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Book Review: A Historical Introduction to the Philosophy of Science

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Professor Losee released an expanded fourth edition of his *A Historical Introduction to the Philosophy of Science* in 2001. This ambitious volume traces the history of thinking on the nature of the scientific endeavour from its beginning with Aristotle in Ancient Greece, through the Middle Ages and the Enlightenment to the work of contemporary philosophers. In this review, Allan reflects on the strengths and weaknesses of Losee's book. In addition, he provides a helpful summary of each chapter and some commentary on Losee's analysis of the key issues in the philosophy of science.

Book Review

I picked up John Losee's book, A Historical Introduction to the Philosophy of Science (fourth edition), with a view to using it in a series of sessions on the philosophy of science. I was looking for a book that cast a wide net over thinking about the nature and limits of science going back to the schools in Ancient Greece. I wanted a book that was reasonably thorough, yet understandable by readers who were not well-versed in academic philosophy. This book tries to steer that middle ground.

As Losee mentions in his Preface, his book is written for students of the history of science and the philosophy of science. Understandably, then, the book is a bit of a challenge for those not initiated into formal studies in philosophy. Losee assumes the reader is familiar with the history of the physical and biological sciences and has a working knowledge of formal logic and probability theory. My hope for the series was that with some guidance through the book with a more experienced tutor, some of the more difficult sections would be comprehensible.

Losee's book is an excellent overview of the key strands in the philosophy of science over the last two and a half millennia. For this book, Losee focuses on the key characters, moving fluidly from Aristotle, progressing through the scholastic period in Europe, the positivist period in the middle of the last century and ending with contemporary thinkers, such as Feyerabend, Kuhn, Laudan and Lakatos.

As Losee discusses the core arguments from each of the thinkers he addresses, he helpfully illustrates some of the main problems with the view he is explaining. Losee takes pains to not present a caricature of the philosopher he is critiquing. He paints a picture of each philosopher he discusses as a deep thinker with complex and nuanced views.

In each chapter, Losee centres his discussion about particular philosophers on one or two themes. Progressing through the chapters, he shows how each problem in the philosophy of science recurs again and again throughout the history of thinking about science, skilfully weaving it into his treatment of the various historical periods. These recurring themes include:

- the status of the central attributes of entities such as humans
  (Do entities have essences independent of the meanings we attach to terms?)(essentialism versus nominalism)

- the nature of causation
  (Are causes and their effects connected necessarily or only contingently?)(causal necessity versus contingent constant conjunction)

- the form of inductive inference
  (Are theories built piecemeal by observing instances or via axiomatic systems?)(induction by simple enumeration versus hypothetico-deductive reasoning)

- the logical relationship between a theory and supporting evidence (Are observational facts independent of the theories they support?)(theory-independence versus theory-ladenness)

- the importance of the time of discovery of evidence (Is a theory better confirmed if the evidence in its favour was previously unknown?)(historical versus logical theories of confirmation)

- the ontological status of theoretical entities such as quarks (Do such entities really exist or are theories merely instruments of prediction?)(realism versus instrumentalism)

On the down side, this fourth edition contains some distracting typographical errors. In addition, the reader would have benefited from Losee introducing some of the technical terms and formal notation before using them in his text. This book is ambitious in its scope, covering as it does over two millennia of thought on this subject. I would have liked some more context around each of the discussions to make it easier for the philosophically and scientifically uninitiated to understand the technical topic being discussed. However, I appreciate that this would have at least doubled the size of what is already a voluminous text.

In the following sections of this review, I summarize each chapter in Losee's book and add some clarifying comments. Where I add a comment to Losee's text, it is indicated thus in square brackets:

[LA: *text of Leslie Allan's comment*]

At the end of each chapter's notes, I also include a number of questions for readers to consider. It is hoped that these questions will prompt further reflection and enquiry.

These notes were used in a series of Philosophy Matters seminars on Losee's book. Register your interest in attending a future seminar series by visiting Philosophy Matters at meetup.com/philosophy-matters

Introduction

[p. 1] Philosophers and scientists do not agree on the nature and subject matter of the philosophy of science (e.g., is it the study of scientific achievement in its historical context or is it a reformulation as a deductive discipline?).

[pp. 1–2] There are four views of the philosophy of science:

1. formulation of world-views based on scientific theories (ontological categories/implications of theories)
2. exposition of the presuppositions and predispositions of scientists (sociology of science)
3. analysis and clarification of scientific theories (elucidation of meanings of concepts)
4. investigation of the distinction between science and non-science, scientific procedures, the nature of scientific explanation and the truth-value of laws and principles

Losee's book is devoted to the fourth view—that philosophy of science is a second-order discipline—while treating aspects of the second and third view.

[p. 3] There is no hard distinction between science and the philosophy of science (e.g., both seek to answer what makes a scientific explanation acceptable).

It is important to review both what scientists have said about the scientific method and their actual scientific practice.

Ch. 1: Aristotle's Philosophy of Science

Philosophers discussed in this chapter: Aristotle (384–322 BCE)

[p. 4] Aristotle was the first philosopher of science. He set up the Peripatetic School in Athens, covering epistemology (philosophy of knowledge), physics, biology, ethics, politics and aesthetics. In the philosophy of science, principle works are his Posterior Analytics, the Physics and the Metaphysics.

Aristotle's Inductive–Deductive Method

[p. 5] For Aristotle, scientific enquiry progresses by an inductive–deductive procedure from observations to general principles and back to observations.

Everything is a union of matter and form. Matter individuates (e.g., makes two identical giraffes two and not one) and form gives commonality among things (e.g., all animals with a four-chambered stomach).

[p. 6] **Inductive stage of scientific enquiry**

There are two types of induction:

1. induction by simple enumeration from individual objects/events to generalization about a species, or from individual species to a generalization about a genus
2. induction by intuition from phenomena to what is an essential property (like "seeing")

[p. 7] **Deductive stage of scientific enquiry**

Deductive syllogism consists of two premises and one conclusion.

Syllogism is deductively valid and consists of major (P), minor (S) and middle (M) terms.

If 'Every S is included in M'

AND 'Every M is included in P',

then (necessarily) 'Every S is included in P'

Major premise: one class is included/excluded from another class

—all/some S are/are not included in P

'All S are P' indicates P as an essential property and is the basis for scientific explanation. The interposition of the middle term between individual S and property P allows explanation for S having property P (e.g., why planets shine steadily).

The validity of a syllogism is solely down to the structural relationship between the premises and the conclusion.

Empirical Requirements for Scientific Explanation

[p. 8] For Aristotle, for a true scientific explanation:

1. the premises of the syllogism must be true
2. the premises must be indemonstrable (some are axiomatic/unprovable to avoid an infinite regress of explanations)
3. the premises must be better known than the conclusion (the general laws of a science are self-evident)
4. the properties named in the premises are the causes of the attribution in the conclusion (distinguishes true causes from accidental correlations) [p. 9]

[p. 9] For a true causal relation, the attribute is:

1. true of every instance of the subject (no exceptions)
2. true of the subject precisely and not as part of a larger whole
3. 'essential to' the subject

Losse objects to criterion 1. The lack of an exception cannot be determined for most properties (e.g., all dense objects sink).

Losse objects to criterion 3. As Aristotle has not given criteria for being 'essential to', this only pushes the need for explanation one step back.

[p. 10] For Aristotle, each science has its own distinct subject genus and set of predicates and hierarchy of deductive principles.

General level: principles of identity, non-contradiction and excluded middle (applies to all sciences)

First principles of physics: natural motion/violent motion; no vacuum (not deducible from other premises)

[p. 11] An acceptable scientific explanation must specify all four causes:

1. formal cause: specifies pattern of the process (design)
2. material cause: specifies type of substance
3. efficient cause: specifies what brings about transition
4. final cause: specifies the reason for the process (telos)

[LA: For more on Aristotle's four causes, see https://en.wikipedia.org/wiki/Four_causes]

Both animate and inanimate objects have a telos (e.g., fire rises to its 'natural place') that may not be conscious or involve a cosmic purpose, but are always deterministic.

Aristotle criticized:

- the atomists who neglected formal and final causes by focusing on the motion of atoms
- the Pythagoreans who only focused on formal causes by explaining in terms of mathematical relationships

The Demarcation of Empirical Science

[p. 12] Aristotle demarcated empirical science from pure mathematics by saying the former deals with change while the latter deals with the unchanging. Pure mathematical forms are abstracted from physical processes and have no objective existence. The first principles of science and their deductive consequences are necessarily true.

The Necessary Status of First Principles

Losee thinks Aristotle is committed to:

1. some properties inhere essentially in individuals [LA: e.g., Socrates would not be a man if he were not a mammal]
2. an identity of structure between sentences describing essential properties and their real counterparts in nature
3. scientists correctly intuit the relationship between language and reality

[p. 13] Losee concludes that Aristotle's distinction between generalizations that are necessarily true and others that are accidental is plausible. However, Aristotle failed to specify a way to verify which properties are essential.

Questions to Consider:

1. *What is it to be an 'essential' property of a thing? How can we tell?*
2. *Was Aristotle a scientist, a proto-scientist or not a scientist at all? Why?*
3. *Does Aristotle's syllogistic form of explanation really explain why a thing has a particular property or a particular event happens?*

Ch. 2: The Pythagorean Orientation

Philosophers discussed in this chapter: Plato (428/7–348/7 BCE); Ptolemy (c.100–c.178 CE)

The Pythagorean View of Nature

[pp. 14–15] The 'Pythagorean Orientation' is the view that the fundamental structure of the universe consists in mathematical harmony. Galileo was a Pythagorean.

[p. 15] Pythagoras (or followers) (6th Century BCE) noticed mathematical ratios in musical harmonies are independent of the physical instrument. Likewise, motions of the stars and planets show 'harmony of the spheres'.

Plato and the Pythagorean Orientation

[pp. 15–16] Losee thinks Plato is unfairly criticized for being anti-science in recommending exclusively pursuit of abstract ideas. Plato thought that rational order lies behind observed empirical phenomena.

[p. 16] Platonism in the Middle Ages and Renaissance corrected religious objections to science and the excessive attention to academic texts. The Christian West melded Plato's creator god (Demiurge) with the Christian creator god to articulate a god who applied a mathematical form to matter.

[pp. 16–17] Plato suggested the five elements are each aligned with a geometrical shape:

1. fire – tetrahedron
2. earth – cube
3. air – octahedron
4. water – icosahedron
5. celestial matter – dodecahedron

[p. 17] Plato suggested transformations between water, air and fire are the result of dissolutions of the triangular faces.

The Tradition of "Saving the Appearances"

Geminus (1st Century BCE) complained that Pythagoreans, in drawing mathematical relations between observed phenomena, were not really providing an explanation. They are only 'saving the appearances'. For Geminus, it is the physicist who truly explains by appeal to essential natures.

[p. 18] Ptolemy devised two mathematical models that derived equally well the retrograde motion of the planets (1. Epicycle-Deferent Model; 2. Moving-Eccentric Model). Astronomers can choose either model for predicting celestial motions.

[p. 19] Ptolemy oscillated between saying his mathematical models (a) were computational devices only and (b) truly described the motion of celestial bodies.
[LA: We'll see this disagreement occur throughout the history and philosophy of science as the debate between an 'instrumentalist' view of scientific theories and a 'realist' view.]

Proclus (5th Century CE) insisted that astronomers not be content with 'saving the phenomena' and get scientific by deducing motions from the (Aristotelian) self-evident axioms that simple motion is either (a) perfectly circular or (b) toward/away from the centre of the universe.

Questions to Consider:

1. *To what extent does Plato's mathematical explanation for the transformation of water, air and fire prefigure modern chemistry and physics with its postulation of lattice-style atomic bonds?*
2. *Do mathematical relations really reveal the nature of things, or do mathematical models merely 'save the phenomena'?*

Ch. 3: The Ideal of Deductive Systematization

Philosophers discussed in this chapter: Euclid (fl. 300 BCE); Archimedes (287–212 BCE)

[p. 20] Ancient writers considered science a deductive system of axioms, definitions and theorems, along the lines of Euclid's geometry and Archimedes' statics.

[p. 20–1] Deductive systematization requires:

1. axioms and theorems are deductively related
2. axioms are self-evident truths
3. theorems agree with observations

[p. 21] Some philosophers of science disagree with 2. and 3.

Euclid and Archimedes deduced theorems from axioms by:

1. *reductio ad absurdum* arguments – assume theorem to be proved is not true and then deduce a contradiction (e.g., Archimedes' proof that 'weights that balance at equal distances from a fulcrum are equal')
2. method of exhaustion – show consequences of contraries of a theorem inconsistent with axioms (e.g., Archimedes' proof that the area of a circle is equal to the area of a certain right triangle)

Euclid's geometry failed first requirement that theorems are deduced from axioms. This was fixed by Hilbert in the 19th Century.

[p. 21–2] Aristotle and Pythagorean tradition agreed with second requirement that axioms are self-evident truths.

[p. 22] Thinkers saving the appearances in mathematical astronomy disagreed with Aristotle and Pythagoreans—all that is required is that the deductive consequences agree with observations.

The third requirement that the theorems agree with observations poses problems not recognized by Euclid and Archimedes. Some of the terms in the deductive system apply to idealized entities that cannot occur in nature (e.g., 'rod' that does not bend and has perfectly uniform weight distribution).

[p. 23] Archimedes' focus on ideal entities reflects the Platonic view that the phenomenal realm (the observed world) is but an 'imitation' or 'reflection' of the 'real world'. This dualism was important for Galileo and Descartes.

Questions to Consider:

1. *Must the axioms of mathematics and geometry be self-evident to be accepted? What of the postulates used in scientific theories?*
2. *What is the relation between the idealized items in a set of axioms and the real world?*
3. *Is our observed world simply a world of appearance?*

Ch. 4: Atomism and the Concept of Underlying Mechanism

Philosophers discussed in this chapter: Democritus and Leucippus

[p. 24] The atomists Democritus and Leucippus rejected the Platonic notion that the observed world is an imperfect copy of the real world. Objects and relations in the world of sense and in the 'real world' are only different in kind.

What is real is atoms moving in the void. These motions cause our perceptual experiences (colour, smell, taste).

The properties of atoms are:

1. size
2. shape
3. impenetrability (indivisibility)
4. motion
5. various combinations and associations with other atoms

Atomists were highly influential on later scientific thinking: how observed changes are explained by more elementary processes. This was confirmed in the 17th Century by Gassendi, Boyle, Newton and others.

Atomists realized how qualities and processes at one level cannot be explained by postulating the same at a deeper level (e.g., coloured atoms do not explain colour of objects).

[pp. 24–5] Atomists sought to reduce qualitative changes at the macroscopic level to quantitative changes at the atomic level (e.g., salty taste due to the setting free of large, jagged atoms; fire penetrating bodies due to the rapid motions of tiny, spherical fire-atoms [p. 24].

Classical atomism was resisted because its uncompromising materialism:

1. challenged man's self-understanding about sensation and thought
2. left no place for spiritual values
3. banished the notion of purpose from science

It was also resisted because its explanations could not be verified (e.g., could not explain why salt dissolves in water while sand does not).

Questions to Consider:

1. *Were the atomists more successful at explaining sensations, such as colour and taste, compared with the Pythagoreans and the Aristotelians?*
2. *How has the atomists' reductive nature of explanations carried forward to current scientific theories?*

Ch. 5: Affirmation and Development of Aristotle's Method in the Medieval Period

Philosophers discussed in this chapter: Robert Grosseteste (c. 1168–1253); Roger Bacon (c. 1214–92); John Duns Scotus (c. 1265–1308); William of Ockham (c. 1280–1349); Nicolaus of Autrecourt (c. 1300–after 1350)

[p. 27] From 1150, Aristotle's works on science and scientific method gained greater authority from being translated into Latin.

[p. 28] Aristotle's ideas dominated European thought for the next three centuries.

The Inductive–Deductive Pattern of Scientific Inquiry

Robert Grosseteste and Roger Bacon repackaged Aristotle's inductive–deductive loop as the 'Method of Resolution and Composition':

- inductive stage = 'resolution' of phenomena into constituent elements
- deductive stage = 'composition' in which elements are combined to reconstruct the original phenomena

E.g., Grosseteste applied this procedure to explaining spectral colours.

[pp. 28–9] For Roger Bacon, the 'Method of Resolution and Composition' requires active experimentation to collect all relevant facts (e.g., breaking a magnetic needle to create two new magnets [p. 29]) [Bacon's 'Second Prerogative'].

[p. 29] To add to Aristotle's inductive procedure, Grosseteste suggested the inductive *Method of Difference* to test for causal power (e.g., administer test herb in situations where no other purgative agent is present). This principle was later adopted as 'Mill's Joint Method of Agreement and Difference' [LA: John Stuart Mill in the 19th Century].

[pp. 29–30] John Duns Scotus also added the *Method of Agreement* in which combinations of circumstances are examined for a common circumstance concomitant with a particular effect. Duns Scotus claimed this method can only show 'aptitudinal union' (regularity) and not causal necessity as God can intervene at any time to produce a different effect. William of Ockham agreed.

[pp. 30–1] William of Ockham's *Method of Difference* allowed in an ideal case that a cause can be established from a *single* test in which two sets of circumstances are identical except in one case a particular circumstance is present with the effect and in the other that circumstance is absent along with its effect. In practice, it's very difficult to be sure that the two sets of test circumstances are identical except for that one particular circumstance.

Evaluation of Competing Explanations

[p. 31] Grosseteste and Bacon, following Aristotle, recognized that an effect can be deduced from more than one set of premises.

They recommended that at the end of Aristotle's inductive–deductive procedure, further testing be done to new situations [Bacon's 'First Prerogative'].

[pp. 31–2] Freiberg in the 14th Century applied this method in his experimental setup testing his theory of rainbows. He successfully reproduced a secondary rainbow with reversed colours and the 11 degree separation between primary and secondary rainbows.

[p. 32] Grosseteste and Bacon often ignored their own advice for further experimentation, relying instead on *a priori* [prior to experience] deduction.

Grosseteste suggested applying a deductive method of falsification to eliminate competing hypotheses that entail the same effect.

If hypothesis *H* entails consequence *C* and it is discovered that *C* is not the case, then conclude that hypothesis *H* is false.

If *H* then *C*; not *C* therefore not *H* (*modus tollens*)

[p. 33] Grosseteste used the *modus tollens* form of argument in an attempt to disprove the hypothesis that the sun generates heat by conduction. But his attempted falsification failed because his stated consequence *C* (adjacent celestial matter undergoes a change of quality) was not shown by him to be false.

Although other philosophers and mathematicians used the *modus tollens* form of argument, Grosseteste was the first to apply it systematically. The method of falsification also became very influential (e.g., John Buridan) [p. 34].
[LA: This method of elimination became the cornerstone of Karl Popper's famous philosophy of science born in the 1940s known as 'Falsificationism'.]

[p. 34] Grosseteste also defended the principle that nature always chooses the simplest path. William of Ockham thought this principle limited God's power and so recommended the criterion of simplicity (later known as 'Ockham's Razor') for formulating concepts and constructing theories. He used this principle to eliminate the concept of 'impetus' in referring to moving bodies.

The Controversy about Necessary Truth

[p. 35] Aristotle thought that the first principles of the sciences are (a) self-evident and (b) necessarily true (could not be otherwise).

John Duns Scotus argued that first principles are necessarily true simply in virtue of the meanings of the words (granting that by sense experience we must first learn the meanings of the words) (e.g., 'opaque bodies cast shadows' is necessarily true.)

For Duns Scotus, there are two types of necessary truths:

1. first principles and their deductive consequences
2. statements of aptitudinal unions of phenomena (e.g., all ravens *can* be black)

For Duns Scotus, empirical generalizations are contingent truths (e.g., all ravens examined have been black).

[p. 36] For Duns Scotus, necessarily true generalizations must be deduced from first principles. (e.g., ‘Opaque bodies cast shadows’ and ‘Earth is an opaque body between sun and moon’ entails ‘Moon is frequently eclipsed’.)

Nicolaus of Autrecourt rejected Aristotle’s view that the first principles of the sciences (from induction) are known with certainty (i.e., are necessarily true).

[pp. 36–7] Nicolaus restricted certain knowledge to the combined premises and conclusion of a deductive argument (e.g., If *A* and *B* and *C*, then *A*) and to articles of faith. A deductive argument is valid if and only if asserting the premises and denying the conclusion is a contradiction.

[p. 37] As valid deductive arguments reveal no new information that was not already in the premises, Nicolaus argued that scientific demonstrations do not show necessary causal relations between cause and effect. Inductive arguments and statements about causes do not entail that a correlation between two events will hold in the future.
[LA: Nicolaus’ arguments expressing uncertainty about necessary causal relations gains a big foothold with David Hume’s famous skeptical argument against causal necessity in the 18th Century.]

Questions to Consider:

1. *What were the chief contributions to the development of the scientific method in the medieval period?*
2. *Does the method of falsification decisively knock out a competing theory from further consideration for all time?*
3. *Are causal relations necessary, and, if so, in what sense?*

Ch. 6: The Debate over Saving the Appearances

Philosophers discussed in this chapter: Nicolaus Copernicus (1473–1543); Johannes Kepler (1571–1630)
[LA: Note Copernicus and Kepler were deeply religious, but at opposite poles, with Copernicus being Catholic while Kepler was Protestant.]

Osiander on Mathematical Models and Physical Truth

[p. 40] Andreas Osiander supported Copernicus' sun-centred model of the solar system as a superior mathematical model for saving the appearances.

Copernicus's Pythagorean Commitment

Copernicus, as a Pythagorean, thought his model really true of the solar system. Even though his model had a similar degree of accuracy to Ptolemy's for predicting motions of the planets, he argued superiority based on 'conceptual integration':

- Ptolemy had a separate model for each planet
- The Copernican model explained the magnitudes and frequencies of the retrograde motions of the planets

Bellarmine v. Galileo

[p. 41] Cardinal Bellarmine (1615) set Galileo's Pythagorean realism against saving the appearances (where accurate mathematical models do not reveal physical truths).

The Jesuit Christopher Clavius (1581) claimed that true theorems can be deduced from false axioms and that that is all Copernicus achieved.

Galileo's *Dialogue Concerning the Two Great World Systems* advanced arguments for the physical truth of the Pythagorean Copernican system and claimed future experiments will vindicate it.

Kepler's Pythagorean Commitment

[pp. 41–2] Kepler's *Mysterium Cosmographicum* (1596) tried to prove a correlation between the distance of a planet from the sun with the radii of a spherical shell circumscribed by one of the five regular solids (revealing God's mathematical blueprint).

[p. 42] Later, using Tycho Brahe's [pronounced *Teeco Bray*] more accurate planetary data, Kepler accepted his theory was false, and yet remained committed to the Pythagorean mathematical harmony of the spheres.

[p. 43] Kepler went on to develop his three laws of planetary motion, the Third Law being a striking vindication of his Pythagorean approach. Kepler described other mathematical relations that were suspect (e.g., Kepler's Distance–Density Relation).
[LA: Note how Kepler broke radically with the Aristotelian/Ptolemaic idea that celestial bodies moved naturally in perfect circles.]

[p. 43] Non-Pythagoreans regard Kepler's distance–density correlation a coincidence.

Bode's Law

[p. 44] The Pythagorean Johann Titius (1772) roughly proved a simple mathematical formula for calculating planetary distances.

[p. 45] Johann Bode's championing of this Pythagorean relation led to its naming as 'Bode's Law'.

William Herschel's discovery (1781) of Uranus and the asteroids Ceres and Pallas (1801/1802) seemed further vindication of Bode's Law. The later discovery of Neptune finally disproved Bode's Law.

However, a contemporary Pythagorean could continue to accept Bode's Law using its fit with Pluto's orbit in conjunction with the hypothesis that Neptune is a captured planet.

Questions to Consider:

1. *How important is conceptual integration in evaluating competing theories?*
2. *Do scientific theories really tell us about the nature of reality, or are they merely calculating devices?*
3. *How can we tell the difference between coincidental mathematical relations in nature and necessary correlations?*

Ch. 7: The Seventeenth-Century Attack on Aristotelian Philosophy

Philosophers discussed in this chapter: Galileo Galilei (1564–1642); Francis Bacon (1561–1626); René Descartes (1596–1650)

I. Galileo

[p. 46] Galileo made many telescopic observations of astronomical objects that questioned the Church’s Aristotelian world-view (e.g., sunspots, Earth’s moon, Jupiter’s moon).

The preface and conclusion to his *The Dialogue Concerning the Two Chief World Systems* (1632) treated the Ptolemaic and Copernican systems as merely mathematical instruments, while the remainder argued for the reality of the Copernican system.

[pp. 46–7] After Galileo recanted before the Inquisition, his *Dialogues Concerning Two New Sciences* (1638) demonstrated the failure of Aristotle’s physics.

The Pythagorean Orientation and the Demarcation of Physics

[p. 47] As a Pythagorean, Galileo identified only mathematical properties of matter as ‘primary qualities’:

- shape
- size
- number
- position
- ‘quantity of motion’

Secondary qualities (colours, tastes, odours, sounds) exist only in the mind of the perceiver.

Galileo demarcated science from non-science by excluding from science:

- Aristotelian teleological ‘explanations’ (e.g., ‘natural motion’)
- secondary qualities

[pp. 47–8] Galileo replaced Aristotle’s qualitatively described spaces (centre of universe, sub-lunar, celestial) with a quantitative system of coordinates. Contradictorily, he also argued that celestial bodies (including the Earth) move naturally in perfect circles.

Theory of Scientific Procedure

[p. 48] Galileo agreed with Aristotle's two-stage inductive–deductive method going from observations to general principles and back to observations.

[p. 49] But he criticized Aristotelians who ignored observations by starting with first principles, holding back scientific progress (e.g., sunspots, immutability).

Galileo deduced motions of falling bodies and real pendulums by first conceptualizing them as idealized bodies. Two examples are:

1. free fall in a vacuum extrapolated from bodies falling through fluids of decreasing density
2. ideal pendulum idealized as hanging from a 'mass-less' string with no air resistance

Galileo emphasized this importance of creative imagination, over and above induction by simple enumeration and the methods of agreement and difference.

Galileo exemplified Grosseteste's and Roger Bacon's advice by deducing from his hypothesis about projectile motion two observational facts that were not used to construct his theory:

1. maximum projectile range is 45 degrees [postdiction]
2. equal projectile range is achieved from angles equidistant from 45 degrees [prediction]

Such successful novel deductions constitute strong confirmation of the theory and genuine scientific explanation.
[LA: For more on the requirement that the observational evidence be independent of theory construction, see Sec. 4 of my 'Towards an Objective Theory of Rationality' at www.rationalrealm.com/philosophy/epistemology/objective-theory-rationality-page6.html]

[p. 50] Galileo, however, was ambivalent about the need for further testing of a hypothesis beyond the initial confirmation (*contra* Grosseteste and Roger Bacon).

[pp. 50–1] Despite his expressed ambivalence, Galileo is credited with performing many experiments on falling bodies, floating bodies and observations of stars (e.g., using inclined planes and pendulums).

[pp. 51–2] On the other hand, early on, Galileo dismissed experimental evidences that did not confirm his theory of:

- the relationship between velocity of fall and density, and
- the timing and frequency of tides

He dismissed the counter-evidences as due to 'unnatural accidents' and 'secondary causes' [LA: known as '*ad hoc* hypotheses'].

The Ideal of Deductive Systematization

[p. 53] Galileo's dual legacy is his actual working examples of:

1. novel mathematical deduction as explaining physical phenomena, and
2. abstracting from material phenomena to construct idealized entities

Questions to Consider:

1. *What do you think of the distinction between primary (mathematical) qualities and secondary (phenomenal) qualities? Is it useful? Is it real?*
2. *Did Galileo do away completely with Aristotle's notion of 'natural motion'?*
3. *How important are idealised models of natural systems to scientific explanation?*
4. *How should a scientist proceed when an experimental result conflicts with their hypothesis?*

II. Francis Bacon

[p. 55] In 1620, Francis Bacon published his *Novum Organum*, the formulation of a new scientific method to replace the Aristotelian approach (*Organon*: compilation of Aristotle's writings). The publication of Bacon's, *New Atlantis* (1627) prefigured the modern scientific enterprise as a collective activity of a community of scientists.

The Controversy over the Value of Bacon's Contribution

Bacon's legacy as a champion and innovator in scientific method is disputed. The disputants agree that:

1. Bacon offered no concrete examples of his new method
2. Bacon's literary ability led many scholars to see him as a leading figure in the scientific revolution
3. Bacon's originality, if any, is his theory of scientific method

Criticism of Aristotelian Method

[p. 56] In *Novum Organum*, Bacon identified four prejudices ('Idols') that distort perception of nature:

1. Idols of the Tribe: innate tendency to hasty generalization and overemphasize confirmations
2. Idols of the Cave: learned biases
3. Idols of the Market-Place: tendency to use common meanings of words that impede concept-formation
4. Idols of the Theatre: received philosophical dogmas and methods (e.g., Aristotle's principles)

Bacon accepted Aristotle's inductive–deductive loop from observations to general principles and back to observations and saw science as a practical enterprise.

[pp. 56–7] Bacon criticized the Aristotelians' use of the inductive phase for:

1. foregoing systematic experimentation/instrumentation for haphazard data collection
2. generalizing too hastily, leaping to too general first principles
3. relying on induction by simple enumeration and ignoring negative instances

[p. 57] Bacon criticized the Aristotelians' use of the deductive phase for:

1. failing to define precisely the terms used in the deductive syllogisms
2. providing inadequate inductive support for the first principles

Bacon failed to distinguish Aristotle's procedure that emphasized the arriving at first principles by observation and induction from that of later pseudo-Aristotelians who short-circuited the procedure by starting with first principles.

"Correction" of Aristotelian Method

[p. 58] Bacon's 'new' method of science featured a pyramid of increasingly general inductive correlations ('Ladder of Axioms') and a method of excluding accidental correlations ('Method of Exclusion').

At the base of the pyramid, the scientist collects observational facts. Next, the scientist looks for correlations of increasing generality ('Ladder of Axioms').

The scientist excludes accidental correlations by inspecting Tables of Presence, Absence, and Degrees ('Method of Exclusion').

[p. 59] This 'Method of Exclusion' is superior to Aristotle's induction by simple enumeration for its ability to isolate accidental correlations from essential via the scientist looking for absences and relative intensities of correlations.

For Bacon, 'Prerogative Instances' of correlations are especially suited to identifying essential correlations. One such 'Prerogative Instance' is the 'Instance of the Fingerpost': a 'crucial' observation is used to decide between competing explanatory hypotheses. The hypothesis that entails the 'crucial' observation wins out against all the other hypotheses that entail the opposite of the observation (e.g., observing non-opposing tides on opposite coasts falsifies the 'basin hypothesis' of tides).
[LA: This method prefigured the later notion of a single 'crucial experiment' deciding between competing hypotheses.]

Bacon overemphasized the significance of a 'crucial' observation in deciding between hypotheses as there may be other unknown explanatory hypotheses that are also in agreement with the observation [LA: known later as the 'Duhem–Quine Underdetermination Thesis'].

Bacon's 'Instance of the Fingerpost' is not new as this deductive method of eliminating hypotheses had antecedents in the ideas of Aristotle, Grosseteste and Roger Bacon.

The Search for Forms

[pp. 59–60] At the apex of Bacon's induction pyramid were the metaphysical general principles he named 'Forms'. These are irreducible qualities ('simple natures') that occur in various combinations to produce effects (much like Aristotle's material and efficient causes)—not to be confused with Platonic forms or Aristotle's formal causes.
[LA: For more on Aristotle's four causes, see https://en.wikipedia.org/wiki/Four_causes]

[p. 60] Bacon thought of Forms in atomistic terms, but rejected the atomist attribution of impact and impenetrability to atoms and the idea of a continuous void. He did attribute 'forces' and 'sympathies' to atoms.

For Bacon, propositions describing Forms are universally true and the converse also true (e.g., ‘heat’ is identical to a ‘rapid expansive motion of small particles’; thus, when heat is present, so is this rapid expansive motion, and conversely so).
[LA: This deducibility of statements about micro-particles from statements about phenomena is a precursor to the later-known ‘reductionist’ view of scientific explanation.]

In some places, Bacon spoke of ‘Forms’ as physical ‘laws’. But unlike our modern notion of a ‘physical law’, Bacon’s conception of a ‘law’ remained distinctly Aristotelian in that for Bacon, physical laws:

1. are decreed by a power, as with civil laws
2. are not expressed by mathematical relations
3. express the relations between substances with properties and powers

Bacon as Propagandist for Organized Scientific Research

[p. 61] In contrast to Aristotle who saw knowledge of nature as an end in itself, for Bacon, the goal of scientific discovery is to gain power over nature for the betterment of humankind. Bacon objected morally to Aristotle’s passivity and to the Aristotelians’ resistance to progress.

[p. 62] In the pursuit of such progress, Bacon advocated for the funding of cooperative scientific projects. His vision was only realized generations later with the founding of the Royal Society.

Bacon also forced the separation between scientific enquiry and teleology/natural theology. For Bacon, disputes about final causes of natural phenomena only impedes scientific and human progress.

Questions to Consider:

1. *How would you contrast Galileo’s approach to scientific method with Francis Bacon’s?*
2. *How useful is Francis Bacon’s four classes of ‘Idols’ in minimizing psychological and perceptual biases?*
3. *Is it possible, or even desirable, to eliminate all biases when making scientific observations?*
4. *Can a single ‘crucial’ observation ever decisively eliminate a competing theory?*
5. *What are the similarities and differences between Francis Bacon’s notion of ‘Forms’ and the contemporary scientific understanding of physical laws and forces?*
6. *Do you agree with Francis Bacon that the scientific enterprise has the moral purpose of improving human lives?*

III. Descartes

[p. 63] After laying the foundations of analytic geometry (1618), Descartes had a dream in which he was called by the Spirit of Truth to reconstruct human knowledge on incorrigible foundations (similar to mathematics).

In various publications, Descartes argued for a mechanistic view of nature centred on impact and pressure of constituent particles.

Inversion of Francis Bacon's Theory of Procedure

[p. 64] Descartes inverted the procedure of enquiry in Francis Bacon's pyramid of knowledge by placing certainly known first principles as the first step [see diagram on p. 69].

Through Descartes method of doubt, he concluded the certainty of his own existence and the existence of a Perfect Being. Such a perfect being would not systematically deceive humans about the world external to the mind.

Descartes' criteria for certainty was that an idea be:

1. clear – immediately present to the mind (e.g., idea of 'bentness' of a stick partially immersed in water), and
2. distinct – both clear and unconditioned (e.g., understand law of refraction that applies to case of 'bentness' of a stick)

Primary Qualities and Secondary Qualities

[p. 65] Examining the changing attributes of a melting lump of wax, Descartes concludes the real and only 'essence' of the wax is its extension. This knowledge about the essence of material bodies is an intuition of the mind.

Descartes followed Galileo in contrasting the 'primary quality' of extension with the subjective 'secondary qualities' (colours, sounds, tastes, odours).

While rejecting the possibility of a vacuum as a contradiction in terms, Descartes accepted classical atomism and restricted scientific enquiry to what can be described mathematically. Here, he synthesised the Archimedean, Pythagorean and atomist points of view.

[p. 66] Descartes equivocated on his meaning of 'extension' between 'being filled by matter' [p. 65] and meaning a relationship a body has to other bodies. His conception of 'extension', then, was not clear and distinct (a major problem for his physics).

The General Scientific Laws

[pp. 66–7] From Descartes' understanding of extension, he derived three important *a priori* physical principles:

1. all motion is caused by impact or pressure (denying occult action-at-a-distance, such as magnetic and gravitational forces)
2. all motion is a cyclical rearrangement of bodies in a closed loop (as there is no vacuum) [p. 67]
3. motion is conserved perpetually (as God would not let the universe run down)

[p. 67] From this principle of conservation of motion, Descartes derived three subsidiary laws of motion about inertia (Law I), natural motion in a straight line (Law II) and conservation of motion on impact (Law III (A)/Law III (B)).
[LA: As modern as these laws are, Descartes conservation of motion laws were later superseded by the law of conservation of energy.]

From these three laws, Descartes further derived seven rules of impact for specific kinds of collisions. These laws turned out to be mistaken as Descartes took size to be a determining factor and not weight.

[p. 68] Descartes system of deduction from first principles is wide in scope (shown on p. 69].

Empirical Emphases in Descartes's Philosophy of Science

Although wide in scope, knowledge of first principles is limited in application as the same principles apply to an indefinitely many circumstances. As Descartes conceded to Francis Bacon, the scientist must still seek correlations among discrete phenomena (e.g., knowledge of anatomical structure for deductions in physiology).

[p. 69] A second role for observation for Descartes was suggesting hypotheses about mechanisms, usually employing an analogy with everyday experience (e.g., motions of planets analogous to bits of cork in a whirlpool).

[p. 70] Sometimes Descartes' analogies led him to ignore existing evidence (e.g., he ignored Harvey's evidence that the heart contracts with each pulse).

Descartes recognized that an observation may be deduced from more than one set of explanatory premises (consisting of laws of nature plus circumstances plus hypothesis). He specified that other observations be found that are deduced from one hypothesis but not the others. Descartes often did not follow his own advice, seeing experimentation as useful in formulating a hypothesis while ignoring their use in verifying a hypothesis.

[p. 71] Descartes gave due weight to the need for certainty and to the complexity of phenomena.

Malebranche saved Descartes principle of the preservation of 'quantity of motion' by interpreting it as 'momentum'. Descartes' general laws can only explain phenomena when used in conjunction with discrete facts and lower-level hypotheses. In that way, discrepancies between Descartes general laws and observation can always be fixed by modifying the lower-level hypotheses while leaving Descartes' principles intact. That flexibility guaranteed the use of Descartes' system into the next century.

Questions to Consider:

1. *Can a priori reasoning alone validate first principles in science?*
2. *Is Descartes' inversion of the procedure of enquiry an improvement on Francis Bacon's pyramid of knowledge?*
3. *Do scientists need God to guarantee the reliability of our perceptions of the external world?*
4. *How important are analogies in science for building theories? If they are important, what role do they play?*
5. *Is the fact that a general law can always be saved from falsification by a modification of a lower-level hypothesis detrimental to the notion of objective scientific knowledge?*

Ch. 8: Newton's Axiomatic Method

Philosophers discussed in this chapter: Isaac Newton (1642–1727)

[p. 72] In two years (1655–57), Newton had:

- formulated the binomial theorem
- developed the calculus ('method of fluxions')
- constructed the first reflecting telescope
- discovered the universal nature of gravitational attraction

[LA: Note that Newton appeared at the tail end of the giants, Galileo Galilei, Francis Bacon and René Descartes.]

Newton appointed Professor of Mathematics at Cambridge in 1669.

Newton elected fellow of the Royal Society in 1672 and President in 1703.

Robert Hooke accused Newton of appropriating his theoretical explanation of elliptical planetary motion.

Leibniz quarrelled with Newton on who developed the calculus.

The Method of Analysis and Synthesis

[p. 73] In his *Opticks*, Newton opposed Descartes' derivation of basic physical laws from metaphysics, insisting on experiment and observation instead.

Newton improved upon Aristotle's inductive–deductive procedure and the 'Method of Analysis and Synthesis' advanced by Grosseteste, Roger Bacon, Galileo and Francis Bacon by emphasizing the importance of:

- confirmation of deduction from the Synthesis stage, and
- deduction of consequences beyond the original inductive evidence [novel prediction]

Newton's method was vindicated with his experiments on light and prisms. By the Method of Analysis, he induced the theory of light as composed of different refracted colors.

This was not Newton using induction by simple enumeration, but theorizing about the nature of light.

[p. 74] By the Method of Synthesis, Newton deduced further consequences of his theory: that red light from a prism will be bent at a certain angle through a second prism. This was confirmed by experiment.

[pp. 74–5] Newton claimed in his *Mathematical Principles of Natural Philosophy* that he formulated his three laws of motion using the Method of Analysis; a process of induction of general laws from particular propositions.

[p. 75] But there are two senses of 'induction':

1. intuitive insight about ideal bodies (*qua* Aristotle) –broad sense
2. simple enumeration and methods of agreement and difference –restricted sense

Newton could not have derived his laws using induction in the restricted sense (e.g., no bodies described by the first law exist or can be observed on Newton's own account).

So, Newton's laws of motion are obtained inductively only in the broad sense; by abstraction from particulars.

Newton's concepts of Absolute Space and Absolute Time are also abstractions from their physical measurement. For Newton, Absolute Space and Absolute Time are ontologically prior to bodies and their motions.

[p. 76] Newton conceded that it may not be possible to measure Absolute Time as there may be no equable motion to use as a basis of measurement. He recommended using the eclipses of Jupiter's moons and the vibrations of pendulums as standard measures.

Against Descartes, Newton advanced a theological argument for the existence of Absolute Space as a receptacle for creation *ex nihilo*.

Newton also advanced his rotating bucket experiment as a physical argument for the existence of Absolute Space as there is no fixed correlation between acceleration and deformation of the water (see table on p. 77).

[p. 77] Newton concluded from his rotating bucket experiment that the acceleration of the water is with respect to Absolute Space. Ernest Mach and others objected that the acceleration may be with respect to the fixed stars.

Newton conceded that even if his experiment demonstrated absolute motion with respect to Absolute Space, he could not specify a system of co-ordinates for locating bodies in such space.

Newton's discussion of this problem illustrates his axiomatic method of analysis instead of his propounded inductive method.

An Axiomatic Method

Three stages in Newton’s Axiomatic Method:

1. formulating an axiom system with axioms, definitions, and theorems
2. specifying a procedure for correlating theorems of the axiom system with observations
3. confirming the deductive consequences of the empirically interpreted axiom system

[pp. 77–8] *Stage 1: Formulate axiom system*

In Newton’s theory of mechanics, the three axioms are:

1. Every body is at rest or rectilinear motion unless acted upon by a force.
2. Change of motion is proportional to the force impressed and is in line with that force.
3. Every action is opposed by an equal and opposite reaction.

[p. 78] Newton distinguished his axiomatic ‘absolute magnitudes’ from their experimental ‘sensible measures’.

[p. 79] *Stage 2: Correlate theorems with observations*

Newton’s Theory of Colour-Mixing relied on Pythagorean speculation and not inductive generalization for his pie slicing of the ‘principal colours’. Granting this non-empirical axiom, Newton failed to provide a procedure for empirically determining the ‘number of rays’ for each colour.

On the other hand, with the ‘Rules of Correspondence’ used in his mechanics, Newton did provide links between statements about Absolute spatial and temporal intervals with events measured in the physical world. He used the centre of gravity of the solar system as the Absolute reference point for measuring distance.

[p. 80] For measuring Absolute Time intervals, Newton favoured linking these with the swings of a pendulum as this procedure delivered measurements that are more regular. [LA: Note how Newton’s choice of measuring instrument here presumes that the acceleration of balls down a plane is ‘regular’, which is precisely what he wished to prove with the use of the pendulum.]

[p. 81] Newton’s most important contribution to the theory of scientific method and deductive systematization was his distinction between the axiom system and its application to experience.

Stage 3: Confirm deductive consequences of interpreted axiom system

Newton recognized that the degree of confirmation between the theorems and empirical observation can be increased by progressive modification of the original empirical assumptions (e.g., modification of assumption that earth is a homogeneous sphere to better account for the moon's motion).

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Newton failed to distinguish clearly his two theories of scientific procedure:

1. Method of Analysis and Synthesis – generalizing from the results of observation and experiment
2. Axiomatic Method – using creative imagination to create a formal system

"Hypotheses Non Fingo"

[p. 82] Newton agreed with Galileo in restricting the subject of physics to primary qualities ('manifest qualities').

Newton eschewed entertaining 'hypotheses' in his scientific work. For Newton:

hypothesis = statements about 'occult qualities' that cannot be measured

theory = invariant relations among manifest qualities

For Newton, his theory of colours had conclusive experimental evidence about refractive properties while avoiding any 'hypothesis' about the nature of light as waves or corpuscles.

For Newton, his theory of gravitational attraction was established while avoiding any 'hypothesis' about the underlying cause of the attraction. He rejected Descartes' Vortex Hypothesis.

[pp. 82–3] Inconsistently, Newton sometimes entertained hypotheses (e.g., the ether), while insisting that the sole purpose of such hypotheses is to direct future research.

The Rules of Reasoning in Philosophy

[p. 83] Newton nominated four regulative principles ('rules of reasoning in philosophy') for finding *fruitful* explanatory hypotheses:

1. Only admit causes of natural things that are both true and sufficient to explain their appearances.
2. To the same natural effects, assign the same causes.
3. Assume the qualities of bodies found in experiments to apply to all bodies.
4. Assume generalizations using induction to be true until contradicted by new observations.

[pp. 83–4] William Whewell objected that Rule 1's reference to 'true' cause may be either too restrictive or too vague and suggested that 'true' cause refer to a cause embedded within a theory that had diverse supports.

John Stuart Mill suggested that 'true' cause refer to a cause with independent evidence in its favour.

[p. 84] For Rule 3, Newton accepted the qualities that apply to all bodies as including extension, hardness, impenetrability, mobility and inertia.

Newton's insistence that these qualities also apply to the micro-constituents of bodies set off future research programmes in chemistry and electromagnetism.

The Contingent Nature of Scientific Laws

[pp. 84–5] Newton rejected Descartes' deduction of scientific laws from metaphysical principles and their necessity. He saw scientific knowledge as contingent and provisional.

Questions to Consider:

1. *Why do you think scientists regard authorship of a theory as so important?*
2. *Did Newton link successfully his axioms with observable quantities?*
3. *What do you take as the difference between the Method of Analysis and Synthesis and the Axiomatic Method?*
4. *Is Losee's distinction between two senses of 'induction' defensible?*
5. *How are the terms 'theory' and 'hypothesis' used by scientists today different compared with Newton's usage?*
6. *What do you see as Newton's greatest achievements in the theory of scientific procedure?*

Ch. 9: Analyses of the Implications of the New Science for a Theory of Scientific Method

I. The Cognitive Status of Scientific Laws

Philosophers discussed in this section: John Locke (1632–1704); Gottfried Wilhelm Leibniz (1646–1716); David Hume (1711–76); Immanuel Kant (1724–1804)

Locke on the Possibility of a Necessary Knowledge of Nature

[p. 87] Locke specified two conditions for necessary *a priori* knowledge of nature:

1. knowledge of configurations and motions of atoms
2. knowledge of how motions of atoms produce primary and secondary qualities

[p. 88] For Locke, 1. is unachievable due to the minuteness of atoms, and 2. is unachievable because we are ignorant of how the power of atomic motions work (need divine revelation?)

Locke sometimes posited an unbridgeable epistemological gap between atomic motions and subjective ideas. Subsequently, he had no interest in hypothesizing about atomic motions, advocating instead Baconian inductivism.

At other times, Locke drew back from skepticism and posited a necessary connection between atomic motions in the external, mind-independent world and our subjective experience of secondary qualities.

Locke used his term, 'idea', to bridge the gap between the external and internal worlds.
[LA: Locke's 'idea' is synonymous with our term 'percept' where a 'perception' is had by a 'perceiver' standing in a causal relation to an object in the external world.]

[p. 89] Later, Berkeley and Hume demanded a justification for belief in this causal link to external objects.

Leibniz on the Relationship between Science and Metaphysics

Leibniz used his scientific contributions to inform his metaphysics, and vice versa.

Three examples:

1. He used his metaphysical principle of continuity to correct Descartes' theory of motion after impact.
2. His application of the differential calculus using *extremum* principles to cases of light refraction was motivated by his theological metaphysical principle of maximum simplicity/perfection.

[LA: In calculus, an *extremum* is a point at which a value is a maximum or a minimum.]

3. [p. 90] He sought a correlation for his metaphysics of monadic activity in the physical law of conservation of *vis viva* (mv^2).
   [LA: In Leibniz's metaphysics, 'monads' are mind-like simple substances endowed with perception and active force/'appetite'. For a basic intro, see https://en.wikipedia.org/wiki/Monadology. For a more technical treatment, see https://plato.stanford.edu/entries/leibniz/.]

Leibniz saw the natural universe and its causal laws as the embodiment of the Perfect Being's teleological principles (e.g., *extremum* principles reflect natural processes' teleological striving for a minimum or maximum value).

Leibniz eschewed Locke's epistemological uncertainty over 'real essences' of things for the necessary truths revealed by his teleological metaphysics of perfection.

[p. 91] At one point, Leibniz used an analogy with the generation of an infinite series to argue that empirical laws could be derived from his metaphysical principles. Losee objects that analogy breaks down because the position in the series must be specified. Leibniz leaves unresolved the link between the metaphysical and physical realms.

Hume's Scepticism

Hume extended Locke's skepticism about formulating necessary principles of nature founded on atomic interactions.

[p. 92] *Contra* Locke, for Hume, even if we knew the internal micro-constituency of bodies, we would remain ignorant of necessary connections between phenomena. All we know is contingent constant conjunctions between physical events.

Hume denied necessary knowledge of empirical laws based on:

1. an unbridgeable epistemic divide between 'relations of ideas' and 'matters of fact'

2. all knowledge of matters of fact is based on sense impressions

3. necessary knowledge of natural laws presupposes knowledge of necessary connections in nature

On 1., Hume thought relations of ideas are necessary truths because denying any one results in self-contradiction (e.g., Euclid's theorems in geometry). On the other hand, matters of fact are always contingent as it's not self-contradictory to deny any and all.

On 2., for Hume, the necessary truth of the relations of ideas is independent of empirical evidence and is either (1) known intuitively from meanings of terms (e.g., Euclid's axioms), or (2) known demonstratively (e.g., Euclid's theorems).

[p. 93] Truth of matters of fact about events, on the other hand, are not determined by reflecting on the meaning of words alone.

Hume sharpened Newton's distinction between an axiomatic system and its application to experience, and posed a problem for naïve Pythagoreanism.

Contra Descartes, Hume argued there are no innate ideas, such as of mind, body, God, world. Mirroring Aristotle, all ideas come from antecedent sense impressions (both internal and external). Hume allowed for operations of the mind on impressions: compounding, transposing, augmenting, diminishing.

Hume's thesis is both a psychological hypothesis and a theory of empirical significance. Hume excluded from empirical significance: 'vacuum', 'substance', 'perduring selfhood', 'necessary connectedness of events'.

[p. 94] Hume was not a naïve Baconian inductivist. Although Hume recognized Newton's axiomatization of physics from creative insight, he denied it the status of necessary truth.

Whereas Bacon and Locke discussed necessary connections among properties, Hume focused on events. He argued that event B following event A is never necessary as it is not self-contradictory to deny the causal proposition.

Hume concluded that our knowledge of 'causes' does not extend beyond a *de facto* conjunction of two types of event. Our feeling of causal 'necessity' results from habit and is unjustified.

[p. 95] Hume defined 'causal relation' as both:

1. a constant conjunction between two kinds of events (objective)
2. the mind leading to anticipate a second type of event following an event of a different type (subjective)

However, in his *An Enquiry Concerning Human Understanding*, Hume offered in addition a counterfactual definition of an objective causal relation (i.e., if A had not been, B would not exist). Losee offers a counterexample to Hume describing two synchronous pendulum clocks; that if the first clock is stopped, the second will not stop. Hume's two definitions pull apart.

Hume's uneasiness with his definition is also shown in his *A Treatise of Human Nature* where he includes versions of the Methods of Agreement, Difference, and Concomitant Variations.

Losee notes that the Method of Difference establishes a causal connection on the basis of just two observations, *contra* Hume's definition. [LA: For the Method of Difference, see Losee pp. 30–1.] Hume responded that the belief is still based on custom.

[pp. 95–6] Having demolished proof of necessary causal connections, Hume maintained a confidence in science as giving probable knowledge and saw 'custom' as a legitimate guide to knowledge.

Kant on Regulative Principles in Science

[p. 96] Kant accepted Hume's conclusion that sense experience cannot justify belief in causal necessity. *Contra* Hume, he argued the mind provided structure to experience.

Kant specified three stages in knowing about physical reality (see diagram on p. 97):

1. 'Forms of the Sensibility' structure 'sensations' with respect to Space and Time → 'perceptions'
2. 'Categories of the Understanding' relate the ordered 'perceptions' to each other via concepts of Unity, Substantiality, Causality and Contingency → 'judgements of experience'
3. 'Regulative Principles of Reason' organize the 'judgements of experience' into a single system of knowledge

[p. 97] Kant charged Hume with ignoring the importance of the organization of scientific knowledge into deductive systems. Kant's 'Regulative Principles of Reason' function to prescribe how scientific theories ought to be organized.

[pp. 97–8] Kant prescribed criteria of acceptability for:

- empirical laws – must be incorporated into higher-level deductive systems (e.g., incorporation of Kepler's laws into Newtonian mechanics)
- theories – must be testable with power to predict novel experience via binding together empirical laws using new entities or relations

[p. 98] For Kant, an important mark in a scientific theory's favour is its fertility for extending knowledge to new experiences and connecting previously thought disparate phenomena.

Kant pointed to three 'analogies of experience' as necessary conditions for objective empirical knowledge:

1. Conservation of Substance – substance is conserved throughout all changes
2. Principle of Causality – a rule specifies the antecedent circumstances for every event
3. Community of Interaction – substances perceived as coexistent in space interact with one another

Kant thought his three 'analogies of experience' translate respectively to the three principles of mechanics:

1. Conservation of Matter
2. Principle of Inertia
3. Equality of Action and Reaction

[p. 99] Kant held that his three principles of mechanics (‘Regulative Principles of Reason’):

- ought to guide the search for empirical laws, and
- set the criteria for adequate scientific explanation and objective empirical knowledge

[LA: Note how here Kant attempts to elevate Newton’s Three Laws of Motion into necessary *a priori* knowledge in a vein similar to Descartes’ Rationalist project.]

Kant promoted the Principle of Purposiveness as a further regulative principle not borrowed from experience and without which the systematization of knowledge is not possible.

[p. 100] The Principle of Purposiveness is the presuppositions of (1) parsimony of paths, (2) continuity, (3) parsimony of interaction types, (4) comprehensible hierarchies, (5) ascending hierarchies.

Kant suggested three other regulative principles for taxonomic ordering:

1. Principle of Homogeneousness – disregard specific differences so species are grouped into genera (to prevent explosion of species and genera)
2. Principle of Specification – emphasize specific differences so species are divided into subspecies (to prevent hasty generalization)
3. Principle of the Continuity of Forms – assume a gradual transition from species to species (to balance first two principles)

Against naïve empiricism, Kant also supported the use of idealized entities in scientific theories (e.g., ‘pure earth’, ‘pure water’, ‘pure air’) to aid systematic organization and scientific explanation.

[p. 101] Kant saw his Principle of Purposiveness as regulative (i.e., investigate *as if* laws of nature were arranged by a super-human ‘understanding’) and not as genuinely teleological.

For Kant, teleological explanations are useful for:

1. their heuristic value in the search for causal laws
2. supplementing available causal interpretations

Kant was doubtful that causal explanations could be given for all life processes because they show a reciprocal dependence of part and whole.

For Kant, purposiveness is a regulative principle only that aids the systematic organization of empirical laws.
[LA: This is unclear. How can it be a regulative principle only helping to find causal laws

when explanations pointing to ‘purpose’, for Kant, may be an unavoidable supplement to causal explanations?]

By making teleology a regulative principle only, Kant achieved the integration of teleological and mechanistic emphases that Leibniz wanted.

Questions to Consider:

1. *How much do you think proper explanations of physical phenomena depend on metaphysical principles?*
2. *How convinced are you of Hume’s argument that the Method of Difference does not contradict his definition of ‘causal relation’ based on ‘constant conjunction’?*
3. *Is there another type of ‘necessity’ other than logical necessity that can account for necessity between cause and effect?*
4. *What role does experience play in Kant’s theory of knowledge?*
5. *Did Kant successfully integrate teleological and causal explanations in his theory of scientific explanation?*

II. Theories of Scientific Procedure

Philosophers discussed in this section: John Herschel (1792–1871); William Whewell [pronounced 'HUGH-ell'] (1794–1866); Émile Meyerson (1859–1933)

[pp. 103–4] Major works were:

Herschel: *A Preliminary Discourse on the Study of Natural Philosophy* (1830)

Whewell: *History of the Inductive Sciences* (1837)

Philosophy of the Inductive Sciences (1840)

Meyerson: *Identity and Reality* (1907)

John Herschel's Theory of Scientific Method

[p. 104] Herschel's *Preliminary Discourse on Natural Philosophy* was the most comprehensive work to date. He distinguished between the 'context of discovery' (induction/wild guess?) and the 'context of justification' (criteria for acceptability), with the former irrelevant to the later.

For Herschel, scientific discoveries of laws come from: (see diagram on p. 105)

1. induction (Baconian method); or
2. hypotheses formulation

Step 1: Subdivide complex phenomena into their parts/aspects and focus on those properties crucial for explanation. (Examples: for motion of bodies, focus on force, mass, velocity; for sound, focus on vibration, transmission, reception, production).

[p. 105] Laws of Nature include:

1. correlations of properties (e.g., Boyle's Law and law of doubly refracting substances)
2. sequences of events (e.g., Galileo's laws of free fall and trajectory of projectiles)

and are not boundless (e.g., Boyle's Law applies only where temperature is constant).

[p. 106] Laws of nature are discovered by:

1. induction from experimentation (e.g., Boyle's inverse law for gases)
2. hypotheses formulation (e.g., Huygens' postulation of elliptical propagation of light ray)

Step 2: Construct a theory that incorporates previously unconnected laws either by:

1. inductive generalization (i.e., Bacon's hierarchy of scientific generalizations); or

2. hypotheses formulation (e.g., Ampère's postulation of circulating electric currents in magnets)

Creative theories, although not inductively derived, are tested by their experimental consequences.

[p. 107] An experiment is a severe test for a law or theory when an observation is:

1. an extreme case of a law (e.g., falling coin and feather as test for Galileo's law of falling bodies)

2. an unexpected result not within the original design of the law or theory (e.g., elliptic orbits of binary star, discrepancy between calculated and observed velocities of sound systems)

3. a decider between competing hypotheses ('crucial experiment') (e.g., between attraction to Earth and internal mechanism theories of acceleration; between atmospheric pressure and 'abhorrence of a vacuum' theories of mercury rise)

Losee points out that an experiment is truly a 'crucial' decider between hypotheses only if every possible alternative hypothesis is inconsistent with the observed results. This oversimplification led scientists to accept Foucault's conclusion in support of Huygens' wave theory of light against Newton's theory.

[p. 108] Herschel rightly pointed to the methodological significance of scientists searching for falsifying instances of their theories.
[LA: Herschel's emphasis on falsification as a test for theories was a prelude to Karl Popper's philosophy of 'Falsificationism' in the mid-twentieth century.]

Whewell's Conclusions about the History of the Sciences

In writing his history of science, Whewell developed a sophisticated historical methodology focused on polarity of 'facts' and 'ideas'.

For Whewell, a 'fact' is an item of knowledge used for the formulation of laws and theories.

[pp. 108–9] A 'fact' includes both:

1. report of a perceptual experience of individual objects

2. scientific law or theory incorporated into a more general theory (e.g., Kepler's Laws)

[p. 109] For Whewell, an 'idea' is a rational principle that binds together 'facts'. He affirmed Kant's thesis that 'ideas' are not derived from sensations, but prescribe to them (e.g., space, time, cause, 'vital forces').

There is no ‘pure fact’ divorced from all ‘ideas’ as all ‘facts’ involve ideas of space, time and number. When we label a ‘fact’, we are ignoring how it integrates a sense experience with theory (e.g., the ‘fact’ that one year is 365 days integrates ideas of time, number, recurrence).

Theory is a conscious inference while Fact is an unconscious inference. The Fact/Theory distinction is still useful as a psychological distinction and for interpreting the history of science.
[LA: This entanglement of fact and theory in observation later came to be termed the ‘theory-ladenness’ of observation statements.]

Whewell’s Pattern of Scientific Discovery consists of three overlapping stages: (see diagram on p. 110)

1. prelude: collection/decomposition of facts; clarification of concepts
2. inductive epoch: conceptual pattern is superinduced on the facts
3. sequel: consolidation/extension of the integration of the facts

[p. 110] *Stage 1: Decomposition of Facts and Explication of Conceptions*

Decomposition of facts is clearly and distinctly reducing to relations among ‘elementary’ facts (e.g., space, time, number, force) and measuring quantitatively.

[pp. 110–11] Explication of conceptions is the progressive clarification of concepts showing their logical relations to fundamental ideas.

[p. 111] Fundamental ideas are expressed by a set of axioms, with derivative conceptions (e.g., ‘accelerating force’) helping to understand their ‘necessary cogency’ ‘clearly and steadily’.

Recognizing an idea ‘clearly and steadily’ is only done in hindsight following the historical success of a theory (e.g., the progressive clarification of the concept of ‘inertia’ by Galileo, Descartes and Newton).

Useful scientific conceptions are also ‘appropriate’ to the facts. Some conceptions can be ruled out *a priori* as not ‘appropriate’ (e.g., use of mechanical/chemical principles in physiology). Other conceptions are admitted when the laws and theories in which they are embedded are confirmed.

Stage 2: Colligation of Facts

The scientist superinduces (inductive ‘binding together’) a conception upon a set of facts (e.g., Kepler’s Third Law binds planets’ periods of revolution and distances from the sun using the mathematical relations of squares and cubes).

[p. 112] For Whewell, ‘induction’ is not the application of rules to generate hypotheses, but the use of creative insight. Induction is framing several tentative hypotheses and selecting the right one.

Whewell did recognize certain regulative principles in selecting hypotheses (simplicity, continuity, symmetry) and specific inductive methods in formulating quantifiable laws (e.g., least squares, residues).

Stage 3: Tributary—River Analogy

Whewell saw the history of science as a progressive incorporation of earlier successes into more comprehensive theories—as tributaries flow into a river (e.g., Kepler's Laws, Galileo's Law of Free Fall, motions of tides, etc., into Newton's theory).

[pp. 112–13] Even rejected theories (e.g., Phlogiston Theory) contributed to the progressive development of their successors.

[p. 113] Whewell's Inductive Table illustrates how a 'consilience of inductions' leads from a myriad of specific facts to progressively increasing levels of generalization.

[pp. 113–14] But this increasing level of generalization is not simply a summation/enumeration of lower-level generalizations. Incorporation is via conceptual integration using new concepts (e.g., force, inertial motion, Absolute Space, Absolute Time).

[p. 114] For Whewell, this 'consilience of inductions':

- only happens when the theoretical concepts attempting the binding are up to the task; and
- is a test for the acceptability of a scientific theory

Example of successful consilience: Newtonian elastic collisions in a gas bind together the empirical laws of Boyle, Charles and Graham.

In sympathy with Kant, Whewell distinguished the *form* from the *content* of knowledge. Hence, he regarded some physical laws as necessarily true. [L.A.: Recall Kant's three *a priori* principles of mechanics on p. 99.]

Along with Hume, Whewell thought it not contradictory for a conjunction of events to be otherwise. [L.A.: See p. 94 for Hume's skepticism over necessary causal connections.]

[p. 115] Whewell tried to resolve this paradox by holding that Newton's laws of motion exemplify the *form* of the Idea of Causation. As the Idea of Causation is a necessary prerequisite for objective knowledge, this necessity flows to Newton's laws.

The experimental confirmation of Newton's laws specifies the *content* of the three axioms implicit in the Idea of Causation; viz.: (1) universality, (2) proportionality, (3) reciprocity.

The content provided by Newton's laws is:

- matter has no internal cause of acceleration
- forces are compounded in certain ways
- certain definitions of 'action'/'reaction' are appropriate

For Whewell, this exemplification of the *a priori* Ideas by the fundamental laws of nature is a gradual clarification as the history of science progresses. Although certain about the necessity of Newton's general laws of mechanics, he was less certain about other general laws.

Meyerson on the Search for Conservation Laws

Meyerson credited Whewell for being the first to explain the *a priori* necessity of the fundamental laws of motion (Newton's laws).

For Meyerson, there are two types of scientific laws:

1. empirical laws – specify how system changes when conditions are modified
2. causal laws – apply Law of Identity to specify what does not change in an interaction

[p. 116] Empirical laws allow prediction while causal laws allow understanding.

As causal laws state an identity, they imply a necessary truth.
As causal laws are empirical, they imply a contingent truth.

So, a particular causal law may turn out to be false (e.g. conservation of mass, conservation of parity). But the Law of Identity remains intact.

Losee points out that the Law of Identity is a tautology [A = A] that entails nothing about the real world. But Meyerson thought it a 'significant' tautology that serves as an essential directive principle that leads to understanding nature. Exemplars are atomic theory and the conservation laws of mechanics.

Example of a challenge to this directive principle is Carnot's Principle (Second Law of Thermodynamics) in which entropy (disorder) increases in a closed system. Meyerson concluded entropy is not a 'substance' and Carnot's Principle is not a law of nature.

Questions to Consider:

1. *In Herschel's system, how crucial is a 'crucial experiment' really?*
2. *How important is it for a scientist to try to falsify their theory?*
3. *Is there a substantive distinction between a 'fact' and a 'theory'? If so, what is it?*
4. *How useful is Whewell's three stage Pattern of Discovery?*
5. *Can you see Whewell's 'consilience of inductions' happening across scientific disciplines as well?*
6. *Did Whewell resolve satisfactorily the paradox of the necessity of the fundamental laws of nature?*
7. *Did Meyerson explain successfully how causal laws are a priori necessary using the Law of Identity?*

III. Structure of Scientific Theories

Philosophers discussed in this section: Pierre Duhem (1861–1916); Norman R. Campbell (1880–1949); Mary B. Hesse (1924—); R. Harré (1927—)

[p. 118] Major works were:

Duhem: *The Aim and Structure of Physical Theory* (1906)

Campbell: *Physics: The Elements* (1919)

Foundations of Science (1957)

Pure Geometry and Physical Geometry

[p. 118] Non-Euclidean geometries in the 19th Century drew attention to the distinction between an axiom system and its application to experience.

[p. 119] Lobachevsky and Riemann's non-Euclidean geometries had differing axioms about points and lines and entailed theorems that the sum of angles in a triangle is greater/less than 180 degrees.

For Helmholtz, *a priori* axioms of 'pure geometry' only become empirically significant when conjoined with specific principles of mechanics showing how 'point', 'angle', etc. are to be measured.

Duhem on the Binding Together of Laws

Duhem agreed with Whewell that successful scientific theories bind together various experimental laws, but insisted that theories are not explanatory (i.e., they do not point to reality behind phenomena).

[p. 120] For Duhem, a scientific theory is exhausted by its axioms and the 'rules of correspondence' that link some of the terms in the axioms with experimental measures. The theory is not arrived at by inductive inference from particular observations/empirical laws. Also, the picture/model plays no part in the deductive system.

Example: kinetic theory of gases links the theoretical 'molecule', 'velocity', 'mass' and root-mean-square velocity to measured gas pressure and temperature. The theory binds together and deduces Boyle's, Charles' and Graham's empirical laws.

For Duhem, the model of elastic collisions between point-masses may serve as a heuristic for future research.
[LA: How can the model be a heuristic for future research if the model is a fiction?]

Duhem agreed with Whewell that there are no facts devoid of theory. All experiments are interpreted using a theory (e.g., a pointer reading of 3.5 is interpreted as a particular amount of current in a circuit).

[p. 121] For Duhem, unavoidable experimental error means a given measurement is consistent with indefinitely many 'theoretical facts'. He rejected Newton's dictum that theory be arrived at by inductive generalization from observation statements.

Campbell on 'Hypotheses' and 'Dictionaries'

(pp. 121–2) For Campbell, a physical theory is composed of:

1. hypothesis – non-empirical set of axioms and theorems
2. dictionary – relates terms in hypothesis to empirical truths

(p. 122) Campbell agreed with Duhem that not every theoretical term requires a dictionary link to an empirical measure in order for the theory to have empirical significance (e.g., in the kinetic theory of gases, there is no entry for mass and velocity of an individual molecule).

(p. 123) Campbell distinguished between:

1. mathematical theories – every term correlated directly with an empirical measure (e.g., physical geometry)
2. mechanical theories – some terms only correlated via functions (e.g., kinetic theory of gases)

(pp. 123–4) *Contra* Duhem, Campbell held that extended analogy (positive-plus-neutral) plays a crucial role in scientific explanation (e.g., van der Waals extension of the kinetic theory of gases to volume of and forces between particles).

(p. 124) Campbell draws an example from the mathematical relationship between electrical resistance and temperature to show that deduction of an empirical law is necessary for a theory but not sufficient as:

1. the hypothesis-plus-dictionary was constructed only to deduce the empirical law
2. indefinitely many such hypothesis-plus-dictionary can deduce the same law/s

(p. 125) But for a mathematical theory, the analogy is where the theory from which the experimental laws are deduced is of the same mathematical form as the laws (e.g., Fourier's theory of heat conduction).

For Campbell, constructing successful analogies requires imagination and not simply induction from experimental laws. A successful theory must be:

1. internally consistent
2. able to deduce experimental laws
3. heuristically useful

(p. 126) Hempel rejected Campbell's insistence on analogical explanation with the example of an *ad hoc* theory (analogous to Ohm's Law) that also deduced the mathematical relationship between electrical resistance and temperature.

Hempel argued that this *ad hoc* theory has no explanatory power as it deduces only one empirical law—it fails in conceptually integrating a number of empirical laws. Although the analogy may be heuristically useful in guiding future research.

(p. 127) Losee points out that Hempel's counterexample of an analogical theory with no explanatory power does not refute Campbell's claim that a successful analogy is necessary for scientific explanation.

Hesse on the Scientific Use of Analogies

Hesse proposed two types of independent relations between an analogue and what is explained:

1. horizontal – similarity relations between the properties of the analogue and what is explained
2. vertical – same causal/functional relations for the analogue and what is explained

Example: properties of sound and properties of light (see diagram on p. 127). Each type of relation may be challenged.

(p. 128) But this example is different to Hempel's counterexample in which the horizontal and vertical relations are dependent ('formal analogy'). On the other hand, a 'material analogy' with independent relations invites reasons for accepting the analogy beyond mere formal identity.

Harré on the Importance of Underlying Mechanisms

(p. 129) In contrast with the formal, deductive Duhem–Hempel view, Harré put scientific models (such as Copernicus') at the centre. Harré noted three components of a scientific theory:

1. statements about the model – postulates existence of theoretical entities and theorizes their behaviour
2. transformation rules – comprises causal hypotheses and modal transforms
3. empirical laws (e.g., PV/T = constant)

A model's existential hypotheses (e.g., there exists capillaries, radio waves, neutrinos) drives scientific progress.

(p. 130) For Harré, trying to confirm a model's existential hypothesis can either:

1. lead to demonstration (e.g., Mendeleef's prediction of Scandium, Gallium, Germanium), or
2. fail demonstration (e.g., planet interior to Mercury, the ether), or
3. fail recognition (e.g., Galen's pores occupied by continuous muscle in heart), or
4. lead to recategorization (e.g., 'caloric' substance reinterpreted as average kinetic energy)

For Harré, advances in understanding underlying mechanisms comes from analogies that generate existential hypotheses. To that extent, Campbell's and Hempel's deductive systems for electrical resistance versus temperature are inadequate as explanations.

Questions to Consider:

1. *How does the distinction between pure geometry and physical geometry impact Kant's 'Forms of the Sensibility' that structure our perception of space?*
2. *Is Duhem right in thinking that scientific theories are not explanatory?*
3. *What is the role of analogical thinking in science? Do analogies serve an explanatory function?*
4. *Must a scientific theory posit an existing theoretical entity to drive progress?*
5. *From the work of these philosophers of science, is induction by simple enumeration dead?*

Ch. 10: Inductivism v. the Hypothetico-Deductive View of Science

Philosophers discussed in this chapter: John Stuart Mill (1806–73); William Stanley Jevons (1832–82)

Mill's Inductivism

(p. 133) Inductivism posits that:

1. scientists *discover* laws and theories by generalizing inductively from observations and experiment
2. scientists *justify* scientific laws and theories with evidence using inductive principles

(pp. 133–4) Mill's four inductive methods are:

1. Method of Agreement
2. Method of Difference (most important)
3. Method of Concomitant variations
4. Method of Residues

Losee objects that with Mill's Method of Difference:

1. no two instances are exactly alike bar one circumstance (e.g., place, time)
2. not all circumstances are equally relevant (e.g., handling method versus sunspot activity)

(p. 134) Mill responded that considering only a small number of circumstances is justified by experience.

(p. 135) Losee notes that *prior* to applying Mill's Method of Difference schema, the scientist must devise a hypothesis about *which* circumstances are relevant for controlled experimentation.

Mill saw his Method of Agreement limited by:

1. not knowing all of the relevant circumstances
   [LA: e.g., poor school performance due to socio-economics and not race]
2. different causes may give the same effect (e.g., *B* caused *a* in 1 and 3, and *D* caused *a* in 2)

Mill suggested varying the circumstances in order to increase the probability of cause.

(pp. 135–6) Mill thought the possibility of the plurality of causes is not a problem for his Method of Difference when applied to a single instance.

Losee objects that Mill's reference to 'a cause in this instance' contradicts his notion of a cause as an invariant set of circumstances preceding a particular effect.

(p. 136) Jevons objects that Mill unjustifiably assumed that a correlation in a single experiment will happen in other experiments (contradicting the principle of inductive inference).

In spite of Mill's overstatement on the inductive method, he agreed with Whewell on the importance of hypothesis generation.

Mill described two types of multiple causation:

1. various causes continue to produce their own separate effects
2. resultant effect is other than the effects produced separately:
    a. resultant effect is the 'vectorial sum' of the causes present (Composition of Causes)
    b. resultant effect differs in kind from the otherwise separate effects

(pp. 136–7) Mill held his four inductive methods can be applied to types 1. and 2.b. above, but not to type 2.a. (Composition of Causes).

(p. 137) Mill thought they cannot be applied to Composition of Causes cases (e.g., vectorial addition of separate forces along a parallelogram) as same motion can be generated from infinitely many sets of forces.

(p. 138) For Composition of Causes cases, Mill recommended the Deductive Method in three stages:

1. formulation of a set of laws (by induction from observation *or* by hypothesis)
2. deduction of a statement of resultant effect from a combination of laws
3. complete verification by deductive consequences agreeing with observations *and* no other hypothesis entailing same consequences

For an example of complete verification, Mill cited Newton's inverse-square law of gravitation. But neither Newton nor Mill tried to prove that there is *no* other possible successful hypothesis [LA: this problem formulated later as the 'Duhem–Quine Underdetermination Thesis'].

In other cases, Mill recognized that a future theory may deduce successfully what a current theory deduces (e.g., Young and Fresnel's wave theory of light) in addition to explaining what is currently not explained.

(p. 139) Although Mill recognized the importance of deductive methods for science, he thought the justification of theories is done by using inductive logic alone.

Mill distinguished casual relations from accidental relations in that casual relations are *both* invariable (constantly conjoined) and unconditional (necessary).

(pp. 139–40) Mill suggested a relation is unconditional if it will continue as long as the ultimate laws of nature persevere. His test for an unconditional relation is:

1. vary the conditions for the invariable sequence (leaving ultimate laws of nature constant)
2. if the effect fails under some variations, then the relation is not unconditional

(e.g., day does not cause night)

(p. 140) Losee objects that Mill has not specified which laws are the ultimate laws of nature.

Mill vacillated between all four of his inductive methods being capable of proving causal connection and only his Method of Difference.

For Mill's Method of Difference to prove causal connection, he requires proof that a connection is both invariable and unconditional. Mill's attempted proof is that:

1. the positive and negative instances differ in just one relevant circumstance
2. the principle of universal causation is true

Philosophers of science accept that Mill failed to justify 1. as he failed to show that no other circumstance could be relevant to occurrence and non-occurrence.

And Mill failed to justify 2. as he failed to escape the vicious circle of trying to prove the law of universal causation.

(p. 141) Mill attempted to prove the law of universal causation as a necessary truth from the fact that it has has no exception in an extremely wide variety of observed circumstances.

Losee objects that pointing to no exceptions to date is not a (formal, logical) proof that it could not be otherwise. Without a proof of the necessity of the law of universal causation, Mill failed to show how the Method of Difference proves causal connections.

Jevons' Hypothetico-Deductive View

(pp. 141–2) Jevon's rejected Mill's arguments that theories are justified by inductive logic. For Jevons, a hypothesis is justified by:

1. demonstrating it is not inconsistent with other well-confirmed laws
2. showing its consequences are consistent with observations

Contra Mill, 2. requires deductive reasoning, not inductive.

Questions to Consider:

1. *Did Mill overstate the importance of inductive methods in scientific discovery?*
2. *Is the Method of Difference an inductive method or, more realistically, a deductive method?*
3. *Was Mill right to think that justifying scientific hypotheses only requires inductive reasoning?*
4. *How useful is Mill's test for casual relations vis-à-vis accidental relations?*

Ch. 11: Mathematical Positivism and Conventionalism

Philosophers discussed in this chapter: George Berkeley (1685–1753); Ernst Mach (1838–1916); Pierre Duhem (1861–1916); Henri Poincaré (1854–1912); Karl Popper (1902–94)

Berkeley's Mathematical Positivism

(p. 144) Berkeley pointed out Newton's hypocrisy in his suggesting what forces are 'in themselves' instead of seeing them as useful mathematical constructions for calculating the motions of bodies. For Berkeley, 'material substances' and forces have no real existence.

(p. 145) Instrumentalism is the view that scientific laws are nothing but computational devices for the description and prediction of phenomena. The terms in these laws and their functional dependencies need not refer to anything that exists in nature.

For Berkeley, only minds and their ideas exist ('Idealism'). Only minds have causal power, not material forces.
[LA: For a critique of Berkeley's Idealism, see my 'The Existence of Mind-Independent Physical Objects' at
www.rationalrealm.com/philosophy/metaphysics/mind-independent-physical-objects.html
For a critique of a contemporary version of Idealism argued by Donald Hoffman, see my 'Hoffman's Conscious Realism: A Critical Review' at
www.rationalrealm.com/philosophy/metaphysics/hoffman-conscious-realism.html]

Berkeley rejected Galileo's, Descartes' and Newton's distinction between primary qualities of material bodies (e.g., extension, position, motion) and secondary qualities of subjective perceptual experience (e.g., heat, brightness) as the former are also only given in perceptual experience.

Berkeley also rejected Newton's Absolute Space as spatial interval is meaningless without our perception of bodies and their motion relative to one another.

(p. 146) Berkeley criticized Newton's bucket experiment for not demonstrating absolute circular motion in a bucket, suggesting instead that terrestrial motion be made relative to the fixed stars. For Berkeley, 'Absolute Space' is a useless fiction and should be eliminated from physics.

Mach's Reformulation of Mechanics

Mach mimicked Berkeley's instrumentalist critique of Newton. For Mach, Snel's law of refraction is a 'compendious rule' for the mental reconstruction of the various instances of refraction.

Mach proposed a Principle of Economy as a regulative principle in science for summarizing the greatest numbers of facts using comprehensive theories to deduce empirical laws.

(p. 147) Like Berkeley, Mach eschewed the reality of primary qualities, atoms and electric charges, allowing only phenomena (hence, 'Phenomenalism').

Mach divested Newtonian mechanics of 'metaphysical' presuppositions by reformulating as:

1. three contingent empirical generalizations
2. *a priori* definitions of 'mass-ratio' and 'force'

(pp. 147–8) For Mach, confirmation of his empirical generalizations requires procedures for measuring spatial intervals against the background of 'fixed' stars and temporal intervals by physical processes.

(p. 148) Losee objects that Mach's reformulation of Newton's empirical generalizations is not subject to experimental falsification, as Mach had claimed. Mach's first generalization about 'contrary accelerations in the direction of their line of junction' may be saved from disconfirmation by claiming the problematic experiment was not conducted within a closed system.

Duhem on the Logic of Disconfirmation

Duhem emphasized how empirical generalizations can be made true by convention ('Conventionalism').

For Duhem, scientific predictions (*E*) are logically deduced conjointly from:

1. (*L*) statements of the relevant empirical laws
2. (*C*) statements of the antecedent conditions

(p. 149) If a prediction is not confirmed, either *L* or *C* may be false. Laws *L* may still be true in spite of the seeming disconfirming evidence of not *E*.

For Duhem, even where antecedent conditions *C* is taken to be true by scientists, any one of the hypotheses stated in empirical laws *L* may be rejected while saving the others. Which hypotheses to save by convention is decided by the objective judgment of scientists.

One reason for saving a hypothesis from disconfirmation is that it is essential in a number of other confirmed theories.

(pp. 149–50) Duhem criticized Francis Bacon's notion of a 'crucial experiment' (e.g., Foucault's supposed experiment falsifying the corpuscular theory of light). Adjustments could be made elsewhere in the Newton/Laplace corpuscular theory and wave proponents failed to prove that the wave theory is the only possible alternative.

Poincaré's Conventionalism

(p. 150) Poincaré rejected Kant's and Whewell's appeal to necessary *a priori* scientific truths. For Poincaré, scientists agree by convention that certain physical laws are true by definition.

Poincaré showed how a decisive test of Newton's law of inertia would require an impossibility; that each body in the universe reassume its earlier position and velocity. In fact, scientists assume that bodies undergoing test are 'reasonably isolated' from the rest of the universe. Test discrepancies can be attributed to an incompletely isolated system.

(p. 151) For Poincaré, 'inertial motion' (Newton's first law) means the motion of a body whose acceleration depends only on its position and the positions and velocities of neighbouring bodies.

However, for Poincaré, Newton's first law was also an empirically significant generalization holding approximately for 'almost isolated' systems.

Poincaré also analysed 'force' and 'mass' in Newton's second and third laws as being both definitional and empirical generalizations for 'almost isolated' systems.

(p. 152) For Poincaré, such conventional definitions are not arbitrary. They are justified by their fruitfulness in future research.

Poincaré thought physical relations will always be described by Euclidean geometry as it is the simplest to apply. Any discrepancy with experimental tests can always be attributed to the bending of light rays.

But Hempel pointed out that the principle of simplicity applies to the conjunction of pure geometry and physical hypotheses. Overall simplicity may be got by adopting a non-Euclidean geometry.

Popper on Falsifiability as a Criterion of Empirical Method

(p. 153) Popper recognized that a theory can always be saved either by rejecting the observational evidence, adding auxiliary hypotheses or modifying the rules of correspondence.

Popper eschewed saving a theory by methodological fiat. He set a meta-criterion for proper methodological rules: that no rule protect a statement in science against falsification.

So, when adding auxiliary hypotheses to save a theory, only those be added that increase the falsifiability of the theory (e.g., allowable Paulis exclusion principle *vis-à-vis* bad Lorentz contraction hypothesis).

(p. 154) Popper proposed a clear demarcation between a scientific theory and non-science. A scientific theory:

1. is exposed to the possibility of falsification
2. has withstood serious attempts at experimental refutation

For Popper, a serious test compares a deductive consequence of the hypothesis (plus initial conditions and auxiliary hypotheses) with a 'basic statement' recording an observation by multiple observers in a specific region of time and space.

As basic statements may record faulty observations, Popper conceded that the acceptance of such statements by the scientific community is by convention.

For Popper, the worth of a physical law or theory is measured by the number, diversity and severity of tests it has passed. Most philosophers of science accept this as a qualitative account of justification.

(p. 155) Popper provided a quantitative measure of justification. For any two comparable theories, one is closer to the truth (more verisimilitude) compared with the other if it either has more truth content than the other or has less false content than the other.

However, Tichy and Miller proved that if both theories are false, then neither condition is satisfied. Popper failed in his attempt to quantify theory acceptability for known false theories and hence failed to show how science progresses.

Popper insisted that passing severe tests does not show a theory to be true or approximately true and resisted appeals to inductive inference from past successes.

Losee complains that Popper's appeal to the evolutionary fitness of theories passing severe tests, then, is misguided. Popper has given us no reason to select for further applications theories that have survived such tests over and above failed theories.

(p. 156) In the end, Popper accepted a 'whiff of inductivism' based on the assumption that reality must be in some respects similar to what our theories tell us it is. He argued that it would be a fantastic coincidence for our theories to make spectacularly unlikely predictions that proved to be true unless there was some truth to them.

Poppers critics claimed that he had abandoned the anti-inductivist programme.

Questions to Consider:

1. *Are scientific theories only instruments for prediction, or do they point to a posited reality behind the phenomena?*
2. *What role does convention play in scientific confirmation and falsification of theories?*
3. *Is Popper's acceptance of conventionalism with regard to his 'basic statements' a serious objection to his objectivist account of science?*
4. *Did Popper succeed in giving a quantitative measure for the comparative worth of theories?*
5. *Was Popper's attack on inductivism successful?*

Ch. 12: Logical Reconstructionist Philosophy of Science

Philosophers discussed in this chapter: Percy Williams Bridgman (1882–1961); Carl Hempel (1905–97); Ernest Nagel (1901–87)

A Hierarchy of Language Levels

(p. 159) From the 1940s, philosophers of science sought to take Campbell's lead in reconstructing science as built on the foundations of a logical axiomatic system. They concerned themselves only with the logic of justification and not with the context of discovery.

(pp. 159–60) The logical reconstructionists identified four levels in the language of scientists, from apex to base:

1. Theories – laws are theorems in deductive systems
2. Laws – Invariant/statistical relations between concepts
3. Values of concepts – values assigned to scientific concepts
4. Primary experimental data – observational statements about readings

The logical reconstructionists concluded that:

1. Each level interprets the level below.
2. Predictive power increases from base to apex.
3. Principal division is between the bottom three observational levels and the theoretical/non-observable level at the top.
4. Statements at the observational levels are test cases for theoretical statements.

Operationalism

(p. 160) Drawing on Einstein's analysis of simultaneity of two events, Bridgman argued that scientific concepts (third level) only gain significance by being linked to operations that assign them specific values.

(p. 161) For Bridgman, operational definitions link concepts (third level) to primary experimental data (fourth level) via the logical operational schema:

$$(x)\ [\mathrm{O}x \supset (\mathrm{C}x \equiv \mathrm{R}x)]$$

'For all operations performed, this particular concept applies only if this result occurs.'

[LA: the logical notation is:

(x)	for all x (where x is a variable)
$\subset$	conditional (if ... then)
$\equiv$	bicondional (if and only if)
O, C, R	predicates]

E.g., for all objects brought near a neutral electroscope, the object is electrically charged if and only if the leaves of the electroscope diverge.

Bridgman allowed some theoretical concepts even though the operations to determine their value were 'paper and pencil' calculations using a mathematical theory (e.g., 'stress').

(p. 162) Bridgman recognized two limitations of his operationalism:

1. impossibility of specifying all the circumstances required for an operation (e.g., for gas pressure, specifying sunspot activity is irrelevant)
2. of necessity, some operations remain unanalyzed (e.g., for measuring relative weight, not necessary to specify operations for making beam balances)

(pp. 162–3) For Bridgman, decisions to consider particular operations unanalysed by the scientific community are provisional only. All operations are, in principle, further analyzable.

The Deductive Pattern of Explanation

(p. 163) Hempel and Oppenheim saw scientific explanation as a logical deduction of a description of a phenomenon from general laws in conjunction with statements of antecedent conditions (e.g., Why does an oar appear bent in water?).

Logical reconstructionists used this pattern of deduction to:

- explain a value of a scientific concept (third level) by reference to a law (second level); and
- confirm a law (third level) by reference to the value of a scientific concept (second level)

(p. 164) Antecedent conditions include both boundary conditions and initial conditions.

Two examples:

1. explanation of the expansion of a heated balloon using Gay-Lussac's Law [LA: V = pressure; T = absolute temperature]
2. explanation of the dominance a species of finch on an offshore island

(p. 165) For Hempel and Oppenheim, for a successful deductive explanation:

- the conditional premise must be a true law
- statements about initial and boundary conditions must be true

Hempel and Oppenheim allowed for some explanations to be statistical/inductive (e.g., probability of a patient recovering from an infection).

Nomic v. Accidental Generalizations

(p. 166) Logical reconstructionists accepted Hume's skepticism about the necessary nature of scientific laws.

But Hume's analysis using constant conjunction fails to distinguish lawlike universals from accidental universals (e.g., Losee's synchronous ticking of two pendulum clocks).

Two problems for Hume's 'constant conjunction' account of scientific laws:

1. Genuine scientific laws (but not accidental generalizations) support counterfactual conditional statements.
2. Some scientific laws refer to non-existent idealized entities.

(pp. 166–7) Braithwaite solved the problems by pointing out that genuine scientific laws (and not accidental generalizations) are deduced from higher-level hypotheses, evidence for which is independent from the evidence for the lower level law (e.g., barium flame colour deduced from postulates of atomic theory).

(p. 167) Nagel supported Hume's account of scientific laws by distinguishing four characteristics of genuine laws:

1. Laws are not made true simply by virtue of the entity referenced not existing.
2. The scope of prediction of a law is not restricted.
3. Entities referred to in laws are not restricted to existing in particular times or places.
4. Laws are mutually and indirectly supported by other laws within the same axiomatic system.

The Confirmation of Scientific Hypotheses

(pp. 167–8) Hempel proposed three phases in the logical confirmation of a scientific theory:

1. collect experimental results
2. analyze whether the experimental results confirm a hypothesis
3. decide whether the hypothesis is true in light of the results

Phase 2 is the problem of how to confirm a theory as true for which Hempel thought the solution lay in the application of formal logic.

(p. 168) The raven paradox shows how both a black shoe (∼Ra · Ba) and a white glove (∼Ra · ∼Ba), counter-intuitively, logically support the scientific law: 'All ravens are black'.

[LA: the logical notation is:

∼	negation (not ...)
·	conjunction (... and ...)
a	constant (this shoe; this glove)
R	raven
B	black]

(p. 169) Hempel sought to dismiss the raven paradox by rejecting our common intuitions about confirmation of laws. He maintained that:

1. 'All ravens are black' is about **all** objects in the universe (for all objects, if it is a raven, then it is black); and
2. Our background knowledge about ravens includes there being many more non-black objects than ravens (hence, greater chance of disconfirmation if we focus on ravens).

(p. 170) Carnap's project of quantitatively measuring the degree of theory confirmation sought to:

1. develop an artificial language of measurement
2. use probability theory to assign degrees of confirmation
3. show how the calculated values align with our intuitions

Counter-intuitively, Carnap's mathematical function for degree of confirmation rendered universal conditionals with infinitely many possible instances with a probability of zero.

Carnap's controversial solution was to redirect from confirmation of a universal generalization over large numbers to confirmation of the next observed instance of the generalization.

The Structure of Scientific Theories

(p. 171) The logical reconstructionists continued Campbell's 'hypothesis-plus-dictionary' analysis of the relationship between axiomatic theories and observation statements.

(p. 172) Hempel likened the relationship to that of a safety net (axioms) supported by rods from below (observation statements), with not every knot (undefined theoretical term) in the net separately supported by a rod.

To the question about what is a sufficient amount of support for a theory, Hempel looked to a theory of confirmation. He conceded in 1952 that finding such a theory is a future project.

Braithwaite and Koertge see the empirical significance of a theory as seeping upwards from observation statements to the defined and undefined terms in the axiom system.

Theory Replacement: Growth by Incorporation

(pp. 172–3) The logical reconstructionists noted in the history of science increasing explanatory breadth through 'growth by incorporation' of lower-level laws.

(p. 173) Nagel identified two types of incorporation/reduction:

1. homogeneous reduction – incorporated and incorporating theories substantially use the same concepts (e.g., Galileo's law of falling bodies by Newtonian mechanics)

2. heterogeneous reduction – incorporated and incorporating theories use different concepts, where incorporating theory refers to micro-structure of objects (e.g., classical thermodynamics by statistical mechanics)

For Nagel, the formal and non-formal conditions for heterogeneous reduction are:

1. connectability – theoretical terms in the reduced theory are linked to terms in the reducing theory

2. derivability – experimental laws in the reduced theory are deducible from the reducing theory

3. empirical support – evidence supporting the theoretical assumptions of the reducing theory are in addition to that supporting the reduced theory

4. fertility – theoretical assumptions of the reducing theory suggest development of the reduced theory

(p. 174) Bohr likened the absorption of lower-level theories into those at a more general level as an expanding nest of Chinese boxes that illustrate his Correspondence Postulate.

Bohr, and later Agassi, articulated the extension of the Correspondence Postulate as criteria for the successful absorption of one theory by its successor. For one theory to succeed another, the new theory must:

1. have greater testable content than its predecessor; and
2. be in asymptotic agreement with its predecessor where its predecessor succeeded

Questions to Consider:

1. *How successful is Bridgman's operationalism at avoiding the necessity of providing an analysis for every aspect of an operation used in an experiment?*
2. *Did Braithwaite successfully distinguish genuine scientific laws that support counterfactuals from accidental generalizations?*
3. *Do you think the raven paradox is a defeater for Hempel's view of theory confirmation?*
4. *How important do you think is growth by incorporation to the progress of science?*

Ch. 13: Orthodoxy under Attack

Philosophers discussed in this chapter: Paul Feyerabend (1924–98); Nelson Goodman (1906–98); Stephen Toulmin (1922—); Herbert Feigl (1902–88)

Is There a Theory-Independent Observational Language?

(p. 178) The logical reconstructionists make three assumptions:

1. Observation reports are true or false independent of the theories they support.
2. Observation reports provide proper tests for theories.
3. Terms in theories obtain their meanings from observation reports.

Contrawise, Feyerabend argued that observation reports get their meaning from theories (e.g., with a change in theory of colour observation, the property ‘colour’ changes from being an intrinsic property to being relational).

(p. 179) Achinstein illustrated how the observational/theoretical distinction depends on the context of the observation (e.g., observing muscle tissue unaided or with optical instruments). The context is the relevance of the accompanying effects (e.g., seeing smoke as fire).

The Duhem–Quine thesis further undermines the logical reconstructionists’ strict observation-theory distinction:

1. There is no straightforward ‘empirical content’ of an individual observation statement.
2. Any statement can be saved from falsification with enough adjustments elsewhere in the theory.
3. There is no sharp distinction between synthetic statements (empirical) and analytic statements (non-empirical).

(p. 180) The logical reconstructionists’ ‘Safety-Net’ analogy is mistaken as the supporting rods (observation reports) for the net are not independent of the net (axiomatic system) they support.

Doubts about the Covering-Law Model of Explanation

Hempel defended his view that a properly scientific explanation subsumes a phenomenon under general laws, either by deduction (DN) or statistical inference (IS).

(p. 181) Hempel conceded that deduction using ‘indicator laws’ and inference using statistical laws are not *sufficient* for scientific explanation (e.g., patient developing measles and recovering from a cold). But covering under a general law is *necessary* for explanation.

(pp. 181–2) Scriven objected that deduction from covering laws is not necessary either (e.g., explaining a bridge collapse). Hempel pointed out that leaving out laws in the explanation still presumes those laws in the explanation.

(p. 182) Salmon objected that the covering-law model fails to explain improbable events (e.g., developing leukemia from radiation exposure).

A Non-Statement View of Theories

Suppe rejected the logical reconstructionists' view of theories as collections of formal statements. He likened a theory to a proposition where a single proposition can be expressed by many different sentences.

(p. 183) For example, how quantum theory is equally 'expressed' by both Schrödinger's wave mechanics and Heisenberg's matrix mechanics.

For Suppe, theories are non-linguistic entities that:

- point to a class of phenomena
- model the phenomena using idealized systems
- make counterfactual claims

Giere thought these idealized systems had explanatory power by hypothesizing a structural relationship between the idealized model and the natural system (e.g. Kinetic Theory of Gases and Galileo's Theory of Falling Bodies).

Cohen pointed out how later developments increase the sophistication of the idealized model to better explain real systems (e.g. Newton's development of his model of planetary motion).

(pp. 183–4) Contra the logical reconstructionists, scientific explanation is not by deduction of empirical laws from theories. Sellars pointed out how theories explain *why* phenomena obey empirical laws the way they do (e.g., the kinetic theory explains why at high pressures gases diverge from the Boyle–Charles law).
[LA: Note how critics such as Suppe, Giere and Cohen rejected Duhem's (see p. 120) anti-realist view of theories.]

Goodman's "New Riddle of Induction"

(p. 184) Goodman attacked Nicod's Criterion (The Principle of Instance Confirmation, see p. 168) with the example of grue. 'All emeralds are grue' is as equally supported as 'All emeralds are green' by observation statements.

(pp. 184–5) Goodman noted that 'All emeralds are grue' is not a lawlike generalization; only accidental.
[LA: But it *can* be posited as a lawlike generalization.]

(p. 185) In 1953, Goodman suggested lawlike generalizations refer to predicates that do not involve a particular space or time. He rejected this demarcation as the 'grue' paradox can be restated without such involvement. Losee also notes that some scientific laws *do* involve reference to a particular space or time (e.g., Kepler's First Law).

Goodman solved his 'grue' paradox by restricting allowed predicates to those that have been used successfully in predictions in the past.

(p. 186) Thereby, Goodman elevated the importance of the history of a scientific theory to its degree of confirmation.

In 1964, Hempel conceded that the logical reconstructionist program of trying to show how theory confirmation is a function solely of logical/syntactical relations between observation statements and theoretical statements has failed.

Doubts about the Chinese-Box View of Scientific Progress

Feyerabend rejected Nagel's logical reconstructionist claim that empirical laws are reduced to higher-level theories.

Two examples:

1. Galileo's law that vertical acceleration of a falling body is constant is not deducible from Newtonian physics as distance between body and earth reduces.
2. Newtonian physics is not deducible from General Relativity Theory as meanings of 'length' are different in the two theories (semantically incommensurable).

(p. 187) Putnam suggested a patch of Nagel's reductionism: reducing theory only need only approximate observational results of reduced theory.

Feyerabend countered that with Putnam's move, the logical reconstructionists have given up on explaining the actual history of theory replacement. As observation reports confirming a high-level theory are not theory-independent, theories are not objectively comparable (observationally incommensurable).

Whewell's tributary–river analogy, the logical reconstructionists' Chinese-box view and Bohr's Correspondence Principle all see scientific theory replacement as continuous.

(p. 188) Contrawise, Toulmin pointed to conceptual changes in what is seen as 'Ideals of Natural Order'. Natural order specifies what phenomena require explanation (e.g., Newton's First Law saw uniform rectilinear motion as not requiring explanation, *contra* Aristotle).

For Toulmin, an anomaly is a resistance to attempts at explanation via a 'natural order' schema (e.g., the motion of projectiles for Aristotelian mechanics). When anomalies mount, a fitter theory replaces it by revolution.

For Toulmin, The logical reconstructionist programme cannot explain theory change as the standards of intelligibility/reasonableness change with a change in theory.

(p. 189) Hanson saw revolutionary theory change as a shift in *gestalt*; a way of seeing the world.

Feyerabend and Feigl on the Death of Orthodoxy

Feyerabend saw the logical reconstructionist programme as useless to the living scientist as it cannot help her decide between competing theories. This is due to two false assumptions:

1. A theory-independent observation language is available to adjudicate between theories.
2. A theory can be consistent with all the known facts.

In reality, every theory has counter-instances.

(p. 190) Feyerabend recommended instead philosophers of science study the history of science.

Losse asks of Feyerabend what distinguishes philosophy of science from the history and practice of science. Feyerabend sought to diminish the specialness of science as a separate form of enquiry.

Feigl, on the other hand, sought to salvage the gains of the logical reconstructionist programme as explaining how theories are tested and compared. This is possible because:

1. empirical laws are deducible from theories
2. many empirical laws are stable and approximately true

(p. 191) Feigl conceded that empirical laws may be corrected by a high-level theory (e.g., an astrophysical theory may correct physical optics), but, none the less, there are thousands of stable empirical laws.

Feigl suggested that these relatively stable empirical laws are what test theories. Although the meanings of terms in empirical laws change with successive theories, as Feyerabend pushed, historically, theories are appraised by their absorption of laws.

Smart observed that Feigl's account ignored the history of biology that has no empirical laws; only generalizations that are restricted to the earth.

(pp. 191–2) Adjustments to these generalizations in biology do not necessitate revolutions. Ruse pointed out that the same is the case with generalizations in physics (e.g., laws of Kepler, Snel, Boyle, Ohm).

(p. 192) Beatty argued that there are no empirical laws in evolutionary biology because the same initial conditions and selective pressures can result in different types of organisms. This variability of outcomes is because of:

1. chance events (e.g., mutations, earthquakes)
2. the functional equivalence of different adaptions (e.g., larger body *vis-à-vis* burrowing)

Unlike empirical laws, biological generalizations do not support counterfactual claims.

Sober and Carrier responded that biological generalizations are at the higher level of supervenient properties. A supervenient property changes if and only if some lower-level property changes.

(p. 193) The supervenient property 'fitness' supervenes upon physical characteristics or behaviour and is measured by biologists to use in biological explanations (e.g., predator–prey, sex-ratios, anemia).

Brandon emphasized how supervenient properties do not figure in low-level biological laws, but in the schema: an organism is better adapted than a rival in a particular environment if and only if it is better able to survive.

Brandon connected the 'fitness' schema to biologies of specific organisms where:

1. biological entities are in 'chance set-ups with respect to reproduction'
2. levels of adaptedness differ in a common selective environment
3. adaptive differences are heritable

The 'fitness' schema, although not directly empirical, makes substantive existential claims and acts a directive principle for evolutionary biologists (much like Newton's second law and the law of conservation).

Questions to Consider:

1. *Were the logical reconstructionists wrong in thinking there is a strict observation/theory dichotomy?*
2. *Is Goodman's 'grue' paradox an illustration of the Duhem–Quine Underdetermination Thesis?*
3. *Can a gradual process theory of scientific theory change be defended?*
4. *Are there low-level empirical laws in evolutionary biology? If not, does it matter?*
5. *Is evolutionary biology a genuine science? If so, how?*

Ch. 14: Theories of Scientific Progress

Philosophers discussed in this chapter: Thomas Kuhn (1922–96); Imre Lakatos (1922–74); Larry Laudan (1941—)

Kuhn on "Normal Science" and "Revolutionary Science"

(p. 197) Philosophers of science came to accept that the logical reconstructionist reduction of scientific practice to exercises in formal logic did not capture what actual scientists do.

(p. 198) Kuhn built on the work of Toulmin and Hanson to reconstruct the history of science as distinct periods where a period of 'normal science' is punctuated by 'revolutionary science' in which an earlier 'paradigm' is replaced ([1962] *The Structure of Scientific Revolutions*).

For Kuhn, most science is carried out as puzzle solving 'normal science' in which scientists:

1. close the measurement gap between observations and the paradigm's predictions
2. extend the scope of the paradigm to other phenomena
3. determine the values of universal constants
4. formulate quantitative laws that explain the paradigm
5. work out how to apply the paradigm to new areas of interest

A competing paradigm arises when the dominant paradigm faces unsolved anomalies. The revolution against a dominant paradigm is sudden and unplanned, like a Gestalt switch.

(p. 199) Kuhn rejected the logic of falsification as there are three terms in the rejection of a paradigm:

1. established paradigm
2. rival paradigm
3. observational evidence

But for Kuhn, competing paradigms are not measured against the same observation statement as there is no paradigm-independent observation language. Conceptual differences mean common terms have different meanings (e.g., 'space', 'time', and 'matter' mean different things in General Relativity compared with Newtonian physics).

Scheffler objected that Kuhn made revolutions in science subjective.

(p. 200) For Scheffler, although competing paradigms have different methods of classification, they both refer to the same objects in the real world. Genuine progress in science happens because the new paradigm better describes those same objects.

For Scheffler, Kuhn failed to distinguish between 'seeing x' and 'seeing x-as-something-or other'.
[LA: For example, while you see the *gestalt* drawing as a duck and I see it as a rabbit, we are still both seeing the same object.]

Scheffler instanced how Special Relativity Theory replaced Newtonian Mechanics even though the 'mass' of each electron within a synchrotron is defined differently in each paradigm. Never the less, both paradigms refer to the same objects in the synchrotron.

Kuhn insisted on a rational standard for paradigm-replacement that included:

1. constructive accommodation of crisis-inducing anomalies
2. increase in quantitative precision of measurement

(pp. 200–1) Shapere and Buchdahl also criticized Kuhn for equivocating between two meanings of 'paradigm':

1. broad sense – 'disciplinary matrix' as a constellation of shared beliefs, values, techniques
2. narrow sense – 'exemplar' as particular presentation of a theory

(p. 201) If Kuhn means 'paradigm' in the narrow sense, then the distinction between normal science and revolutionary science is greatly diminished. If he means it in the broad sense, then it is too vague to be useful to historians of science.

In his 1962 *Postscript*, Kuhn conceded he equivocated, but maintained that historians of science will find both kinds of 'paradigm'; both exemplars and disciplinary matrices.

In expecting historians to reveal a large number of relatively small groups of researchers (micro-communities), Kuhn made a number of concessions:

1. revolutions may happen within a micro-community without causing a broader revolution
2. one paradigm may replace another within a micro-community without a prior crisis
3. a crisis can be diverted by scientists by shelving the anomaly till a later time
4. periods of 'normal science' within a micro-community may include debate over fundamental metaphysical commitments

With these concessions, Losee thinks Kuhn has blurred his previously sharp distinction between normal science and revolutionary science.

(p. 202) In disarming his critics with his *Postscript*, Kuhn's thesis is no longer radical or objectionable. Kuhn's 'normal science' is no longer the dogmatic and monolithic enterprise he originally depicted it as.

Lakatos on Scientific Research Programmes

Lakatos agreed with Kuhn on the continuity of science: science progresses in an 'ocean of anomalies' (e.g., anomalous motion of Mercury).

Lakatos criticised Popper's falsificationism for not distinguishing between refutation and rejection. Popper responded that rejection depends on availability of alternative theories.

(p. 203) Lakatos unfairly criticised Kuhn for thinking that revolutions in science are like an irrational 'mystical conversion'.

Lakatos, like Popper, sought to write a rational reconstruction of the history of science. For Popper, the unit of theory appraisal is the individual theory. For Lakatos, it is a 'research programme' extending over a period of time with modifications along the way (see diagram).

For Lakatos, a 'research programme' consists of:

- negative heuristic – 'hard core' that resists falsification by methodological fiat (convention)
- positive heuristic – strategy for directing research to overcome anomalies (p. 204)

(p. 204) The positive heuristic builds a 'protective belt' of auxiliary hypotheses around the non-falsifiable hard core of the programme (e.g., Newtonian Research Programme adjustments to solve anomalies with planetary and lunar orbits).

For Lakatos, a negative test does not refute an entire research programme (*contra* Popper), but directs attention to modifying the set of auxiliary hypotheses.

Contra Duhem and Kuhn, Lakatos argued that a research programme as a series of theories can be appraised objectively as either a 'progressive problem-shift' or a 'degenerating problem-shift'.

(p. 205) A series of theories within a research programme is a 'progressive problem-shift' when the later theory:

1. accounts for the success of the earlier theory
2. has more empirical content than the earlier theory
3. has some of its excess empirical content corroborated by experiment

For criterion 1., the asymptotic agreement of calculations with the earlier theory counts as an instance of accounting for the successes of the earlier theory (e.g., Ideal Gas Theory, Bohr Theory of the Hydrogen Atom).

A problem for Lakatos' objective appraisal criteria is that a currently degenerating research programme may, with more research, stage a comeback to become progressive (e.g., Prout's programme on atomic weights).

(p. 206) Feyerabend objected that for Lakatos' criteria to be of practical guidance for scientists for when to abandon a degenerating research programme, they require a time limit for application.

Lakatos responded that Feyerabend ignored the distinction between:

1. appraising a research programme according to methodological criteria
2. deciding whether to continue working on a research programme

Because an appraisal may change over time, Lakatos insisted that it is not the role of the philosopher of science to recommend to scientists which programmes to work on.

For Lakatos, while a scientist may rationally choose to work on a degenerating research programme, they must recognize the risk they are taking on by doing so. Lakatos advocated a public register of the successes and failures of each research programme.

Laudan on Problem-Solving

In Laudan's [1977] *Progress and its Problems*, he saw the unit of progress in science as the solved problem. Problems in science are of two types:

1. empirical problems – about the structure or relations of objects within the discipline
2. conceptual problems – about incompatibilities between theories and incongruities between a theory and methodological presuppositions

An example of a conceptual problem was the incongruity between Newton's axiomatization of mechanics and his insistence on the inductive method.

(p. 207) For Laudan, this conceptual problem solving allows for evolving standards of rationality (in Newton's case, from an induction by simple enumeration standard to a hypothetico-deductive standard).

The logicist view is that progress in science is judged against a fixed standard of rationality. Laudan inverted that process by showing how progress in problem-solving in science modifies the standard of rationality.

For Laudan, progress in science is achieved by:

1. increasing the number of solved empirical problems (even where the solution is only approximate)
2. resolving anomalies (including anomalies where a theory fails to explain an observation that a rival theory explains)
3. resolving conceptual conflicts among theories

Laudan explained how anomalies can be removed by:

1. revising an anomalous observation statement
2. adding an auxiliary hypothesis
3. revising the theory

Questions to Consider:

1. *Was Scheffler right in criticising Kuhn for not recognizing that competing paradigms still refer to the same objects?*
2. *How successful was Kuhn in defending a sharp distinction between 'normal science' and 'revolutionary science'?*
3. *What do you understand by the 'rational reconstruction of science'?*
4. *Is Lakatos' methodology of scientific research programs an improvement on Popper's falsificationism?*
5. *Did Lakatos successfully defend his objective criteria of research programme appraisal against Feyerabend's objection?*
6. *Is Laudan right in thinking that progress in science modifies our standards of rationality?*
7. *What other examples can you think of where solving a problem in science has improved scientists methodological principles of reasoning?*

Ch. 15: Explanation, Causation, and Unification

Philosophers discussed in this chapter: Wesley Salmon (1925—); Peter Railton (1950—); Philip Kitcher (1947—)

Salmon's Causal Model

(p. 210) Because the Covering-Law Model and the Deductive-Nomological Pattern (DN) ignore real causal relations, they fail to explain how the correlations in the flagpole and barometer cases are not causally related (see p. 182). Similarly, the Inductive-Statistical Pattern (IS) cannot explain rare cases of leukemia.

Salmon argued that scientific explanations specify causal mechanisms (e.g., effect of gamma rays on cells).

(p. 211) For Salmon, a 'cause' triggers the production and propagation of a structure and is analyzed with reference to 'process', 'intersection' and 'probability'.

A 'process' is the persistence of an entity, quality, or structure (e.g., motion of bodies) and is of two types:

1. 'causal processes' that transmit modifications ('marks')
2. 'pseudo-processes' that do not transmit modifications (e.g., search light swept through red filter)

For Salmon, new structure is produced whenever causal processes intersect such that their modifications persist in time.

This intersection is of two types:

1. 'conjunctive fork' arising from special background conditions
   – a particular effect of the cause does *not* change the probability of another particular effect from the same cause (e.g., contracting leukemia from atomic bomb blast)
2. 'interactive fork' from direct physical interactions
   – a particular effect of the cause does change the probability of another particular effect from the same cause (e.g., colliding billiard balls) (p. 212)

(pp. 211–12) In a **conjunctive fork** of two effects from a single cause:

1. the probability (P) of both effects occurring given the cause equals (=) the product (X) of the probabilities (P) of each of the effects occurring given the cause
2. the probability (P) of both effects occurring given the *absence of the cause* (=) the product (X) of the probabilities (P) of each of the effects occurring given the *absence of the cause*

3. the probability (P) of the *first* effect occurring given the cause is greater than (>) the probability (P) of the first effect occurring given the *absence of the cause*

4. the probability (P) of the *second* effect occurring given the cause is greater than (>) the probability (P) of the second effect occurring given the *absence of the cause*

[LA: For a primer on probability theory, see www.britannica.com/science/probability-theory]

(p. 212) Reichenbach demonstrated that these four conjunctive fork conditions imply that:

5. the probability (P) of both effects occurring is greater than (>) the product (X) of the probabilities (P) of each of the effects occurring

Formula 5. grounds Reichenbach's 'Principle of the Common Cause':

> Where the probability of the joint occurrence of effects is greater than would be expected if the two effects are statistically independent, posit a common cause.

Salmon supports this 'Principle of the Common Cause' as a directive principle for scientists looking for conjunctive forks.

In an **interactive fork** of two effects from a single cause:

1. the probability (P) of both effects occurring given the cause is greater than (>) the product (X) of the probabilities (P) of each of the effects occurring given the cause

An example of 1. is a cue ball colliding with an eight ball with the effect of rebounding at 45 degrees.

Salmon successfully reconciles two distinct approaches to causal relations:

1. singularity view of a process as propagating structure

2. regularity view of individual processes generating modifications of structure

(p. 213) The reconciliation occurs in that conjunctive forks and interactive forks can only be seen as statistical regularities.

Kitcher provided a counterexample to Salmon's analysis of causation (projectile moving between a vehicle and its shadow).

Salmon abandoned his modification/mark-transmission criterion for causal relatedness in the face of Cartwright's counterexample that demonstrated Salmon needed to include counterfactuals in his analysis.

Salmon then adopted Dowe's 'conserved quantity' theory of causation that identified:

- causal process = world line of an object with conserved quantity
- causal interaction = intersection of world lines with exchange of conserved quantity

Conserved quantities = quantities remaining constant within closed systems (e.g., mass-energy, electric charge)

On Dowe's 'conserved quantity' view, the nitrogen/α-particle reaction and the decay of Radium are causal interactions as charge is conserved through the world lines.

(pp. 213–15) However, Salmon accepting Dowe's 'conserved quantity' theory leaves unexplained how the emission of a particular α-particle is 'caused' by a decaying Radium atom and not just the probability of its emission.

Railton's Deductive–Nomological–Probabilistic Model

(p. 215) Railton's probabilistic explanatory model includes three factors:

1. Deductive–Nomological argument for probability of α-particle emission (see p. 214)
2. causal account of underlying mechanism for this probability
3. information about specific emission

Even though the explanation includes that which is to be explained, Railton insisted the account is still explanatory as it explains a highly improbable, non-causal and indeterministic event in terms of:

1. low but finite probability
2. actual atomic decay during that time
3. quantum-mechanical tunnelling

Several philosophers extend non-causal type explanations to non-quantum physics as well.

(p. 216) Such non-causal, non-temporal explanations occur in cases of:

1. static equilibrium and equation of state (e.g., thermodynamics of gases)
2. classification (e.g., why Fido is a dog)
3. evolution (e.g., sexual equilibrium)

Kitcher and Maxwell on Explanatory Unification

Kitcher tipped the question of 'causal relatedness' on its head: We accept '*x* causes *y*' only after we accept an explanation of *y* in terms of *x*.

Thus, Kitcher turned our attention to comparing the adequacy of rival explanatory theories. He thought the better explanation was the one that unified previous explanations by:

1. minimizing the number of patterns of derivation of earlier laws
2. maximizing the number of conclusions generated

(p. 217) For Kitcher, sometimes a principled trade-off needs to be made between satisfying conditions 1. and 2. where they clash.

Maxwell argued that the aim of unification presupposes the universe is comprehensible in terms of invariant and universal laws that cohere.

Einstein's Special Theory of Relativity embodied this unification aim by marrying the seemingly discordant Newtonian Mechanics (dynamics) and Electromagnetic Theory (electrodynamics).

(p. 218) For Maxwell, Einstein's 'aim-oriented empiricism' is more adequate than the 'standard empiricism' that only judged theories by their degree of agreement with observations. That is the lesson from Goodman's paradox (see pp. 184–6) resulting from the generation of *ad hoc* theories that equally marry with observational data.

Salmon sought to combine Kitcher's unification model with causal models of explanation. The unification model aims at systematizing empirical knowledge while causal models complementarily uncover nature's hidden mechanisms.

Questions to Consider:

1. *Is 'causation' about propagation and modification of structure, as Salmon maintains?*
2. *Is Reichenbach's 'Principle of the Common Cause' a useful directive principle for scientists looking for a cause of statistical events?*
3. *Do you think Salmon successfully reconciled the two different approaches to analyzing causation?*
4. *Does Dowe's 'conserved quantity' theory of causation rescue Salmon's approach to indeterminate processes?*
5. *Must all scientific explanations be causal? What other non-causal explanations can you think of?*
6. *Is 'aim-oriented empiricism' a substantive improvement on 'standard empiricism'?*

Ch. 16: Confirmation, Evidential Support, and Theory Appraisal

Philosophers discussed in this chapter: Clark Glymour (1942—); Thomas Kuhn (1922–96); Imre Lakatos (1922–74)

Bayesian Confirmation Theory

(p. 220) Goodman's 'New Riddle of Induction' based on his 'grue' paradox forced some philosophers to abandon the Logical Reconstructionist's syntactical theory of confirmation for a quantitative probability theory.

(p. 221) The axioms of the probability calculus entail:

1. The probability for a sentence (*A*) being true is greater than or equal to 0.
2. The probability for a tautology (*t*) (logical truth) being true is 1 (certainty).
3. The probability for either of two inconsistent sentences (*A*, *B*) being true equals the addition of the probabilities of the individual sentences being true (P(*A*)+ P(*B*)).
4. The probability for a sentence (*A*) being true given another sentence (*B*) being true equals the probability of both sentences being true (P(*A*) + P(*B*)) divided by the probability of the latter sentence (*B*) being true.

1. to 4. entail Bayes' Theorem:

> The probability for a sentence (*A*) being true given another sentence (*B*) being true equals the product (X) of
> the probability for sentence (*B*) being true given sentence (*A*) being true
> **and** the probability for sentence (*A*) being true, divided by
> the probability for sentence (*B*) being true.

Bayes' Theorem is adapted by some to give a quantitative measure of evidential support for a scientific hypothesis (*h*) given an observational evidence statement (*e*):

> The probability for a hypothesis (*h*) being true given evidence statement (*e*) being true equals the product (X) of
> the probability for evidence statement (*e*) being true given hypothesis (*h*) being true **and** the probability hypothesis (*h*) being true, divided by
> the probability for evidence statement (*e*) being true.

The probability for evidence statement (*e*) being true is the sum (+) of the individual probabilities of the evidence statement (*e*) being true given each of the hypotheses competing with hypothesis (*h*).

Bayes' Theorem accords with our intuition that the degree of confirmation given by evidence statement (*e*) to a hypothesis (*h*) equals

> the probability for hypothesis (*h*) being true given evidence statement (*e*) being true minus (–) the initial probability of hypothesis (*h*) being true

(p. 222) and that the degree of confirmation:

- increases the more probable that evidence statement (*e*) is true given hypothesis (*h*) is true; and
- decreases the more probable that one of the hypotheses competing with hypothesis (*h*) is true.

Given this relation:

> The probability of a hypothesis being true given an evidence statement (*e*) being true relative to a competing hypothesis being true given the same evidence statement (*e*) being true
> is equal to
> the product (X) of the probability for evidence statement (*e*) being true given hypothesis (*h*) being true **and** the probability hypothesis (*h*) being true
> relative to
> the product (X) of the probability for evidence statement (*e*) being true given the competing hypothesis (*h**) being true **and** the probability for hypothesis (*h**) being true.

For Bayesians to solve the 'grue' paradox, they need to accord a higher prior probability to the 'green hypothesis'.
[LA: Or find an evidence statement entailed by the 'green' hypothesis that is not entailed by the 'grue' hypothesis, or vice versa; e.g., find a time-variant colour mechanism in emeralds.]

Three ways for Bayesians to interpret 'the probability of a hypothesis':

1. 'frequency interpretation' – frequencies of occurrence in a long-run series of trials
2. 'logical interpretation' – logical relations between hypotheses and evidence statements
3. 'subjectivist interpretation' – measures of rational belief

Losee thinks Bayesians' subjectivist interpretation poses the problem of how to assign degrees of rational belief to a hypothesis. Bayesians respond that scientists applying Bayes' Theorem to new evidence converge in their assessment of posterior probability (as with assessing the probability of drawing a white ball at random from an urn).
[LA: For a suggestion for a satisfactory interpretation of 'prior probability', see my 'Bayes' Theorem and Theory Appraisal' at www.rationalrealm.com/philosophy/reflections/bayes-theorem-theory-appraisal.html]

(p. 223) But Bayes' Theorem treats the repetition of the same experiment as important as varied and severe tests of a hypothesis (e.g., tests of the law of refraction). Bayesians respond that Bayes' Theorem is not designed to assess confidence in particular experimental results. [LA: But that's not the objection.]

Glymour complained that Bayesians have only offered a theory of personal learning, not a theory of scientific reasoning.

(p. 224) Glymour also objected that for positive evidence already known at the time of the formulation of a hypothesis, Bayes' Theorem counts it as of zero value. Bayesians Howson and Urbach responded that Bayes' Theorem should be applied counterfactually; *as if* the evidence was newly discovered.

Bayesian Garber suggested that prior known evidence adds support for the hypothesis because it becomes known that the hypothesis entails the evidence statement. Losee objects that a hypothesis does not entail the evidence statement alone.

(p. 225) For Gerber, support for a theory is of two types:

1. new evidence raises theory's posterior probability
2. new discovery that theory entails prior known evidence

Miller noted that in the face of new counter-evidence, a scientist may adjust the prior probabilities to save their favoured hypothesis (e.g., creationist *ad hoc* adjustment).

Bayesians fatally lack a rule for when to disallow such *ad hoc* revisions. Many such revisions were legitimate in the history of science (e.g., Darwin on the fossil record, Copernicus on stellar parallax, Galileo on telescopic magnification).

Glymour on "Bootstrapping"

(p. 226) Glymour pointed out how a scientific theory may receive supporting evidence from another part of the same theory ('bootstrapping') (e.g., Newton's Second Law supported his universal law of gravitation via the motions of Jupiter's moons).

(p. 227) *Contra* historical theories of confirmation, Glymour's Bootstrap Model is in the Logical Reconstructionist semantic-relations tradition that sees the time that evidence is discovered as irrelevant to the support it gives to a theory.

Lakatos on Comparative Confirmation

For Lakatos, prior known evidence adds support for a hypothesis when:

1. the hypothesis (plus relevant conditions plus auxiliary hypotheses) entails the prior known evidence; and
2. either the main rival hypothesis (plus relevant conditions plus auxiliary hypotheses):
    a. entails the negation of the prior known evidence; or
    b. entails neither the prior known evidence nor the negation of the prior known evidence

An example is how old experiments showing weight gain on combustion confirmed Lavoisier's Oxygen Theory as they contradicted the rival Phlogiston Theory.

Theory Appraisal

(p. 228) Duhem and Campbell showed how the deducibility of lower-level laws does not confirm a theory. As a solution, Kuhn offered the following prescriptive and historically descriptive criteria for theory acceptance:

1. internal consistency
2. agreement with observations
3. simplicity
4. breadth of scope
5. conceptual integration
6. fertility

Losee agrees that criterion 1. (consistency) is a necessary condition for theory acceptance.

(p. 229) Losee argues that the deducibility of an observation report (criterion 2.) is debated by scientists in specific cases.

Similarly, criterion 3. (simplicity) is also vague (e.g., Is power of the independent variable or the number of variables the criteria?)

Kuhn recognized that his criteria conflict in certain cases (e.g., agreement with observations conflicts with simplicity of relationship between variables).

Kuhn's criterion 4. (breadth of scope) is well-supported by the historical record (e.g., Newtonian Mechanics, Electromagnetic Theory of Light).

(p. 230) Kuhn's criterion 5. (conceptual integration) is also well-supported by the historical record (e.g., Copernicus' dealing with retrograde motion).

So is Kuhn's criterion 6. (fertility). McMullin identified two types of fertility of a theory:

1. 'proven fertility' – successful adaptation to historical pressures (expanding explanatory power, resolving anomalies)
2. 'potential fertility' – successful adaptation to future pressures

A theory may be fertile either by:

1. showing how to make progressive modifications to the theory itself (e.g., Bohr's atomic theory); or
2. applying itself successfully to a new type of phenomena (e.g., LaPlace's theory of heat) (p. 231)

(p. 231) Debates about the fertility of LaPlace's theory of heat may indicate that 'application to new phenomena' is a function of scientists' surprise.

Zahar argued for an objective basis for 'novel facts'. For Zahar, a fact is a 'novel fact' if the theory under appraisal was constructed to solve problems that did not include that fact.

Novel facts supporting a theory are of two kinds:

1. novel prediction – only known to be true after theory formulation (e.g., Mendeleeff's predictions of new elements; Maxwell's prediction of gas viscosity relation)

2. novel postdiction – known to be true before theory formulation, but not part of theory formulation (e.g., LaPlace's deduction of discrepancy between calculated and actual sound velocities)

(p. 232) Zahar also argued the Michelson–Morley null result over the speed of light in an hypothesized ether as an example of 'novel' support for Einstein's Special Relativity Theory.

Losee insists we keep separate:

1. truth of predictivist thesis – a newly known fact provides greater support to a theory compared with a fact known prior and accommodated during theory construction

2. problem of undesigned scope – whether novel deducibility is measurable and ought to be a criterion of theory acceptance

Brush's historical research shows that scientists have not accepted a theory *primarily* on the basis of novel facts.

(pp. 232–3) Kuhn insisted that scientists apply his several criteria for theory choice, although there is no set algorithmic calculation. This is because with scientists' varied and idiosyncratic personalities, they disagree on how to weigh the criteria when they conflict.

McAllister on Aesthetic Standards

(p. 233) *Contra* Kuhn's 'idiosyncratic factors' determining scientists' theory appraisal, McAllister advocated a 'rationalist' approach that first accepted Kuhn's *inviolable* 'logico-empirical' criteria:

- internal consistency
- agreement with data
- novel prediction

But McAllister further added aesthetic standards of theory choice, *revisable* by scientists during periods of scientific revolution:

- visualizability
- symmetry
- explanatory simplicity
- ontological parsimony

(pp. 233–4) For McAllister, Kepler, Bohr and Heisenberg were revolutionary in rejecting the current aesthetic standards. But Copernicus was not as he accepted Platonic–Aristotelian uniform circular motion. Likewise, Einstein was not as he accepted the aesthetic symmetry considerations of the day.

(p. 234) Losee objects that even if we accept McAllister's criteria for revolutionary theory change, it's uncertain what counts as an 'aesthetic standard' (e.g., Copernicus may be counted as overturning the Platonic–Aristotelian aesthetic of the crystalline sphere).

Further, de Regt noted that Einstein, with his Special Relativity Theory, rejected the 'aesthetic' standard of absolute containers. McAllister's 'aesthetic' standard is too loose a criterion for theory choice.

Questions to Consider:

1. *Of the three ways of interpreting 'the probability of a hypothesis', which do you think is the most promising for Bayes' Theorists?*
2. *Is Gerber's modification of the application of Bayes' Theorem legitimate?*
3. *Are Kuhn's criteria for theory appraisal sufficiently precise to act as a useful guide for scientists?*
4. *Has Zahar accounted adequately for how old facts can objectively support a new theory?*
5. *How much do think aesthetic standards play in scientists' evaluation of alternative theories?*

Ch. 17: The Justification of Evaluative Standards

Philosophers discussed in this chapter: Imre Lakatos (1922–74); Thomas Kuhn (1922–96); Larry Laudan (1941—); Neurath (1882–1945)

(p. 236) Philosophers of science reconstruct progress in science differently:

1. Francis Bacon – successive inductive generalizations on expanding factual base
2. Karl Popper – sequence of bold conjectures that survive refutation
3. Imre Lakatos – articulation of scientific research programmes

Lakatos's Incorporation Criterion

Lakatos's criteria of theory replacement:

- incorporation of older rival theory by explaining its previous successes
- addition of corroborated excess context (experimentally verified novel facts)

Lakatos suggested applying the same criteria to reconstructions of scientific progress using two steps:

1. rationally reconstruct the history of scientific progress according to each methodology
2. compare each rational reconstruction against the history of science

Result: favour the methodology that reconstructs more of the history of science as rational.

(p. 237) Lakatos claimed his criterion superior to Popper's falsificationist methodology as Popper's reconstruction renders, for example, Newtonians continued research in the face of the falsifying orbit of Mercury as irrational.

Kuhn on the Circularity of Lakatos' Appraisal

Kuhn charged Lakatos's procedure as circular because Lakatos held that:

1. Philosophies of science imply rational reconstructions of scientific growth.
2. Each reconstruction delimits an 'internal history' of science from its 'external history'.
3. The history of science serves as a standard for the evaluation of rival methodologies.
4. Every 'history of science' is an interpretation of the historical record from a particular standpoint.

(p. 238) For Kuhn, there is no methodologically neutral vantage point for evaluating methodologies. Lakatos rejected Popper's falsificationism using Lakatos's own biased reconstruction of science.
[LA: Even though there is no completely methodology-neutral way of writing the internal history of science, Carnap, Hempel, Popper, Kuhn, Lakatos, Feyerabend, *et al*, by and large agree on which episodes count in the internal history of science. In this case, they agree that Newton's work on dynamics in the face of counter-instances is part of the internal history of science. None the less, as I argue in my 'Imre Lakatos: A Critical Appraisal' at www.rationalrealm.com/philosophy/epistemology/imre-lakatos-critical-apraisal.html, Lakatos's meta-methodology is incomplete as a normative theory of rationality.]

Losee observes that in Lakatos's favour, Lakatos grants that even his methodology may be superseded in future by a new methodology that reconstructs more of the internal history of science.

Laudan's "Standard-Case" Model

To remedy the circularity of Lakatos's meta-methodology, Laudan suggested favouring the methodology that reconstructs as rational the most number of standard-case episodes in the history of science.

Laudan's candidates for standard-cases approved by scientists today are:

1. Newtonian Mechanics beat Aristotelian Mechanics in 1800.
2. Kinetic Theory of Heat beat the Caloric Theory in 1900.
3. General Theory of Relativity beat Newtonian Mechanics in 1925.

(p. 239) Losee sees Laudan's choice of best methodology as open-ended as:

1. methodologies in the future may reconstruct more of the standard-cases as rational
2. scientists in the future may agree on a different list of standard-cases

For Losee, a further problem is that Laudan's meta-methodology allows for the *ad hoc* inclusion of evaluative principles that explain a single recalcitrant standard-case.

But to block such an *ad hoc* move is to invoke a higher-order principle of rationality. Laudan has not escaped the problem of circularity he had hoped to avoid.

The Sociological Turn

(p. 240) For Lakatos and Laudan:

- standards of rationality explain the 'internal history of science'
- social and political forces explain the 'external history of science'

In the 1970s and 80s, Bloor, Barnes and Shapin's 'Strong Programme' sought to explain both the internal and external history of science using the same social forces of power and conformity.

But 'Strong Programme' theorists ignore the fact that sometimes the cause of a scientist's belief is an anterior belief that a reason is correct (e.g., Aristotle's belief about how fish conceive).

(p. 241) Explaining Aristotle's belief by reference to reasons is more informative than explaining by reference to social reinforcement. Another case is Rutherford's belief about the density of nuclei.

Losee thinks it implausible that social factors can account for *all* aspects of theory change in science.

Normative Naturalism

(pp. 241–2) Normative naturalists hold that:

- evaluative standards are assessed just like any scientific theory
- philosophy of science does not sit above science
- evaluative standards are provisional
- evaluative standards are truly normative

(p. 242) Neurath's Normative Naturalism stated that:

1. empirical inquiry, evaluation standards and their selection are *not* outside of science proper
2. there are no inviolable/transempirical principles in science
3. all propositions in science can be disputed
4. no propositions in science are foundational, not requiring validation from other propositions
5. every proposition in science requires justification
6. knowledge claims are subject to the pragmatic social and political requirements of the day
7. the normative claims of reason comes from the requirement for explanatory coherence

Neurath saw growth in science as analogous to the rebuilding of a ship at sea, plank by plank.

(p. 243) Underlying Neurath's 'Boat' analogy are three assumptions:

1. scientific enquiry is ongoing with no end in sight
2. pressures from within science and without from society require continual scientific adjustments
3. there is no rock-solid outside-of-science source for adjustments

For Neurath, evaluative principles are themselves learned during the boat's voyage. Even observation-statements can be questioned (hence Neurath's anti-foundationalism).

An observation report may be rejected if:

1. other observers observe something contrary
2. the observer was not in the right state for an accurate observation
3. the observer is proved unreliable
4. the observer is too committed to their theory
5. the observer is anxious to please their research group

Contra the Logical Reconstructionists, Neurath concluded that observation reports are not the incorrigible foundation for science.

(p. 244) Quine built on Neurath's 'Boat' analogy with his 'field of force' analogy in which experience constrains how adjustments are made to the system.
[LA: Imagine lines of force impinging on a sphere directed towards the centre of the sphere and in which observation statements sit at the periphery, scientific laws sit further in, theories sit even deeper within the sphere and where the laws of logic sit at the centre.]

When there is a conflict between a theory and an observation, Quine teaches that scientists exercise a choice between:

- making minimal adjustments at the periphery of the force field; or
- making drastic adjustments deep within the force field that affect all regions

In choosing a response, Quine recommended the normative principles of:

- simplicity (Ockham's Razor)
- conservatism (minimal adjustments to the system)

Justification and Inviolable Principles

Lakatos and Laudan assumed a hierarchy (ladder) of levels of justification:

1. laws and theories (lowest level)
2. evaluative standards
3. inviolable justificatory principles (highest level)

(p. 245) *Contra* Lakatos and Laudan, Shapere's 'non-presuppositionist' view saw all levels subject to change and criticism. For Shapere, as there is no suprahistorical standpoint for evaluating standards of rationality, judgments of rationality are dependent on the historical context.

In 1984, Laudan came to accept Laudan's historical relativism. He came to reject the 'ladder of justification' model for a 'reticulational model' in which theories, methodological principles and cognitive aims work reciprocally (see diagram on p. 246).

(pp. 245–6) Cognitive aims include the mutually inconsistent aim of Newtonian science to allow only 'manifest qualities' (observed properties) *vis-à-vis* theories positing unobserved entities.

(p. 246) Laudan preferred his 'reticulational model' over the evaluative relativism of 'Kuhnian holism'. For Kuhn, during periods of revolution scientists replace theories, methodological rules and cognitive aims as a complete package.

Contra Kuhn, Laudan saw replacements as rational and piecemeal. For Laudan, methodological rules are hypothetical imperatives: If goal y is the aim, then do x (see table on p. 247 for examples).

(p. 247) Laudan used inductive reasoning to support means-end correlations because:

- it uses empirical generalization
- philosophers universally accept it as a 'criterion of choice'
- it exemplifies learning from experience

Losee points to exceptions to Laudan's inductive generalization of reliable means-end correlations (see table on p. 248).
[LA: But these are not examples of the highest-order and posited inviolable methodological rules at the top of the ladder of justification. Each is a heuristic for a particular, historically situated research programme.]

(p. 248) Doppelt objected that Laudan's reticulational model is missing rational criteria for when to modify cognitive aims. Laudan offered two criteria for rational change. The newly posited cognitive aim must be:

1. realizable (Losee questions whether this criterion is reasonable)
2. consistent with the values guiding theory choice

Doppelt responded that in cases where a cognitive aim clashes with values, scientists may ditch one or the other. Either mutually exclusive choice is rational on Laudan's model.

(p. 249) Laudan continued to maintain that his model was an objective standard of rationality.

Contra Kuhn's and Laudan's relativism, Worrall resurrected the hierarchical model of justification by insisting on three fixed principles of evaluation:

1. test theories against plausible rivals
2. prefer non *ad hoc* accounts to *ad hoc* accounts
3. give greater weight to a causal hypothesis where an experiment attempts to shield against other possible causal factors

Worrall likened these three principles to the *modus ponens* rule of inference in formal logic.

Lauden countered that Worrall's principles are substantive and therefore subject to revision. Lauden dispelled the charge of relativism by maintaining that the rules of justification change only under the rational constraints of realizability and consistency.

Worrall conceded that some methodological principles have changed, but insisted that some principles are not revisable. Otherwise, it makes no sense to say that we learn more about how to conduct scientific enquiry.

Questions to Consider:

1. *Is Kuhn right in thinking that Lakatos's justification of his methodology by appealing to the history of science is circular?*
2. *Is Laudan's meta-methodology an improvement on Lakatos's? If so, how? If not, why not?*
3. *Is the 'Strong Programme' in the sociology of science a form of epistemological relativism?*
4. *Can social factors account for all of the decisions of scientists about theory choice?*
5. *What evaluative principles can you think of that illustrate Neurath's point that evaluative principles are learned on the journey of science?*
6. *How useful is Quine's force field analogy? Are there unquestionable foundations in science?*
7. *Did Laudan succeed in defending an objectivist account of principles of rationality?*
8. *If there are inviolable standards of rationality, what are they?*

Ch. 18: The Debate over Scientific Realism

Philosophers discussed in this chapter: Richard Boyd (1942—); Ian Hacking (1936—); Bas C. Van Fraassen (1941—); Arthur Fine (1937—)

(p. 252–3) Realists and instrumentalists debate over:

1. what kinds of facts scientists are aiming at
2. how to account for the progress of science throughout history

Truth Realism

(p. 253) The realist answers:

1. scientists should aim to devise theories that describe the structure of the universe
2. scientific progress shows (a) the universe has a structure that is independent of human thought and (b) theories are increasingly accurate

In the 1970s, realists pointed to the success of plate tectonics and the theory of DNA structure.

Putnam argued the increasing predictive success of theories shows greater approximation to truth and that posited theoretical entities (e.g., electrons, genes) really exist (‘No Miracle’ Argument).

Boyd emphasized how, for example, the methodological principle of assigning properties to theoretical entities yields increasing predictive success/instrumental reliability.

(p. 254) Boyed offered an ‘abductive’ argument (inference to the best explanation) for realism:

1. If successive theories converge upon truth, then principles of scientific method are instrumentally reliable.
2. Principles of scientific method are increasingly more instrumentally reliable.

Therefore: It is probable that successive theories typically converge upon truth.

Non-realists like Laudan, in contrast, point to how the long-term predictive success of a scientific theory is no guarantee of its truth (e.g., Ptolemaic planetary models, phlogiston theory, electromagnetic ether).

Laudan also complained that realists have not explained what they mean by ‘approximate truth’/‘progress toward truth’. Universal generalizations may be true, but *per* Hume, can’t be proved to be true.

Entity Realism

(p. 254–5) Harré sought to overcome the problems with 'truth realism' by identifying three kinds of entities:

> Realm 1: observable entities (e.g., Mars, Atlantic trench)
>
> Realm 2: possibly observable entities with amplified human senses and posited by 'iconic theories' (e.g., micro-organisms, X-ray stars)
>
> Realm 3: not possibly observable entities even with amplified human senses (e.g., neutrinos)

(p. 255) While Realm 2 entities easily satisfy scientists' existence criteria, Realm 3 entities are only detected indirectly by triggered events and so are more open to question in specific cases.

Hacking supported 'entity realism' by pointing out how scientists manipulate unobservable entities to produce new phenomena and to investigate unrelated areas of enquiry.

(p. 256) For example, the electron microscope uncovers the optically invisible structure of some proteins.

Contrariwise, Leplin points to entities that cannot satisfy Hacking's test of manipulability. For example, quarks act as triplets and cannot be isolated individually.

For Leplin, physicists elevate unifying explanatory power over empirical confirmation via novel predictions.

Losee observes that even though 'entity realism' does not require that every theoretical entity refer to an object in the real world, none the less, its failure to support existence claims for the entities postulated by today's fundamental theories is a problem.

Van Fraassen's Constructive Empiricism

(p. 257) Instrumentalists hold that:

1. theories are calculating devices that facilitate the organization and prediction of observations
2. only observation statements are true or false
3. theories are only 'useful' or 'not useful'

With Van Fraassen's 'constructive empiricism', the aim of science is not the 'truth' of a theory, but its 'empirical adequacy'. A theory is empirically adequate if it saves the relevant phenomena.

For Van Fraassen, a statement about an observable is true or false if its value can in principle be determined by the *unaided* human senses. Craters on Neptune counts as an observable as humans can, in principle, travel to Neptune.

[LA: But does this distinction hold in cases like the craters on Neptune? Astronauts arriving at Neptune would require many aids to compensate for the lack of oxygen on Neptune and its enormous gravitational pull. Viewing its craters would require a highly controlled environment just to keep them alive. Furthermore, theoretical entities cannot be dispensed with. Even an astronaut looking through her specially-designed visor assumes the veracity of the laws of optics that describe how photons reflected by the craters travel through her visor to arrive at her eyes. If these theories of optics cannot be regarded as 'true', then how is the astronaut's observation of the craters corroborated as 'true'.]

Van Fraassen conceded that statements about the motions of electrons are both empirically adequate *and* capable of being true or false. But, for van Fraassen, scientists ought to remain agnostic about their existence.

Sober objected that van Fraassen's 'constructive empiricism' gives contradictory advice, depending on how a theory is stated (e.g., as 'food web' or 'eat but not eaten').

(p. 258) Hacking opposed van Fraassen's restriction of observables to what is perceived unaided with counter-examples of grids used in microscopic observation.

Hacking's pointing to the successful detection and manipulation of theoretical entities to investigate unrelated phenomena further refutes the instrumentalist account of these entities (e.g., electrons).

Fine's Natural Ontological Attitude

Fine agreed with Hacking that scientists posing truth claims about theoretical entities promote progress in science.

Fine distinguished two kinds of realism:

1. 'local' realism – hypothesizes existence of theoretical entities in specific cases
2. 'global' realism – assumes some theories truly represent the structure of the world

(p. 259) 'Convergent realism', as a variant of 'global' realism, posits that successive theories better approximate the truth.

Global anti-realists insist that no scientific theory is true or approximates the truth.

Fine's 'Natural Ontological Attitude' (NOA) puts the epistemic and ontological conclusions of scientists on par with common sense claims, while admitting that such claims are neither incorrigible nor invariably progressive in all respects.

For Fine, the endeavour to uncover globally 'the aim of science' requires therapy. Realism is outer-directed to correspondence with the world, while anti-realism is inner-directed to human relations with concepts and truth.

The 'Natural Ontological Attitude' accepts the evolving standards for judging truth-claims among scientists.

Cartwright on Truth-claims about Causal Mechanisms

(p. 259–60) Realists remain persuaded by the ‘No Miracle’ Argument. While anti-realists remain persuaded by the historical ‘Pessimistic Meta-Induction’ Argument.

(p. 260) Cartwright capitalized on how idealised theoretical entities are known not to exist (e.g., massless charges, ideal pendulums, absolute vacuums), thereby making the fundamental laws of physics false.

Cartwright regarded low-level phenomenological laws, on the other hand, as true. She also agreed with Hacking that such laws are evidence for theoretical entities where such entities cause the known law-like regularities (e.g., curves in a cloud chamber).

Structural Realism

(p. 261) With his ‘Structural Realism’, Worrall steered a middle path between Truth Realism/Entity Realism and anti-realism. Structural Realism eschews claims to the truth/approximate truth of theories and the existence of theoretical entities.

Worrall claimed only a mathematical mapping between theories and physical structures.

For example, although Fresnel and Maxwell had different ontological commitments in their two competing theories (Maxwell’s had no ether), their mathematical structures are the same.

Worrall noted that with some competing successive theories, mathematical form is shared under limiting conditions (e.g., Special Relativity and Newtonian mechanics).

(p. 262) Psillos objected that Worrall assumed without argument that a persisting structure is an indicator of real relations between objects.

As objective structural relations require entities to be so related, Chakravartty concluded that Structural Realism assumes Entity Realism. Conversely, if theoretical entities exist, there must be structural relations binding them for them to be detectable reliably. Thus, Entity Realism assumes Structural Realism.

Ladyman pointed out a further problem for realism: in modern physics, the ontological status of subatomic entities is ambiguous (wave-particle duality). Ladyman’s solution is to grant existence to invariant structures only (e.g., elementary particles with invariant quantities).

Ladyman’s solution is more inclusive than Hacking’s in allowing for the existence of neutrinos and quarks.

Harré and Madden see the atomistic metaphysical position favouring Entity Realism, while modern ‘Great Field’ theory favours Structural Realism. Physicists’ preference today for the Great-Field metaphysic lends more weight to Structural Realism.

Questions to Consider:

1. *If universal generalizations cannot be proved, then how would a realist make sense of 'closer approximations to the truth'?*
2. *Is Entity Realists' inability to indentify every theoretical entity with a successful novel prediction a serious problem for Entity Realism?*
3. *Does van Fraassen's sharp distinction between theoretical entities on the one hand and in principle observable entities on the other withstand scrutiny?*
4. *Does the fact that there are no idealised theoretical entities in nature make the fundamental laws of physics false?*
5. *Is Structural Realism a more defensible version of realism compared with Truth Realism and Entity Realism? If so, why?*

Ch. 19: Descriptive Philosophies of Science

Philosophers discussed in this chapter: Gerald Holton (1922—); David Hull (1935—)

(p. 264) Except for Fine, philosophers of science have been prescriptive in developing rational standards for evaluating scientific theories.

(p. 265) From the 1980s, some philosophers advanced one of two forms of a non-normative, descriptivist approach:

1. Modest descriptivists aim to reconstruct piecemeal the actual evaluative standards of scientists.
2. Robust descriptivists offer a theoretical explanation of scientific judgments by appealing to underlying evaluative principles.

Holton on Thematic Principles

(p. 266) Holton noted that contemporary scientists ignored the pronouncements of philosophers of science. From his historical studies, Holton identified these thematic principles of discovery and justification:

1. explanatory principles (e.g., Bohr's Principle of Complementarity)
2. directive principles (e.g., seek micro-structure)
3. evaluative standards (e.g., parsimony, simplicity, incorporation)
4. ontological assumptions (e.g., atomism, plenism)
5. high-level substantive hypotheses (e.g., quantization of energy)

For Holton, scientists' avowed principles may not match their actual methodological practice (e.g., Newton, Darwin).

Although thematic principles are revised by the scientific community over time (e.g., mass conservation), it is these shared commitments that bind scientists into the one enduring enterprise.

(p. 267) Following Whewell, Holton recommended a three-dimensional interpretative framework for describing the activity of scientists:

1. empirical content (facts)
2. analytical content (ideas)
3. thematic content (principles)

For Holton, only by attending to the thematic content can we find out:

1. what gives continuity to science in spite of radical shifts in theory and practice

2. why scientists tenaciously hold on to a theory/principle in the face of contrary evidence

3. why scientists with the same information accept contrary theories

(p. 268) While Holton focused on the role of general theoretical presuppositions, others explored experimental practice. Franklin identified strategies scientists use to separate 'genuine' experimental results from artefacts, including:

1. demonstrate that the apparatus accounts for known phenomena (e.g., spectroscopic data on solar absorption lines)

2. show that an experimental procedure accounts for known features (e.g., infrared spectroscopic data on an organic substance)

3. employ different types of instruments to generate experimental results (e.g., optical, polarizing, electron microscopes on minute objects)

4. argue that features of an experimental result establish it as a genuine fact (e.g., telescopic observations of moons of Jupiter)

5. argue that the well-established operation of an instrument warrants its applications to new phenomena (e.g., radio signals in astronomy)

(p. 269) Losee also emphasizes that sometimes results from different instruments are mutually reinforcing (e.g., molecular weights, Planck's Constant).

Pickering concluded that the acceptance of an experimental fact requires three elements:

1. material procedure (specified in advance)

2. model of the operation of the instrument (to determine correct operation)

3. model of the phenomena under investigation (to compare with results)

The lack of these three elements hindered the acceptance of Galileo's telescopic observations of sun spots. Galileo's critics objected that:

1. there is no plausible optical theory about the telescope

2. the telescope magnifies all terrestrial objects, but only some celestial objects (planets)

3. some telescopic observations of celestial objects are inconsistent with naked-eye observations (e.g., horns on Venus)

Galileo attempted to support the three elements of experimental practice by appealing to the fact that the observed spots:

1. change shape from oval at the Sun's periphery to round at its centre
2. increase in velocity as they move from the periphery to the centre

(p. 270) Galileo countered the resistance of critics wed to the geostatic model by pointing to the change in orientation of the spots with the Earth's seasons. This change is explained by the heliostatic model, but left a puzzle on the geostatic model.

Toulmin on Conceptual Evolution

Toulmin applied the Darwinian Theory of Evolution as an explanation of the development of concepts in science. For Toulmin, mirroring Kuhn, in a scientific revolution, the paradigm that wins is the one best adapted to the pressure presented from anomalies to explain nature.

Hull on Selection Processes

(p. 271) Hull amplified on Toulmin with his 'General Theory of Selection Processes' in which there are:

- replicators: concepts that are copied and transferred (akin to genes)
- interactors: scientists/research groups subject to competition within an environment (akin to individual)
- lineages: sequences of replicators (akin to genetic history)

For Hull, the 'fittest' conceptual innovations (replicators) survive pressure from within the scientific community.

Fitness is a balance between adaptation to present pressures and adaptability (fertility) to future pressures. Scientist's judgments of 'fitness' are provisional.

(pp. 271–2) Hull draws explicit parallels between biological evolution, his Theory of Selection Processes and items from the history of science (see table).

(p. 272) For a concept to be in the same lineage as another, its adoption by scientists must be causally related to that other previous concept (e.g., Darwin's and Wallace's theories *vis-à-vis* Matthew's).

For Hull, his General Theory of Selection Processes is not just an interpretation of the history of science, but a theory of science as it provides answers to three key questions:

1. Science is highly successful because the self-interests of individual scientists to publish coincide with the aims of the discipline.
2. Scientists are concerned about priority and proper citation because it contributes to their individual 'fitness'.
3. The self-policing activities of science are very effective because undermining professional standards is self-defeating.

L. J. Cohen on the Inappropriateness of the Evolutionary Analogy

(p. 273) Cohen objected to Toulmin's and Hull's evolutionary analogy on two grounds:

1. coupled processes: in nature, variants are produced blindly, whereas, in science, variants are produced intentionally with solutions in mind

2. complex interrelations: in nature, individuals represent their species, whereas, in science, research programmes are a complex mix of concepts, relations, theories, rules and standards

Toulmin and Hull conceded Cohen's points, but insisted that the disanalogies were not significant.

Ruse on Epigenetic Rules

(p. 274) Two views drawing on evolutionary biology:

1. Evolutionary-Analogy View (Toulmin/Hull) = analogy between differential reproductive success of genes and of scientific theories/standards

2. Evolutionary-Origins View (Ruse) = epigenetic rules have been encoded in *homo sapiens* that direct scientific progress

Ruse added to the epigenetic rules that guided human 'colour' language, deep linguistic structure and incest prohibition further rules that stipulate:

1. formulating internally consistent theories

2. seeking 'severe tests' of theories (Popper)

3. developing 'consilient' theories (Whewell)

4. using logic and mathematics in formulating and evaluating theories

Critics point to numerous failures in human reasoning (e.g., affirming the consequent, 'gambler's fallacy', conjunction fallacy).

Ruse responded that humans do reason according to the rules when necessary for survival and reproduction. Losee objects that Ruse's response is *ad hoc*.

(pp. 274–5) Losee further objects that the Evolutionary-Origins View that scientists' aim for pattern recognition/successful prediction because it leads to adaptive advantage fails to account for scientists' striving for more than just to 'save the appearances'.

(p. 275) For example, the more complicated Virial Expansion law is more accurate than the theoretically explained Ideal Gas Law.

The Evolutionary-Origins View advocate may respond that the speed of predictions is also an evolutionary advantage. O'Hear objects that the scientists' search for true explanations does not reduce to predictive effectiveness, survival and reproduction.

(pp. 275–6) It may turn out that scientists' dispassionate search for truth is driven by the same forces that drive evolutionary adaption to the environment, but Losee is skeptical.

Descriptive Philosophy of Science and the History of Science

The descriptive approach seeks to subsume the philosophy of science under the history of science, with an emphasis on evaluative practice. But Losee thinks that philosophers of science have an additional aim; that of uncovering general principles of evaluation that transcend historical instances.

Questions to Consider:

1. *Is it possible for descriptivists to provide a non-normative account of scientific practice?*
2. *Why do you think many scientists ignore the prescriptions of philosophers of science?*
3. *How is it that legendary scientists such as Newton and Darwin say one thing and do another?*
4. *How important do you think Holton's thematic principles are to an adequate explanation of scientific practice?*
5. *Are Pickering's criteria for scientists' acceptance of an experimental result exhaustive?*
6. *To what extent do you think an evolutionary account of science succeeds? Where does it fail?*