:

- What is it that scientists *do* when they do science?
- This offers a practice-orientated re-formulation of the familiar philosophical question about the scientific method.
- (Follow-up question, to be addressed in Session 2: How is scientific work represented in scientific publications?)

Some standard answers to the main question:

- Inductivism
- Falsificationism
- Normal science (Thomas Kuhn)
- Methodological anarchism (Paul Feyerabend)

1. Inductivism

What is inductivism?

The central inductivist idea:

scientific knowledge is based on unbiased observations;
such reliance on observations distinguishes science from other systems;
scientists should seek to prove their theories with observations.

Empiricism as the common-sense criterion of demarcation

Induction in empirical science: generalisation from observations
Inductivism as the ideal form of empiricism

Historical location of inductivism

17th-century revolt against speculation and received doctrines
Francis Bacon
The ideal of certainty, proven knowledge
model of mathematics
attempt to meet that standard in empirical science
The inductivist spirit:
start with unbiased experience,
and allow nothing else into your system of knowledge.

Induction as a demarcation criterion

> Hans Reichenbach : The principle of induction "determines the truth of scientific theories. To eliminate it from science would mean nothing less than to deprive science of the power to decide the truth or falsity of its theories. Without it, clearly, science would no longer have the right to distinguish its theories from the fanciful and arbitrary creations of the poet's mind." "The principle of induction is unreservedly accepted by the whole of science and ... no man can seriously doubt this principle in everyday life either." (quoted in Karl Popper, Logic of Scientific Discovery, rev. ed. (London: Hutchinson, 1968), pp. 28-29; p. 427 in Curd and Cover)

Problem: Why scientific discoveries cannot be made by inductivism

1. Observations without presuppositions are impossible.
   - Conceptual expectations (the "duck-rabbit" metaphor)
   - E.g. the sun rising vs. the earth rotating
   - E.g. bubble-chamber pictures
   - One could retreat to sense-data,
     - but they do not lead us anywhere very interesting.

2. Observations themselves do not direct generalizations.
   - Observing a black raven? bird? animal? thing?
   - The “all swans in the Northern Hemisphere” case
   - The “grue” business
   - Classification is necessary for generalisation, and
     - classification is inherently theory-laden

3. Making connections require theoretical expectations.
   - The case of the tides
   - The impossibility of noting all factors

4. Simple generalisation from observations cannot yield theories about unobservable entities.

2. Falsificationism

Karl Popper’s rejection of inductivism

Popper's doubts about systems of great “explanatory power",

> “those impressive and all-explanatory theories which act upon weak minds like revelations” (Popper, “Science: Conjectures and Refutations”, in Curd & Cover, p. 9)

Always being right is actually a weakness

- Vague astrological predictions
- Adler’s verdict on the unknown case (ibid., pp. 5-6).
  - Popper concluded: “It is easy to obtain confirmations, or verifications, for nearly every theory -- if we look for confirmations” (ibid., p. 7).
- The need to subject theories to harsh tests (cf. Polanyi’s dog)
- The double futility of inductive confirmation:

the accumulation of positive evidence fails to amount to proof;
each piece of positive evidence itself may be deceptive.

The young Popper in the social and intellectual climate of interwar Vienna

Defeat, followed by a rejection of the old;
political, intellectual, cultural turmoil.

New ideas and trends:
Marxism/communism, fascism/racialist view of history,
modernism, logical positivism, psychoanalysis,
new physics, atonal music, etc.

Desire for criteria for judging what was a good/scientific system of belief.

Falsification as the scientific method

True science is falsifiable:

"The criterion of the scientific status of a theory is its falsifiability, or refutability, or testability." (ibid., p. 7) "The criterion of falsifiability is a solution to this problem of demarcation, for it says that statements or systems of statements, in order to be ranked as scientific, must be capable of conflicting with possible, or conceivable, observations. . . ." (ibid., p. 9)

The critical spirit

"Sticking one's neck out"
Against "immunizing tactics" or "conventionalist twists"
"Every genuine test of a theory is an attempt to falsify it" (ibid., p. 7)
Contrast between Marxism and general relativity

Popper's demarcation criterion ("proposal (D)")

"it is this second boldness*, together with the readiness to look out for tests and refutations, which distinguishes 'empirical' science from nonscience, and especially from prescientific myths and metaphysics." *In a footnote there Popper indicates what exactly he means by "boldness": "the boldness of predicting aspects of the world of appearance which so far have been overlooked but which it must possess if the conjectured reality is (more or less) right, if the explanatory hypotheses are (approximately) true." In a later passage he explains further: "It is the boldness of a conjecture which takes a real risk -- the risk of being tested, and refuted; the risk of clashing with reality." (Popper in P. A. Schilpp, ed., The Philosophy of Karl Popper, pp.980-981)

The methodological form of proposal (D)

"Propose theories which can be criticized. Think about possible decisive falsifying experiments--crucial experiments. But do not give up your theories too easily--not, at any rate, before you have critically examined your criticism." (ibid., p.984)

Problems of falsificationism

Basic descriptive problem: Most scientists don't seem to be doing their best to falsify their own theories.

Popper's "great men" defence of his demarcation criterion

"It is the working of great scientists which I have in mind as my paradigm for science." "It is science in this heroic sense that I wish to study." "I only wish to draw a simple picture of the kind of men I have in mind, and of their activities....these are men of bold ideas, but

highly critical of their own ideas; they try to find whether their ideas are right by trying first to find whether they are not perhaps wrong. They work with bold conjectures and severe attempts at refuting their own conjectures." (Philosophy of Karl Popper, pp.977-8)

Further descriptive problem: Non-Popperian behaviour by "great" scientists

- Robert Millikan: oil-drop experiment
- Albert Einstein
  - Criticism of Kaufmann's experiment
  - Eclipse experiment ("I would have been sorry for the Lord…")
- Rumford vs. the Calorists (Berthollet, etc.)
  - Rumford: heat has no detectable weight.
    - Calorists: caloric is a subtle fluid.
  - Rumford: by friction, heat is generated from motion.
    - Calorists: combined caloric is released by mechanical agitation.
  - Rumford ("cannon-boring experiment"): heat by friction is limitless.
    - Calorists: the experiment ends when all the metal is ground up.

Normative Problem: it may be a good thing to keep a falsified theory (cf. the discovery of Neptune).

Popper's response: do give theories a chance to mature:

"if you avoid falsification at any price, you give up empirical science in my sense. But I found that, in addition, supersensitivity with respect to refuting criticism was just as dangerous: there is a legitimate place for dogmatism, though a very limited place. He who gives up his theory too easily in the face of apparent refutations will never discover the possibilities inherent in his theory."

Doesn't this negate Popper's whole idea? Popper's further response:

"As always, science is conjecture. You have to conjecture when to stop defending a favourite theory, and when to try a new one" (Philosophy of Karl Popper, p.984)

3. Normal Science

Paradigms and the transition to normal science

The chaos of pre-paradigm research

Paradigm as winning exemplar: transition to science

Paradigm as promise:

"The success of a paradigm...is at the start largely a promise of success....Normal science consists in the actualization of that promise." (Thomas Kuhn, The Structure of Scientific Revolutions, second ed., pp.23-24)

The benefits of having a paradigm:

narrowing and focusing, enabling esoteric research;
freedom to disregard foundational questions.

The nature of normal science

Normal science is paradigm-based research

"In this essay, 'normal science' means research firmly based upon one or more past scientific achievements, achievements that some particular scientific community acknowledges for a time as supplying the foundation for its further practice." (p.10)

> In other words, it is “a strenuous and devoted attempt to force nature into the preformed and relatively inflexible box that the paradigm supplies” (p.24)

What constitutes the activity of normal science?

1. Generation of facts
2. Increasing the fit between theory and observation
   Popper's refutations are Kuhn's anomalies:
   material to work on, not reasons to quit.
3. Paradigm articulation

Normal science is NOT interested in:

1. Genuine novelties
2. Critique of the paradigm
   Testing of conjectures only occurs within a paradigm.
   Failure is failure of the scientist, not the failure of the science.
3. Aimless gathering of facts

Normal science as puzzle-solving ("anomaly busting")

Excitement of the game
Assured solutions
Existence of rules, which may be unspoken,
generated in the process of emulating the paradigm.

Has Kuhn solved the demarcation problem?

Kuhn thought that paradigm-based research was the key characteristic of science as we know it:

> “it is normal science, in which Sir Karl’s sort of testing does not occur, rather than extraordinary science which most nearly distinguishes science from other enterprises. If a demarcation criterion exists (we must not, I think, seek a sharp or decisive one), it may lie just in that part of science which Sir Karl ignores.” “In a sense, to turn Sir Karl’s view on its head, it is precisely the abandonment of critical discourse that marks the transition to a science.” (Thomas Kuhn, "Logic of Discovery or Psychology of Research?", in Imre Lakatos and Alan Musgrave, eds., Criticism and the Growth of Knowledge (Cambridge: Cambridge University Press, 1970), p.6; on p. 14 in Curd and Cover)

Popper regarded normal science as the worst of science:

> " 'Normal' science, in Kuhn's sense, exists. It is the activity of the non-revolutionary, or more precisely, the not-too-critical professional: of the science student who accepts the ruling dogma of the day . . . . In my view the 'normal' scientist, as Kuhn describes him, is a person one ought to be sorry for. . . . The 'normal' scientist, in my view, has been taught badly . . . . He has been taught in a dogmatic spirit: he is a victim of indoctrination. . . . I admit that this kind of attitude exists . . . . I can only say that I see a very great danger in it and in the possibility of its becoming normal . . .: a danger to science and, indeed, to our civilization. And this shows why I regard Kuhn's emphasis on the existence of this kind of science as so important." (Karl Popper, "Normal Science and Its Dangers", in Lakatos and Musgrave, pp.52-53)

Paul Feyerabend thought Kuhn's demarcation was too loose:

> “if the existence of a puzzle-solving tradition is so essential, if it is the occurrence of this property that unifies and characterizes a specific and well recognizable discipline; then I

> do not see how we shall be able to exclude say, Oxford philosophy, or, to take an even more extreme example, organized crime from our considerations.... Every statement which Kuhn makes about science remains true when we replace 'normal science' by 'organized crime'." ("Consolations for the Specialist" in Lakatos and Musgrave, pp.199-200)

Popper vs. Kuhn on normal science and progress

Popper: the dogmatism inherent in normal science prevents innovation.

Kuhn: normal science enables esoteric research and cumulative growth, and also leads to scientific revolutions.

What does normal science achieve?

Progress in normal science does not occur by testing the paradigm, but by taking the paradigm as valid and building on it.

Increase in detail and precision through paradigm articulation; paradigm-based research promotes esoteric knowledge.

"The success of a paradigm...is at the start largely a promise of success.... Normal science consists in the actualization of that promise". (Kuhn, The Structure of Scientific Revolutions, 2nd ed., pp.23-24)

Does a paradigm always validate itself?

No, normal science in fact prompts major innovations, in the long run:

> "research under a paradigm must be a particularly effective way of inducing paradigm change" (Structure, p.52)
> Cf. James Clerk Maxwell

Adherence to a paradigm provides no guarantee of producing observations that conform to it:

> "The instability of experience, a verifiable result of the Galilean proposition that theories violate the senses, by no means supports the thesis that nature tamely fashions herself according to our ideas and actions. In actuality, nature resists and rebels and in so doing behaves very improperly by the standards of idealistic doctrines." (Enrico Bellone [1976], A World on Paper: Studies on the Second Scientific Revolution, translated by M. and R. Ciacconi (Cambridge, Mass.: The MIT Press, 1980), p. 155)

Sharper expectations are less likely to be met (cf. Popper on testability):

> "Anomaly appears only against the background provided by the paradigm" (Kuhn, Structure, p.65)

How paradigms unravel:

Crisis leads to a "proliferation of divergent articulations"
(cf. pre-science, before any paradigm is established).

Examples of discomfort (Kuhn, Structure, pp. 83-84):

Copernicus (astronomy as "monster");
Pauli before quantum mechanics.

The character of extraordinary science:

> "The proliferation of competing articulations, the willingness to try anything, the expression of explicit discontent, the recourse to philosophy and to debate over fundamentals, all these are symptoms of a transition from normal to extraordinary research." (p.91)

The structure of a scientific revolution, in brief:

there is a crisis arising out of normal science;
an entirely novel kind of solution is proposed;
the novel solution generates a new paradigm;
the new paradigm replaces the old paradigm.

Good ideas don't start a new paradigm without a pre-exiting crisis.

e.g. Aristarchus on heliocentrism,
Kinetic theory of heat (Rumford, Davy, etc.),
Critique of absolute space (Leibniz, Mach).

4. Methodological Anarchism

Paul Feyerabend on the scientific method

Rejection of the search for 'The Scientific Method':

(1) there is no such thing;
(2) it would be harmful to impose one.

Argumentative strategy:

'Give me any presumably universal methodological rule,
and I'll show you counter-examples from the history of science,
moreover from very "good" science.'

> "Given any rule, however 'fundamental' or 'necessary' for science, there are always circumstances when it is advisable not only to ignore the rule, but to adopt its opposite. For example, there are circumstances when it is advisable to introduce, elaborate, and defend *ad hoc* hypotheses, or hypotheses which contradict well-established and generally accepted experimental results, or hypotheses whose content is smaller than the content of the existing and empirically adequate alternative, or self-inconsistent hypotheses, and so on." (Against Method, pp.23-24)

The case of "counter-induction"

Feyerabend wants to show problems with even the most obvious of methodological rules: "Theories should not violate known facts."

Summary heading of Ch. 5 of *Against Method* (1st edition, p.55)

> "No theory ever agrees with all the facts in its domain, yet it is not always the theory that is to blame. Facts are constituted by older ideologies, and a clash between facts and theories may be proof of progress. It is also a first step in our attempt to find the principles implicit in familiar observational notions."

Other relevant passages

(p.67) “a theory may be inconsistent with the evidence, not because it is incorrect, *but because the evidence is contaminated*.”
(p.67) “It is this *historico-physiological character of the evidence*, the fact that it does not merely describe some objective state of affairs *but also expresses some subjective, mythical, and long-forgotten views* concerning this state of affairs, that forces us to take a fresh look at methodology.”
(p.67) “A straightforward and unqualified judgement of theories by ‘facts’ is bound to eliminate ideas *simply because they do not fit into the framework of some older cosmology*.”
(p.68) “the first step in our criticism of customary concepts and customary reactions is to step outside the circle and either to invent a new conceptual system, for example a new theory, that clashes with the most carefully established observational results and confounds the most plausible theoretical principles, or to import such a system from outside science, from religion, from mythology, from the ideas of incompetents, or the ramblings of madmen. This step is, again, counterinductive. Counterinduction is thus both a *fact* --science could not exist without it-- and a legitimate and much needed *move* in the game of science.”

Feyerabend's general epistemological position

The lesson from counter-induction is meant to apply generally.

Albert Einstein: The scientist “must appear to the systematic epistemologist as a type of unscrupulous opportunist . . . .” (quoted in Against Method, p.18)

Feyerabend's "anarchistic" theory of knowledge:

neither falsificationism nor normal science,
nor insistence on increasing empirical content.

SOURCES FOR FURTHER STUDY

Introductory Textbooks

- A. F. Chalmers, What Is This Thing Called Science?, 3rd ed. (Buckingham: Open University Press, 1999), or 2nd ed. (St Lucia: University of Queensland Press, 1982).
- Carl G. Hempel, Philosophy of Natural Science (Englewood Cliffs: Prentice-Hall, 1966).
- John Losee, A Historical Introduction to the Philosophy of Science, 3rd ed. (Oxford: Oxford University Press, 1993).
- Alan Musgrave, Common Sense, Science and Scepticism: A Historical Introduction to the Theory of Knowledge (Cambridge: Cambridge University Press, 1993).
- Samir Okasha, Philosophy of Science: A Very Short Introduction (Oxford: Oxford University Press, 2002).

Useful Anthologies

- Martin Curd and J. A. Cover, eds., Philosophy of Science: The Central Issues (New York and London: Norton, 1998).
- E. D. Klemke, Robert Hollinger, and A. David Kline, eds., Introductory Readings in the Philosophy of Science, revised ed. (Buffalo: Prometheus Books, 1988).
- Joseph J. Kockelmans, ed., Philosophy of Science: The Historical Background (New York: The Free Press, 1968).
- Ryan Tweeney, Michael Doherty and Clifford Mynatt, eds., On Scientific Thinking (New York: Columbia University Press, 1981).
- Arthur Zucker, ed., Introduction to the Philosophy of Science (Upper Saddle River, N.J.: Prentice Hall, 1996).

Reference works

- The Oxford Companion to Philosophy, ed. by Ted Honderich (Oxford: Oxford University Press, 1995).
- The Pimlico History of Western Philosophy, ed. by Richard H. Popkin (London: Pimlico, 1999). First published as The Columbia History of Western Philosophy (New York: Columbia University Press, 1998).
- The Stanford Encyclopedia of Philosophy, online at http://plato.stanford.edu/.

More on Popper, Kuhn and Feyerabend

Popper

- Karl Popper, Conjectures and Refutations, 3rd ed. (London: Routledge, 1969), pp. 33-41 (sections 1.i-1.iii). Also reprinted in the Curd and Cover anthology, pp. 3-10.
- Paul Arthur Schilpp, ed., The Philosophy of Karl Popper, 2 vols (La Salle: Open Court, 1974).

Kuhn

- Thomas S. Kuhn, The Structure of Scientific Revolutions, 2nd ed. (Chicago: University of Chicago Press, 1970), pp. 10-51 (chs. 2-5); the third edition is essentially the same as the second.
- Imre Lakatos and Alan Musgrave, eds., Criticism and the Growth of Knowledge (Cambridge: Cambridge University Press, 1970). Especially note Kuhn's paper "Logic of Discovery or Psychological Research?", pp. 1-23 (also reprinted in the Curd and Cover anthology, pp. 11-19); Popper's paper, "Normal Science and Its Dangers", pp. 51-58; and also John Watkins, "Against 'Normal Science'", pp. 25-37; Paul Feyerabend, "Consolations for the Specialist", pp. 197-230.
- Paul Hoyningen-Huene, Reconstructing Scientific Revolutions: Thomas S. Kuhn's Philosophy of Science (Chicago: University of Chicago Press, 1993), Ch. 5 (pp. 167–196).

Feyerabend

- Paul Feyerabend, "How to Defend Society Against Science", Radical Philosophy, No. 11 (1975), pp. 3-8; reprinted in the Klemke, Hollinger and Kline anthology, pp. 34-44.
- Paul Feyerabend, Against Method (London: New Left Books, 1975); later editions are fine.
- Paul Feyerabend, Science in a Free Society (London: Verso, 1978), Part 2, Sections 1-10 (pp. 73-107).

And Lakatos, too!

- Imre Lakatos, "Science and Pseudoscience", in Philosophical Papers, vol. 1 (Cambridge: Cambridge University Press, 1977), pp. 1-7. Also reprinted in the Curd and Cover anthology, pp. 20-26.
- Paul R. Thagard, "Why Astrology is a Pseudoscience", in Curd and Cover, pp. 27-37