History and Philosophy of Science

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Contents

§ 1. Mechanics in antiquity and middle ages

§ 1.1. Discussion questions

1.1. Aristotle's view of physics involves what we would consider "scientific" claims:

- Elements have a natural place; this is a driver of motion.
- Motion is fundamentally distinct from rest (contrary to modern inertia).
- A void is impossible.
- The universe is finite (bounded by the sphere of the stars).

As well as "metaphysical" claims:

- Motion is merely one instance of the more general concept of change.
- Position is a quality of a body (comparable to colour, heat, wetness, etc.).
- Qualities are superimposed on matter (not reducible to material explication in the manner of atomism).

Argue that these sets of beliefs are extensively interdependent and mutually supportive.

1.2. Buridan's impetus theory is purely qualitative in character. Why is it interesting to study such things quantitatively? Why are modern physics textbooks obsessed with plugging numbers into formulas instead of reasoning verbally and philosophically like Buridan?

Unification of terrestrial and celestial domains. Even Galileo didn't have a good selling point.

1.3. Buridan's impetus theory anticipates the modern notions of inertia and momentum. Discuss.

Yes, qualitatively. 102.

1.4. Medieval physics was focussed on explaining why a rock, once thrown, keeps going after it no longer has contact with the thrower. Modern physics fundamentally change the question: the thing to be explained is not why the rock keeps going but why it eventually stops. Discuss.

In support of this: 100.1, 103.7. Focus on mathematics helps this conceptual transition.

1.5. Is there an unresolved ambiguity as to whether impetus is circular or rectilinear in Buridan's account? Compare for example the circular motion of the planets with a rock in a sling (and what happens when it is released).

Planets discussed at 103.6. Problem can be resolved by positing crystalline spheres (similar to Buridan's mill stone example).

§ 1.2. Reader

ANDREW EDE & LESLEY CORMACK, *A History of Science in Society: A Reader*, Broadview Press, 2007.

3.4. Buridan.

THOMAS KUHN, What Are Scientific Revolutions?, in Lorenz Kruger, Lorraine J. Daston, and Michael Heidelberger (eds.), *The Probabilistic Revolution, volume I: Ideas in History*, MIT Press, 1987, 7–22.

[When] I first read some of Aristotle's physical writings ..., I rapidly discovered that Aristotle had known almost no mechanics at all. ... As I was reading him, Aristotle appeared not only ignorant of mechanics, but a dreadfully bad physical scientist as well. About motion, in particular, his writings seemed to me full of egregious errors, both of logic and of observation. These conclusions were unlikely. Aristotle, after all, had been the much admired codifier of ancient logic. ... In addition, Aristotle had often proved an extraordinarily acute naturalistic observer ... in biology, especially. ... How could his characteristic talents have deserted him so systematically when he turned to the study of motion and mechanics? Equally, if his talents had so deserted him, why had his writings in physics been taken so seriously for so many centuries after his death? Those questions troubled me. I could easily believe that Aristotle had stumbled, but not that, on entering physics, he had totally collapsed. Might not the fault be mine rather than Aristotle's, I asked myself. ...

Feeling that way, I continued to puzzle over the text, and my suspicions ultimately proved well-founded. I was sitting at my desk with the text of Aristotle's *Physics* open in front of me and with a four-colored pencil in my hand. Looking up, I gazed abstractedly out the window of my room—the visual image is one I still retain. Suddenly the fragments in my head sorted themselves out in a new way, and fell into place together. My jaw dropped, for all at once Aristotle seemed a very good physicist indeed, but of a sort I'd never dreamed possible. Now I could understand why he had said what he'd said, and what his authority had been. Statements that had previously seemed egregious mistakes, now seemed at worst near misses within a powerful and generally successful tradition. ...

When the term 'motion' occurs in Aristotelian physics, it refers to change in general, not just to the change of position of a physical body. Change of position, the exclusive subject of mechanics for Galileo and Newton, is one of a number of subcategories of motion for Aristotle. Others include growth (the transformation of an acorn to an oak), alterations of intensity (the heating of an iron bar), and a number of more general qualitative changes (the transition from sickness to health). ... All varieties of change are seen as like each other, as constituting a single natural family. ... All are thereafter changes-of-state; their end points and the elapsed time of transition are their salient features. Seen in that way, motion cannot be relative and must be in a category distinct from rest, which is a state. ...

A second aspect of Aristotle's physics ... is the centrality of qualities to its conceptual structure. ... Aristotelian physics inverts the ontological hierarchy of matter and quality that has been standard since the middle of the seventeenth century. In Newtonian physics a body is constituted of particles of matter, and its qualities are a consequence of the way those particles are arranged, move, and interact. In Aristotle's physics, on the other hand, matter is very nearly dispensable. It is a neutral substrate, present wherever a body could be—which means wherever there's space or place. A particular body, a substance, exists in whatever place this neutral substrate, a sort of sponge, is sufficiently impregnated with qualities like heat, wetness, color, and so on to give it individual identity. ...

Position is thus, like wetness or hotness, a quality of the object, one that changes as the object moves or is moved. Local motion (motion tout court in Newton's sense) is therefore change-of-quality or change-of-state for Aristotle, rather than being itself a state as it is for Newton. But it is precisely seeing motion as change-of-quality that permits its assimilation to all other sorts of change—acorn to oak or sickness to health, for examples. ... The conception of motion-as-change and the conception of a qualitative physics prove deeply interdependent, almost equivalent notions.

Another aspect of Aristotle's physics—one that regularly seems ridiculous in isolation—begins to make sense as well. Most changes of quality, especially in the organic realm, are asymmetric, at least when left to themselves. An acorn naturally develops into an oak, not vice versa. A sick man often grows healthy by himself, but an external agent is needed, or believed to be needed, to make him sick. One set of qualities, one end point of change, represents a body's natural state, the one that it realizes voluntarily and thereafter rests. The same asymmetry should be characteristic of local motion, change of position, and indeed it is. The quality that a stone or other heavy body strives to realize is position at the center of the universe; the natural position of fire is at the periphery. That is why stones fall toward the center until blocked by an obstacle and why fire flies to the heavens. They are realizing their natural properties just as the acorn does through its growth. Another initially strange part of Aristotelian doctrine begins to fall into place. ...

Aristotle's doctrine about the vacuum or void ... displays with particular clarity the way in which a number of theses that appear arbitrary in isolation lend each other mutual authority and support. Aristotle states that a void is impossible: his underlying position is that the notion itself is incoherent. By now it should be apparent how that might be so. If position is a quality, and if qualities cannot exist separate from matter, then there must be matter wherever there's position, wherever body might be. But that is to say that there must be matter everywhere in space: the void, space without matter, acquires the status of, say, a square circle.

That argument has force, but its premise seems arbitrary. Aristotle need not, one supposes, have conceived position as a quality. Perhaps, but we have already noted that that conception underlies his view of motion as change-of-state, and other aspects of his physics depend on it as well. If there could be a void, then the Aristotelian universe or cosmos could not be finite. It is just because matter and space are coextensive that space can end where matter ends, at the outermost sphere beyond which there is nothing at all, neither space nor matter. That doctrine, too, may seem dispensable. But expanding the stellar sphere to infinity would make problems for astronomy, since that sphere's rotations carry the stars about the earth. Another, more central, difficulty arises earlier. In an infinite universe there is no center—any point is as much the center as any other—and there is thus no natural position at which stones and other heavy bodies realize their natural quality. Or, to put the point in another way, one that Aristotle actually uses, in a void a body could not be aware of the location of its natural place. It is just by being in contact with all positions in the universe through a chain of intervening matter that a body is able to find its way to the place where its natural qualities are fully realized. The presence of matter is what provides space with structure. Thus, both Aristotle's theory of natural local motion and ancient geocentric astronomy are threatened by an attack on Aristotle's doctrine of the void. ...

Revolutionary changes are somehow holistic. They cannot, that is, be made piecemeal, one step at a time. ... In the case of

Aristotelian physics, one cannot simply discover that a vacuum is possible or that motion is a state, not a change-of-state. An integrated picture of several aspects of nature has to be changed at the same time. . . .

The central characteristic of scientific revolutions is that they alter the knowledge of nature that is intrinsic to the language itself and that is thus prior to anything quite describable as description or generalization, scientific or everyday. To make the void or an infinite linear motion part of science required observation reports that could only be formulated by altering the language with which nature was described. Until those changes had occurred, language itself resisted the invention and introduction of the sought-after new theories.

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§ 1.3. Podcasts

VIKTOR BLÅSJÖ, *Opinionated History of Mathematics*, podcast. See also the corresponding sections of Viktor Blåsjö, *Galileo, ignoramus: Mathematics versus Philosophy in the Scientific Revolution*, arXiv:2102.06595.

- "Galileo bad, Archimedes good"
- "Galilean science in antiquity?"
- "Historiography of Galileo's relation to antiquity and middle ages"

§ 2. Early modern mechanics

§ 2.1. Discussion questions

2.1. Galileo used logic to prove Aristotle wrong and arrive at the correct law of fall. Discuss.

Galileo's argument: Aristotle says heavy $\implies$ faster. Hence heavy + light $\implies$ even faster. But also light (= slow) object retards the heavy one, so heavy + light $\implies$ slower.* Contradiction. (* Not actually Aristotle's opinion.)

2.2. Galileo used experiment to prove Aristotle wrong and arrive at the correct law of fall. Discuss.

Experiment disproves Aristotle's law, as was pointed out already in antiquity. Galileo's correct law is in far from perfect agreement with experiment, due to air resistance.

2.3. How and why did Galileo fall short of the modern notion of inertia?

2.4. Explain and criticise Galileo's theory of tides.

Problems: Inconsistent with relativity of motion (implies that experiment in ship cabin can distinguish between rest and uniform motion). Implies one high tides per day, instead of actual two (due to secondary factors, according to Galileo).

2.5. Galileo's dismissal of the lunar theory of tides is similar to how some people today dismiss what they consider pseudo-science (vaccines causing autism, homeopathy, creationism, etc.). Discuss.

2.6. Was Galileo's use of friction (for example to explain why his law of fall does not agree with experiments) an instance of "explaining" something by merely giving a name to it?

§ 2.2. Podcasts

VIKTOR BLÅSJÖ, *Opinionated History of Mathematics*, podcast. See also the corresponding sections of Viktor Blåsjö, *Galileo, ignoramus: Mathematics versus Philosophy in the Scientific Revolution*, arXiv:2102.06595.

- "The case against Galileo on the law of fall"
- "Galileo's errors on projectile motion and inertia"
- "Why Galileo is like Nostradamus"
- "Galileo's theory of tides"

§ 3. Early astronomy

§ 3.1. Discussion questions

3.1. Why did classical astronomy describe planetary motions by combinations of circles?

Metaphysics (circle perfect, eternal, cyclic, complete). Trigonometry/mathematical viability? Gear planetaria?

3.2. Were Greek astronomers "realists" (i.e., believed their planetary models corresponded to physical reality) or "instrumentalists" (i.e., considered their theories to be nothing more than recipes for calculation and prediction)?

Arguments for realism: Ptolemy on planetary distances. Terrestrial physics arguments against earth's motion. Physical astronomy (e.g. smaller body orbits larger).

3.3. How and why does Osiander argue that Copernicus's work is instrumentalist?

Division of labour between astronomer and philosopher. Computational efficiency all that matters for astronomy. Venus variations in brightness not observed.

3.4. How do the systems of Ptolemy and Copernicus differ regarding the distance variations of the moon and Venus? What does this tell us about realism versus instrumentalism?

Ptolemy's moon model implies too big size fluctuations; fixed by Copernicus. Copernicus's Venus model implies big size fluctuations, which were not observed. Moon model might seem to suggest Ptolemy instrumentalist. But then by the same logic Copernicus would be instrumentalist about Venus, which we know is false.

3.5. What were the pros and cons of Copernicus's sun-centered system as compared with Ptolemy's earth-centered system at the time Copernicus published his book? What evidence or arguments compelled astronomers to switch?

Pro: Better explanations of retrograde motion, bounded elongation. Predicts planetary distances (but unverifiable). Con: Predicts parallax, but not observed. Predicts size variations in e.g. Venus, but not observed. Incompatible with Aristotelian physics and other received wisdom.

3.6. Was Copernicus's system superior to that of Ptolemy in terms of accuracy? Simplicity? Aesthetics? Fitting a wider range of facts?

No. Sort of (explanatory simplicity only, not less complex models). Sort of (e.g. planetary distances locked, more satisfying explanations). No.

3.7. What were the pros and cons of Tycho Brahe's hybrid system, as compared with the Copernican and Ptolemaic systems?

For: no parallax/wasted space; terrestrial physics. Against: bigger body orbits smaller body.

3.8. Was Copernicus revolutionary or conservative?

Revolutionary: heliocentrism. Conservative: principle of uniform circular motion.

3.9. How do we know that the earth is spherical?

Seen by how view of heaven changes as we move east-west or north-south. Shape of earth seen on moon mentioned by Copernicus (top right 129); confusingly as refutation of those who posited "drum-shaped" earth.

3.10. To prove that the earth is spherical, Sacrobosco claims that stars "rise sooner for orientals than for westerners." How could this be ascertained at the time?

It could not. Consider Berlin-Amsterdam. Almost straight west. About 30 minutes sun-time difference. About 6 hours by car. Note Ptolemy's 45.4: this is known because of eclipses.

3.11. How do we know that the earth is negligibly small in relation to the sphere of the stars, and located at the center of that sphere?

We always see half the sphere of the stars.

3.12. On what grounds does Ptolemy argue that the earth is immobile?

Absence of parallax effects. Whirling would throw animals off. If rotating, objects would not fall below where they were dropped.

3.13. Ptolemy's text suggests that contemporaries disagreed with his view that the earth is immobile. What can we infer about them and their arguments from Ptolemy's text?

Many of them ("all those who ..."). Implausible for heavy body to simply hover in space, but maybe not to orbit (cf. rock in sling). They already had inertia-style arguments.

3.14. Explain why Copernicus's sun-centered model gives more natural explanations of the retrograde motions of the outer planets.

Closer to sun $\implies$ faster. Inner planets passing outer ones like faster runner on inner track in a stadium.

3.15. Explain why the sun-centered model explains why the inner planets (Mercury and Venus) never deviate too far from the sun while the other planets can be found in any position relative to the sun.

Line of sight from earth to inner planet always points "inwards."

3.16. Explain why the relative distances to the planets are not determined in an earth-centered model of the solar system, but are so in a sun-centred model.

Consider what happens if the radius of one planetary orbit (e.g. Saturn) is varied in either model. See figures of this below.

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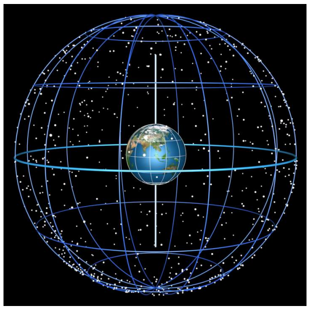
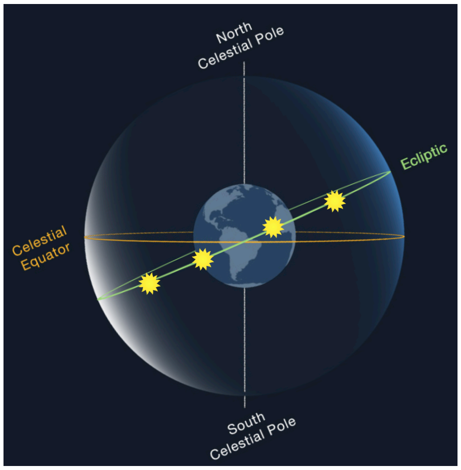
§ 3.2. The basic facts of naked-eye astronomy

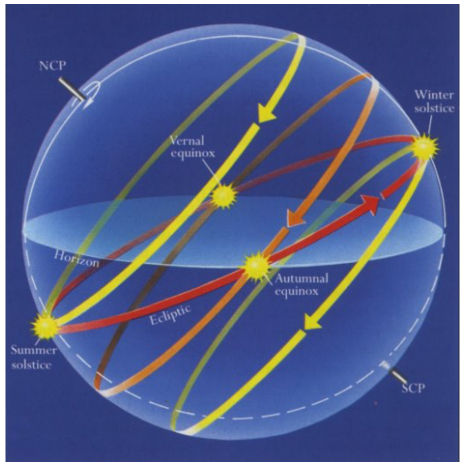
Think of the sky as a sphere: the *celestial sphere*. Think of all the stars as shiny points nailed onto this sphere. We call them the *fixed stars*. Since we cannot judge depth or distance of heavenly objects by eye, we can just as well imagine everything taking place on a single surface like this. The earth is at the center of the sphere. The radius of the earth can be considered zero in relation to the celestial sphere. The celestial sphere rotates one full revolution in 24 hours. The axis of rotation passes through the north and south poles of the earth.

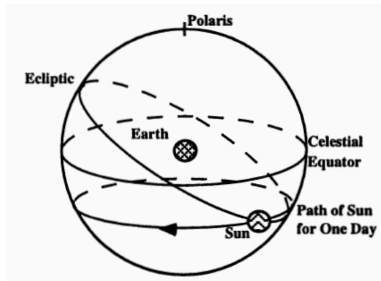


The sun can also be thought of as attached to this sphere, like a sticker. It follows along with the 24-hour rotation of the celestial sphere. But in addition to this it has its own, much slower motion. It's as if someone removes the sun sticker at the end of each day and puts it back again in almost the same spot by not quite. If we forget the daily rotation of the celestial sphere and focus only on the motion of the sun relative to the fixed stars, we find that the sun moves in a circle and comes back to its original position after one year. This circle—the path of the sun's motion in the course of a year—is called the *ecliptic*.

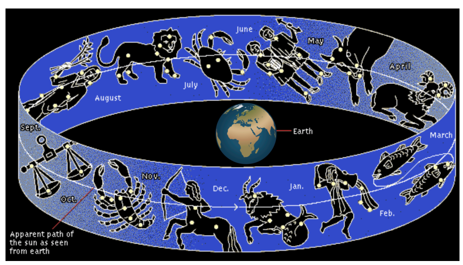


So while the sun circles around us every day, more interesting for astronomical purposes is its intrinsic motion with respect to the fixed stars.





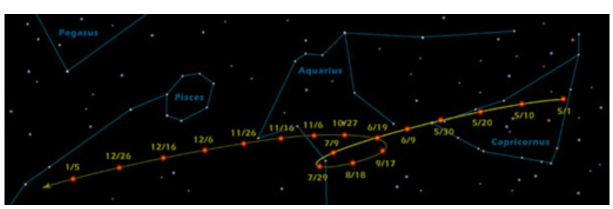
We keep track of the position of the sun with respect to the stars by recording the various constellations it passes through in the course of a year. This is the *zodiac* known from astrology.



The planets and the moon behave like the sun in these respects. They too are “moveable stickers” on the celestial sphere that spin around with it every day, and also on top of that have their own much slower motion relative to the fixed stars. The latter motion is the astronomically interesting one. From now on, whenever we speak of the motions of the heavenly bodies we mean this latter motion only.

Altogether there are seven heavenly bodies, that is to say seven things that move with respect to the stars, as far as naked-eye astronomy is concerned: the sun, the moon, Mercury, Venus, Mars, Jupiter, Saturn. The paths of these bodies are all approximately the same as that of the sun—the ecliptic. The planets used to be called “wandering stars” because they look like fixed stars, only they move.

The motions of these bodies is not very regular. Sometimes they go faster, sometimes slower. Planets even sometimes *retrograde*, that is, go backwards for a while with respect to the stars:

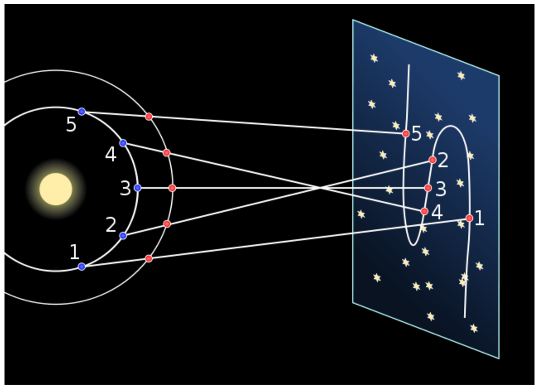


The fundamental task of classical astronomy is to work out a theory that enables us to calculate where the heavenly bodies are going to be at any point in time.

Benefits of the Copernican system include obvious and immediate explanations of the following, which are not naturally explained in geocentric astronomy:

- Retrograde motion, including: why retrogression coincides with opposition and maximum luminosity (for superior planets); why retrogression is more frequent the further the planets is from us; why the retrograde arc is greater the closer the planet is to us.

  This follows from first principles in the Copernican system, since retrograde motion is due to the earth speeding past the slower, outer planets.

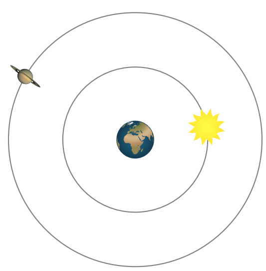


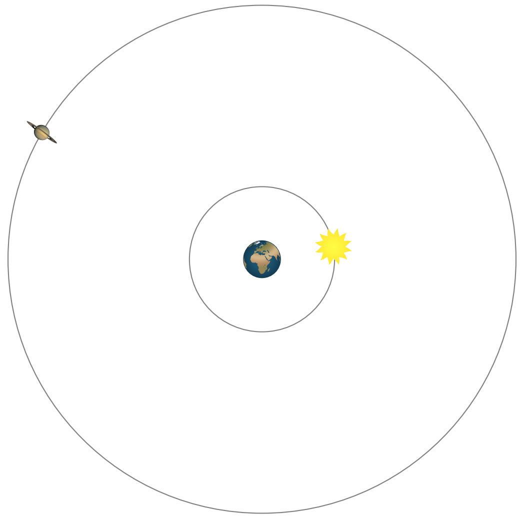
- Why inferior planets (Mercury and Venus) are never seen far from the sun.

  This follow from the fact that their orbits are enclosed in the orbit of the earth.

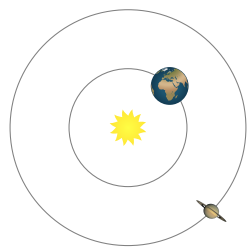
- Peculiarities of Ptolemy, including: why Ptolemy's planets all have a solar component (namely, to explain bounded elongation or why retrogression coincides with opposition) but the moon does not; why orbital periods are not constant as seen from the earth.

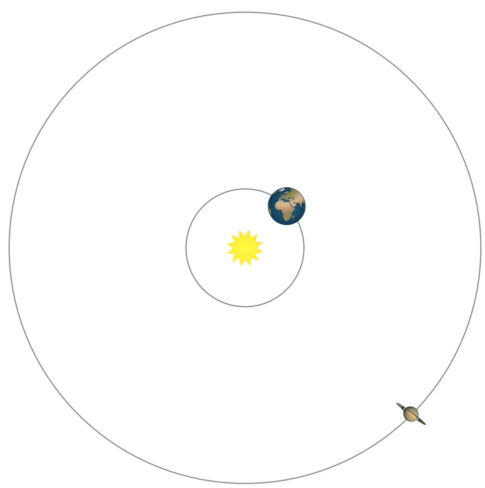
Another "advantage" of Copernicus's system is that is "locks" the planetary distances. The relative distances to the planets are not determined in an earth-centered model of the solar system since the following configurations observationally equivalent (from earth we can only see in what direction planets are located, not how far away they are):





But relative distances are determined in a sun-centred model, since the following configurations are *not* observationally equivalent:



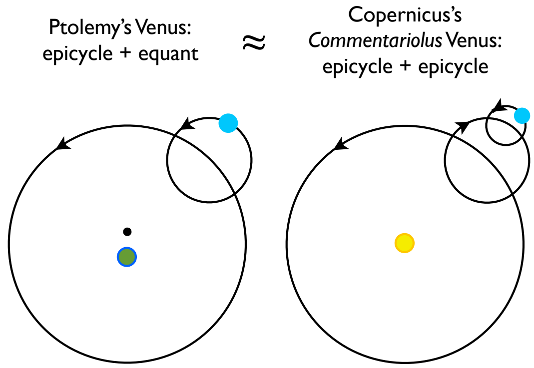


Problems with Copernicus' system in 1543 included:

- Rapid motion of the earth supposedly inconsistent with physical experience.
- Heliocentrism incompatible with Aristotelian worldview, the Bible.
- Unsuccessful predictions of:
  - Parallax; variation in bisection of the stellar sphere; variation of brightness of the stars.
  - Greater variations in planetary distances from earth than observed (expected to be visible as variations in luminosity). For instance, Venus being sometimes very close and sometimes very far away, compared to its more or less uniform distance from earth in Ptolemy's system.

Copernicus was in some respects very conservative and hardly a prototype revolutionary at all. Notably, he disapproved strongly of Ptolemy's use of the equant, which he considered a betrayal of the principle of perfect circular motion. Thus, rather than trying to innovate and discard tradition, he was keen to go *back* to the original vision of circular motion of classical antiquity—a reactionary philosophical principle that was universally recognised as complete nonsense just a century or so later. The connection between the elimination of the equant and Copernicus's main achievement—the switch to heliocentrism—is not very strong or direct, however. Heliocentrism itself had also been advocated in antiquity, but almost all works related to this are lost. Copernicus was aware of a few allusions to heliocentrism in ancient sources, which he cited.

In terms of predictive accuracy and technical simplicity, Copernicus's theory was pretty much on par with Ptolemy's. Indeed, Copernicus's basic goal was essentially to reproduce the same net effects as Ptolemy's system, only with equant-free, heliocentric models.



NICOLAUS COPERNICUS, *On the Revolutions of Heavenly Spheres*, 1543, translated by C. G. Wallis, Prometheus Books, 1995.

On planetary distances: "[Previous astronomers] have not been able to discover or to infer the chief point of all, i.e., the form of the world and the certain commensurability of its parts. But they are in exactly the same fix as someone taking from different places hands, feet, head, and the other limbs—shaped very beautifully but not with reference to one body and without correspondence to one another—so that such parts made up a monster rather than a man." (5) In Copernicus's system, by contrast, a "correlation binds together so closely the order and the magnitudes of all the planets and of their spheres or orbital circles and the heavens themselves that nothing can be shifted around in any part of them without disrupting the remaining parts and the universe as a whole." (6)

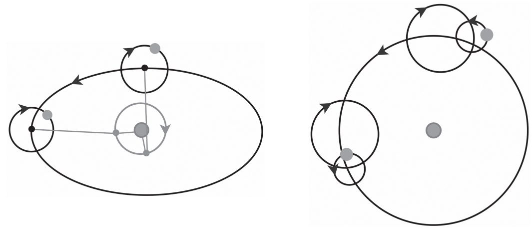
"I have no doubt that talented and learned mathematicians will agree with me." (6) "[I will make everything] clearer than day—at least for those who are not ignorant of the art of mathematics." (24) "If perchance there are certain idle talkers . . . , although wholly ignorant of mathematics, . . . [who] dare to reprehend and to attack my work; they worry me so little that I shall . . . scorn their judgements." (7) These are the kinds of people who "on account of their natural stupidity hold the position among philosophers that drones hold among bees." (4) "The studious need not be surprised if people like that laugh at us. Mathematics is written for mathematicians." (7)

NICOLAUS COPERNICUS, Commentariolus, c. 1510, quoted from Noel M. Swerdlow, The derivation and first draft of Copernicus's planetary theory: A translation of the Commentariolus with commentary, *Proceedings of the American Philosophical Society*, cxvii (1973), 423–512..

"Ptolemy . . . envisioned certain equant circles, on account of which it appeared that the planet never moves with uniform velocity. . . . Therefore a theory of this kind seemed neither perfect enough nor sufficiently in accordance with reason. . . . I often pondered whether perhaps a more reasonable model composed of circles could be found from which every apparent irregularity would follow while everything in itself moved uniformly, just as the principle of perfect motion requires." (434–435)

"[Ptolemy has] fallen into . . . obvious errors [in his theory of the moon]. For it follows [from it] that . . . it should appear about four times larger [at some times than others], unless in addition an increase and decrease in the size of its body is rashly maintained. . . . If, however, one investigates more carefully, he will find that [the apparent size] differ only very slightly . . . , and consequently he will not easily doubt our more reasonable theory." (461)

Schematic comparison of the lunar models of Ptolemy (left) and Ibn al-Shatir and Copernicus (right):



§ 3.4. *Reader*

Andrew Ede & Lesley Cormack, *A History of Science in Society: A Reader*, Broadview Press, 2007.

3.9. Sacrobosco, from “The Earth is a Sphere” (122) to “The Earth Immobile” (123).

2.1.1. Ptolemy. Section 1: skip. Sections 3–6: say basically the same thing as Sacrobosco; you do not need to read both in detail. Section 7: interesting. Section 8: can basically be skipped since the essential ideas here are described in the notes above.

4.1. Copernicus. Osiander’s Foreword: read. Chapters 1–3: skip (repeats the same arguments as in Ptolemy/Sacrobosco for a third time! stupid selection made by the editors). Chapter 4–end: skim quickly, but don’t worry about the technical details.

§ 3.5. *Podcasts*

Viktor Blåsjö, *Opinionated History of Mathematics*, podcast. See also the corresponding sections of Viktor Blåsjö, *Galileo, ignoramus: Mathematics versus Philosophy in the Scientific Revolution*, arXiv:2102.06595.

- “Heliocentrism in antiquity”
- “Heliocentrism before the telescope”

§ 4. Physical and telescopic astronomy

§ 4.1. *Discussion questions*

4.1. How did telescopic evidence change the credibility of the Ptolemaic, Copernican, and Tychonic systems? (Key phenomena are: moons of Jupiter; phases of Venus; sunspots.)

Moons of Jupiter shows that there are multiple centers of circular motion, thus removing one argument for the Ptolemaic system. Phases of Venus show that Venus orbits the sun (compatible with Tycho or Copernicus, not Ptolemy). The paths of sunspots easier to explain heliocentrically, according to Galileo, although the wobbling of the earth's axis shows that geocentric explanation cannot be dismissed.

4.2. The terrestrial and celestial realms are fundamentally different. One should not assume that the science of one is applicable to the other. What led people to accept or reject this point of view?

This was Aristotle's view. Went with his theory of four elements etc. Heavens eternal, cyclic, rule-bound; earth chaotic, transient. Goes with divine perfection. Contra: Moon a rock. Starting from religion, it makes sense to assume division. Starting from science, it makes sense to assume unity.

4.3. The purpose of astronomy is to calculate and predict appearances ("instrumentalism"). It is pointless to speculate about physical forces in the heavens. Discuss. What led astronomers to reject this point of view?

Physics experimentally accessible, sky not. Scientists have often assumed unity of terrestrial and celestial physics, first as analogies (rock in sling etc.), then as exact numerical science (Newton: moon test, proofs of Kepler's laws).

4.4. Many things Kepler "explained" by design arguments are now considered the outcome of pure chance and hence are not explained at all by modern science. But shouldn't science, as it progresses, explain *more and more*, rather than *less and less*?

E.g. number of planets, their distances, size of moon and sun at eclipse, number of moons of Jupiter.

§ 4.2. *Podcasts*

VIKTOR BLÅSJÖ, *Opinionated History of Mathematics*, podcast. See also the corresponding sections of Viktor Blåsjö, *Galileo, ignoramus: Mathematics versus Philosophy in the Scientific Revolution*, arXiv:2102.06595.

- "The telescope"
- "Blemished sun"
- "Phases of Venus"
- "Galileo's theory of comets is hot air"

§ 5. Scientific Revolution?

§ 5.1. Discussion questions

5.1. What was the most distinctive ingredient that set the "Scientific Revolution" apart from previous centuries? Give arguments for and against the following being key:

- The application of mathematics to the physical world.
- Empiricism.
- Experimental method.
- Technology and craft knowledge.
- Realism.
- The "mechanical philosophy."

All arguably neglected in the Aristotelian tradition, but heavily emphasised in the Galilean. All arguably always accepted in the mathematical tradition, but for a long time in Europe it was thought that philosophy, not mathematical science, was the highest knowledge and the best way to understand the essential nature of the world. Ancient Greek examples of the above: astronomy, statics (lever etc.), optics (laws of reflections, refraction; properties of curved mirrors), hydrostatics (equilibrium conditions for floatation of paraboloids), harmonics (musical chords correspond to simple numerical ratios), zoology (studying embryonic development by opening eggs etc.), advanced machines and automata (including explanations of organic phenomena in such terms), atomism.

5.2. Compare and contrast the Scientific Revolution with Greek thought. What led Greek thinkers such as Plato and Aristotle to their different emphasis? What led people to transition away from their points of view?

Plato glorified pure philosophy and mathematics, contrasting their nobility and perfection with the imperfect physical world and petty everyday concerns. Aristotle's philosophy was fundamentally qualitative. It was felt by many (esp. in European Middle Ages, Renaissance) that such philosophising constituted core knowledge about how the world is constituted, whereas mathematical approaches to nature were seen as mere computational means to an end (e.g., navigation at sea, calendric calculations, etc.).

5.3. How should we interpret precursors of the Scientific Revolution in Greek times? Were they isolated occurrences whose modern appeal is misleading, or did the real Scientific Revolution occur in ancient Greece?

Difficult to say definitively because so few sources have survived and so much is lost. The Aristotelian tradition still going strong in Galileo's day should not be assumed to be the state of knowledge in Greek times. Arguably, indications about lost sources suggests that Hellenistic scientists were closer to a "Galilean" worldview than an "Aristotelian" one.

§ 5.2. Mechanical philosophy readings

ROBERT BOYLE, *A Free Enquiry into the Vulgarly Received Notion of Nature*, 1686, Cambridge University Press, 1996.

Allegorical statement of the critical method: "For I am wont to judge of opinions as of coins: I consider much less, in any one that I am to receive, whose inscription it bears, than what metal it is made of. It is indifferent enough to me whether it was stamped many years or ages since, or came but yesterday from the mint. Nor do I regard through how many, or how few, hands it has passed for current, provided I know by the touchstone or any sure trial purposely made, whether or no it be genuine, and does or does not deserve to have been current. For if upon due proof it appears to be good, its having been long and by many received for such will not tempt me to refuse it. But if I find it counterfeit, neither the prince's image or inscription, nor its date (how ancient soever), nor the multitude of hands through which it has passed unsuspected will engage me to receive it. And one disfavouring trial, well made, will much more discredit it with me than all those specious things I have named can recommend it." (5)

The fundamental conflict between the two views of nature is brought out by another allegory: "They seem to imagine the world to be after the nature of a puppet, whose contrivance indeed may be very artificial, but yet is such that almost every particular motion the artificer is fain (by drawing sometimes one wire or string, sometimes another) to guide, and oftentimes overrule, the actions of the engine; whereas, according to us, it is like a rare clock … where all things are so skilfully contrived that the engine being once set a-moving, all things proceed according to the artificer's first design, and the motions of the little statues that at such hours performs these or those things do not require (like those of puppets) the peculiar interposing of the artificer or any intelligent agent employed by him, but perform their functions upon particular occasions by virtue of the general and primitive contrivance of the whole engine." (12-13)

As for the actual arguments against the teleological view of nature, they consist most importantly in "diverse phenomena which do not agree with the notion or representation of nature that I question" (63), such as:

Nature abhors vacuum only inconsistently. “When a glass bubble is blown very thin at the flame of a lamp and hermetically sealed while it is very hot, the cause that is rendered why it is apt to break when it grows cold, is that the inward air (which was before rarefied by the heat), coming to be condensed by the cold, lest the space deserted by the air that thus contracts itself should be left void, nature with violence breaks the glass in pieces. But, by these learned men’s favour, if the glass be blown but a little stronger than ordinary, though at the flame of a lamp, the bubble (as I have often tried) will continue unbroken, in spite of nature’s pretended abhorrency of a vacuum, which needs not at all to be recurred to in the case.” (65) “And why does she furiously break in pieces a thin sealed bubble, such as I come from speaking of, to hinder a vacuum? If in case she did not break it, no vacuum would ensue. And on the other side, if we admit her endeavours to hinder a vacuum not to have been superfluous, and consequently foolish, we must confess that where these endeavours succeed not, there is really produced such a vacuum as she is said to abhor. So that, as I was saying, either she must be very indiscreet to trouble herself and to transgress her own ordinary laws to prevent a danger she need not fear, or her strength must be very small—that is, not able to … break a tender glass bubble, which perhaps a pound weight on it would … crush in pieces.” (66-67)

Bouncing ball wasteful. “For if (for example) you let fall a ball upon the ground, it will rebound to a good height, proportionable to that from whence you let it fall, or perhaps will make several lesser rebounds before it come to rest. If it be now asked, why the ball, being let out of your hand, does not fall on this or that side, or move upwards, but falls directly towards the centre of the earth by that shortest line … which is the diameter of the earth prolonged to the centre of gravity of the ball? It will be readily answered that this proceeds from the ball’s gravity, i.e. an innate appetite whereby it tends to the centre of the earth the nearest way. But then I demand, whence comes this rebound, i.e. this motion upwards? For it is plain, it is the genuine consequence of the motion downwards, and therefore is increased according as that motion in the ball was increased, by falling from a greater height. So that it seems that nature does in such cases play a very odd game, since she forces a ball, against the laws of heavy bodies, to ascend divers times upwards, upon the account of that very gravity whose office it is to carry it downwards the directest way. And at least she seems, in spite of the wisdom ascribed to her, to take her measures very ill, in making the ball move downwards with so much violence, as makes it divers times fly back from the place she intended it should go to. As if a ball which a child can play with and direct as he pleases were so unwieldy a thing that nature cannot manage it, without letting it be hurried on with far greater violence than her design requires.” (67-68)

Bubbles in water inconsistent with natural place theory. “For if a bubble happens to arise from the bottom of a vessel to the upper part of it, we are told that the haste wherewith the air moves through water proceeds from the appetite it has to quit that preternatural place and rejoin the element, or great mass of air detained at the very surface of the water by a very thin skin of that liquor, together with which it constitutes a bubble. Now I demand how it comes to pass, that this appetite of the air—which, when it was at the bottom of the water, and also in its passage upwards, is supposed to have enabled it to ascend with so much eagerness and force as to make its way through all the incumbent water (which possibly was very deep)—should not be able, when the air is arrived at the very top of the water, to break through so thin a membrane of water as usually serves to make a bubble, and which suffices to keep it from the beloved conjunction with the great mass of the external air, especially since they tell us that natural motion grows more quick, the nearer it comes to the end or place of rest, the appetites of bodies increasing with their approaches to the good they aspire to, upon which account falling bodies, as stones, etc., are said (though falsely) to increase their swiftness the nearer they come to the earth. But if, setting aside the imaginary appetite of the air, we attribute the ascension of bubbles to the gravity and pressure upwards of the water, it is easy hydrostatically to explicate why bubbles often move slower when they come near the surface of the water, and why they are detained there; which last phenomenon proceeds from this: that the pressure of the water being there inconsiderable, it is not able to make the air quite surmount the resistance made by the tenacity of the superficial part of the water. And therefore in good spirit of wine, whose tenacity and glutinousness is far less than that of water, bubbles rarely continue upon the surface of the liquor, but are presently broken and vanish. … I shall add that I have often observed that water, in that state which is usually called its natural state, is wont to have store of aerial particles mingled with it … as may appear by putting a glass full of water into the receiver of the new pneumatical engine. For the pressure of the external air being by the pump taken off, there will from time to time disclose themselves in the water a multitude of bubbles, made by the aerial particles that lay concealed in that liquor. … so little appetite has air in general to flee all association with water and make its escape out of that liquor, though when sensible portions of it happen to be underwater, the great inequality in gravity between those two fluids makes the water press up the air.” (82-83)

Nature changeable and forgetful in caring to restore springy bodies. “If, for example, you take a somewhat long and narrow plate of silver that has not been hammered or compressed … you may bend it which way you will, and it will constantly retain the last curve figure that you gave it. But if, having again straightened this plate, you give it some smart strokes of a hammer, it will by that merely mechanical change become a springy body: so that if with your hand you force it a little from its rectitude, as soon as you remove your hand it will endeavour to regain its former straightness. … Now upon these phenomena I demand why, if nature be so careful to restore bodies to their former state, she does not restore the silver blade or plate to its rectitude when it is bent this way or that way before it be hammered? And why a few strokes of a hammer (which, acting violently, seems likely to have put the metal into a preternatural state) should entitle the blade to nature’s peculiar care, and make her solicitous to restore it to its rectitude when it is forced from it? And why, if the springy plate be again ignited and refrigerated of itself, nature abandons her former care of it, and suffers it quietly to continue in what crooked posture one pleases to put it into? ... I shall add to what I was just now saying, that even in sword blades it has been often observed that though if, soon after they are bent, the force that bent them be withdrawn, they will nimbly return to their former straightness. Yet if they ... be kept too long bent, they will lose the power of recovering their former straightness and continue in that crooked posture, though the force that put them into it cease to act. So that it seems nature easily forgets the care she was presumed to take of it at first." (86-87)

ROBERT BOYLE, About the excellency and grounds of the mechanical hypothesis (1674), in *Selected Philosophical Papers of Robert Boyle*, Hacket, 1991, 138–154.

Arguments for corpuscularism:

"Intelligibleness or clearness of Mechanical principles and explications." Other theories "are either so general and slight, or otherwise so unsatisfactory, that, granting their principles, it is very hard to understand or admit their applications of them to particular phenomena."

Unlimited scope. "When I consider the almost innumerable diversifications that compositions and decompositions may make of small number, not perhaps exceeding twenty, of distinct things, I am apt to look upon those who think the Mechanical principles may serve indeed to give an account of the phenomena of this or that particular part of natural philosophy, as statics, hydrostatics, the theory of planetary motions, &c., but can never be applied to all the phenomena of things corporeal—I am apt, I say, to look upon those, otherwise learned, men as I would upon him that should affirm that, by putting together the letters of the alphabet, one may indeed make up all the words to be found in one book, as in Euclid or Virgil, or in one language, as Latin or English, but that they can by no means suffice to supply words to all the books of a great library, much less to all the languages in the world."

Primacy of its principles. "Of the principles of things corporeal, none can be more few, without being insufficient, or more primary, than matter and motion," "neither of them being resoluble into any things whereof it may be truly, or as much a tolerably, said to be compounded." "So that the fear that so much of a new physical hypothesis as is true will overthrow, or make useless, the Mechanical principles, is as if one should fear that there will be a language proposed that is discordant from, or not reducible to, the letters of the alphabet."

Scalability of nature. "And he that looks upon sand in a good microscope will easily perceive that each minute grain of it has as well its own size and shape as a rock or mountain. And when we let fall a great stone and a pebble from the top of a high building, we find not but that the latter as well as the former moves conformably to the laws of acceleration in heavy descending bodies." "And therefore to say that, though in natural bodies whose bulk is manifest and their structure visible the Mechanical principles may be usefully admitted, they are not to be extended to such portions of matter whose parts and textures are invisible, may perhaps look to some as if a man should allow that the laws of mechanism may take place in a town clock, but cannot in a pocket watch, or ... as if, because the terraqueous globe is a vast magnetical body ... one should affirm that magnetical laws are not to be expected to be of force in a spherical piece of loadstone that is not perhaps in inch long."

Against agents. "They that, to solve the phenomena of nature, have recourse to agents ... tell us nothing that will satisfy the curiosity of an inquisitive person, who seeks not so much to know what is the general agent that produces a phenomenon, as by what means, and after what manner, the phenomenon is produced." It is the latter that matters, as witnessed, for example, by the fact that ground corn is the same "whether the corn be ground by a water-mill or a windmill, or a horse-mill, or a hand-mill; that is, by a mill whose stones are turned by inanimate, by brute, or by rational, agents"; or again by the uselessness to "a sober physician, that comes to visit a patient reported to be bewitched, receives of the strange symptoms he meets with and would have an account of, if he be coldly answered that it is a witch or the devil that produces them."

WILLIAM GILBERT, *De Magnete*, 1600, quoted from the Dover edition, 1991.

Condemnation of the past. "What business have I in that vast ocean of books? ... By the more silly ones among them the crowd and most impudent people get intoxicated [and] declare themselves to be philosophers ... [But] neither Greek arguments nor Greek words can assist in finding truth." (Preface)

"For example, they asserted that a loadstone rubbed with garlic does not attract iron; nor when it is in the presence of a diamond. The like of this is found in Pliny and Ptolemy's Quadripartitum; and errors have steadily been ... accepted—even as evil and noxious plants ever have the most luxuriant growth—down to our day, being propagated in the writings of many authors who, to the end that their volumes might grow to the desired bulk, do write and copy all sorts about ever so many things about which they know naught for certain in the light of experience." (I.I, 2-3)

The same refuted by experiment. "[The claim regarding diamonds] is contrary to our magnetic rules; and hence we made the experiment ourselves with seventy-five diamonds in presence of many witnesses ... yet never was in granted to me to see the effect mentioned." (III.XIII, 218)

A fact that can be established by experiment, on the other hand, is "that the globe of the earth is magnetick." For the behaviour of a compass needle in various places around the earth is precisely mirrored by its behaviour in the vicinity of a spherical loadstone. Even the compass' deviations from perfect north can be accounted for on this theory. For the earth is not perfectly spherical, and if one cuts out "oceans" in the spherical loadstone one finds that the needle deviates towards the "continents," as in reality (IV.II).

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§ 5.3. Podcasts

VIKTOR BLÅSJÖ, *Opinionated History of Mathematics*, podcast. See also the corresponding sections of Viktor Blåsjö, *Galileo, ignoramus: Mathematics versus Philosophy in the Scientific Revolution*, arXiv:2102.06595.

- "Mathematics versus philosophy, then and now"
- "Galileo was the first to ... what exactly?"
- "More things Galileo didn't do first"

§ 6. Science and religion

§ 6.1. Discussion questions

6.1. Did new astronomical and scientific discoveries in the 17th century weigh for or against religion?

For: Kepler, planetary distances, eclipse sizes, moon of Jupiter; Newton, stability of the solar system; biological complexity implies design.

6.2. To what extent was Galileo's conflict with the church about fundamentally irreconcilable differences of their belief systems, and to what extent about accidental aspects of the particular social context?

The church was not very concerned with technical astronomy (works by Copernicus, Galileo, etc. had long been tolerated). Conflict had much to do with who had the right to interpret the Bible: a very important issue for social rather than scientific reasons (counterreformation by the Catholic church against Protestantism). Many with similar scientific beliefs as Galileo were profoundly religious (Kepler, Newton).

6.3. Did Kepler's religious and mystical beliefs help or hinder his science?

Made him look for the wrong kinds of explanations, according to modern view. But was a strong motivation to do science, including the "right" questions from a modern point of view such as the ultimate laws of planetary motion, which there were not many other reasons to study and which played a crucial role in the transition toward modern science.

6.4. What aspects of 17th-century religious framings of science can be reconciled with a modern atheistic outlook?

Exploring truth associated with duty, nobility. Seek fundamental "blueprint" of the universe.

6.5. Did mechanistic natural philosophy lead to atheism?

Probably to some extent. Blood transfusions and other biological experiments showed that even the most "vitalist" phenomena were susceptible to mechanical manipulation.

§ 6.2. Reader

ANDREW EDE & LESLEY CORMACK, *A History of Science in Society: A Reader*, Broadview Press, 2007.

4.2.2. Galileo.

§ 6.3. Galileo's private beliefs

DAVID WOOTTON, *Galileo: Watcher of the Skies*, Yale University Press, 2010.

Galileo was officially a Catholic, but Wootton argues that there were "two Galileos, the public Catholic and the private sceptic" (249). "The only decisive document we have" is a 1639 letter to Galileo from "Benedetto Castelli, Galileo's old friend, former pupil and long-time intellectual companion" (247). "If anyone was in a position to know if Galileo was or was not a believer it was Castelli." (248) "Castelli has heard news of Galileo that has made him weep with joy, for he has heard that Galileo has given his soul to Christ. Castelli immediately refers to the parable of the labourers in the vineyard (Matthew 20.1–16): even those who were hired in the last hour of the day received payment for the whole day's work. ... Then ... he turns to the crucifixion, and in particular to the two thieves crucified on either side of Christ (Luke 23.39–43). One confessed Christ as his saviour and was saved; the other did not and was damned. ... Castelli's invocation of the parable of the labourers in the vineyard and of the two thieves crucified alongside Christ is clear and unambiguous. He believes Galileo is coming to Christianity at the last moment, but not too late to save his soul. There is no conceivable interpretation of this letter which is compatible with the generally held view that Galileo was, throughout his career, a believing Catholic." (247) One may add that God plays an essential role in the scientific systems of Kepler, Descartes, Leibniz, and Newton, but no part whatsoever in Galileo's.

§ 6.4. Kepler

MAX CASPAR, *Kepler*, Dover, 1993.

"Aesthetic-artistic consideration of the universe" (382). "I consider it my duty and task ... to advocate ... what I ... have recognized as true and whose beauty fills me with unbelievable rapture on contemplation." (298). "I may say with truth that whenever I consider in my thoughts the beautiful order, how one thing issues out of and is derived from another, then it is as though I had read a divine text, written onto the world itself … saying: Man, stretch thy reason hither, so that thou mayest comprehend these things" (152).

Mathematics a means to this end. "Kepler consciously renounced [Archimedean] rigor and wanted to take over from Archimedes only so much as 'is sufficient for the pleasure of the lovers of geometry.'" (234). "Don't sentence me completely to the treadmill of mathematical calculations and leave me time for philosophical speculations, which are my sole delight. Each one has his own particular pleasure, one the tables and nativities, I the flower of astronomy, the artistic structure of the motions." (308).

Man's cognitive abilities designed for this purpose. "The world partakes of quantity and the mind of man grasps nothing better than quantities for the recognition of which he was obviously created." (96). "Nature loves these relationships in everything that is capable of thus being related. They are also loved by the intellect of man who is an image of the Creator." (94).

The universe designed for this purpose. "The earth's axis in inclined to the ecliptic in consideration of the people distributed over the whole surface of the earth, so that the change of the heavenly phenomena should extend to all places on the earth and consequently all people have a share in it. … Sun and moon have the same apparent sizes, so that the eclipses, one of the spectacles arranged by the Creator for instructing observing creatures in the orbital relations of the sun and the moon, can occur. The earth moves around the sun to make it possible for man to get to know the world and its dimensions." (296).

Reception of the above. These ideas were quite well received e.g. in the case of the *Mysterium Cosmographicum*: "Professor Georg Limnäus in Jena … is ecstatic that at last someone had again revived the time-honoured Platonic art of philosophising. … [Tycho Brahe] takes unusual pleasure in the book: … the zeal, the fine understanding and acumen ought to be praised [even though] certain details give him pause." (69-70). It was different with the more modern physics of the *Astronomia Nova*: "Kepler ran up against rejection and lack of understanding on all sides. Maestlin, Fabricius, Longomontanus and others shook their heads." (135).

CHARLOTTE METHUEN, *Kepler's Tübingen: Stimulus to a Theological Mathematics*, Scolar Press, 1998.

The human mind is created to do mathematics. According to Melanchthon, the atomistic doctrines of creation by chance "wage war against human nature, which was clearly founded to understand divine things" (76); astronomical observations are as natural to a human being as "swimming to a fish or singing to a nightingale" (85).

The purpose of scientific study is therefore twofold. (1) "inflaming their souls with love and enthusiasm for the truth and rousing them to understanding of the noblest things" (Melanchthon, 73). (2) Astronomers are "priests of the book of nature" (Kepler, 206n3). The existence of God follows from the universe's "beauty, order, and all things which have been founded for settled purposes" (Heerbrand, 137). "God desired that knowledge of the wonderful courses and powers should lead us towards knowledge of the divine" (Melanchthon, 76).

JOHANNES KEPLER, *Mysterium Cosmographicum: The Secret of the Universe*, 1596, quoted from the ABaris Books translation, 1981.

What is the purpose of astronomy? "As we do not ask what hope or gain makes a little bird warble, since we know that it takes delight in singing because it is for that very singing that a bird was made, so there is no need to ask why the human mind undertakes such toil in seeking out these secrets of the heavens. … The reason why there is such a great variety of things, and treasuries so well concealed in the fabric of the heavens, is so that fresh nourishment should never be lacking for the human mind, and it … should have in this universe an inexhaustible workshop in which to busy itself." (55)

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§ 6.5. England

ADAM D. RICHTER, On food and fossils: natural philosophy, mathematics, and biblical history in the works of John Wallis, *The Seventeenth Century*, DOI: 10.1080/0268117X.2018.1541424.

Leading mathematician John Wallis worked extensively on calculating biblical chronology (according to which the earth is about 6000–7000 years old) and refused to accept "a time wherein the Earth should have been tossed & turned upside down, & the tops of the Alps become a sea; onely to enable us to give an account of some Fish-shels found there."

Isaac Newton, *Philosophical Writings*, Cambridge Texts in the History of Philosophy, edited by Andrew Janiak, Cambridge University Press, 2004.

Blind fate could never make all the planets move one and the same way in orbits concentric, some inconsiderable irregularities excepted, which may have risen from the mutual actions of comets and planets upon one another, and which will be apt to increase, till this system wants a reformation. Such a wonderful uniformity in the planetary system must be allowed the effect of choice. (138)

The first contrivance of those very artificial parts of animals, the eyes, ears, brain, muscles, heart, lungs, midriff, glands, larynx, hands, wings, swimming bladders, natural spectacles, and other organs of sense and motion; and the instinct of brutes and insects, can be the effect of nothing else than the wisdom and skill of a powerful ever-living agent. (138)

Isaac Newton, *Philosophiae Naturalis Principia Mathematica*, 1687, translated by Andrew Motte.

GENERAL SCHOLIUM. ... It is not to be conceived that mere mechanical causes could give birth to so many regular motions [as are observed in the solar system]. ... This most beautiful system of the sun, planets, and comets, could only proceed from the counsel and dominion of an intelligent and powerful Being. ... Blind metaphysical necessity, which is certainly the same always and every where, could produce no variety of things. All that diversity of natural things which we find suited to different times and places could arise from nothing but the ideas and will of a Being necessarily existing. ... And thus much concerning God; to discourse of whom from the appearances of things, does certainly belong to Natural Philosophy.

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§ 6.6. Newton biography

Niccolo Guicciardini, *Isaac Newton and Natural Philosophy*, Reaktion Books, 2018.

What should one make of "the two Newtons" (19)—the mathematician and the mystic? Newton was a veritable "last of the Sumerians, a mystical restorer of esoteric pagan wisdom" (19), who spent more time "bent over a steaming crucible in search of the philosopher's stone" (104) than he did calculating planetary orbits. It is tempting for us to ask what drove such a brilliant scientific mind to such an obsession with alchemy and the hermeneutics of ancient religious texts.

But perhaps we are asking the wrong question. Perhaps the anomaly to be explained is not Newton's mysticism but his mathematics. "Newton's interests in alchemy, chronology, biblical prophecies and Church history ... would not have seemed unusual to his contemporaries" (20). "Prestigious status [was] conferred on practitioners in such ... erudite field[s]" (190). Mathematics, on the other hand, was treated with some suspicion. It was dangerously close to having "atheistic consequences" insofar as reducing the physical world to "inescapable rules" left creation "independent of the providential action of God" (40). Its claim to absolute certainty was also undesirable: "Many natural philosophers in the Royal Society wished to allay any fears that they might propose 'unquestionable' or 'dogmatic' conclusions. Politically opinionated philosophers or dogmatic theologians were not admitted in the society, which instead promoted innocuous, moderate scepticism. ... Any discourse presuming to certainty was looked upon with suspicion." (83-84)

Indeed, Newton's interest in esoteric and religious subjects seems to have been very genuine, whereas he ended up in mathematics almost involuntarily, "not due to any choice on his part but to the fact that his mind was extraordinarily equipped for mathematical inventiveness" (35).

Once in mathematics, Newton let it dictate the lead in all his work to an unprecedented extent. He dismissed qualitative and philosophical modes of doing science, deriding such speculations as "no difficult matter" (88). Instead he held that "mathematical regularities deduced with certainty from phenomena" (88) were sufficient in themselves, even though this meant his science was "devoid of physical meaning" (8) in the eyes of many contemporaries. Similarly, the deeply mathematical Principia was largely "foreign ... to the [Royal] Society, whose members espoused the Baconian ideal of an experimental philosophy accessible for public discussion and beneficial to human welfare" (153).

"Philosophy was for Newton a necessity rather than a vocation, a defensive strategy rather than a chosen line of research." (180) When he did engage in it, his method was to extrapolate philosophical conclusions from mathematical results. The material nature and causal structure of e.g. planetary motion should not be sought in philosophical reflections but in mathematical results.

For example, gravity "operates not according to the quantity of the surfaces of the particles upon which it acts (as mechanical causes use to do) but according to the quantity of the solid matter which they contain" (210), and hence the vortex theory of planetary motion should be dismissed. Thus a metaphysical conclusion is drawn as a corollary of a mathematical result—a form of argument giving mathematics a priority over other fields that few of Newton's contemporaries would have granted. Similarly, "Newton's defence of absolute space and time is based on his conviction that these notions are presupposed by good scientific practice" (166), that is to say, the mathematical theory demands them and this is reason enough to postulate them—in contrast to people like Descartes and Leibniz who found this abhorrent on purely epistemological grounds.

Mathematics affords religious conclusions as well. The stable trajectories of the solar system are so unlikely to have arisen by chance that they must be attributed to "an intelligent agent ... very well skilled in Mechanics and Geometry" (200), who furthermore "intervenes with a 'continuous miracle' to stop the stars from falling ... onto each other" (202).

The history of ancient civilisations is another field where conventional arguments and evidence must yield to mathematics. "Newton brought to chronology methodological innovations that depended upon his expertise in natural philosophy. ... [He] received sharp criticisms from the humanists ..., who thought that the discipline should be founded on antiquarian and textual evidence. Newton instead resorted to a complex argument in which the hermeneutics of classical sources were mingled with astronomical calculations on the precession of the equinoxes." (190)

Newton's somewhat elitist emphasis on mathematics as the subject to rule them all, and readiness to radically dismiss much contemporary work on the basis of this principle, fit well with his personality and beliefs.

Newton was "convinced that he was part of a circle of chosen ones" (130) and had little respect for those who were not. He kept much of his work secret—an "extremely peculiar approach to the publication and promotion of his ideas" (20). Attempts to engage with others usually led to "violent dispute" (195), which Newton approached with "a fierce, polemical vehemence" (136). He was convinced most people lived in sin and delusional beliefs. He adhered to a heretical religious faith, "which, if it had become public, would have completely compromised his academic and political career" (28). "Manuscripts survive in which Newton explains the rules for maximizing concentration in research and avoiding the temptations of worldly pleasures which he then lists in ... detail ... bordering on the pornographic." (34) "As far as we know, Newton remained a virgin" all his life (33).

"The position of professor allowed him to devote himself to research, almost undisturbed" for much of his life (76), his teaching being "perhaps almost non-existent" (79). His misanthropic tendencies were no less marked when he became a more public figure. "During his long presidency" he ruled the Royal Society "as an absolute monarch" (187-188), for instance "forcibly" confiscating the records of the Royal Observatory when the Astronomer Royal did not collaborate to his liking (187). He took "a morbid pleasure in sending counterfeiters to the gallows" (185) when his appointment at the Mint put him in a position to do so.

Newton's dislike of his fellow man was accompanied by great respect for ancient wisdom (one is tempted to call it an "equal and opposite" force in the manner of his third law). In mathematics, he had "a critical attitude, bordering on contempt, with regard to the algebraic methods of the 'Moderns'," but "nothing short of veneration" for "the 'Ancients'" (52). In religion, he was convinced that "corrupters ... had 'twisted the meaning' of Holy Writ" (125). The Ancients "knew truths that concerned not only God ... but Creation itself," including "astronomical and physical knowledge that Newton tried to 'restore'" (137). "This ancient wisdom ... was kept hidden from the common people through an esoteric language" (138). For example, "ancient Greek-Roman mythology concealed instructions for alchemical recipes," according to an allegory in which "the gods were associated with the planets, [and] the planets with metals" (114).

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§ 6.7. 18th century

Colin Maclaurin, *Collected Letters of Colin Maclaurin [1698–1746]*, Birkhauser, 1982.

"I am satisfied that the interests of true Science and true Religion are united, & that they do real prejudice to Mankind who endeavour to represent them as opposite in any measure." (427) "I believe it will be easily granted by all who are acquainted with the History of Learning that there is no other order or Class of Learned Men that has produced fewer writers on the side of Infidelity, or fewer adversarys to natural or revealed Religion than that of the Mathematicians. The greatest Men among them have distinguished themselves as firm in the belief, and ornaments to the practice of Christianity." (426)

George Sarton, Laplace's Religion, *Isis*, 33(3), 1941, 309–312.

It has often been told that Napoleon asked Laplace why he had not mentioned God in his treatise on celestial mechanics, and that Laplace answered "Sire, I did not need that hypothesis." I do not know when and where that story originated, for it is not found in the biographies of the great mathematician. ... It is clear that General Bonaparte was interested in the Mecanique celeste. ... Laplace ... had been his examiner at the Ecole militaire. ... It is not impossible that he teased its author with that famous query and that Laplace answered it as tradition has it. That is plausible enough, and characteristic of both men, but only if the anecdote is properly interpreted. ...

[Laplace's] answer was a subtle way of rebuking a cynical question, so subtle that [the revolutionary authorities who were overseeing the executions of many religious figures] could not take offence at it and could even misunderstand it [as a disavowal of religion]. ...

We are well acquainted with Laplace's religion. He was a conventional Catholic, taking his religion for granted. ... [A letter he wrote to his son reads:] "May God watch over you. Keep Him always in mind."

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§ 6.8. Biological experiments

MARJORIE HOPE NICOLSON, *Pepys' Diary and the new science*, University Press of Virginia, 1965.

The Royal Society in London was a center of experimental science in the 17th century. One theme was to study life by mechanical means. It was discovered that the new vacuum pump, for instance, could be used to suffocate animals. "Out of the glasse the Ayre being screwed, Pusse dyed and ne're so much as mewed" (117)—except that "we could not quite kill her, with such a way" for "the air being in upon her revives her immediately" (65). Another example is "that notable experiment of opening a dog, and laying bare his lungs, and blowing into him with bellows, keeping him thus alive as long as we pleased" (66).

"Much against my will staid out the whole church ... but I did entertain myself with my perspective glass up and down the church, by which I had the great pleasure of seeing and gazing at a great many very fine women; and with that, and sleeping, I passed away the time till sermon was done." (22-23, from the diary of Samuel Pepys, later president of the Royal Society)

MARIE BOAS HALL, *Promoting Experimental Learning: Experiment and the Royal Society*, Cambridge University Press, 2002.

The Royal Society experimented with blood transfusions in the 1660s. At first "dogs were used; the donor was bled until it died, whereupon the recipient was sewn up and when set free showed itself very lively." A flurry of inter-species blood transfusions followed, with enough success that a human subject was found who agreed to have his blood replaced with that of a sheep. "The subject ... not only survived being given the blood of a sheep, but two months later he read a paper to the Society describing the effects which he had experienced."

WALTER GRATZER (ED.), *Bedside Nature 1869-1953: Genius and Eccentricity in Science*, W.H. Freeman, 1999.

"The Influence of a Tuning-Fork on the Garden Spider." "Last autumn, while watching some spiders spinning their beautiful geometrical webs, it occurred to me to try what effect a tuning-fork would have upon them. On sounding an A fork and lightly touching with it any leaf or other support of the web or any portion of the web itself I found that the spider, if at the centre of the web, rapidly slews round so as to face the direction of the fork, feeling with its fore feet along which radial thread the vibration travels. Having become satisfied on this point, it next darts along that thread till it reaches either the fork itself or a junction of two or more threads the right one of which it instantly determines as before. If the fork is not removed when the spider has arrived it seems to have the same charm as any fly for the spider seizes it, embraces it, and runs about on the legs of the fork as often as it is made to sound never seeming to learn by experience that other things may buzz besides its natural food.

If the spider is not at the centre of the web at the time that the fork is applied, it cannot tell which way to go until it has been to the centre to ascertain which radial thread is vibrating ... The spider never leaves the centre of the web without a thread along which to travel back. If after enticing a spider out we cut this thread with a pair of scissors the spider seems to be unable to get back without doing considerable damage to the web generally, gumming together the sticky parallel threads in groups of three and four.

By means of a tuning-fork a spider may be made to eat what it would otherwise avoid. I took a fly that had been drowned in paraffin and put it into a spider's web and then attracted the spider by touching the fly with a fork. When the spider had come to the conclusion that it was not suitable food and was leaving it, I touched the fly again. This had the same effect as before, and as often as the spider began to leave the fly I again touched it and by this means compelled the spider to eat a large portion of the fly.

The supposed fondness of spiders for music must surely have some connection with these observations." (63)

"Suicide of a Scorpion." "One morning a servant brought to me a very large specimen of this scorpion ['the common Black Scorpion of Southern India'], which, having stayed out too long in its nocturnal rambles, had apparently got bewildered at daybreak, and been unable to find its way home. To keep it safe, the creature was at once put into a glazed entomological case. Having few leisure moments in the course of forenoon, I thought I would see how the prisoner was getting on, and to have a better view of it the case was placed in a window, in the rays of a hot sun. The light and heat seemed to irritate it very much, and this recalled to my mind a story I had read somewhere, that a scorpion, on being surrounded with fire, had committed suicide. I hesitated about subjecting my pet to such terrible ordeal, but taking a common botanical lens, I focused the rays of the sun its back. The moment this was done it began to run hurriedly about the case, hissing and spitting in a very fierce way. This experiment was repeated some four or five times with like results, but on trying it once again, the scorpion turned up its tail and plunged the sting, quick as lightning, into its own back. The infliction of the wound was followed by a sudden escape of fluid, and a friend standing by me called out, 'See, it has stung itself; it is dead'; and sure enough in less than half a minute life was quite extinct. I have written this brief notice show (1) That animals may commit suicide; (2) That the poison of certain animals may be destructive to themselves." (41)

J. L. HEILBRON, *Elements of Early Modern Physics*, University of California Press, 1982.

In 1703, when Newton became its president, the Royal Society "wished to revive the healthy old custom, then disused for a generation, of showing experiments at the weekly meetings. The custom had been neglected not from disinterest or poverty but because it demanded a continual inventiveness that ultimately emptied even the ablest. To arouse the interest and stimulate the thinking of the heterogeneous fellowship required demonstration luciferous in philosophy, useful in art, ingenious in contrivance, and surprising and amusing in execution." (168). Similarly, electricity was taken up by "playful German professors" (179) who liked to "kill flies with sparks from their fingers" (177).

Soon the discovery of the Leyden jar enabled scientists to administer much more powerful shocks, which they immediately tried on themselves. Musschenbroek wrote to his friends at the Paris Academy explaining "how they too could blast themselves with electricity." "'I thought I was done for,'" he wrote, "adding precise directions for realizing the 'terrible experiment' and advice not to try it." The academicians could of course not resist "blasting themselves" right away and "reported bleedings, temporary paralysis, concussions, convolutions, and dizziness," with one person even "warning that his wife was unable to walk after he used her to shorten a Layden jar." (184)

"Science is a social enterprise. Let a gentleman hold the jar and a lady the PC; both feel the shock when they touch. How many others can be inserted in the train? Academician L. G. Le Monnier tried 140 courtiers, before the king; Nollet shocked 180 gendarmes in the same presence, and over 200 Cistercians in their monastery in Paris. 'It is singular to see the multitude of different gestures, and to hear the instantaneous exclamations of those surprised by the shock.' Only persons in the train felt the commotion; those in side chains branching from the main line felt nothing. Thus electricians discovered that the discharge—to use the word they introduced for the climax of the Leyden experiment—goes preferentially along the best conducting circuit … In one demonstration only those at the extremes of the chain felt the shock, which appeared to avoid one of the company suspected 'of not possessing everything that constitutes the distinctive character of a man.' Some wits deduced that eunuchs cannot be electrified [but] three of the king's musicians, 'whose state was not equivocal,' held hands, and jumped as other men. The shy shock was found to occur only when the train stood on moist ground; apparently the discharge went through the arms and legs of the extreme members and completed its course in the soil." (186)

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§ 6.9. Podcasts

VIKTOR BLÅSJÖ, *Opinionated History of Mathematics*, podcast. See also the corresponding sections of Viktor Blåsjö, *Galileo, ignoramus: Mathematics versus Philosophy in the Scientific Revolution*, arXiv:2102.06595.

"Galileo and the Church"

§ 7. Newtonian mechanics

§ 7.1. Discussion questions

7.1. Explain Newton's proof of the law of equal areas. Which of Newton's laws are used in this proof?

Assumes inertia and central force, but does not assume specific strength of this central force (i.e., inverse square law).

7.2. Explain Newton's moon test. Which of Newton's laws are used in this argument?

Assumes inverse square law, in addition to the above.

7.3. Why should we believe Newton's laws?

They account for numerous phenomena in a unified way, including "Galilean" terrestrial physics and Kepler's law of astronomy for the solar system and planet-moon systems. The "moon test" confirms the unity of terrestrial and celestial domains.

7.4. Why was Newtonian mechanics so abhorrent to physicists in the Cartesian tradition? Why did these physicists refuse to change their principles despite the success of Newtonian mechanics?

In the Cartesian tradition, starting principles of science should be intuitive, undoubtable, elemental. Newton's gravitational law of action at a distance in not like that. It can be arrived at only after the fact, by analysing many specific facts. Newton's system works for calculations, but does it provide scientific understanding? Not if this demands that everything be deduced from simple principles (as Descartes and Leibniz thought it should be; as geometry is).

7.5. How did Newton address the concerns raised by the Cartesian tradition?

Philosophically: Analytic method. "Meddle not" with causes. Scientifically: Planetary motion follows exact mathematical laws forever; not compatible with resistance. Pendulum slows almost as fast as in air, suggesting aether; but this happens whether the bob is hollow or dense with whatever matter, which goes against aether theory.

7.6. Newtonian mechanics flew in the face of the mechanical philosophy, as expressed for example by Boyle and Galileo. Discuss.

Action at a distance is a mysterious concept. There is nothing like it in our everyday experience and it cannot be verified by experiment. Eliminating forces with those characteristics from science was precisely what people like Boyle and Galileo wanted to do, with good reason.

7.7. Several key laws in Newtonian mechanics amount to tautologies or definitions in disguise rather than statements with empirical content. Discuss.

Examples: law of inertia, law of freely falling objects, $F = ma$. Consider what an experiment disproving such a law would look like. In fact, the laws are consistent with any observational data. Supposed refuting examples are readily interpreted in accordance with the law.

7.8. Newton's laws are not deduced from the phenomena but are rather logically inconsistent with the very phenomena they are purportedly based upon. Discuss.

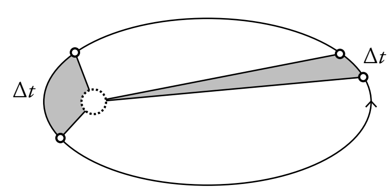
Technically speaking, yes, but only in marginal ways (e.g., the sun is not exactly stationary since it is being pulled a bit by the planets, but this is a very small effect).

7.9. What was the impact of Newtonian mechanics on philosophy of science in the 18th century?

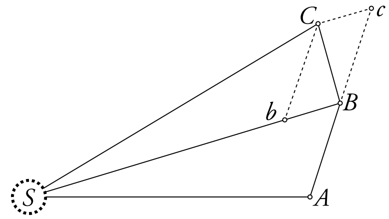
Distrust of Cartesian-style philosophical systems and the use of such reasoning in the sciences. Less speculation and "hypotheses." Search for a final system seen as dogmatic, hubristic. Instead more focus on starting with analysis of data and inductively forming laws from there. Seen as more humble, open-minded, suited for gradual progress.

§ 7.2. The law of equal areas

Isaac Newton's *Philosophiae Naturalis Principia Mathematica* (1687) is arguably the most important scientific work of all time. The very first proposition in this work is Kepler's law of equal areas. The law says that planets sweep out equal areas in equal times:



Newton's proof goes as follows.



In an infinitely small period of time the planet has moved from A to B. If we let an equal amount of time pass again then the planet would continue to c if it was not for the gravity of the sun, which intervenes and deflects the planet to C. Since the time it takes for the planet to move from B to C is infinitely small, the gravitational pull has no time to change direction from its initial direction BS, thus causing cC to be parallel to BS. It follows that triangles SAB, SBc, SBC all have the same area. In other words, the area covered in the first instance of time is equal to the area covered in the second, equal instance of time. Hence the speed at which the planets sweeps out area remains constant.

This argument is further explained in the video `https://www.youtube.com/watch?v=M4gCrEhFMB8`.

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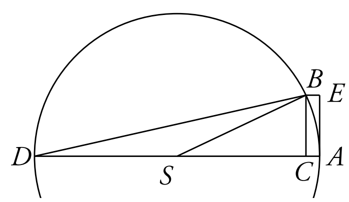
§ 7.3. The moon test

The moon is kept in its orbit by the earth's gravitational pull, or so your high school textbook told you. How do you know that it is really so? How do you know that the moon is not towed about by a bunch of angels? This question doesn't seem to arise in today's authoritarian classrooms, but Newton gave an excellent answer if anyone is interested.

"That force by which the moon is held back in its orbit is that very force which we usually call 'gravity'," says Newton (*Principia*, Book III, Prop. IV). And his proof goes like this. Consider the hypothetical scenario that "the moon be supposed to be deprived of all motion and dropped, so as to descend towards the earth." If we knew how far the moon would fall in, say, one second, then we could compare its fall to that of an ordinary object such as an apple. Ignoring air resistance, the two should fall equally far if dropped from the same height.

Of course we cannot actually drop the moon, but we can deduce what would happen if we did. Here is a picture of the moon's orbit, with the earth in the center:



Suppose the moon moves from A to B along a circle with center S in an infinitely small interval of time. If there were no gravity the moon would have moved along the tangent to the circle to some point E instead of to B (BE is parallel to ASD because the time interval is infinitely small so gravity has no time to change direction). We see that ABC and ABD are similar triangles. Thus $AC/AB = AB/AD$, or in other words, (diameter of the orbit)/(arc)=(arc)/(distance fallen). The only unknown in this equation is the sought quantity: how far the moon falls in one second. Adjusting for gravity, we can compute how far the moon would fall if dropped at the surface of the earth, where gravity is 60^2 times stronger since the moon is 60 earth radii away. We can compare the result with the fall of an apple, and we find that the moon and the apple indeed fall the same distance.

This argument is further explained in the video `https://www.youtube.com/watch?v=gJhn7WmXWVY`.

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§ 7.4. Principia

ISAAC NEWTON, *Philosophiae Naturalis Principia Mathematica*, 1687, translated by Andrew Motte.

All the difficulty of philosophy seems to consist in this—from the phaenomena of motions to investigate the forces of nature, and then from these forces to demonstrate the other phaenomena. (Preface)

BOOK I.

LAW I. Every body perseveres in its state of rest, or of uniform motion in a right line, unless it is compelled to change that state by forces impressed thereon.

LAW II. The alteration of motion is ever proportional to the motive force impressed; and is made in the direction of the right line in which that force is impressed.

LAW III. To every action there is always opposed an equal reaction: or the mutual actions of two bodies upon each other are always equal, and directed to contrary parts.

PROPOSITION I. THEOREM I. [Kepler's law of equal areas.] The areas, which revolving bodies describe by radii drawn to an immovable centre of force ... are proportional to the times in which they are described.

PROPOSITION XI. PROBLEM VI. [Kepler's law of ellipses implies inverse square law of gravity.] If a body revolves in an ellipsis; it is required to find the law of the centripetal force tending to the focus of the ellipsis. ... The centripetal force is reciprocally ... in the duplicate ratio of the distance.

PROPOSITION XIV. THEOREM VI–VII. [Kepler's third law stating how orbital period T and orbital radius R are related: namely R^3/T^2 is the same for all orbiting bodies subject to inverse-square attraction toward a central body.] If several bodies revolve about one common centre, and the centripetal force is reciprocally in the duplicate ratio of the distance of places from the centre; ... I say, that the periodic times in ellipses are in the sesquiplicate ratio of their greater axes.

BOOK III.

RULE I. We are to admit no more causes of natural things than such as are both true and sufficient to explain their appearances.

RULE II. Therefore to the same natural effects we must, as far as possible, assign the same causes.

RULE IV. In experimental philosophy we are to look upon propositions collected by general induction from phaenomena as accurately or very nearly true, notwithstanding any contrary hypotheses that may be imagined, till such time as other phaenomena occur, by which they may either be made more accurate, or liable to exceptions.

This rule we must follow, that the argument of induction may not be evaded by hypotheses.

PHAENOMENON I. [Jupiter's moons satisfy Kepler's laws of area and orbital time.] That the circumjovial planets, by radii drawn to Jupiter's centre, describe areas proportional to the times of description; and that their periodic times, the fixed stars being at rest, are in the sesquiplicate proportion of their distances from, its centre.

PHAENOMENON II. [Saturn's moons satisfy Kepler's laws of area and orbital time.] That the circumsaturnal planets, by radii drawn to Saturn's centre, describe areas proportional to the times of description; and that their periodic times, the fixed stars being at rest, are in the sesquiplicate proportion of their distances from its centre.

PHAENOMENON III. That the five primary planets, Mercury, Venus, Mars, Jupiter, and Saturn, with their several orbits, encompass the sun.

PHAENOMENON IV. [Solar system satisfies Kepler's law of orbital time.] That the fixed stars being at rest, the periodic times of the five primary planets, and (whether of the sun, about the earth, or) of the earth about the sun, are in the sesquiplicate proportion of their mean distances from the sun.

PHAENOMENON V. [Solar system satisfies Kepler's law of area.] That the primary planets, by radii drawn to the earth, describe areas no wise proportional to the times; but that the areas which they describe by radii drawn to the sun are proportional to the times of description.

PHAENOMENON VI. [The moon satisfies Kepler's law of area.] That the moon, by a radius drawn to the earth's centre, describes an area proportional to the time of description.

PROPOSITIONS. [Book I shows that Newton's Laws 1–3 and the inverse square law of gravity fits all the phenomena. The Rules of Book III imply that we should accept these laws as established from the phenomena.]

GENERAL SCHOLIUM. ... We have explained the phenomena of the heavens ... by the power of gravity, but have not yet assigned the cause of this power. ... I have not been able to discover the cause of those properties of gravity from phaenomena, and I frame no hypotheses; for whatever is not deduced from the phaenomena is to be called an hypothesis; and hypotheses, whether metaphysical or physical, whether of occult qualities or mechanical, have no place in experimental philosophy. In this philosophy particular propositions are inferred from the phenomena, and afterwards rendered general by induction. ... To us it is enough that gravity does really exist, and act according to the laws which we have explained, and abundantly serves to account for all the motions of the celestial bodies.

RENÉ DESCARTES, *Principles of Philosophy*, 1644, translated by R. P. Miller, Springer, 1982.

"One must begin by searching for … first causes, that is, for Principles [which] must be so clear and so evident that the human mind cannot doubt of their truth when it attentively considers them … And then, one must attempt to deduce from these Principles the knowledge of the things which depend upon them, in such a way that there is nothing in the whole sequence of deductions which one makes from them which is not very manifest." (xvii-xviii)

"Whoever is searching for truth must, once in his life, doubt all things." (I.1)

"We can indeed easily suppose that there is no God, no heaven, no material bodies; and yet even that we ourselves have no hands, or feet, in short, no body; yet we do not on that account suppose that we, who are thinking such things, are nothing: for it is contradictory for us to believe that that which thinks, at the very time when it is thinking, does not exist. And, accordingly, this knowledge, *I think, therefore I am*, is the first and most certain to be acquired by and present itself to anyone who is philosophizing in correct order." (I.7)

"The knowledge of remaining things depend on a knowledge of God," because the next things the mind feels certain of are basic mathematical facts, but it cannot trust these judgments unless it knows that its creator is not deceitful. Thus "the mind … discovers [in itself] certain common notions, and forms various proofs from these; and as long as it is concentrating on these proofs it is entirely convinced that they are true. Thus, for example, the mind has in itself the ideas of numbers and figures, and also has among its common notions, *that if equals are added to equals, the results will be equal*, and other similar ones; from which it is easily proved that the three angles of a triangle are equal to two right angles, etc." But the mind "does not yet know whether it was perhaps created of such a nature that it errs even in those things which appear most evident to it." Therefore "the mind sees that it rightly doubts such things, and cannot have any certain knowledge until it has come to know the author of its origin." (I.13)

The existence of God is established to Descartes's satisfaction by several arguments, most notably the following. "Just as, for example, the mind is entirely convinced that a triangle has three angles which are equal to two right angles, because it perceives that the fact that its three angles equal two right angles is necessarily contained in the idea of a triangle; so, solely because it perceives that necessary and eternal existence is contained in the idea of a supremely perfect being, the mind must clearly conclude that a supremely perfect being exists." (I.14) And all the more since it is "very well know from [our] natural enlightenment" "that that which is more perfect is not produced by an efficient and total cause which is less perfect; and moreover that there cannot be in us the idea or image of anything, of which there does not exist somewhere (either in us or outside us), some Original, which truly contains all its perfections. And because we in no way find in ourselves those supreme perfections of which we have the idea; from that fact alone we rightly conclude that they exist, or certainly once existed, in something different from us; that is, in God." (I.18)

"It follows from this that all the things which we clearly perceive are true, and that the doubts previously listed are removed" (I.30), since "God is not the cause of errors," owing to his perfection, seeing as "the will to deceive certainly never proceeds from anything other than malice, or fear, or weakness; and, consequently, cannot occur in God." (I.29) "Thus, Mathematical truths must no longer be mistrusted by us, since they are most manifest." (I.30)

In the same way we can be sure that material objects exist, since otherwise "it would be impossible to devise any reason for not thinking Him a deceiver" (II.1). But the argument forces upon us the restriction "that the nature of body does not consist in weight, hardness, color, or other similar properties; but in extension alone" (II.4), since a body can easily be conceived to be deprived of its secondary properties (cf. also II.11), but not its extension.

Physics, therefore, must be based on a theory of extended matter and nothing else. Two key characteristics of Cartesian physics follow quite naturally from this starting point, and are indeed introduced almost immediately: relativity of space (II.13-14) and contact mechanics (II.36-52).

The first is a quite unavoidable corollary of Descartes's starting point, since his perspective does not admit the possibility of space as a concept separate from body. Thus he is compelled to argue that "the names 'place' or 'space' do not signify a thing different from the body which is said to be in the place; but only designate its size, shape and situation among other bodies" (II.13). "So when we say that a thing is in a certain place, we understand only that it is in a certain situation in relation to other things" (II.14).

A second rather straightforward consequence of Descartes's starting point is that contact mechanics is the fundamental phenomena in terms of which all other physics must be construed. And indeed Descartes offers a detailed account of contact

	Descartes, Leibniz Continental rationalism	Newton British empiricism
The search for knowledge starts with …	intuitively clear primitive notions	the rich diversity of phenomena
… and consists in …	deducing the diversity of phenomena from them.	reducing them to a few simple principles.
Intrinsic justification of the axiomatic principles is …	immediate by their intuitive nature	external to the matter at hand
… and is therefore …	the crucial epistemological cornerstone of the entire enterprise.	of secondary importance at best.
In the case of physics, the axiomatic principles are …	the laws of contact mechanics	Newton's three force laws and the law of gravity
… which are established by means of …	their intuitively immediate nature.	induction from the phenomena.
This is the method of …	synthesis.	analysis.

Table 1: From Blåsjö, 206.

mechanics almost at once, in II.36-52.

VIKTOR BLÅSJÖ, *Transcendental curves in the Leibnizian calculus*, Elsevier, 2017.

"The defining characteristic of Cartesian physics is its insistence on explaining everything in terms of contact mechanics. Leibniz agreed completely: 'A body is never moved naturally except by another body that touches and pushes it … Any other kind of operation on bodies in either miraculous or imaginary.' Whence his famous conflict with Newton on the nature of gravity. Leibniz condemns very fiercely the notion of gravity as a primitive cause: 'I maintain that the attraction of bodies, properly called, is a miraculous thing, since it cannot be explained by the nature of bodies.'" (58)

ARTUR DONOVAN, LARRY LAUDAN, RACHEL LAUDAN (EDS.), *Scrutinizing Science: Empirical Studies of Scientific Change*, Kluwer, 1988.

The Copernican hypothesis generated a new problem for astronomy: if the earth is one planet among many, planetary motions could no longer be explained by imputing a natural, circular motion to celestial bodies. Descartes solved this problem by postulating fluid vortices … [that] provided a mechanism for planetary motion. … Descartes' theory was wonderfully successful in accounting for the gross phenomena of a heliocentric system; among others, the vortex theory explained the appearance and disappearance of comets, the motions of the planets in approximately circular orbits, and the movement of all the planets in the same plane and in the same direction around the sun. (87)

The problem that Newton encountered [in the 1680s] is simple and obvious in retrospect but one that proved insurmountable within the context of the mechanical philosophy: the observed area law of Kepler should not fit so closely with the exact area law derived mathematically by Newton if the heavens are filled with a retarding medium. Unless the medium is somehow disposed to move with exactly the same variable speed that the planetary body exhibits, the planet should encounter enough resistance from the medium to cause an observed deviation from the mathematical prediction, just as projectiles in the terrestrial atmosphere are observed to deviate from mathematical prediction.

Newton's realization that no form of the hypothetical gravitational aether of mechanical philosophy could be reconciled with actual celestial motions must have been rather a shock to him. From the time of his introduction to mechanical philosophy in the 1660s until the early autumn of 1684 he left an extensive record of his aethereal speculations. … All involved the impact or pressure of fine particles of matter imperceptible to the senses. … Newton marshalled such empirical evidence as he could for the existence of the aether. He cited the Boylean calcination experiments in which metals gained weight when heated in hermetically sealed glasses. "It is clear", Newton said, "that the increase is from a most subtle saline spirit" that came through the pores of the glass. He cited pendulum experiments in vacuo in which the motion of the bob was damped almost as quickly as in air. He cited the case of iron filings arranged in curved lines by a loadstone: "I believe everyone who sees [that] … will acknowledge that these magnetic effluvia are of this [aethereal] kind." (72)

Newton had understood the in vacuo pendulum experiments to indicate the presence of aether within the glass even after the air was removed. By definition, aether had the ability to penetrate the pores of glass and all other terrestrial bodies. Therefore, the pendulum motion "ought not to cease unless, when the air is exhausted, there remains in the glass something much more subtle which damps the motion of the bob". For Newton that experiment had indicated the presence of a gravitational aether that had acted in conjunction with the air to slow the motion of the bob when air was present and had acted almost as effectively to damp the motion when air was absent. But the experiment had only been designed to separate the effects of air and aether; with the very presence of the postulated aether in question Newton needed further evidence. Was there any way he could experimentally justify the hypothesis that a gravitational aether remained within the evacuated container? Was there a property of the aether that could be detected by its varying reactions under experimentally varied conditions?

The answer was yes. Since, by the hypothesis, aether penetrated not only the pores of the glass container but also every other terrestrial body, it should interact with the internal parts of the bodies it permeated. By holding constant the retardation due to the presence of air (presumed to act only on the surface of the pendulum bob) and varying the quantity of matter within the interior of the bob (where the aether alone was presumed to interact with particles of matter), one should expect an increase in aethereal retardation with an increasing quantity of matter. If, that is, the gravitational aether really existed. Newton reported just such a set of experiments in the Principia, in which he had first used an empty box as a bob, then filled the same box with lead or another metal. The total resistance experienced by the empty box was to the total resistance experienced by the full box as 77 to 78; therefore the increased quantity of matter made very little if any difference, certainly not enough to justify the existence of the gravitational aether. (74)

"I think that the resistance of pure aether is either non-existent or extremely small. … Comets … are indifferently carried through all parts of our heaven with an immense velocity, and yet they do not lose their tails nor the vapour surrounding their heads, which the resistance of the aether would impede and tear away. Planets will persevere in their motion for thousands of years, so far are they from experiencing any resistance." (Newton, 75–76)

"If there were any aerial or aetherial space of such a kind that it yielded without any resistance to the motions of comets or any other projectiles I should believe that it was utterly void. For it is impossible that a corporeal fluid should not impede the motion of bodies passing through it, assuming that … it is not disposed to move at the same speed as the body." (Newton, 78)

ISAAC NEWTON, *Philosophiae Naturalis Principia Mathematica*, 1687, translated by Andrew Motte.

GENERAL SCHOLIUM. … The hypothesis of vortices is pressed with many difficulties. … The motions of the comets are exceedingly regular, are governed by the same laws with the motions of the planets, and can by no means be accounted for by the hypothesis of vortices; for comets are carried with very eccentric motions through all parts of the heavens indifferently, with a freedom that is incompatible with the notion of a vortex.

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§ 7.6. Path to discovery

ZEV BECHLER (ED.), *Contemporary Newtonian Research*, Reidel, 1982.

Newton's breakthroughs in physics came quite late in his life. For a long time he "took a Cartesian track" (116), unproductively. In 1679 Hooke wrote to Newton for help with the mathematical aspects of his hypothesis "of compounding the celestiall motions of the planetts of a direct motion by the tangent & an attractive motion towards the centrall body" (36). But "Newton was still mired in very confusing older notions" (35) and wrote a reply with a rather basic error in it. To get Newton going Hooke had to explicitly suggest the inverse square law and plead that "I doubt not but that by your excellent method you will easily find out what that Curve [the orbit] must be" (37). Only then "Newton quickly broke through to dynamical enlightenment … following [Hooke's] signposted track" (117).

The experimental aspect of Newton's work on mechanics did not go so well, and sometimes hampered rather than aided the development of his theory. The famous moon test, for example, first came out negatively (owing to a bad value for the radius of the earth), which "made Sir Isaac suspect that this Power was partly that of Cartesius's Vortices" (34).

OFER GAL, *Meanest Foundations and Nobler Superstructures: Hooke, Newton and "the Compounding of the Celestiall Motions of the Planetts"*, Springer, 2002.

Hooke wanted to "shew, that circular motion is compounded of an endeavour by a direct motion by the tangent, and of another endeavour tending to the center'" (37), thereby "compounding the celestiall motions of the planetts of a direct motion by the tangent & an attractive motion towards the central body" (2). Hooke's programme "occasioned my findings" (17) on planetary motion, Newton admitted. "It is difficult to overstate the novelty of Hooke's Programme." (ix) "For Kepler as well as Galileo, for Descartes himself, as well as for Gassendi and the Cartesians Mersenne and Huygens, … as well as for Newton's own favorite Borelli, the explication of the planetary motions had always included rotation as a primary cause." (2) The inverse square relation between gravity and distance, however, "was rather common" (9).

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§ 7.7. Philosophy

ISAAC NEWTON, *Philosophical Writings*, Cambridge Texts in the History of Philosophy, edited by Andrew Janiak, Cambridge University Press, 2004.

As in mathematics, so in natural philosophy, the investigation of difficult things by the method of analysis, ought ever to precede the method of composition. This analysis consists in making experiments and observations, and in drawing general conclusions from them by induction. … For hypotheses are not to be regarded in experimental philosophy. And although the arguing from experiments and observations by induction be no demonstration of general conclusions; yet it is the best way of arguing which the nature of things admits of. … By this way of analysis we may proceed from compounds to ingredients, and from motions to the forces producing them; and in general, from effects to their causes, and from particular causes to more general ones, till the argument end in the most general. This is the method of analysis, and the synthesis consists in assuming the causes discovered, and established as principles, and by them explaining the phenomena proceeding from them, and proving the explanations. (139)

[I] show that there is a universal gravity and that all the phenomena of the heavens are the effect of it and with the cause of gravity [I] meddle not but leave it to be found out by them that can explain it, whether mechanically or otherwise. (116) For my own part, I have so little fancy to things of this nature. (11)

Since celestial motions are more regular than if they arose from vortices and observe other laws, so much so that vortices contribute not to the regulation but the disturbance of the motions of planets and comets; and since all phenomena of the heavens and of the sea follow precisely, so far as I am aware, from nothing but gravity acting in accordance with the laws described by me; and since nature is very simple, I have myself concluded that all other causes are to be rejected and that the heavens are to be stripped as far as may be of all matter, lest the motions of planets and comets be hindered or rendered irregular. But if, meanwhile, someone explains gravity along with all its laws by the action of some subtle matter, and shows that the motion of planets and comets will not be disturbed by this matter, I shall be far from objecting. (108–109)

You sometimes speak of gravity as essential and inherent to matter. Pray do not ascribe that notion to me; for the cause of gravity is what I do not pretend to know. (100) It is inconceivable that inanimate brute matter should, without the mediation of something else, which is not material, operate upon and affect other matter without mutual contact, as it must be, if gravitation … be essential and inherent in it. … That gravity should be innate, inherent, and essential to matter, so that one body may act upon another at a distance through a vacuum without the mediation of anything else, … is to me so great an absurdity, that I believe no man who has in philosophical matters a competent faculty of thinking can ever fall into it. (102)

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§ 7.8. Philosophical perspective

PIERRE DUHEM, *The Aim and Structure of Physical Theory*, 1914, trans. Phillip Wiener, Princeton University Press, 1954.

Certain fundamental hypotheses of physical theory cannot be contradicted by any experiment, because they constitute in reality definitions, and because certain expressions in the physicist's usage take their meaning only through them.

When a heavy body falls freely, the acceleration of its fall is constant. Can such a law be contradicted by experiment? No, for it constitutes the very definition of what is meant by "falling freely." If while studying the fall of a heavy body we found that this body does not fall with uniform acceleration, we should conclude not that the stated law is false, but that the body does not fall freely, that some cause obstructs its motion, and that the deviations of the observed facts from the law as stated would serve to discover this cause and to analyze its effects.

HENRI POINCARÉ, *La Science et l'Hypothèse*, 1902, quoted from the Dover edition, *Science and Hypothesis*, 1952.

The law of inertia is not an experimental fact (91–97). "Have there ever been experiments on bodies acted on by no forces? And, if so, how did we know that no forces were acting? The usual [empirical illustration] is that of a ball rolling for a very long time on a marble table; but why do we say that it is under the action of no force?" And conversely, let's say that the ball does deviate from its path, and that we cannot find any force to blame this on. Does that falsify the law of inertia? No, it only means that we do not understand the force in question. A force was acting on the ball by definition, since force is mass times acceleration. There is no way to define force independent of $F = ma$ (97–101). Thus the law of inertia is true by definition, as is $F = ma$. But to define force as mass times acceleration we first need to know what mass is. "There is no escape" except the following definition, "which is only a confession of failure: Masses are co-efficients which it is found convenient to introduce into calculation" (103). "This convention, however, is not absolutely arbitrary; it is not the child of our caprice. We admit it because certain experiments have shown us that it will be convenient" (136).

PIERRE DUHEM, *The Aim and Structure of Physical Theory*, 1914, trans. Phillip Wiener, Princeton University Press, 1954.

Newton . . . , in the "General Scholium" which crowns his Principia, . . . rejected . . . vigorously as outside of natural philosophy any hypothesis that induction did not extract from experiment; when he asserted that in a sound physics every proposition should be drawn from phenomena and generalized by induction. . . . Did not Newton follow this method when he established the system of universal attraction, thus adding to his precepts the most magnificent of examples? Is not his theory of gravitation derived entirely from the laws which were revealed to Kepler by observation . . . ?

This first law of Kepler's, "The radial vector from the sun to a planet sweeps out an area proportional to the time during which the planet's motion is observed," did, in fact, teach Newton that each planet is constantly subjected to a force directed toward the sun.

The second law of Kepler's, "The orbit of each planet is an ellipse having the sun at one focus," taught him that the force attracting a given planet varies with the distance of this planet from the sun, and that it is in an inverse ratio to the square of this distance.

The third law of Kepler's, "The squares of the periods of revolution of the various planets are proportional to the cubes of the major axes of their orbits," showed him that different planets would, if they were brought to the same distance from the sun, undergo in relation to it attractions proportional to their respective masses.

The experimental laws established by Kepler and transformed by geometric reasoning yield all the characteristics present in the action exerted by the sun on a planet; by induction Newton generalized the result obtained; he allowed this result to express the law according to which any portion of matter acts on any other portion whatsoever, and he formulated this great principle: "Any two bodies whatsoever attract each other with a force which is proportional to the product of their masses and in inverse ratio to the square of the distance between them." The principle of universal gravitation was found, and it was obtained, without any use having been made of any fictive hypothesis, by the inductive method the plan of which Newton outlined.

Let us again examine this application of the Newtonian method, this time more closely; let us see if a somewhat strict logical analysis will leave intact the appearance of rigor and simplicity that this very summary exposition attributes to it.

In order to assure this discussion of all the clarity it needs, let us begin by recalling the following principle, familiar to all those who deal with mechanics: We cannot speak of the force which attracts a body in given circumstances before we have designated the supposedly fixed term of reference to which we relate the motion of all bodies; when we change this point of reference or term of comparison, the force representing the effect produced on the observed body by the other bodies surrounding it changes in direction and magnitude according to the rules stated by mechanics with precision. That posited, let us follow Newton's reasoning.

Newton first took the sun as the fixed point of reference; he considered the motions affecting the different planets by reference to the sun; he admitted Kepler's laws as governing these motions, and derived the following proposition: If the sun is the point of reference in relation to which all forces are compared, each planet is subjected to a force directed toward the sun, a force proportional to the mass of the planet and to the inverse square of its distance from the sun. Since the latter is taken as the reference point, it is not subject to any force.

In an analogous manner Newton studied the motion of the satellites and for each of these he chose as a fixed reference point the planet which the satellite accompanies, the earth in the case of the moon, Jupiter in the case of the masses moving around Jupiter. Laws just like Kepler's were taken as governing these motions, from which it follows that we can formulate the following proposition: If we take as a fixed reference point the planet accompanied by a satellite, this satellite is subject to a force directed toward the planet varying inversely with the square of the distance. If, as happens with Jupiter, the same planet possesses several satellites, these satellites, were they at the same distance from the planet, would be acted on by the latter with forces proportional to their respective masses. The planet is itself not acted on by the satellite.

Such, in very precise form, are the propositions which Kepler's laws of planetary motion and the extension of these laws to the motions of satellites authorize us to formulate. For these propositions Newton substituted another which may be stated as follows: Any two celestial bodies whatsoever exert on each other a force of attraction in the direction of the straight line joining them, a force proportional to the product of their masses and to the inverse square of the distance between them. This statement presupposes all motions and forces to be related to the same reference point; the latter is an ideal standard of reference which may well be conceived by the geometer but which does not characterize in an exact and concrete manner the position in the sky of any body.

Is this principle of universal gravitation merely a generalization of the two statements provided by Kepler's laws and their extension to the motion of satellites? Can induction derive it from these two statements? Not at all. In fact, not only is it more general than these two statements and unlike them, but it contradicts them. … The principle of universal gravity, very far from being derivable by generalization and induction from the observational laws of Kepler, formally contradicts these laws. If Newton's theory is correct, Kepler's laws are necessarily false.

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§ 7.9. Reception and influence

LARRY LAUDAN, *Science and Hypothesis: Historical Essays on Scientific Methodology*, Reidel, 1981.

The half-century following the publication of the Principia was marked by a growing antipathy to hypotheses and speculation. (112) Virtually every preface to major scientific works in this period included a condemnation of hypotheses and a panegyric for induction. … As a contemporary noted, "The [natural] philosophers of the present age hold hypotheses in vile esteem." Philosophers of science and epistemologists were, if anything, even more enthusiastic in their condemnation of hypothetical inference. (10)

COLIN MACLAURIN, *An Account of Sir Isaac Newton's Philosophical Discoveries*, London, 1748.

[Before Newton,] One ill-grounded maxim was imagined, to support another, and fiction was grafted upon fiction. Hypotheses were invented, nor for reducing facts or observations of a complicated nature to rules and order, (for which purpose they may be of service) but as principles of science. These were of so great authority as not to be overturned by contradictory observations, or by the extravagant consequences that arose from them; but the author, charm'd with his rhapsody, proceeded, without minding these, to the conclusion of his fable. Thus one age or sect could not but destroy, for the most part, the labour of another. (94)

They who have indulged themselves in inventing systems and compleating them, tho' they have sometimes set out in a manner that has appeared plausible, yet, in pursuing those schemes, such consequences have arisen as could not fail to disgust all but such as were intoxicated with the deceit. (95) We may also learn … from the bad success of so many fruitless attempts, to be less fond of perfect and finished schemes of natural philosophy; to be willing to stop when we find we are not in a condition to proceed farther; and to leave to posterity to make greater advances, as time and observation shall enable them. (91)

But it has appeared sufficiently, from the discoveries of … Sir Isaac Newton, that the fault has lain in the philosophers themselves, and not in philosophy. A compleat system indeed was not to be expected from one man, or one age, or perhaps from the greatest number of ages; could we have expected it from the abilities of any one man, we surely would have had it from Sir Isaac Newton: but he saw too far into nature to attempt it. (96)

PETER JIMACK, The French Enlightenment I: science, materialism and determinism, in Stuart Brown (ed.), *Routledge History of Philosophy, Volume V: British Philosophy and the Age of Enlightenment*, Routledge, 1996, Chapter 9.

The French Enlightenment is not just a convenient label devised by historians of philosophy, and the thinkers to be discussed in this chapter and the next were for the most part conscious of belonging to a movement. … On many topics …, they held broadly similar views and differed only in matters of detail. The very term 'philosophes' came to be used to designate the thinkers who held these views, and the philosophes actually saw themselves as a kind of brotherhood involved in a campaign, a group of 'frères' who shared the same attitudes and aspirations. … It has been argued that some of the principal figures of the French Enlightenment were largely gifted vulgarizers, rather than original thinkers. While this is no doubt an exaggeration, it does draw attention to the way in which they picked up and developed ideas that had been expressed by sometimes lone voices in previous centuries. …

[Voltaire's Letters on the English (1733) may be considered a marker of the beginning of the French Enlightenment.] Voltaire … introduced Locke and Newton to the French public and praised English religious toleration and political freedom, implicitly contrasting them with the very different situation in France. … The French Enlightenment … movement could be said to have been brought to a natural close by the outbreak of the French Revolution. In many ways, of course, the Revolution was the outcome of this wave of intellectual attacks on authority, though retrospectively, the fact that it occurred has inevitably affected the way the intellectual movement itself is perceived—often as more revolutionary, and particularly more specifically political, than it actually was.

If there was one work which, more than any other, embodied the ideals and attitudes of the Enlightenment, it was the Encyclopédie. … [The editors] wanted to make known to the public at large all the huge strides that had recently been made in human knowledge of every conceivable kind, and this comprehensive survey was to be written by appropriate experts in each field. …

Mistrust of … sterile metaphysical speculation about absolute reality and … emphasis on the scientific and the utilitarian were shared by many. … Thinkers convinced of the ultimate intelligibility of the universe and imbued with confidence in man's capacities to decode it had little patience with the metaphysical theories of Spinoza or Leibniz, for example, about such matters as pre-established harmony. The knowledge that interested the thinkers of the Enlightenment was not metaphysical, but scientific, knowledge of the material world of nature. … Philosophy … should not be concerned with axiomatic truths like 'the part is smaller than the whole', since they are self-evident and thus useless; nor with vain metaphysical enquiry into such matters as the nature of movement. The true philosopher sensibly supposes the existence of movement and tries to discover how it operates in practice. … Their principal inspiration in this field was undoubtedly Newton. … The lavish praise bestowed on him [in the Encyclopédie] expressed a view which was widely shared in France. …

It was above all Newton's methods which were to serve as a model for scientific investigation[:] the observation of phenomena followed by the attempt to discover the principles or laws underlying them. … Metaphysicians such as Descartes, Malebranche and Leibniz had gone astray because the systems they had proposed were not based on observation of the natural world.

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§ 7.10. Reader

ANDREW EDE & LESLEY CORMACK, *A History of Science in Society: A Reader*, Broadview Press, 2007.

5.4.1. Newton. These selections are not very good. You can skip most of it. But it is interesting to compare Newton's definitions of mass and force with the critique of these concepts in §7.8.

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§ 7.11. Podcasts

VIKTOR BLÅSJÖ, *Opinionated History of Mathematics*, podcast. See also the corresponding sections of Viktor Blåsjö, *Galileo, ignoramus: Mathematics versus Philosophy in the Scientific Revolution*, arXiv:2102.06595.

"The mathematicians' view of Galileo"

§ 8. The Chemical Revolution

§ 8.1. Discussion questions

8.1. What led to the belief that the following reactions all analogous? That is, that calcification is “slow burning”?

$$\begin{aligned} \text{wood} &\rightarrow \text{ash} \\ \text{iron} &\rightarrow \text{rust} \\ \text{metal} &\rightarrow \text{calx (powdery substance)} \end{aligned}$$

Look similar. Require heat. Metal is produced from ore in the presence of charcoal (= combustible = phlogiston-rich).

8.2. What are the basic observable facts regarding the role of air and weight in the reaction metal → calx? How do the old and new chemistry explain them?

Weight increases. Air decreases in volume. Phlogiston: Weight increase due to secondary factor (maybe some air/water sticking to the calx, like water makes sand heavier without changing its composition). Air shrinks when it absorbs phlogiston, like cotton wool absorbing water. New: Oxygen goes from air to calx.

8.3. Lavoisier’s theory explains the above reactions in a simple way from first principles. But for other phenomena it needs less elegant secondary assumptions. Example:

$$\text{calx} \rightarrow \text{metal + air that doesn't support combustion}$$

Why is this a problem? How did Lavoisier solve it?

Calx = metal + oxygen, and oxygen supports combustion. But this reaction requires charcoal which contaminates the air.

8.4. Just as Lavoisier failed to extract oxygen from the calx → metal reaction (8.3), so he failed to extract oxygen from marine acid. Compare and contrast the two cases.

Both core predictions from L’s theory. First case: L right; anomaly due to secondary factor; core theory correct. Second case: L wrong; absolutely sure theory was right, secondary factor at play, but proven wrong later.

8.5. Consider the reactions:

$$\begin{aligned} \text{metal + acid} &\rightarrow \text{salt + inflammable air} \\ \text{calx + acid} &\rightarrow \text{salt} \end{aligned}$$

What were the pros and cons for phlogistonists of saying: inflammable air = phlogiston?

Pros: Fits perfectly with calx = metal − phlogiston. Makes phlogiston concretely exhibitable, not mysterious. Cons: Airs saturated with phlogiston should prohibit burning (required to explain why burning stops in enclosed space), not burn themselves. (Flammable gas = modern hydrogen.)

8.6. If calx = metal − phlogiston (8.1), and inflammable air = phlogiston (8.5), then it would seem to follow that:

$$\text{calx + inflammable air} \rightarrow \text{metal}$$

This was dramatically confirmed by Priestley. Discuss the significance of this.

Same as pros of previous question, but now underwritten by confirmation of novel prediction.

8.7. This reaction (8.6) is actually:

$$\begin{array}{rrl} \text{calx+} & \text{inflammable air} \rightarrow & \text{metal} \\ \text{metal + oxygen+} & \text{hydrogen} \rightarrow & \text{metal + water} \end{array}$$

Why had Priestley missed this? How could it be confirmed?

Experiments with gasses need to take place in contained vessels, which typically meant domes of glass inverted over a liquid. Using mercury instead of water, the water byproduct became observable.

8.8. Lavoisier used the new theory of water to explain the reactions in 8.5. How?

Acid is part water. An intermediary reaction in the first case is metal + water = calx + hydrogen. So oxygen has left the water, leaving hydrogen, and entered the metal, making it calx. Confirmed novel prediction: this doesn’t happen with concentrated acids (less watery).

8.9. “Lavoisier inaugurated the Chemical Revolution by violating Newton’s rule banning the proliferation of hypotheses.” Discuss.

Alternative hypotheses are always possible. Permitting them means that no conclusion or principle can ever be considered established from the phenomena. Newton disproved Cartesian physics, not merely offered another alternative to a functioning theory. Lavoisier admits that the phlogiston theory explains the phenomena “in a very nice manner.”

8.10. What did it make sense for Lavoisier to postulate caloric as a part of oxygen gas? What was the problems with this view?

Caloric explains the heat/light generated in combustion in a (quasi?) material way. Caloric is what makes things gases, and what enables combustion. Problem: If caloric is what makes something gaseous, how to explain reactions that produce lots of gas and lots of heat? Or combustions of other gases not releasing heat/light? Also: Combustion without gas (e.g. gunpowder [oxygen present in solid state]).

8.11. The phlogistonists better anticipated some aspects of modern chemistry than Lavoisier. Discuss.

Energy (combustibles contain potential/chemical energy, basically = phlogiston). Also before Lavoisier re: combustion = calcification; experimental identification of oxygen, composition of water; weight analysis.

8.12. How does Lavoisier’s program relate to the atomistic and Aristotelian traditions of antiquity?

Atomism was metaphysics, not explanatory. Aristotle more pragmatic; parallels Lavoisier’s operationalist definition of elements.

8.13. How is the “Duhem thesis” relevant to the Chemical Revolution?

Phlogistonists theory not falsified by calx (= metal − phlogiston) being heavier than metal.

8.14. Why did Lavoisier win the war with the phlogistonists? What are some simplistic answers that have been given to this question, and what is wrong with them?

Lakatosian-type explanation (progressive research program). Failed explanations: inductivism, falsification takes experiments to be too unequivocal; alleged mysticism of phlogiston (some could point to phlogiston concretely and weigh it, and see end of Priestley reading; Lavoisier had caloric); simplicism ill-defined, tainted by presentism.

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§ 8.2. Scientific content

Alan Musgrave, Why did oxygen supplant phlogiston? Research programmes in the Chemical Revolution, in Colin Howson (ed.), *Method and Appraisal in the Physical Sciences*, Cambridge University Press, 1976, 181–209.

4. Phlogiston before Lavoisier.

The [phlogiston] programme originated with [the] claim that combustibles contained an ‘inflammable principle’ which they released upon burning. Since metals upon heating turned into powdery substances like ashes, it was also claimed that they too contained the ‘inflammable principle’, and that calcination was slow combustion. And since metallic ores, when heated with charcoal, turned into metals, it was said that in smelting ores we supply the ‘inflammable principle’ to them. The ‘principle of inflammability’ was thus transferred from a combustible (the charcoal) to a metal. (187)

The phlogiston programme … gave a unified explanation of the apparently distinct phenomena of combustion and calcination. But there were many anomalies. Some well-known facts about combustion were not explained: why does combustion soon cease in an enclosed volume of air, and why is the volume of air reduced by it; why won’t things burn at all in a vacuum? Worse still, other well-known facts seemed to refute the theory: why, if calcination is the release of phlogiston, do calxes weigh more than the original metals?

The first two anomalies were dealt with by adding auxiliary hypotheses. Phlogiston must be carried away from a combustible by the air, and a given volume of air can only absorb a certain amount of it. Hence nothing will burn in a vacuum, and combustion soon ceases in a confined space. As for the reduction in volume of the air, we need only suppose that air saturated with phlogiston (‘phlogisticated air’) takes up less room than ordinary air (just as cotton-wool saturated with water takes up less room than ordinary cotton-wool).

The third anomaly, the weight increase of calxes, was more troublesome, but it did not compel rejection of phlogistonism. We have here a nice example of the Duhem thesis [cf. §8.3]. … The thesis that combustion and calcination involve the release of phlogiston … alone does not entail that calcination will lead to a weight loss. … To derive such a prediction we need the following additional premisses: phlogiston has weight, nothing weighty is added to the metal as it calcinates, and if something weighty is removed in a process, and nothing weighty added, then the result will weigh less than the original. The observed weight increase contradicts the conjunction of phlogiston theory with these additional premisses. (188)

Clearly, phlogistonists had several options for accommodating the weight increase. All of them, and a few more besides, were explored. In 1772 de Morveau said that if phlogiston is lighter than air, then removing it from a body immersed in air will cause that body to weigh more. … Earlier, several phlogistonists harked back to Aristotelianism and ascribed negative weight or ‘levity’ to phlogiston. … This hypothesis of the levity of phlogiston has caused much levity among historians ever since; [many phlogistonists such as] Scheele, Priestley, Cavendish and Kirwan thought it funny too, and would have nothing to do with it. The third broad option was to say that the weight of calxes was augmented by something added to them as their phlogiston was released. … Before the phlogiston programme got underway, Boyle claimed that the weight of calxes was augmented by ‘fire particles’ which stuck to them. … Several phlogistonists took over this ready-made solution to the anomaly. Earlier still, Rey claimed that the weight increase of calxes ‘comes from the air … which … mixes with the calx … and becomes attached to its most minute particles: not otherwise than water makes heavier sand which you throw into it and agitate, by moistening and adhering to the smallest of its grains’. … Priestley incorporated Rey’s explanation into phlogistonism: the ‘air’ which is ‘precipitated’ into the calx is phlogisticated or ‘fixed air’ (together, perhaps, with some water) which has been formed by the calcination process. (189–190)

Metals [submerged] in acids [bubble and turn into] salts. In 1766 Cavendish immersed [metals] in … acids, and collected the gas given off. He found that it was eleven times lighter than common air, and highly inflammable. Cavendish concluded that when metals are immersed in acids ‘their phlogiston flies off … and forms inflammable air’. If this was correct then metallic calxes [that is, metals having been deprived of their phlogiston beforehand] immersed in acids should form the same salts, but no ‘inflammable air’ should be released. This was confirmed by experiment—a brilliant success. (190)

The identification of phlogiston with inflammable air (hydrogen) did pose a few puzzles. ‘Airs’ rich in phlogiston were supposed to *inhibit* combustion, yet pure phlogiston burns! When things burn they are supposed to *release* phlogiston, so it would seem that when phlogiston burns it is released from itself! All this made Cavendish tentative about identifying ‘inflammable air’ with phlogiston; and in 1784 he proposed a new version of phlogistonism in which this identification was denied. Yet the appeal of his 1766 version of the theory was obvious. As Kirwan put it, phlogiston ‘was no longer to be regarded as a mere hypothetical substance, since it could be exhibited in an aerial form in as great a degree of purity as any other air’. (190–191)

5. The birth of the oxygen programme.

In 1772 Lavoisier burned sulphur and phosphorus in air confined …, and noted the reduction in the volume of the air and the increase in weight of the sulphur and phosphorus. … [He concluded that:] ‘This increase of weight arises from a prodigious quantity of air that is fixed [that is, absorbed by the burning substance] during combustion. … I am persuaded that the increase in weight of metallic calxes is due to the same cause.’ (191)

But how could Lavoisier explain the fact that combustion ceases before an enclosed volume of air is ‘fixed’? He elaborates the theory further by postulating that ordinary air is composed of two very different substances, a ‘pure part’ which supports combustion and another part (‘mephitic air’) which does not. His theory now predicted that if we reduce a calx to a metal, the ‘pure part’ of the air should be released. [That is to say, the air produced this way should be very capable of supporting combustion. But this is not what happens. The air produced by such a] reduction of calxes always yielded ‘fixed air’, will not support combustion.

Undaunted, Lavoisier explains this apparent refutation away: ‘The principle which combines with metals during their calcination, which increases their weight and constitutes them in the state of a calx, is nothing other than the … purest part of the air and such that, if the air, after having engaged in a metallic combination, becomes free again, it appears in an eminently respirable state. … The majority of calxes are not to be reduced … without the immediate contact of a carbonaceous material. … The charcoal which is used is completely destroyed in this operation if it is present in suitable proportion; whence it follows that the air which is evolved in metallic reductions with carbon is not a simple substance but in some manner is the result of the combination of the elastic fluid disengaged from the metal and that disengaged from the carbon. Therefore the fact that this fluid is obtained as fixed air gives us no right to conclude that it existed in this form in the metallic calx before its combination with the carbon. These considerations showed me that in order to clear up the mystery of the reduction of metallic calxes it would be necessary to experiment with those calxes which are reducible without the addition of anything.’ (192)

The reasoning is faultless, a new chemical composition for … ‘fixed air’ is suggested, and the discovery of the ‘purest part of air’ (oxygen) is predicted. At this point it would be pleasant to relate that Lavoisier, having boldly predicted oxygen, went on to discover it. But in fact it was discovered before him by Priestley (and before Priestley by Scheele, who did not, however, publish his discovery until 1777). As Mark Twain said, ‘in real life the right thing never happens at the right place at the right time—it is the business of the historian to remedy this mistake’. (192–193)

In rationally reconstructed history, having predicted oxygen, Lavoisier went on to discover it—while in actual history it was discovered by Priestley, who had not predicted it, but who taught Lavoisier how to discover it. Not that Lavoisier did not try: he tells us that in 1774 he tried to obtain ‘the purest portion of the air’ by reducing iron calxes without charcoal, but without success. He must have been distressed to learn of Bayen’s report, in February 1774, that the red calx of mercury could be reduced by heat alone, and that *fixed air was obtained*. If correct, Bayen’s report refuted Lavoisier’s prediction. (193)

Priestley … soon found that Bayen was wrong: the air from the calx could not be ‘fixed air’, for it would not dissolve in water.

After several false trails, each of which he reports in characteristic style, Priestley decides that the air from the mercury calx is a new kind of air, which supports combustion better than ordinary air. He calls it dephlogisticated air. … Priestley's account is full of words like 'surprize' and 'astonishment'. At one point he apologises for 'the frequent repetition of the word surprize', only to continue 'the next day I was more surprized than ever I had been before'! He begins his story by saying that it illustrates how 'more is owing to … chance … than to any preconceived theory in this business'.

Before his discovery was published, Priestley visited Paris and over the dinner table told Lavoisier how he had made it. Lavoisier repeated the experiment, misinterpreted it as Priestley at one stage had also done, and came to a rather curious conclusion which conflicted with his earlier prediction. [Priestley's earlier puzzlement, thus, cannot be attributed to him having the wrong theory.] … Priestley read this first version of Lavoisier's paper, … [and sent] a critique of Lavoisier, gently correcting him. … Lavoisier read the critique, [and] promptly revised his paper …, and concluded what he had earlier predicted[:] Combustion and calcination are combinations of substances with the 'pure part of the air'; 'fixed air' is a combination of the 'pure part of the air' with charcoal; any calx reduced without charcoal will yield the 'pure part of the air'. Thus was oxygen discovered. (194)

Despite its comic aspects the discovery of oxygen was a dramatic success for the oxygen research programme. Was it an equally dramatic failure for phlogistonism? Priestley did not think so. He explained the experiment by saying that, since calxes contain 'fixed air' which contains phlogiston, when they are heated the phlogiston revives the metal and 'dephlogisticated air' is emitted. But, said Priestley, this does not always happen: 'it will be seen, in the course of my experiments, that several … [lead] calxes yield fixed air by heat only, without any addition of charcoal'. Priestley could explain this post hoc by saying that not all of the phlogiston contained in the calx is needed to revive it, so that 'phlogisticated' or 'fixed' air is still driven out. Lavoisier could not explain it; if Priestley was right, then Lavoisier was wrong. (195–196)

Lavoisier did not worry about Priestley's refutations. All he could say (though he did not, so far as I know, bother to say it) was that Priestley's calxes, and Bayen's before him, must have been impure in that they contained some source of carbon. And much later his followers showed that this was correct: lead calxes prepared by Priestley's methods often contain lead carbonates. But in 1775 experiments on the reduction of calxes told as much against Lavoisier's theory as in favour of it. Yet Lavoisier, emboldened by his successes and paying little heed to his failures, came out with his first direct attack on phlogistonism. (196)

Lavoisier's attack is an interesting one from a methodological point of view. … Lavoisier begins by admitting that the phenomena 'are explained in a very nice manner by the [phlogiston] hypothesis of Stahl'. He then says that the 'system of Stahl will be shaken to its foundations' simply by the fact that the same phenomena 'may be explained in just as natural a manner by an opposing hypothesis'. And he proceeds to explain that hypothesis. The argument is quite correct. The proliferation of a rival hypothesis to explain the same phenomena does not, of course, show the existing hypothesis to be false. But it does shake the existing hypothesis to its foundations by showing that its alleged 'foundations in the phenomena' are spurious. Newton had seen the point, and proposed a rule banning the proliferation of hypotheses, adding 'This rule we must follow, that the argument of induction may not be evaded by hypotheses.' Lavoisier inaugurated the Chemical Revolution by violating Newton's rule. (196–197)

6. The oxygen programme extended to acidity.

Having declared war on phlogiston, Lavoisier's next step was to try to conquer territory already occupied by the enemy. The most important occupied territory was that of the formation of acids. Phlogistonists had realised that when certain substances are burned, or deprived of their phlogiston, they formed acids. It was fairly obvious what Lavoisier had to say: acids are formed when certain substances combine with oxygen. (197)

Lavoisier's oxygen theory of acidity predicts that the oxidation of organic compounds should produce acids. This prediction was so well confirmed that Lavoisier could later write that since 1766 'a new field of inquiry has been opened up to chemists'. … And yet, despite its success, the oxygen theory of acidity was false. One of the best-known acids was … 'marine acid'. … Lavoisier tried and failed to extract oxygen from it. His phlogistonist opponents also tried and failed, and they never tired of pointing to the anomaly. Lavoisier was not impressed: in his *Elements of Chemistry* he insists that we 'cannot have the smallest doubt' that marine acid contains oxygen, though he later admits that he cannot extract it and reaches this indubitable conclusion 'from analogy with other acids'. Lavoisier was … wrong, as Davy showed in 1808: marine acid is hydrochloric acid, which contains no oxygen. … Phlogistonists were right about the relationship between marine acid and chlorine. … For a while Davy flirted with the idea of trying to resurrect the phlogiston programme. (198)

Priestley was having great success with the 1766 version of phlogistonism [discussed above]. This version predicted that combustibles and metals contain 'inflammable air' (now identified with phlogiston).

The most impressive experiment of all came in early 1783. If 'inflammable air' is phlogiston, then we ought to be able to reduce calxes to metals by heating them in it. Priestley focussed his burning lens on lead calxes in 'inflammable air' confined over water. Much to his delight, the calxes ran in the form of perfect lead, at the same time that the air diminished at a great rate, the water ascending within the receiver. The conclusion was plain enough: 'I could not doubt but that the calx was actually imbibing something from the air; and from its effects in making the calx into metal, it could be no other than that to which chemists had unanimously given the name of phlogiston'. This experiment was symmetrical with Lavoisier's 1772 experiment on the combustion of phosphorus, and so was Priestley's interpretation of it: the 'inflammable air' must be entering the calx and reviving it. (199)

[Priestley:] "This simple experiment seems to prove, that what we have called phlogiston is the same thing with inflammable air. My experiments are certainly inconsistent with Mr Lavoisier's supposition of there being no such thing as phlogiston, and that it is the addition of air, and not the loss of anything that converts a metal into a calx. ... I reduce [calxes] to a perfect metallic state by nothing but inflammable air, which they imbibe in toto."

Boulton exclaimed: "We have long talked of phlogiston without knowing what we talked about, but now Dr Priestley hath brought ye matter to light. We can pour that Element out of one Vessell into another, can tell how much of it by accurate measure is necessary to reduce a Calx to a Metal. ... In short, this Goddess of levity can be measured and weighed like other Matter." Wedgwood was 'quite delighted with the resurrection of poor Phlogiston, as we had been old friends & I could not so well at my time of life supply his place with another'. (200)

7. The composition of water.

Several chemists, including Priestley, had noticed that when 'inflammable air' burned a 'dew' was deposited on the walls of the vessel. All had ignored it as an irrelevant side-effect. But Cavendish, unhappy with the idea that when phlogiston burned it was released from itself, decided to investigate. ... The 'dew' turned out to be pure water. And Cavendish found that when 'inflammable air' was burned in 'dephlogisticated air', the 'airs' disappeared in the ratio two to one. Cavendish developed yet another version of phlogistonism to account for this.

Lavoisier was delighted: ... the long-sought 'oxide of inflammable air' was actually water. The outstanding anomalies facing his theory could now be solved. ... Lavoisier [could] dispose of [the] apparent refutations [of his theory by] Priestley's experiments on the reduction of calxes in 'inflammable air'. "I observe that Mr Priestley has not paid attention to one capital circumstance which takes place in this experiment, that the lead, far from increasing in weight, on the contrary diminishes by almost a twelfth: it gives up, therefore, some substance. ... Since it is ... obvious that water is a compound of inflammable air and dephlogisticated air, it is clear that Mr Priestley has formed water without suspecting it." Priestley had not noticed the water which was formed because he had done the experiment over water. A novel prediction was obvious: re-do the experiment over mercury and you will detect the water produced in it. Ironically, it was Priestley who confirmed this novel prediction in 1785. (200–201)

Was Priestley's original experiment a 'sloppy' one? Was it careless of him to confine his 'inflammable air' over water, making it impossible to detect the water produced? It was not. Nobody dreamt in 1782 that water might be produced. So why not do the experiment over water, especially since neither 'inflammable air' nor the oxygen which Priestley's opponents expected to be produced was readily soluble in water? Priestley's experimental report of 1782 was false, but that does not mean that his experiment was a sloppy one. Lavoisier was quite right to point out that Priestley had 'not paid attention' to the decrease in weight of the calxes. ... Priestley did not bother to weigh his calxes in this case because for him the qualitative result was enough to refute Lavoisier. ... The point is that Priestley had the wrong theory, not that he made sloppy experiments.

What of Priestley's other confirmations of 1766 phlogistonism, in which he extracted 'inflammable air' from combustibles? Lavoisier could explain these too: the combustibles must have contained some water which was decomposed into oxygen and 'inflammable air'. (202)

The discovery of the composition of water also enabled Lavoisier to account for the production of inflammable air when metals are dissolved in acids. The metals oxidise, decomposing water and releasing inflammable air, and the oxide combines with the acid to form a salt. Calxes, being already oxidised, simply form the salt. Metals dissolved in concentrated nitric or sulphuric acid yield no inflammable air because here water is not decomposed (this was a real anomaly for phlogistonists, who said that the 'inflammable air' came from the metal).

Finally, Lavoisier deduced a startling new prediction: that water, traditionally used to put out fires, should, since it contains oxygen, support slow combustion and yield hydrogen. Iron filings immersed in water did indeed rust and hydrogen was collected.

Encouraged by all these successes, Lavoisier came out with his second attack on phlogistonism. Again, he displays his methodological sophistication: his chief complaint against phlogistonism is that it consists of a series of *ad hoc* devices, mutually inconsistent with each other. ... "Chemists have made a vague principle of phlogiston which is not strictly defined, and which in consequence accommodates itself to every explanation into which it is pressed. Sometimes this principle is heavy and sometimes it is not; sometimes it is free fire and sometimes it is combined with the earthy elements; sometimes it passes through the pores of vessels and sometimes they are impenetrable to it." (203)

But a degenerating programme can soldier on, and phlogistonism did just that. ... Cavendish developed a new version of phlogistonism. ... Cavendish is well aware of Lavoisier's rival theory: he gives an excellent summary of it, and admits that it explains the phenomena 'as well, or nearly as well' as phlogiston theory. But, he insists, 'the commonly received principle of phlogiston explains all phenomena, at least as well as M. Lavoisier's'. (... By the 'commonly received principle of phlogiston' he actually means his new version of the phlogiston programme.) ... Provided we allow that post hoc explanations are as good as predictions (which we should not), Cavendish's claim has substance. ... Cavendish can explain all the experiments qualitatively; he has only to assume that phlogiston is weightless, and all Lavoisier's quantitative results are accommodated too. (203–204)

Philosophers of science who think that the evidential support of a theory depends solely upon the timeless logical relations between theory and evidence will have to say that [Cavendish's] 1784 phlogiston theory had as much evidential support as 1784 oxygen theory. Both theories explained the main facts about combustion and calcination (and both faced some outstanding anomalies). ... But the chemists of the late eighteenth century did not take this view. They saw that 1784 phlogiston theory merely accommodated known facts, many of which had been discovered by testing predictions made within the oxygen programme. They saw that 1784 phlogiston theory was inconsistent with the previous version, and marked a return to the imponderable phlogiston. ... They contrasted this incoherent development with the smooth development of the various versions of the oxygen programme. And one by one they changed their allegiance. (205)

HASOK CHANG, We Have Never Been Whiggish (About Phlogiston), *Centaurus*, 51, 2009, 239–264.

Traditional historiography celebrates Lavoisier as the 'father of modern chemistry', but from a modern perspective ... Lavoisier's theory was quite wrong and not clearly better than the phlogiston theory. ... Three of the major pillars of Lavoisier's new system of chemistry were clearly wrong, judged from the viewpoint of modern chemistry, or even from the viewpoint of 19th-century chemistry:

1. First of all, it is readily conceded by even the most robust Lavoisier-enthusiasts that his theory of acids was mistaken. Not all acids contain oxygen, and the Lavoisierians knew as well as anyone that there were certain acids that had not been shown to contain oxygen. ... In the early 19th century, Lavoisier's oxygen theory of acidity was clearly dead, never to be revived again. Was the theory of acids just an unfortunate non-essential adjunct to the rest of Lavoisier's system, which could safely be discarded? At least Lavoisier himself didn't think so, as we can glimpse from the way he named his beloved 'oxygen', [which etymologically means] the acid-generator.

2. And then there is the core of Lavoisier's 'antiphlogistic' system, namely his theory of combustion. Surely this undeniably essential bit of Lavoisier's system was correct, and is still preserved in modern chemistry? To grant that would be to participate in an amnesia that pro-Lavoisierian historiography has carefully orchestrated. ...

In his influential textbook of chemistry published in 1802, Thomson calmly summarised various devastating objections to Lavoisier's theory of combustion, while clearly not advocating a return to phlogiston. ... Lavoisier understood combustion as involving a decomposition of oxygen gas into oxygen base and caloric, the oxygen base combining with the combustible and the caloric being released as sensible heat. According to Lavoisier's theory, the heat generated in combustion came from the oxygen gas, and it was essential that the oxygen enabling combustion was in a gaseous state to begin with, since it was the abundance of combined caloric which put a gas into the gaseous state. ... Following Lavoisier's view, 'one would naturally suppose, that when the product [of combustion] is a gas, all the caloric and light which existed in the oxygen gas would be necessary for maintaining the gaseous state of the product'. But when charcoal is burned, for example, the product is usually a gas yet the combustion still yields a great deal of heat and light. ... Lavoisier was aware of this problem, but did not provide a convincing solution.

Secondly, 'one would naturally suppose that in every case of combustion the oxygen employed must be in the state of a gas. But this is very far from being the case'. ... Consider the explosion of gunpowder, which happens without the help of ambient oxygen gas, the oxygen being present in the solid state in the nitre (saltpetre) in the gunpowder. ... Lavoisier was very interested in the chemistry of gunpowder, and made a few different attempts to explain its workings in terms of his theory. [In fact,] Lavoisier ... was a commissioner of the Royal Gunpowder Administration from 1775 and in that capacity set up his residence and laboratory at the Paris Arsenal. ... None less than Claude-Louis Berthollet used the gunpowder case as a weapon against Lavoisier's theory of combustion for precisely the reason [mentioned above]. Lavoisier was himself never quite satisfied with the clever yet clumsy defence he was able to give against Berthollet.

Yet another problem for Lavoisier: from his theory, 'one would naturally expect that caloric and light would be emitted during the condensation of other gases as well as oxygen: but this never happens unless oxygen be concerned'. For example, when hydrogen and nitrogen gases combine, there is no heat or light emitted; ammonia gas and hydrochloric acid gas combine to make a 'concrete salt', producing very little heat and no light.

Thomson concluded: 'Upon the whole, it cannot be denied that Lavoisier's theory does not afford a sufficient explanation of combustion'. … Numerous other chemists who accepted that oxygen combined with combustibles remained skeptical of Lavoisier's explanation of the heat and light in combustion; these 'late phlogistonists' often maintained a system in which oxygen and phlogiston happily co-existed, the latter still being given the role of explaining what we would now identify as the energy relations in combustion.

3. Lavoisier's caloric theory in general also ran into problems, and was eventually rejected altogether. … Caloric in Lavoisier's system was not merely a device for explaining the release of heat in combustion; rather, it was an essential element in his cosmology, for example in explaining the three states of matter. Lavoisier clearly considered caloric a cornerstone of his chemical system, putting it (along with light) at the top of his list of chemical elements, and devoting the entire first chapter of his definitive textbook of new chemistry to the elucidation of the nature and role of caloric. … By the time the energy concept and early thermodynamics toppled the caloric theory altogether in the 1840s and the 1850s, Lavoisier's basic picture of the universe was in tatters; later the kinetic theory would fill in the theoretical vacuum in regard to the explanation of the three states of matter.

But surely these incorrect theories are not the only important contributions Lavoisier made to chemistry? That is correct, but if we examine Lavoisier's more lasting contributions, we find that various phlogistonists had priority or superiority.

1. Most famously and obviously, the isolation of the gas that Lavoisier called oxygen had previously been made, and its chemical and biological properties studied quite well, by the phlogistonists Carl Wilhelm Scheele and Joseph Priestley. It was Priestley (and his mice), not Lavoisier, who first discovered that dephlogisticated air was 'eminently respirable', deserving the name of 'vital air'.

2. It was the phlogistonists, going back to Georg Stahl himself, who recognized combustion and calcination as phenomena of the same type.

3. Lavoisier's discovery of the composition of water was largely anticipated by Henry Cavendish, as well as Priestley. … It was Cavendish who first recognized clearly that water was produced when inflammable air (hydrogen) and dephlogisticated air (oxygen) were exploded together, and showed that there was no change of weight in that process, within an accuracy of 0.2 grain out of 24,000 grains.

4. Even Lavoisier's emphasis on precise weight measurements and the balancing of chemical equations by weight was nothing entirely new or so unusual. The water episode just summarized above gives a glimpse of the fact that Cavendish, for one, had the same concern about the preservation of weight, and his skills in weight-measurement were superior to Lavoisier's. … And then if we recall Lavoisier's utter conviction that the weight ratio of oxygen and hydrogen combining to make water was 85:15 (rather than … 8:1), it is difficult to maintain the common lore about how Lavoisier overwhelmed his opponents with his accurate weight-measurements.

5. It is often claimed that Lavoisier introduced the quasi-operationalist concept of the chemical element as an 'undecompouned body'. But this really was nothing new, and various phlogistonists had put it into practice long before Lavoisier. … A number of 18th-century chemists prior to Lavoisier understood the operational definition of an element and distinguished between the ultimate constituents of matter and the last products of chemical analysis. Lavoisier certainly did promote this concept of the chemical element, but he was neither the first to do so, nor completely consistent in sticking to it.

I have already indicated various phlogistonist contributions that were clear anticipations of what is usually credited to Lavoisier and his group. There are also some aspects of the phlogiston theory that were clearly unlike Lavoisier's ideas but could be regarded as consonant with much later modern ideas.

First of all, whiggishly speaking, phlogiston served as an expression of chemical potential energy, which the weight-obsessed oxygen theory completely lost sight of. … [Later chemists held that] 'there can be no doubt' that potential energy was what the earlier chemists 'meant when they spoke of phlogiston'. … [Thus] the major insight from the phlogistonists was that 'combustible bodies possess in common a power or energy capable of being elicited and used', and that 'the energy pertaining to combustible bodies is the same in all of them, and capable of being transferred from the combustible body which has it to an incombustible body which has it not'. … Lavoisier had got this wrong by locating the energy in the oxygen gas (in the form of caloric), without a convincing account of why caloric contained in other gases would not have the ability to cause combustion.

One important thing that the phlogiston theory did well and the oxygen theory did not do so well was to explain the common properties of metals. The phlogistonist explanation was that all metals were rich in phlogiston; now, that has a certain resonance with the modern notion that metals all share the metallic properties because they all have a 'sea' of free electrons.

PAUL NEEDHAM, Compounds and Mixtures, in Robin Findlay Hendry, Paul Needham and Andrea I. Woody (eds.), *Handbook of the Philosophy of Science. Volume 6: Philosophy of Chemistry*, Elsevier, 2012.

Aristotle held that there were four elements: earth, water, air and fire. ... Each element is characterised in terms of properties exhibited in isolation. ... He thought these properties could be reduced to a small number of basic features ... [namely] degrees of warmth and humidity. Further, he thought there are maximal and minimal “contrary extremes” of each of these two fundamental scales, hot and cold being the extremes of warmth, moist and dry those of humidity, and elements are substances with these extremal properties. ...

Aristotle is sometimes regarded as a bad influence on chemistry because of his criticism of atomism and his role as a prime source in the development of alchemy. The mystical ideas with which alchemy infused the study of matter have been taken to reflect on him, as in the picture Robert Siegfried paints of Aristotle in his history of modern chemistry: “the atomic theory of Democritus lost out in antiquity [to Aristotle’s view] because its materialism left no room for the spiritual. The so-called elements of this story were not the material ones of today, but metaphysical causes of the properties of the various bodies experienced.”

But ... atomic theory “lost out” in antiquity for the very good reason that it bore no explanatory relation to observable phenomena, and had no useful part to play in furthering the understanding of nature—a point which Lavoisier and 19th century critics of atomism were quite clear on. This circumstance wasn’t to change until the first decades of the 20th century, before which atomic speculations provided at best “metaphysical causes of the properties of the various bodies experienced”.

[Aristotle and Lavoisier] agree[d] that no help is to be had by resorting to the doctrine of atomism. [Instead they favoured a more operational view of the constituent parts of substances.] Aristotle called elements simple substances, by which he meant[:] “An element ... is a body into which other bodies may be analysed, present in them potentially or in actuality (which of these is still disputable), and not itself divisible into bodies different in form. That, or something like it, is what all men in every case mean by element.” ... This definition of an element ... bears a striking resemblance to Lavoisier’s: “if, by the term elements, we mean to express those simple and indivisible atoms of which matter is composed, it is extremely probable we know nothing at all about them; but if we apply the term elements ... to express our idea of the last point which analysis is capable of reaching, we must admit, as elements, all the substances into which we are capable, by any means, to reduce bodies by decomposition.”

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§ 8.3. Applicable philosophy of science

ALAN MUSGRAVE, Why did oxygen supplant phlogiston? Research programmes in the Chemical Revolution, in Colin Howson (ed.), *Method and Appraisal in the Physical Sciences*, Cambridge University Press, 1976, 181–209.

I will first outline the accounts of the Chemical Revolution given by other methodologies, and show that they are unsatisfactory: each of them must either deem the Chemical Revolution an irrational affair or falsify history so that it squares with its canons of rationality. Then I will argue that the actual story of the Chemical Revolution fits Lakatos’s methodology like a glove.

1. Inductivist accounts.

Inductivism claims that respectable scientific theories are derived from, and proved by, experiments. A naive inductivist account of the Chemical Revolution says that it consisted of replacing premature and erroneous speculation by proven truth. ... The chief difficulty for inductivists is that Lavoisier’s theory, allegedly proven truth, is actually false. Inductivist historians have to prune that theory of its mistaken branches (such as the caloric theory, or the thesis that all acids contain oxygen) in order to make their account plausible. ... Most historians would disavow a crude inductivist account of the Chemical Revolution: it survives explicitly only in illiterate prefaces to chemistry textbooks. (182)

2. Naive falsificationist accounts.

If experiments did not prove Lavoisier’s oxygen theory, perhaps they disproved the rival phlogiston theory? A naive account of the Chemical Revolution says that it was brought about by experiments which decisively refuted the phlogiston theory of combustion. The experiment usually selected for this decisive role is Lavoisier’s experiment of 1772: phosphorus was burned in a volume of air confined over water, the volume of air was reduced, and the product of the combustion weighed more than the original. This decisively refuted phlogiston theory, which claimed that a burning substance emitted phlogiston and hence should decrease in weight. ... Thus, a falsificationist can write that phlogiston theory ‘survived for almost a century, until Lavoisier exposed its fallacies by his study of the changes in weight during combustion’. ... This falsificationist account soon runs up against unpleasant historical facts. First of all, the result of Lavoisier’s 1772 experiment ... was common knowledge long before the phlogiston theory was ever proposed. It was common knowledge that metallic calxes weigh more than the metals from which they have been prepared. ... If Lavoisier’s 1772 experiment refutes phlogiston theory, then phlogiston theory was born refuted. ... Falsificationists will have to say that phlogiston theorists ignored or twisted the facts. ... A second difficulty for falsificationists is that Lavoisier’s experiment of 1775, usually called the ‘discovery of oxygen’, was first performed by Scheele and

Priestley, both of whom were phlogiston theorists and both of whom continued to adhere to the phlogiston theory. Falsificationists have to say that they were too stupid, dogmatic, or blinded by preconceived ideas, to appreciate the logical consequences of their own discovery. ... But *most* chemists continued to adhere to phlogiston theory long after 1775, when Lavoisier is supposed to have disproved it. Perhaps an epidemic of stupidity or dogmatism swept through European chemists, leaving only Lavoisier unscathed! Falsificationism is bankrupt if it must be supplemented with such dubious social psychology as this. (182–184)

Many historians hint that the Chemical Revolution was brought about because Lavoisier had an accurate balance and used it carefully, insisting on the principle of conservation of weight and, more importantly, on the principle that only items which figured on the balance should figure in chemical explanations. His phlogistonist opponents, on the other hand, are supposed to have scorned accurate measurement, performed sloppy experiments, and structured their theory around a mysterious imponderable substance which could never be captured in a bottle. Alas, this sort of tale is almost entirely mythical. The first accurate balance was made for Cavendish, a phlogistonist with a fetish about precision. Lavoisier did not get his most accurate balances until after 1785, when the battle against phlogiston was won. ... Phlogiston was not always claimed to be an imponderable substance which could not be isolated: some phlogistonists identified it with hydrogen which they isolated and weighed. Besides, an element called caloric played an essential role in Lavoisier's own theory, and according to Lavoisier caloric was imponderable and could not be isolated. Historian who like to emphasise Lavoisier's use of the balance engage in double-think. ... Lavoisier's imponderables were fine, phlogistonist imponderables sophistry and illusion! (185)

3. Conventionalist accounts.

Historians who realise that experiments neither proved the oxygen theory nor disproved its rival often fall back on conventionalism or simplicism. ... It is then claimed that the oxygen theory superseded the phlogiston theory because it was a *simpler* 'conceptual scheme'. ... The main problem [from this point of view] ... lies in specifying the exact sense in which the oxygen theory is ... simpler than the phlogiston theory. ... [One possible] straightforward answer [is]: the oxygen theory was simpler because the analyses which it gave of chemical reactions involved fewer elements than those provided by phlogistonists. This can be supported as follows. According to phlogiston theory, when a metal calcinates it loses its phlogiston; but to explain the weight increase, some phlogistonists added that the calx also gains something. Lavoisier, on the other hand, simply dispenses with phlogiston and says that when a metal calcinates it combines with oxygen from the air. The phlogistonist analysis involves three elements, the anti-phlogistonist analysis only two. What better example of Occam's razor could there be? Alas, this argument falsifies history. Lavoisier did not regard oxygen gas as a simple element, but as an 'igneous combination' of an 'oxygen base' and caloric. When a metal calcinates it seizes the oxygen base and sets free the caloric in the form of heat. Here three elements are involved, as in the phlogistonist account. Indeed, [Lavoisier] explicitly repudiated the two-element analysis of calcination. ... Thus, in this very simple sense of 'simple', the oxygen theory was no simpler than the phlogiston theory. (185–186)

PIERRE DUHEM, *The Aim and Structure of Physical Theory*, 1914, trans. Phillip Wiener, Princeton University Press, 1954.

The physicist can never subject an isolated hypothesis to experimental test, but only a whole group of hypotheses; when the experiment is in disagreement with his predictions, what he learns is that at least one of the hypotheses constituting this group is unacceptable and ought to be modified; but the experiment does not designate which one should be changed. ... People generally think that each one of the hypotheses employed in physics can be taken in isolation, checked by experiment, and then, when many varied tests have established its validity, given a definitive place in the system of physics. In reality, this is not the case. Physics is not a machine which lets itself be taken apart; ... Physical science is a system that must be taken as a whole. ... The watchmaker to whom you give a watch that has stopped separates all the wheelworks and examines them one by one until he finds the part that is defective or broken. The doctor to whom a patient appears cannot dissect him in order to establish his diagnosis; he has to guess the seat and cause of the ailment solely by inspecting disorders affecting the whole body. Now, the physicist concerned with remedying a limping theory resembles the doctor and not the watchmaker.

IMRE LAKATOS, Falsification and the Methodology of Scientific Research Programmes, quoted from *The Methodology of Scientific Research Programmes: Philosophical Papers Volume I*, Cambridge University Press, 1980.

"A series of theories is theoretically progressive ... if each new theory has some excess empirical content over its predecessor, that is, if it predicts some novel, hitherto unexpected fact. ... [It] is also empirically progressive ... if some of this excess empirical content is also corroborated, that is, if each new theory leads to the discovery of some new fact. Finally, let us call a problemshift progressive if it is both theoretically and empirically progressive, and degenerating if it is not." (33-34). "Justificationists valued 'confirming' instances of a theory; naive falsificationists stressed the 'refuting' instances; for the methodological falsificationist [i.e. Lakatos] it is the—rather rare—corroborating instances of the excess information which are the crucial ones ... We are no longer interested in the thousands of trivial verifying instances nor in the hundreds of readily available anomalies.” (36).

STRUAN JACOBS, Whewell’s philosophy of science and ethics, in C. L. Ten (ed.), *Routledge History of Philosophy, Volume VII: The Nineteenth Century*, Routledge, 1994, Chapter 2.

[When a scientific theory faces phenomena it cannot explain, it is forced to adapt.] Whewell notices two [ways in which such adaptation can constitute] ‘progressive’ explanatory success . . . . One concerns theories to which hypotheses are added. It may be the case that the hypotheses successively ‘tend to simplicity and harmony; . . . the system . . . [becoming] more coherent as it is further extended’. Observes Whewell, ‘The elements . . . we require for explaining a new class of facts are already contained in our system’, and for him this amounts to an infallible sign of truth.

The other form of ‘progressive’ success is ‘consilience of inductions’. This is achieved when a theory . . . , advanced to causally explain a confirmed generalization . . . , is later discovered also to give a successful explanation of a confirmed law about a class of phenomena . . . different from that for which it was framed. . . .

[These two forms of] success together, Whewell says, ‘irresistibly’ establish a theory’s truth. . . . Whewell’s paradigm cases of progressive explanatory success—universal gravity and the undulatory theory of light—physicists came in the fullness of time to judge as false.

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§ 8.4. Reader

ANDREW EDE & LESLEY CORMACK, *A History of Science in Society: A Reader*, Broadview Press, 2007.

6.4. Priestley. Mostly interesting for its general framing of the conflict between the two systems. The technical details are not very important for us (most of them are already covered in the above readings).

(6.5. Lavoisier. The details here are unimportant for us. Basically skip this reading, but the general idea is interesting: a concrete experimental setup for quantifying heat by how much ice it can melt. This is relevant to Lavoisier’s views on caloric and operationalism discussed above.)

Cut out the elements and compounds, and use them to reenact the reactions from discussion questions 1 (also consider the reaction in an enclosed space) through 8 of this section. Red: phlogiston theory. Blue: Lavoisier's theory.

Simple elements:

phlogiston	calx	acid	oxygen	metal	hydrogen

Compounds (place the substances above in the light spaces):

metal: calx, phlogiston	salt: calx, acid	inflammable air: phlogiston, in gas form
stuff that can burn: something, phlogiston	inflammable air: hydrogen, in gas form	calx: metal, oxygen
standard acid: something, water	concentrated acid: something, oxygen	water: hydrogen, oxygen
salt: calx, something		

Wildcards for use in either theory:

something	"air": any gas, any gas

§ 9. Natural history before Darwin

§ 9.1. Discussion questions

9.1. What were the main arguments for the "Neptunist" view that water has been the most important force shaping the earth's natural history?

Geological evidence that water once covered all the earth. Life originated in water. Water can produce crystals spontaneously, other rocks through erosion and sedimentation.

9.2. What were the main arguments for the "Vulcanist" view that volcanic activity has been the most important force shaping the earth's natural history?

Rocks, mountains generated this way. Mass extinctions, that are furthermore embedded in stone.

9.3. What was the main evidence for the "catastrophist" view that the earth's natural history has been shaped by exceptional, dramatic events?

Mass extinctions. Formation of new species. Mountains made by very old rock suddenly radically displaced. Other mountains produced by volcanic eruptions. Uniformitanism cannot account for origins: species; heat in earth's interior; formation of solar system; creation of languages.

9.4. What was the main evidence for the "uniformist" view that the earth's natural history has been shaped by forces like those we see around us currently operating today?

Currently observable: land rising, species dying. Conceivable how slow forces can cause great effects.

9.5. What general philosophical commitments about how science should operate were called upon in the debate between catastrophism and uniformism?

Uniformism: Like astronomy, explain far away by what can be observed close at hand. Don't postulate unknowns when knowns are sufficient. Objection: this distinction is not well-defined (where to draw the line between slow rise, small volcano, and drastic versions of the same?). Also a preconceived notion. Catastrophism prone to hypothesising, putting forth striking evidence. Uniformism prone to refutation, scepticism. Rejection of hypotheses a common view, but vigorous debate a path to truth (Playfair). Newton's system anti-hypotheses, but grew out of a succession of hypotheses.

9.6. Theories of the origin of species before Lamarck and Darwin were heavily influenced by theological considerations. Discuss.

French Enlightenment hated Christianity; materialist-based atheism was as much a consequence of this hostility as a cause of it (Jimack). Greek thought on this matter more theological than scientific (Sloan): atomists committed to blind chance; Plato and Aristotle postulate perfect forms and teleology. In Christianity, species forms can be considered pre-formed, even if not yet instantiated. Contra: Argument from design arguably scientific.

9.7. What were the scientific strengths and weaknesses of preformationist theories such as the claim that sperm contained miniature persons?

Pro: Mechanism inadequate to explain life. Organs interdependent, must exist together. Microscopy. Contra: Rules out influence of environment. Regeneration of lost parts. Influence of both parents, even when from two species.

9.8. How and when did scientific theories of life go from vitalist (life a "special ingredient" distinct from other physical phenomena) to materialist (life explained in terms of ordinary physical forces)?

The French wanted to do this, seemingly driven more by ideology than evidence. But lack of viable materialist account left theorising quasi-vitalist. Lamarck avoids outright vitalism only by vague appeal to caloric and electric forces. Buffon still has some vitalism to account for spontaneous generation of life in an otherwise materialistic system.

9.9. Does Lamarck claim that acquired traits (such as having larger muscles through extensive use) are hereditary?

Yes. 244 second law. 246 left bottom half.

9.10. In Lamarck's view, the basic force driving species development is changes in habit that the organism adopts in order to be more successful in a given environment. Discuss.

No. Secondary factor only. Primary force is drive toward complexity. In reader, early.

9.11. Compare Lamarck's and Darwin's views on how adaptations emerge (for example, why certain birds have very long legs and necks). What are the strengths and weaknesses of each view based on the evidence available at the time?

L explains why only useful, not random, traits are passed on (both parents need to contribute, 246). D requires lots of time. And if adaptation is random and very slow, shouldn't we see a lot more "unfinished" "work in progress" around us, instead of species perfectly adapted to their environment?

WILLIAM WHEWELL, *Philosophy of the Inductive Sciences*, second edition, London: John W. Parker, 1847, 1.X.3, 665–680.

Attempts to frame a theory of the earth have brought into view two completely opposite opinions:—one, which represents the course of nature as uniform through all ages, the causes which produce change having had the same intensity in former times which they have at the present day;—the other opinion, which sees, in the present condition of things, evidences of catastrophes;—changes of a more sweeping kind, and produced by more powerful agencies than those which occur in recent times. Geologists who held the latter opinion, maintained that the forces which have elevated the Alps or the Andes to their present height could not have been any forces which are now in action: they pointed to vast masses of strata hundreds of miles long, thousands of feet thick, thrown into highly inclined positions, fractured, dislocated, crushed: they remarked that upon the shattered edges of such strata they found enormous accumulations of fragments and rubbish, rounded by the action of water, so as to denote ages of violent aqueous action: they conceived that they saw instances in which whole mountains of rock in a state of igneous fusion, must have burst the earth's crust from below: they found that in the course of the revolutions by which one stratum of rock was placed upon another, the whole collection of animal species which tenanted the earth and the seas had been removed, and a new set of living things introduced in its place: finally, they found, above all the strata, vast masses of sand and gravel containing bones of animals, and apparently the work of a mighty deluge. With all these proofs before their eyes, they thought it impossible not to judge that the agents of change by which the world was urged from one condition to another till it reached its present state must have been more violent, more powerful, than any which we see at work around us. They conceived that the evidence of "catastrophes" was irresistible.

This formidable array of proofs was, in the minds of some eminent geologists, weakened, and at last overcome. This was done by showing that the sudden breaks in the succession of strata were apparent only, the discontinuity of the series which occurred in one country being removed by terms interposed in another locality:—by urging that the total effect produced by existing causes, taking into account the accumulated result of long periods, is far greater than a casual speculator would think possible:—by making it appear that there are in many parts of the world evidences of a slow and imperceptible rising of the land since it was the habitation of now existing species:—by proving that it is not universally true that the strata separated in time by supposed catastrophes contain distinct species of animals:...—and finally, by remarking that though the creation of species is a mystery, the extinction of species is going on in our own day. Hypotheses were suggested, too, by which it was conceived that the change of climate might be explained, which, as the consideration of the fossil remains seemed to show, must have taken place between the ancient and the modern times. In this manner the whole evidence of catastrophes was explained away: the notion of a series of paroxysms of violence in the causes of change was represented as a delusion arising from our contemplating short periods only, in the action of present causes: length of time was called in to take the place of intensity of force: and it was declared that Geology need not despair of accounting for the revolutions of the earth, as Astronomy accounts for the revolutions of the heavens, by the universal action of causes which are close at hand to us, operating through time and space without variation or decay. ...

The doctrine of Uniformity in the course of nature has sometimes been represented by its adherents as possessing a great degree of *a priori* probability. It is highly unphilosophical, it has been urged, to assume that the causes of the geological events of former times were of a different kind from causes now in action, if causes of this latter kind can in any way be made to explain the facts. The analogy of all other sciences compels us, it was said, to explain phenomena by known, not by unknown, causes. And on these grounds the geological teacher recommended "an earnest and patient endeavour to reconcile the indications of former change with the evidence of gradual mutations now in progress."

But on this we may remark, that if by known causes we mean causes acting with the same intensity which they have had during historical times, the restriction is altogether arbitrary and groundless. Let it be granted, for instance, that many parts of the earth's surface are now undergoing an imperceptible rise. It is not pretended that the rate of this elevation is rigorously uniform; what, then, are the limits of its velocity? Why may it not increase so as to assume that character of violence which we may term a catastrophe with reference to all changes hitherto recorded? Why may not the rate of elevation be such that we may conceive the strata to assume suddenly a position nearly vertical? and is it, in fact, easy to conceive a position of strata nearly vertical, a position which occurs so frequently, to be gradually assumed? In cases where the strata are nearly vertical, as in the Isle of Wight, and hundreds of other places, or where they are actually inverted, as sometimes occurs, are not the causes which have produced the effect as truly known causes, as those which have raised the coasts where we trace the former beach in an elevated terrace? If the latter case proves slow elevation, does not the former case prove rapid elevation? In neither case have we any measure of the time employed in the change; but does not the very nature of the results enable us to discern, that if one was gradual, the other was comparatively sudden?

The causes which are now elevating a portion of Scandinavia can be called known causes, only because we know the effect. Are not the causes which have elevated the Alps and the Andes known causes in the same sense? We know nothing in either case which confines the intensity of the force within any limit, or prescribes to it any law of uniformity. Why, then, should we make a merit of cramping our speculations by such assumptions? Whether the causes of change do act uniformly;—whether they oscillate only within narrow limits;—whether their intensity in former times was nearly the same as it now is;—these are precisely the questions which we wish Science to answer to us impartially and truly: where is then the wisdom of "an earnest and patient endeavour" to secure an affirmative reply?

Thus I conceive that the assertion of an *a priori* claim to probability and philosophical spirit in favour of the doctrine of uniformity, is quite untenable. We must learn from an examination of all the facts, and not from any assumption of our own, whether the course of nature be uniform. The limit of intensity being really unknown, catastrophes are just as probable as uniformity. If a volcano may repose for a thousand years, and then break out and destroy a city; why may not another volcano repose for ten thousand years, and then destroy a continent; or if a continent, why not the whole habitable surface of the earth? . . .

There is an opposite tendency in the mode of maintaining the catastrophist and the uniformitarian opinions, which depends upon their fundamental principles, and shows itself in all the controversies between them. The Catastrophist is affirmative, the Uniformitarian is negative in his assertions: the former is constantly attempting to construct a theory; the latter delights in demolishing all theories. The one is constantly bringing fresh evidence of some great past event, or series of events, of a striking and definite kind; his antagonist is at every step explaining away the evidence, and showing that it proves nothing. One geologist adduces his proofs of a vast universal deluge; but another endeavours to show that the proofs do not establish either the universality or the vastness of such an event. The inclined broken edges of a certain formation, covered with their own fragments, beneath superjacent horizontal deposits, are at one time supposed to prove a catastrophic breaking up of the earlier strata; but this opinion is controverted by showing that the same formations, when pursued into other countries, exhibit a uniform gradation from the lower to the upper, with no trace of violence. Extensive and lofty elevations of the coast, continents of igneous rock, at first appear to indicate operations far more gigantic than those which now occur; but attempts are soon made to show that time only is wanting to enable the present age to rival the past in the production of such changes. Each new fact adduced by the catastrophist is at first striking and apparently convincing; but as it becomes familiar, it strikes the imagination less powerfully; and the uniformitarian, constantly labouring to produce some imitation of it by the machinery which he has so well studied, at last in every case seems to himself to succeed, so far as to destroy the effect of his opponent's evidence.

This is so with regard to more remote, as well as with regard to immediate evidences of change. When it is ascertained that in every part of the earth's crust the temperature increases as we descend below the surface, at first this fact seems to indicate a central heat: and a central heat naturally suggests an earlier state of the mass, in which it was incandescent, and from which it is now cooling. But this original incandescence of the globe of the earth is manifestly an entire violation of the present course of things; it belongs to the catastrophist view, and the advocates of uniformity have to explain it away. Accordingly, one of them holds that this increase of heat in descending below the surface may very possibly not go on all the way to the center. The heat which increases at first as we descend, may, he conceives, afterwards decrease; and he suggests causes which may have produced such a succession of hotter and colder shells within the mass of the earth. . . . Other persons also, desirous of reconciling this subterraneous heat with the tenet of uniformity, have offered another suggestion:—that the warmth or incandescence of the interior parts of the earth does not arise out of an originally hot condition from which it is gradually cooling, but results from chemical action constantly going on among the materials of the earth's substance. And thus new attempts are perpetually making, to escape from the cogency of the reasonings which send us towards an original state of things different from the present. Those who theorize concerning an origin go on building up the fabric of their speculations, while those who think such theories unphilosophical, ever and anon dig away the foundation of this structure. As we have already said, the uniformitarian's doctrines are a collection of negatives.

This is so entirely the case, that the uniformitarian would for the most part shrink from maintaining as positive tenets the explanations which he so willingly uses as instruments of controversy. He puts forward his suggestions as difficulties, but he will not stand by them as doctrines. And this is in accordance with his general tendency; for any of his hypotheses, if insisted upon as positive theories, would be found inconsistent with the assertion of uniformity. For example, the nebular hypothesis appears to give to the history of the heavens an aspect which obliterates all special acts of creation, for, according to that hypothesis, new planetary systems are constantly forming; but when asserted as the origin of our own solar system, it brings with it an original incandescence, and an origin of the organic world. And if, instead of using the chemical theory of subterraneous heat to neutralize the evidence of original incandescence, we assert it as a positive tenet, we can no longer maintain the infinite past duration of the earth; for chemical forces, as well as mechanical, tend to equilibrium; and that condition once attained, their efficacy ceases. . . . Thus a perpetual motion is impossible in chemistry, as it is in mechanics; and a theory of constant change continued through infinite time, is untenable when asserted upon chemical, no less than upon mechanical principles. . . .

When geologists had first discovered that the successive strata are each distinguished by appropriate organic fossils, they assumed at once that each of these collections of living things belonged to a separate creation. But this conclusion, . . . Lyell has attempted to invalidate, by proving that in the existing order of things, some species become extinct; and by suggesting it as possible, that in the same order it may be true that new species are from time to time produced, even in the present course of nature. And in this, as in the other part of the subject, he calls in the aid of vast periods of time, in order that the violence of the changes may be softened down: and he appears disposed to believe that the actual extinction and creation of species may be so slow as to excite no more notice than it has hitherto obtained; and yet may be rapid enough, considering the immensity of geological periods, to produce such a succession of different collections of species as we find in the strata of the earth's surface.

Nothing has been pointed out in the existing order of things which has any analogy or resemblance … to that creative energy which must be exerted in the production of a new species. And to assume the introduction of new species as "a part of the order of nature," without pointing out any natural fact with which such an event can be classed, would be to reject creation, by an arbitrary act. Hence, even on natural grounds, the most intelligible view of the history of the animal and vegetable kingdoms seems to be, that each period which is marked by a distinct collection of species forms a cycle; and that at the beginning of each such cycle a creative power was exerted, of a kind to which there was nothing at all analogous in the succeeding part of the same cycle. If it be urged that in some cases the same species, or the same genus, runs through two geological formations, which must, on other grounds, be referred to different cycles of creative energy, we may reply that the creation of many new species does not imply the extinction of all the old ones.

Thus we are led by our reasonings to this view, that the present order of things was commenced by an act of creative power entirely different to any agency which has been exerted since. None of the influences which have modified the present races of animals and plants since they were placed in their habitations on the earth's surface can have had any efficacy in producing them at first. We are necessarily driven to assume, as the beginning of the present cycle of organic nature, an event not included in the course of nature.

If we attempt to apply the same antithesis of opinion (the doctrines of Catastrophe and Uniformity,) to the other subjects of palaetiological sciences, we shall be led to similar conclusions. Thus, if we turn our attention to astronomical palaetiology, we perceive that the nebular hypothesis has a uniformitarian tendency. According to this hypothesis the formation of this our system of sun, planets, and satellites, was a process of the same kind as those which are still going on in the heavens. One after another, nebulae condense into separate masses, which begin to revolve about each other by mechanical necessity, and form systems of which our solar system is a finished example. But we … have as yet only very vague and imperfect reasonings to show that by such condensation a material system such as ours could result; and the introduction of organized beings into such a material system is utterly out of the reach of our philosophy. Here again, therefore, we are led to regard the present order of the world as pointing towards an origin altogether of a different kind from anything which our material science can grasp.

We should be led to the same conclusion once more, if we were to take into our consideration … the history of languages. We may explain many of the differences and changes which we become acquainted with, by referring to the action of causes of change which still operate. But what glossologist will venture to declare that … the influences which mould a language, or make one language differ from others of the same stock, operated formerly with no more efficacy than they exercise now. … As the early stages of the progress of language must have widely different from those later ones of which we can in some measure trace the natural causes, we cannot place the origin of language in any point of view in which it comes under the jurisdiction of natural causation at all.

We are thus led by a survey of several of the palaetiological sciences to a confirmation of the principle formerly asserted, That in no palaetiological science has man been able to arrive at a beginning which is homogeneous with the known course of events.

JOHN PLAYFAIR, *Illustrations of the Huttonian theory of the earth*, Edinburgh, 1802, 510–528.

Among the prejudices which a new theory of the earth has to overcome, is an opinion, held, or affected to be held, by many, that geological science is not yet ripe for such elevated and difficult speculations. They would, therefore, get rid of these speculations, by moving the previous question, and declaring that at present we ought to have no theory at all. We are not yet, they allege, sufficiently acquainted with the phenomena of geology; the subject is so various and extensive, that our knowledge of it must for a long time, perhaps for ever, remain extremely imperfect. And hence it is, that the theories hitherto proposed have succeeded one another with so great rapidity, hardly any of them having been able to last longer than the discovery of a new fact, or a fact unknown when it was invented. …

This unfavourable view of geology, ought not, however, to be received without examination; in science, presumption is less hurtful than despair, and inactivity is more dangerous than error. …

Theories that have a rational object, though they be false or imperfect in their principles, are for the most part approximations to the truth, suited to the information at the time when they were proposed. They are steps, therefore, in the advancement of knowledge, and are terms of a series that must end when the real laws of nature are discovered. It is, on this account, rash to conclude, that in the revolutions of science, what has happened must continue to happen, and because systems have changed rapidly in time past, that they must necessarily do so in time to come.

He who would have reasoned so, and who had seen the ancient physical systems, at first all rivals to one another, and then swallowed up by the Aristotelian; the Aristotelian physics giving way to those of Des Cartes; and the physics of Des Cartes to those of Newton; would have predicted that these last were also, in their turn, to give place to the philosophy of some later period. This is, however, a conclusion that hardly any one will now be bold enough to maintain, after a hundred years of the most scrupulous examination have done nothing but add to the evidence of the Newtonian System. It seems certain, therefore, that the rise and fall of theories in times past, does not argue, that the same will happen in the time that is to come.

The multifarious and extremely diversified object of geological researches, does, no doubt, render the first steps difficult, and may very well account for the instability hitherto observed in such theories; but the very same thing gives reason for expecting a very high degree of certainty to be ultimately attained in these inquiries.

Where the phenomena are few and simple, there may be several different theories that will explain them in a manner equally satisfactory; and in such cases, the true and the false hypotheses are not easily distinguished from one another. When, on the other hand, the phenomena are greatly varied, the probability is, that among them, some of those *instantiae crucis* will be found, that exclude every hypothesis but one, and reduce the explanation given to the highest degree of certainty. It was thus, when the phenomena of the heavens were but imperfectly known, and were confined to a few general and simple facts, that the [heliocentric system] could claim no preference to the Ptolemaic system: The former seemed a possible hypothesis; but as it performed nothing that the other did not perform, and was inconsistent with some of our most natural prejudices, it had but few adherents. The invention of the telescope, and the use of more accurate instruments, by multiplying and diversifying the facts, established its credit; and when not only the general laws, but also the inequalities, and disturbances of the planetary motions were understood, all physical hypotheses vanished, like phantoms, before the philosophy of Newton. Hence the number, the variety, and even the complication of facts, contribute ultimately to separate truth from falsehood; and the same causes which, in any case, render the first attempts toward a theory difficult, make the final success of such attempts just so much the more probable. …

The truth, indeed, is, that in physical inquiries, the work of theory and observation must go hand in hand, and ought to be carried on at the same time, more especially if the matter is very complicated, for there the clue of theory is necessary to direct the observer. Though a man may begin to observe without any hypothesis, he cannot continue long without seeing some general conclusion arise; and to this nascent theory it is his business to attend, because, by seeking either to verify or to disprove it, he is led to new experiments, or new observations. … By this correction of his first opinion, a new approximation is made to the truth; and by the repetition of the same process, certainty is finally obtained. Thus theory and observation mutually assist one another; and the spirit of system, against which there are so many and such just complaints, appears, nevertheless, as the animating principle of inductive investigation. The business of sound philosophy is not to extinguish this spirit, but to restrain and direct its efforts. … It is therefore hurtful to the progress of physical science to represent observation and theory as standing opposed to one another. …

It cannot … be denied, that the impartiality of an observer may often be affected by system; but this is a misfortune against which the want of theory is not always a complete security. The partialities in favour of opinions are not more dangerous than the prejudices against them; for such is the spirit of system, and so naturally do all men's notions tend to reduce themselves into some regular form, that the very belief that there can be no theory, becomes a theory itself, and may have no inconsiderable sway over the mind of an observer. Besides, one man may have as much delight in pulling down, as another has in building up, and may choose to display his dexterity in the one occupation as well as in the other. The want of theory, then, does not secure the candour of an observer, and it may very much diminish his skill. The discipline that seems best calculated to promote both, is a thorough knowledge of the methods of inductive investigation; an acquaintance with the history of physical discovery; and the careful study of those sciences in which the rules of philosophizing have been most successfully applied.

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§ 9.3. Natural philosophy in France

PETER JIMACK, The French Enlightenment I: science, materialism and determinism, in Stuart Brown (ed.), *Routledge History of Philosophy, Volume V: British Philosophy and the Age of Enlightenment*, Routledge, 1996, Chapter 9.

By considering the evidence of geology (and simply ignoring the Bible), [Buffon] boldly drew conclusions about the immense age of the world. In his vision of an organically evolving universe, there was no room for final causes, which he rejected as misleading abstractions inhibiting true scientific enquiry. Whereas the great botanist Linnaeus, his exact contemporary, had a static view of nature, in which all plants and animals were created once and for all in permanent form for the glorification of God, Buffon emphasized the boundless creativity of 'nature' (rather than God): nature worked on a kind of trial and error basis, with its failures as well as its successes, producing monsters doomed to extinction as well as species equipped to survive and prosper by adapting to their environment. … It has been argued that his thought was not really transformist, and that he had no true idea of the evolution of species; nevertheless, by placing the study of biology in an historical perspective, by his vision of a dynamic, changing, natural world, he can truly be said to have anticipated Lamarck and Darwin. But such ideas were in the air. At almost exactly the same time as Buffon, Diderot too … outlined a similar evolutionary account of the animal world: the apparently wonderful way in which existing forms of life are adapted to their needs and their environment, far from being evidence of final causes, is merely the result of a natural process, in which many created forms turned out to be blind alleys, unable to survive or simply unable to reproduce. It is true that this theory had been expounded by Lucretius in his De natura rerum, but its re-emergence in the eighteenth century, in the context of post-Bacon post-Newton science, gave it an entirely new significance. …

One thinker at this time … went even further along the road towards Darwin. … Maupertuis tackled the question of generation by the study of heredity. … The transmission of acquired parental and even ancestral characteristics appeared to confirm the theory that in procreation the seed came from both parents. … Maupertuis explained this process of transmission by a kind of memory retained by the component parts of the maternal and paternal seed, each of which comes from a different part of the body and is destined to reproduce a similar part in the new being. However, chance deviations can then lead to the transformation and multiplication of species: "Could one not in this way explain how, starting from two single individuals, the multiplication of the most disparate species could have occurred? They would have owed their origin merely to a few chance productions …; and as a result of repeated deviations, there would have come about the infinite diversity of animals which we see today." (Maupertuis, 1751) …

La Mettrie's principal point … in L'Homme machine [1748] is that the so-called soul, which falls asleep with the body and needs food to continue functioning, is merely a way of talking about certain functions of the body. Man, then, is just another animal. … Differences of character and mind between men are all physiological in origin, as are differences between men and animals: La Mettrie is convinced (wrongly, as we now know) that the only thing preventing monkeys from learning to speak is their inadequate organs. …

French Enlightenment … thinkers … saw Christianity as fundamentally anti-human and therefore as a force for social evil, and their materialist-based atheism was as much a consequence of this hostility as a cause of it.

PHILLIP SLOAN, The Concept of Evolution to 1872, *The Stanford Encyclopedia of Philosophy*, Spring 2017 Edition, Edward N. Zalta (ed.), https://plato.stanford.edu/archives/spr2017/entries/evolution-to-1872/.

The general idea of the possibility of species change is an old concept. The reflections of … Greek Atomists … formed a Classical heritage on which later speculations could be developed. These Presocratic speculations combined naturalistic myths of origins with the workings of chance-like processes to create a naturalistic account of the origins of existing forms of life. … The Presocratic Atomist speculations were restated by the Roman poet Titus Lucretius (ca. 99–50 BCE) in book five of his On the Nature of Things (De Rerum natura). … Lucretius sets out a speculative account of the gradual origin of living beings from an initial atomic chaos through an undirected process that sorts out the best adapted forms and eliminates those not suited to their conditions.

Such speculative accounts were, however, opposed on several levels by the subsequent mainstream Platonic, Neo-Platonic, Aristotelian, and Stoic philosophical traditions. The writings of Plato (427–327 BCE), particularly his long creation myth, Timaeus, … provided an influential non-Biblical source for arguments against the Atomist tradition. This dialogue serves as the locus classicus for the notion of an externally-imposed origin of living beings through the action of an intelligent Craftsman (demiurgos) who created the cosmos and all living beings in accord with eternal archetypes or forms. … Plato's account initiated … the argument that organic beings could not be explained by chance-like processes either in their origins or in their complex design. Particularly as developed in the influential writings of the Greek physician Claudius Galenus (129–200 CE), a long heritage in the life sciences relied upon anatomy as evidence of rational design. These interpretations of "teleological design" interacted in complex ways with Jewish, Christian, and Islamic Biblical concepts of creation. …

In Aristotle's (384–322 BCE) seminal biological writings, the external teleology of a designer-creator was replaced by an internal teleological purposiveness associated with the immanent action of an internal cause—in living beings their informing soul (psuche)—which functioned as the formal, final and efficient cause of life. Aristotle also did not endorse the concept of an historical origin of the world, affirming instead the eternity of the world order. … Aristotle's apparent metaphysical requirement that the soul-as-form (eidos) be permanent and enduring … seemed … to amount to a denial of the possibility that natural species could change over time in their essential properties, even though local adaptation in "accidental" properties was fully possible. Since individual beings were dynamic composites of a material substrate and an immaterial and eternal form (eidos), the accidental differentiation of the substantial form in individuals did not affect the metaphysical endurance of the species. ...

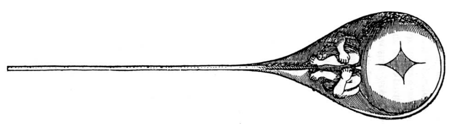
As Aristotle's views were developed in the West ..., these were received into a theological context. ... One issue that reverberates ... is Augustine's concept of a temporalized creation, developed in his work The Literal Interpretation of Genesis. This advocated an interpretation of divine creation through the formation at one moment in time of primordial seeds of each species, rather than their creation fully formed. This allowed Augustine to argue that species emerged sequentially in historical time rather than all at once. This thesis was also endorsed in substance by Thomas Aquinas. As he comments: "New species, if they appear, pre-exist in certain active powers". ...

An outstanding lacuna in Descartes's [mechanical philosophy] was his failure to incorporate the origins of living beings into this naturalistic story of creation by natural laws. ... Descartes attempted on several occasions to work out some linkage between his general natural philosophy and the embryological formation of living beings [without success]. ...

"And indeed all the Laws of Motion which are as yet discovered, can give but a very lame account of the forming of a Plant or Animal. We see how wretchedly Des Cartes came off when he began to apply them to this subject; they are form'd by Laws yet unknown to Mankind, and it seems most probable that the Stamina of all the Plants and Animals that have ever been, or ever shall be in the World, have been formed ab Origine Mundi [from the foundation of the world] by the Almighty Creator within the first of each respective kind." (Garden 1691) ...

The implausibility ... [of] purely mechanical accounts of embryogenesis ... suggested that universal mechanism could not deal with the question of embryological formation. "Mechanistic" epigenesis ... was for various reasons generally rejected in the latter seventeenth century. ... Mechanists ... opted instead for some version of a preformation theory of origins, or more accurately, a "pre-existence" theory of generation. On this view the new organism is not generated in secular time, but has pre-existed since the original creation of the world. ...

The main options one finds expressed in the official medical and professional gynecological literature of the 1670–1740 period ... assumed the pre-existence of forms in miniature, either encased in the ovaries of the female (Ovism), the original version, or after the discovery of spermatozoa in 1677, in the testes of the male (Vermism). ...



The theory of pre-existence was seen to solve many problems. First, it explained the intimate interrelation of structure and function that seemed to require the existence of parts of the organism in an integrated system. The heart presumably could not beat without ennervation, and the nerves could not exist without the heart. Consequently the entire organism must pre-exist, so the argument went. The existence of such integrated systems seemed otherwise difficult to explain by the sequential development of parts. ... Some kind of preformation of the embryo could be reconciled with the best microscopic observations of the late seventeenth and early eighteenth centuries. ...

The pre-existence theory effectively removed the organism from the effects of local circumstance and environmental conditions and placed the origin of species ... at a moment in the original creation. ... The immediate consequence of this theory was a new rigidity given to the concept of species that it had not possessed in the Aristotelian and Scholastic traditions. Pre-existence theory reinforced a sharp distinction between "essential" and "accidental" properties to a degree not implied by the prior tradition. This theory made it difficult to explain obvious empirical phenomena, such as monstrosity, the regeneration of lost parts, the resemblances of offspring to both parents, evidence for geographical variation, racial differences, or even the existence of hybrid forms such as the mule. It seemed necessary to attribute these anomalies to divine action at an original creation. These difficulties in the theory resulted in a variety of criticisms that were eventually to lead to the downfall of preexistence theory. ...

In 1744 ..., Carl von Linné (Linnaeus) offered a speculative theory ... in which he presented a narrative of a historical creation of the present world and its inhabitants by descent from a few original forms that had been created by divine action on a primeval equatorial island. In response to evidence for the sudden creation of new species that was drawn to Linnaeus's attention in 1744, he then developed a theory of how the original forms had likely hybridized to create new species in time. This Linnaean thesis of species origin by the hybridization of original forms was to have a long history, extending to the work of Gregor Mendel. The hybridization theory does not, however, imply a genuine historical change of species in response to external conditions in the way this developed in later transformism, and in some respects it was to form a source of opposition to Darwinian transformism. ...

As a scientific research program with institutional foundations, … the investigations of the transformation of species can be traced to the work of Enlightenment naturalists nearly a century before the publication of Darwin's Origin in 1859. It was particularly among French natural historians associated with the King's garden and natural history collection in Paris that these discussions took on a new form. Through the combination of theoretical reflection and institutional structure, these inquiries were given a precise development that they otherwise would not have attained. This explains, at least in part, why evolutionary theory emerged in the nineteenth century rather than in direct response to the transformations in natural philosophy of the seventeenth. … As the head of the King's Garden and Natural History Cabinet in Paris for the last half-century of the Bourbon monarchy (1739–88), Buffon was institutionally situated to become one of the major theoreticians of the natural-historical sciences of his era. …

Buffon reoriented natural history away from a primarily classificatory project to one which sought to analyze organisms in relation to their conditions of existence, to biogeography, and … historical geology. … Buffon … presented a secular and realist account of the origins of the earth and its life forms. …

Following the lead of his friend Maupertuis, Buffon revived the classical theory of the two seeds to explain animal generation, deriving the origin of the embryo from the mechanical mixture of these ingredients. … He explained the organization of the particles of these two seeds into a structured whole through microforces closely identified with Newtonian attractive forces that formed an organizing force-field, an "internal mould," that assimilated matter in the proper order for embryological development. … For this reason, Buffon was conceptually required to attribute some new powers to matter to account for vital action. … Buffon did not [believe] that an inert and common matter was sufficient for a plausible formulation of a theory of mechanical epigenesis. Vital properties therefore had to be attributed to a specific kind of matter confined to living beings, the organic molecules. … This introduction of the concept of "vital" matter by Buffon … broke with the uniformity of matter assumed by the Newtonian, Gassendist, and Cartesian traditions. …

The internal moulds and organic molecules … were … seen to arise by natural laws from the natural attraction of different shapes of matter and from the changes in matter as the earth cooled from its origin in matter cast off by the sun. Animals first originated by the spontaneous clumping together of these organic molecules on the cooling earth. … Buffon's liberal use of a form of spontaneous generation that allowed for the origin of even major animal groups from the clumping together of organic molecules as the earth cooled, rendered the actual derivation of forms from previous forms unnecessary. … The development of genuine transformist theories by Buffon's successors required a much more restricted use of the possibility of spontaneous generation. …

The encounter with a wide body of new data from the colonies and exploratory voyages returned to Paris during the course of the writing of the Natural History impressed Buffon with the degree to which species seemed to be affected by external circumstances such that from a single source numerous "degenerations," could arise in some groups. … Buffon's theory of the degeneration of species lumped the quadrupeds of both the Old and New worlds into a limited number of primary "families" and "genera" which had degenerated in time in response to migration from common points of origin to new locales. …

Buffon … raised the option of species transformism, only to reject it. … Buffon explicitly raised the possibility that all quadrupeds might have been derived from a single stem which "in the succession of time, has produced, by perfection and degeneration, all the other animals". In a move that has confused commentators ever since, Buffon then rejected this possibility. …

French Transformism After Buffon. … The following claims formed the core of … Lamarck's theory of species transformism:

The origin of living beings is initially through spontaneous generation. This action is confined, however, to the origins of the most structurally-simple forms of life—infusoria. All subsequent forms necessarily have developed in some way in time from the elementary beginning in these simplest microscopic forms. … [This includes for example] the transformation of primates into humans.

The causal agency behind this "ascending," rather than "degenerating," history of life over time is supplied by the activity of dynamic material agencies—caloric and electricity. These material agencies produce the spontaneous generation of the infusorians and also provide the impetus by which these give rise to forms of higher complexity, the radiarians, and so on up the series. Moving beyond the distinction of "inert" and "living" matter of his mentor Buffon, Lamarck's theories generally can be considered "vitalist" in inspiration in that they attribute a genuine dynamism to living matter and grant it the ability to create new forms and structures through its inherent powers. Lamarck's appeal to the causal role of Newtonian aetherial fluids, however, grounded his theory on a concept of active matter rather than on special superadded vital forces, and in this respect it can be termed a theory of vital materialism. …

The best-known feature of Lamarckianism in the subsequent tradition—the theory of transformism via the inheritance of acquired characters—functions as a subordinate, diversifying process through which major animal groups are adapted to local circumstances. Such adaptation is not, however, the primary cause of transformation from group to group. ... Consequently, in contrast to Darwin's later theory, the primary evolution of life is not through local adaptation. ...

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§ 9.4. Reader

Andrew Ede & Lesley Cormack, *A History of Science in Society: A Reader*, Broadview Press, 2007.

7.3. Lamarck.

The following reader selections are less essential. Read them if interested; otherwise rely on the brief summaries I provide below.

6.8. Playfair.

> The earth has changed over time: land was once water (seen in sea shell fossils, sand [= stone crushed by water] far from oceans); rivers have made canyons through rock.
>
> Antiquity of the earth: rock has been formed in layers; composed of segments of earlier rocks; organic fossils even in very old rock.
>
> Neptunist theory: crystalline rocks are more pure and primary than other rocks; crystals generated spontaneously in primeval oceans; other rocks derivative from these by mechanical processes such as grinding, friction, and pressure. But the chronology implied by rock strata does not fit this theory.

7.2. Cuvier.

> Rock strata show that: Land was once covered by ocean for a long period of time. Mountains formed after this, by old rock strata being violently pushed up. Material and organic composition of the strata are subject to long-term changes. Sea life older than land life. Strata of some regions show marine and land animal fossils alternating over the course of time.
>
> Proof of suddenness of catastrophes: Animals frozen almost instantaneously by ice age. Rock strata formed over very long time suddenly pressed up into mountains.

7.5. Lyell.

> Species are not rigidly fixed because: individual differences hereditary; cross-species breeding sometimes possible; geological records show progressive development toward more complex organisms.
>
> Tempting but dangerous to imagine speculative grand theories based on exciting new specimens from all corners of the globe. The evidence is complex, incomplete. No real evidence for one species transforming into another. For example, all dogs are fundamentally similar, and dissimilar from wolves.

§ 10. Darwin and evolution

§ 10.1. Discussion questions

10.1. Why was Darwin's theory not proposed 200 years earlier?

Geological evidence about age of the earth; uniformitanism. Common sense re: design (Newton, Kepler). Unworthy to consider creation the product of trial-and-error and dumb luck (religion but also science: e.g. light chooses quickest path). Institutional factors (gardens, museums) (Sloan).

10.2. Key themes in the debate between catastrophism versus uniformitanism in geology have direct counterparts in the debates on the origin of species. Discuss.

Whether incremental forces can explain major shifts. Whether it is plausible to stipulate vast time scales. Darwin (262 top left) makes the parallel explicit.

10.3. Is Darwin's theory comparable to Aristotle's and Buridan's ways of doing physics (§2) in its qualitative and sometimes even metaphorical character?

Some felt so at the time (e.g. comparison with Goethe, discussed in Sloan below).

10.4. Why does natural variation not "wash out" by "regression to the mean"? For example, if the entire human population has black hair, what is the probability that natural variation, which operates in tiny steps, could lead to large groups of blond people? If a person is born with slightly lighter hair, that person will likely partner with someone more typical of the population overall, and hence the light-hair variation will be diminished again in the next generation.

This was indeed an objection raised against Darwin at the time, by Jenkin as discussed in Sloan below.

10.5. Does group selection blur the boundaries between "blind" selection and the teleological explanations of nature in terms of purpose and design that Darwin's theory supposedly displaced?

Yes, arguably. In its most basic form, evolutionary theory emphasises competition between individuals. But altruistic behaviour (e.g. infertile workers in insect communities, or alarm calling among prey as a predator is approaching) is detrimental to the individual. So altruistic individuals should have poor reproductive success, and hence the trait of altruism should die out. Since it does not, Darwin felt that he had to appeal to group selection: just as competition between individuals produces individuals with adaptive traits, so competition between groups produces groups with adaptive traits—meaning traits that are good to the group, bu not the individual. But the idea of evolution looking at the "big picture" like this seems to run contrary to the core idea of evolution that apparent design is merely the outcome of blind processes at the individual level.

10.6. Why was Darwin so adamant in rejecting the possibility of a species adapting for the sole good of another species?

Because of the issue discussed above. Group selection is already problematic, although maybe necessary. All the more problematic if evolution takes an even more "big picture" perspective. That would erode the distinction between evolutionary theory and intelligent design so far that the two would almost be the same thing.

10.7. If selection can favour a group at the expense of individual organisms, then why can it not favour world population as a whole at the expense of individual species?

This is indeed the slippery slope problem discussed above. It remains an issue of debate to this day how exactly to account for altruism in a manner that does not undermine the core principles of evolutionary theory.

10.8. If there was no limit on food and other resources to sustain life, there would be no natural selection. Discuss.

Darwin (258 left) stresses this. Fierce competition essential for evolutionary progress.

10.9. What happened when European species were introduced to new continents, and what does this say about evolution?

Darwin (258 right). All species vastly overproduce offspring, hence competition.

10.10. Does sexual selection pose a problem for Darwin's theory? If selection favours pointless ornamental traits developed solely to impress mates, does this not undermine the claim that selection explains why species are so cleverly adapted to their environment? Is this analogous to saying that free-market capitalism favours companies providing useful innovations, but also companies specialising in deceptive marketing?

Darwin (259–260) doesn't seem to consider this a problem.

10.11. What evidence does Kelvin adduce that the earth is a cooling rock?

The further you dig, the hotter it gets. Seasonal variations superficial. Ice age should have cooled the earth to a much greater depth than observed, in absence of internal heat source.

10.12. What does the internal temperature of the earth suggest about its age, according to Kelvin?

Earth younger (say <50,000,000) than geologists guess (at least 90,000,000 years). Limit imposed by assumption that the earth's interior is solid. Liquid interior would be likely to have strange consequences in terms of precession/wobbling, lunar gravity. Thin crust would be subject to tidal influences as much as water, hence rise along with the oceans so that no tides would be observed.

10.13. What were the implications of Kelvin's estimate of the age of the earth for Darwinian evolution?

Too little time.

10.14. How old is the earth according to modern science? Why was Kelvin wrong?

1000 times older than estimated by Kelvin. Radioactivity was not yet known to Kelvin.

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§ 10.2. Reader

ANDREW EDE & LESLEY CORMACK, *A History of Science in Society: A Reader*, Broadview Press, 2007.

7.6. Darwin.

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§ 10.3. Age of the earth

7.10. Kelvin. Pages 280–281, and perhaps parenthetically 282–283.

T. W. KÖRNER, *Fourier Analysis*, Cambridge University Press, §58.

[Darwin:] "I am greatly troubled at the short duration of the world according to [Kelvin], for I require for my theoretical views a very long period before the Cambrian formation." "This objection ... is probably one of the gravest yet advanced."

In 1904 Rutherford announced that the radio-active decay of radium was accompanied by the release of immense amounts of energy and speculated that this could replace the heat lost from the surface of the earth. Kelvin's argument would then only give a minimum for the earth's age. [Rutherford:] "The discovery of the radio-active elements ... thus increases the possible limit of the duration of life on this planet, and allows the time claimed by the geologists and biologists for the process of evolution." "[When giving a lecture on this,] I ... spotted Lord Kelvin in the audience and realised I was in for trouble at the last part of the speech dealing with the age of the earth, where my views conflicted with his. To my relief, Kelvin fell fast asleep, but as I came to the important point, I saw the old bird sit up, open an eye and take a baleful glance at me! Then a sudden inspiration came, and I said Lord Kelvin had limited the age of the earth, provided that no new source of heat was discovered. That prophetic utterance refers to what we are now considering tonight, radium! Behold! the old boy beamed upon me."

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§ 10.4. Darwin in context

PHILLIP SLOAN, The Concept of Evolution to 1872, *The Stanford Encyclopedia of Philosophy*, Spring 2017 Edition, Edward N. Zalta (ed.), https://plato.stanford.edu/archives/spr2017/entries/evolution-to-1872/.

A long historiographical tradition has emphasized the endemic developments in British natural history, geology, and British versions of natural theology as the primary background for understanding the origins of Darwin's own views. The new awareness of the importance of issues raised within British medical discussions, and the impact of French and German discussions on the British context have only recently been appreciated in rich detail. ...

Darwin's theory does not deal with the creation of the world and the ultimate origins of life through naturalistic means, and therefore was more restricted in its theoretical scope than its main predecessors deriving from the reflections of Buffon and Lamarck. ... The foremost difference distinguishing Darwin's theory from previous explanations of species change centers on the different way in which he explained how this process occurred. Prior theories, such as Lamarck's, relied on the inherent dynamic properties of matter. ... The developmental change of species was not primarily through an adaptive process. ... Darwin's emphasis ... on the factors controlling population increase, rather than on a causal theory of life, ... accounts for many of the differences between Darwin's theory and those of his predecessors and contemporaries. ...

These differences [are encapsulated] in the concept of natural selection as a central ingredient of Darwinian theory. ... Darwin's exact meaning, and the varying ways he stated the principle ..., however, varied. ...

[His earliest notes] transferred the concept of selection of forms by human agency in the creation of the varieties of domestic animals and plants, to the active selection in the natural world by an almost conscious agency, a "being more sagacious than man (not an omniscient creator)". This agency selects out those features most beneficial to organisms in relation to conditions of life, analogous in its action to the selection by man on domestic forms in the production of different breeds. ... [Darwin] referred to the selective action of a wise imaginary being whose selection was made with greater foresight and wisdom than human selection. ... The domestic breeding analogy ... repeatedly functions for Darwin as the principal empirical example to which he could appeal at several places in the text as a means of visualizing the working of natural selection in nature, and this appeal remains intact through the six editions of the Origin. ...

In the initial definition of natural selection presented in the first edition of Darwin's text, it is characterized as "preservation of favourable variations and the rejection of injurious variations". As Darwin elaborated on this concept in the first edition, he continued to describe natural selection in language suggesting that it involved an intentional selection. ... Criticisms that quickly developed over the overt intentionality imbedded in such passages, however, led Darwin to revise the argument. ... [In later edition,s] he explicitly downplayed the intentional and teleological language ..., denying that his appeals to the selective role of "nature" were anything more than a literary figure. ... He also regrets in his correspondence his mistake in not utilizing the designation "natural preservation" rather than "natural selection" to characterize his principle. [Darwin's] adoption in the fifth edition of 1869 of Herbert Spencer's designator "survival of the fittest" as a synonym for "natural selection" further emphasized this shift of meaning. ...

Anticipating at first publication several obvious lines of objection, Darwin devoted much of the text of the original Origin to offering a solution in advance to predictable difficulties. As Darwin outlined these main lines of objection, they included[:] The apparent absence of numerous slight gradations between species, both in the present and in the fossil record, of the kind that would seem to be predictable from the gradualist workings of the theory. The existence of organs and structures of extreme complexity, such as the vertebrate eye, structures that had since the writings of Galen in Hellenistic antiquity served as a mainstay in the argument for external teleological design, needed some plausible explanation. The evolution of the elaborate instincts of animals and the puzzling problem of the evolution of social insects that developed sterile neuter castes. ... The complex issue of the traditional distinction between natural species defined by interfertility, and artificial species defined by morphological differences, also required a full chapter of analysis in which he sought to undermine the absolute character of the interbreeding criterion as a sign of fixed natural species. ...

To each of these lines of objection Darwin offered his contemporaries plausible, if not for many critics compelling, replies. ... For reasons related both to the condensed and summary form of public presentation, and also to the bold conceptual sweep of the theory, the primary argument of the Origin could not gain its force from the data presented by the book itself. Instead, it presented an argument from unifying simplicity, gaining its force from the ability of Darwin's theory to draw together a wide variety of issues in taxonomy, comparative anatomy, paleontology, biogeography, and embryology under the simple principles worked out in the first four chapters. In important respects, this might be seen as reflecting the impact of Whewell's methodology. ... Dealing with the question of the vertebrate eye ..., for example, Darwin offered a few speculations on how such a structure could have developed by the gradual selection upon the rudimentary eyes of invertebrates. But the primary solution offered was the ability of his theory to draw together in its total argument numerous lines of inquiry that would not otherwise receive a coherent explanation. In such a case one would "admit that a structure even as perfect as the eye of an eagle might be formed by natural selection, although in this case he does not know any of the transitional grades". Here again, one might see Whewell's notion of a "consilience of inductions" at work. ...

Reception of Darwin's Theory. ... The broad sweep of Darwin's claims, the brevity of the empirical evidence actually supplied in the text, and the implications of his theory for several more general philosophical and theological issues, immediately opened up ... controversy. ... [However,] the popular image of a great public outcry against Darwin's work has been shown by careful historical analyses to be generally mythical, or at least in need of careful discrimination by social group, national tradition, and religious affiliation. ...

In France, Darwin's theory was received against the background of the prior debates over transformism. ... These debates had been resolved, at least within official Parisian science, in favor of ... anti-transformism. Darwin was, as a consequence, viewed as involved in a tradition of rejected science by leading figures of French science. As the leading physiologist and methodologist of French Science, Claude Bernard (1813–78) put this in 1865, Darwin's theory was to be regarded with those of "a Goethe, an Oken, a Carus, a Geoffroy Saint Hilaire," locating it within speculative philosophy of nature rather than granting it the status of genuine science. ... In the Germanies, ... the philosophical traditions of German Naturphilosophie, Romanticism, and the Idealism of Fichte and Hegel formed a fertile philosophical ground into which Darwin's developmental view of nature and theory of the transformation of species was often assimilated. ...

The enthusiastic advocacy of Darwinism in Germany by University of Jena professor of zoology Ernst Heinrich Haeckel (1834–1919) made Darwinism a major player in the polarized political and religious disputes of Bismarckian Germany. ... Haeckel advocated a materialist monism in the name of Darwin, and used this as a stick with which to beat traditional religion. Much of the historical conflict between religious communities and evolutionary biology can be traced back to Haeckel's polemical writings, which went through numerous editions and translations, including several English and American editions that appeared into the early decades of the twentieth century. ... Darwin's position on [this] remained unclear ... [and] allowed many before 1872 to see Darwin as more open to religious views than those of some of his popularizers. ...

Darwin developed his claims for explanatory superiority against a doctrine of "special creation," which he posed as the main alternative to his account. This stylized opposition to "creationism," rather than to the traditions of Cuvier and Buffon that ... formed the basis for scientific opposition to transformism, presented Darwin's evolution in opposition to the thesis of a direct supernatural action of an intelligent deity who created each individual species exactly in its present condition. This was a point of considerable criticism by contemporaries such as Richard Owen, who held no such view. But this rhetorical strategy served to define much of the popular debate over Darwin's theory in the succeeding period, and continues to define it to the present. ...

If we concentrate on the reception by workers with professional positions in museums, laboratories, ... universities and membership in elite scientific societies, Darwin's reception was varied. ... Only rarely did members of the scientific elites accept and develop Darwin's theories exactly as they were presented in his texts. ...

Of central importance in analyzing this complex professional reception was the role assigned to normal individual variation and its causes. In the initial public presentation of his theory, Darwin had relied on the novel claim that small individual variations—the kind of differences considered by an earlier tradition as merely "accidental"—formed the raw material upon which, by unlimited addition through the action of natural selection, major changes could be produced sufficient to explain the differences in all the various forms of life over time. The causes of this variation were, however, left unspecified by Darwin. Variation was presented simply as governed by "unknown laws." ... Critics focused attack on the claim that such micro-differences between individuals could be accumulated over time without natural limits. ...

Jenkin cited empirical evidence from domestic breeding that suggested a distinct limitation on the degree of variation, and denied that selection upon this could be taken to the extent assumed by Darwin. Using a loosely mathematical argument, Jenkin argued that the effects of intercrossing would continuously swamp deviations from the mean ... and result in a tendency of the variation in a population to return to mean values over time. For Jenkin, Darwin's reliance on continuous additive deviation was presumed undermined by this argument, and only more dramatic and discontinuous change—something Darwin explicitly rejected—could account for the origin of new species. ... [In response, Darwin] shifted his emphasis away from his early reliance on normal individual variation, and gave new status to what he now called "strongly marked" variation. The latter was now the form of variation to be given primary evolutionary significance, and presumably this was more likely to be transmitted to the offspring, although details are left unclear. In this form it presumably could be maintained in a population against the tendency to swamping by intercrossing. ... The debates over variation placed Darwinism in a defensive posture that forced its supporters into major revisions in the Darwinian research program. ...

Jenkin also argued that the time needed by Darwin's theory was simply inadequate, supporting this claim by appeal to the physical calculations of the probable age of the earth [based on the gradual cooling of the earth's interoior] presented in publications by Jenkins's mentor, ... physicist William Thompson (Lord Kelvin) (1824–1907). On the basis of Thompson's quantitative arguments, Jenkin judged the time since the origins of the solar system to be insufficient for the Darwinian gradualist theory of species transformation to take place. Jenkin's multi-pronged argument gave Darwin considerable difficulties and set the stage for more detailed empirical inquiries into variation and its causes. The time difficulties were only resolved in the twentieth-century with the discovery of radioactivity. ...

Human Evolution and the Descent of Man. The publication of the *Descent and Selection in Relation to Sex* in 1871 created a watershed in the public reception of Darwin's views. ... [Before this,] Darwin's own views on human evolution remained unclear, with only one vague sentence on the issue in the Origin itself. The Descent, however, drew his more radical reflections to the fore, and seemed to many of his readers, even those previously sympathetic to the Origin, to throw Darwin's weight behind materialist and anti-religious forces. ... Darwin's extension of his theory to a range of questions traditionally discussed within philosophy, theology, and social and political theory, hardened the opposition of many religiously-based communities to evolutionary theory. ...

Most striking in comparing the Origin to the Descent was the strong emphasis on the workings of the secondary process of sexual selection in the animal kingdom. Sexual selection—the selection of females by males or vice versa for breeding purposes—had played a minor role in the original argument of the Origin. ... Darwin now developed this secondary form of selection in extensive detail as a factor in evolution that could even work against ordinary natural selection. Sexual selection could now be marshalled to explain both sexual dimorphism and also those character and properties of organisms—elaborate feeding organs, bright colors on fish and birds, seemingly maladaptive structures such as the great horn on the Rhinoceros beetle—that would appear to be anomalous outcomes of ordinary natural selection working to the optimal survival of organisms in nature. ...

TIMOTHY SHANAHAN, *The Evolution of Darwinism: Selection, Adaptation and Progress in Evolutionary Biology*, Cambridge University Press, 2004.

Darwin … considered how the "transmutation" of one species into another could account for some of the observations made during his voyage. For example, finches on the Galapagos Archipelago (six hundred miles due west of Ecuador) differed dramatically from one island to another, yet all resembled finches on the South American mainland in their basic structure, despite the fact that the volcanic islands represented a quite different environment. The resemblance could be explained, Darwin realized, by supposing that a few individuals from the mainland were carried by storms out to the islands, where their descendants then became modified to each different island environment. Over sufficient time, each form had evolved into a new species.

Darwin also realized that this explanation could be generalized. In a world characterized by environmental change, some individuals will vary in a way that better fits them to the new circumstances. With sufficient change, the descendants of these individuals will form new species. Others will fail to adapt and will go extinct, leaving gaps between those forms remaining. This would account for the large differences between some species but not between others.

It wasn't so much Darwin's advocacy of evolution that was novel or disturbing. By 1859 evolutionary ideas had become almost commonplace. Rather, what was disconcerting was the idea that natural selection operating on chance variations produced the diversity and apparent design in nature. Darwin's theory seemed to make evolution more blind and haphazard than anyone had imagined.

One way to appreciate the novelty of these aspects of Darwin's theory is to contrast it with an account of evolution in which chance variation and natural selection are not key explanatory elements.

Lamarck (1744–1829) stands out as the most important evolutionary theorist before Darwin. … According to Lamarck, "Nature, in successively producing all species of animals, beginning with the most imperfect or the simplest, and ending her work with the most perfect, has caused their organization gradually to become more complex". The various classes of organisms we observe today (e.g., insects, fishes, amphibians, reptiles, birds, mammals) were explained as the result of this primary complexifying process. … As organisms move up this ladder of organization, vacant morphological space at the bottom is continually being replenished with lower forms (e.g., worms) arising from spontaneous generation from inanimate matter. In Lamarck's view, biogenesis (the origination of life from nonlife) was not a singular unique event in the history of the earth, but rather a continuous and ongoing process. It follows that different lineages begin their ascent up the ladder of complexity at different times. Thus part of the diversity we observe is simply the result of different lineages having begun at different times, with the secondary result that each has so far progressed to a different stage in its upward ascent. The lineage that includes Homo sapiens is the oldest, because it alone has reached the highest stage of development.

As Lamarck noted, "The organization of animals, in its growing complexity, from the least to the most perfect, presents only an irregular gradation of which the whole extent displays a large number of anomalies or deviations which have no apparent order in their diversity". In order to account for this diversity of forms, Lamarck realized, there must be other forces at work besides the intrinsic drive toward perfection. To explain this level of diversity Lamarck posited a secondary process of adaptation to environmental conditions. To survive, organisms must be able to interact successfully with their environments which are always changing. … This is [based on] the infamous "inheritance of acquired characteristics" doctrine usually associated with Lamarck. … The explanation for why giraffes have long necks is that in the past individual giraffes stretched their necks to reach the higher foliage, this altered feature was passed on to offspring, and the process was repeated until the long-necked creatures of today appeared. … [But this was to Lamarck a minor force subordinated to the main] directional unfolding from lesser to greater complexity. … "The progression in the complexity of organization suffers, here and there, in the general series of animals, from anomalies produced by the influence of the circumstances of the environment, and by those of the habits contracted" (Lamarck).

Despite the fact that Lamarck is now considered to have gotten it almost completely wrong, his theory was nonetheless a serious effort to explain certain accepted but problematic facts about nature. First, many forms uncovered in the fossil record are no longer extant. Likewise, there is no evidence in the fossil record of many of the forms we see today. Clearly there has been a tremendous replacement of organic forms over time. Second, an inspection of extant animals shows that they form a graded series of increasing complexity. Organisms can be more or less arranged along a scala naturae ranging from bacteria to Homo sapiens, with each step along the ladder exhibiting greater complexity. Third, organisms display amazing diversity, which must be explained in some way. Finally, organisms seem exquisitely well-suited for their particular environments. Organic replacement, increasing complexity, diversity, and fitness are four primary biological phenomena Lamarck correctly recognized as in special need of explanation. Providing correct explanations of each is, of course, important, but the importance of correctly identifying and taking seriously the problems to be solved should not be underestimated. Lamarck's contributions in this regard were seminal.

Others before Darwin had attempted to explain the diversity of living things, but Darwin provided a new kind of theory by reversing what could be taken for granted, and what required special explanation. … [Before Darwin] individual variations [were] viewed as a kind of "noise". … Darwin's … theory entails a complete reversal of these approaches. According to this view, individual variability is fundamental, and the existence of types (e.g., species) requires special explanation. Species exist precisely because of naturally occurring variations. Organic variation is the natural result of the absence of interfering forces. Uniformity (species) results from interfering forces (e.g., geographical isolation, which prevents individuals from interbreeding). For example, whereas Aristotle and Lamarck would explain variations in the height of oak trees as due to interfering forces affecting the oak's natural tendency, Darwin would treat the variation as natural, with the fact that the trees instantiate the restricted height distribution they do as in need of explanation. … Aristotle and Lamarck each treat variations as somewhat unfortunate consequences of imperfections in the process; Darwin treats variations as the indispensable precondition of continuing evolutionary development.

Selection is often thought of as operating on individual organisms. … This tidy picture is complicated when one considers Darwin's treatment of certain "special difficulties." … Darwin considers "one special difficulty, which at first appeared to me insuperable, and actually fatal to my whole theory. I allude to the neuters or sterile females in insect-communities". … "For these neuters often differ widely in instinct and in structure from both the males and fertile females, and yet, being sterile, they cannot propagate their kind". "The difficulty lies in understanding how such correlated modifications of structure could have been slowly accumulated by natural selection".

Sterile individuals, by definition, do not reproduce. Instead, they appear to sacrifice their reproductive interests for the benefit of the rest of the hive or colony. If natural selection favors those individuals more proficient at reproducing themselves, then sterile individuals are obviously at a distinct disadvantage relative to their more prolific conspecifics, and should be eliminated from the struggle for existence in short order. … The existence of sterile castes among social insects seems inexplicable on the assumption that all selection is for individually advantageous characteristics. What possible individual advantage can accrue to being sterile? There appears to be none. How, then, is the presence of sterile castes to be explained?

Despite the serious threat it posed to his theory, Darwin apparently thought that this problem could be handled rather easily …: "… if … it had been profitable to the community that a number should have been annually born capable of work, but incapable of procreation, I can see no very great difficulty in this being effected by natural selection. … Thus I believe it has been with social insects: a slight modification of structure, or instinct, correlated with the sterile condition of certain members of the community, has been advantageous to the community: consequently the fertile males and females of the same community flourished, and transmitted to their fertile offspring a tendency to produce sterile members having the same modification."

The key idea in this passage is that in addition to operating on individually advantageous characteristics, selection can also operate on characteristics "profitable to the community." Recent explanations (from the mid-1960s on) of sterile castes among social insects have focused on explanations in terms of benefits conferred on genetic relatives by sterile individuals. … Although they are themselves reproductively disadvantaged by being sterile, nonetheless by helping their relatives to survive and reproduce they are assisting in the propagation of copies of their genes, many of which are shared with close relatives. Instead of passing on their genes directly through producing offspring, sterile individuals do so indirectly through the offspring of their fertile relatives. … [But this] was not clearly understood until well over a century after the publication of the Origin.

[Darwin] writes: "In social animals [natural selection] will adapt the structure of each individual for the benefit of the community; if each in consequence profits by the selected change". This supposes that selection will adapt the structure of each individual to the benefit of the community *only if* such adaptation *also* benefits the individual. So in this case benefit to the individual is primary. However, in the sixth edition of the *Origin* (1872) the passage is changed to read as follows: "In social animals [natural selection] will adapt the structure of each individual for the benefit of the whole community; if the community profits by the selected change". The change of emphasis has now been reversed! Darwin's view is not altogether as clear as we might like, so we are left with some uncertainty in representing his thought.

[Altruism in humans poses a similar problem:]

"It is extremely doubtful whether the offspring of the more sympathetic and benevolent parents, or of those which were the most faithful to their comrades, would be reared in greater numbers than the children of selfish and treacherous parents of the same tribe. He who was ready to sacrifice his life, as many a savage has been, rather than betray his comrades, would often leave no offspring to inherit his noble nature. The bravest men, who were always willing to come to the front in war, and who freely risked their lives for others, would on an average perish in larger numbers than other men. Therefore it seems scarcely possible . . . that the number of men gifted with such virtues, or that the standard of their excellence, could be increased through natural selection."

Darwin's solution to this problem lay in considering benefits accruing to tribes constituted by such virtuous men:

"Although a high standard of morality gives but a slight or no advantage to each individual man and his children over the other men of the same tribe, yet … an advancement in the standard of morality … will certainly give an immense advantage to one tribe over another. There can be no doubt that a tribe including many members who, from possessing in a high degree the spirit of patriotism, fidelity, obedience, courage, and sympathy, were always ready to give aid to each other and to sacrifice themselves for the common good, would be victorious over most other tribes; and this would be natural selection."

Consequently, in addition to selection operating on differences among individual organisms ("organism selection"), Darwin also recognized selection operating on differences among groups of organisms ("group selection").

There is one issue concerning selection that was never an issue for Darwin, namely, whether selection might operate on one species for the good of another. He was absolutely clear that natural selection could never be understood to act in this way: "Natural selection … will adapt the structure of each individual for the benefit of the community; if each in consequence profits by the selected change. What natural selection cannot do, is to modify the structure of one species, without giving it any advantage, for the good of another species."

Later in the same work Darwin put the point in the strongest possible terms: "Natural selection cannot possibly produce any modification in any one species exclusively for the good of another species. … If it could be proved that any part of the structure of any one species had been formed for the exclusive good of another species, it would annihilate my theory, for such could not have been produced by natural selection." "Natural selection will never produce in a being anything injurious to itself." (Chapter 1)

Darwin's bold move of introducing the idea of selection for group benefit … created a troubling tension within his theory. … If the evolution of characteristics detrimental to their possessors is categorically excluded in the one case (between species), why not also in the other (within a given species)? … Both seem to contradict the fundamental logic of Darwin's theory. (Chapter 2)

§ 11. Relativity Theory

§ 11.1. Discussion questions

11.1. What is "relative" in relativity theory?

Space (length) and time are not absolute (universal, objective, the same for everyone), but only have meaning relative to a particular reference frame.

11.2. Relativity theory vindicates 17th-century opposition to the notion of absolute space. Discuss.

Yes. The philosophically dubious aspects of Newtonian physics had been ignored for a long time because it was such an extraordinarily successful theory. But indeed relativity theory made progress precisely by addressing those long-ignored issues.

11.3. Relativity theory is an example of a scientific advance driven by philosophical, rather than internal scientific, considerations. Discuss.

In part, yes. Critiques of e.g. Newtonian absolute space such as that of e.g. Mach were philosophical in character. They challenged conceptual aspects of Newtonian physics, but did not disagree with Newtonian physics about any concrete empirical question.

11.4. What does Einstein mean when he calls relativity theory an "analytic" rather than a "constructive" theory? What are other examples of analytic and constructive theories? What are the strengths and weaknesses of each approach? Cf. §7.5.

Constructive = based on hypotheses (e.g. molecular hypothesis to explain gases). As Descartes/rationalism. In Einstein, less emphasis on plausibility of the hypotheses. "Completeness, adaptability, clearness." Analytic = principles derived empirically; mathematical laws independent of representations. As Newton. Relativity goes here. "Logical perfection, security of foundations."

11.5. Suppose I place two flashlights at opposite ends of a room and place a stick across the room so that it touches both of their on-buttons. When I press down on the stick both flashlights turn on at the same time. How does this relate to Einstein's definition of simultaneity?

Must be impossible because nothing can go faster than c. Also lacks association with clocks.

11.6. Does section "XI. The Lorentz Transformation" of Einstein (1916) say something conceptual, in addition to merely saying that there are formulas for what has already been said?

Consistency. The formulas show that there is a coherent way of translating back and forth between different reference frames, and hence that the theory does not lead to contradictions.

11.7. To what extent does relativity theory require us to reject the assumption that spatial relations can be described by ordinary Euclidean geometry? Distinguish between the special and the general theory.

Special theory: Yes for moving rulers, no for stationary ones. General theory: Yes tout court, because e.g. rotating disc would have non-Euclidean geometry yet be equally valid reference frame.

11.8. Special relativity views the motion of light as fundamentally different than the motion of ordinary objects. Discuss.

Yes, because velocity does not depend on velocity of object firing it.

11.9. In the classical Galilean thought experiment, the person in the cabin cannot tell whether the room is moving or not. But he can tell that the ship is in a gravitational field (that is, in the vicinity of a massive body, the earth). Or can he?

No. Alternative explanation: motion.

11.10. What is the relation between Einstein's theory of relativity and operationalism/positivism?

Both cause and effect. Pre-Einstein positivism such as that of Mach was a big influence on Einstein. The success of Einstein gave these ideas further credibility, and they were then enthusiastically promoted by people like the logical empiricists and Bridgman.

11.11. The transition from Newtonian physics to special relativity, and the transition from special relativity to general relativity, are both examples in which the new theory incorporates the old as a limiting case. Discuss.

General = special in absence of gravity. Newton ≈ special relativity when all speeds are much lover than the speed of light.

Worksheet

(REL) No physical experiment can determine whether a closed system (such as the inside of a train car) is at rest or is moving at uniform velocity.

(ADD) speed of a projectile = speed of projecter + firing speed

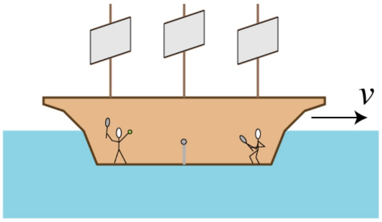
(C) The speed of light (in vacuum) is a universal constant, c.

(ABS T) There is an absolute, universal standard of time, captured by clocks.

(ABS S) There is an absolute, universal standard of space, or length, captured by rulers.

Special relativity accepts REL and C and rejects the others. Let's see why.

11.12. Argue that ADD is what makes REL work, in a classical context such as this:



If my tennis serve on land has speed s, and the ship is going at speed v, then my serve will go at speed $v+s$ when I'm facing forwards, and $v-s$ when I'm facing backwards. But since the room itself has speed v, the speed of my serve in relation to objects in the room is $\pm s$ in either case.

11.13. Projectiles are fired at equal speed from each end of the ship toward an observer in the middle of the room. They reach the observer at the same time. How does the observer explain this? How does an outside observer (stationary at the shore) explain this? Use ADD.

Observer in ship: The projectiles travelled equal distances at equal speeds, so they reached the midpoint at the same time. Outside observer: The projectiles had different speeds: $v+s$ for the one fired forwards and $v-s$ for the one fired backwards. But the observer in the ship was moving toward the projectile fired backwards, hence shortening the distance it needed to travel and increasing the distance the forward-fired projectile needed to cover. Therefore, although the backwards projectile was slower, it nevertheless reached the observer at the same time, since it had a shorter distance to travel.

11.14. Argue that C contradicts ADD.

The tennis balls had speed $v \pm s$, but the light ray always have speed c regardless of how the light source was travelling.

11.15. So how does Problem 11.13 change if the projectiles are light rays, assuming C? If the observer sees one ray before the other, is REL violated?

The observer will not conclude that REL was violated but rather that the two light rays were not fired at the same time. If you want to argue with him and say that they truly were fired at the same time, then you would need to come to an agreement with him about an empirically verifiable definition of simultaneity. But simultaneity cannot be coherently defined except in terms of light rays reaching the midpoint at the same time. So it is impossible to test empirically whether light rays fired at the same time reach the midpoint at the same time, because these two things are equivalent by definition.

11.16. Argue that this problem can be solved by ditching ABS T while retaining REL and C.

The explanation given in the previous problem is based on assuming that C and REL are both true. But it implies that ABS T is false, because light rays that are fired "at the same time" according to the outside observer will not reach the midpoint at the same time, and hence were not fired "at the same time" according to the observer in the ship. Hence different observers disagree about which events happens simultaneously, which means that ABS T is violated.

11.17. So fas we have seen that REL and C force us to give up ADD and ABS T. What about ABS S?

Argue that ABS S must go as well, by considering the question: How long will take a light ray fired from one end of the room to hit the opposite wall? Answer from the point of view of an observer in the ship, and one on the shore.

Observer in the ship: Time it takes for light speed c to cover the length of the room, L. Observer on the shore: Time it takes for light speed c to cover the length of the room, L, plus the time to catch up with how far the ship moved in the meantime. These two must be equal because of REL (otherwise there would be an observationally detectable difference between their reference frames, and hence a way of telling objectively who was "really" moving). Hence L must be different according to the two observers: the observer on the shore thinks L is shorter.

11.18. General relativity is based on an even more radical principle:

(G REL) No physical experiment can determine whether a closed system (such as the inside of a train car) is at rest or is moving in any way, whether uniformly or not.

How could the tennis players in the ship not know whether they are accelerating or not?

All effects they could experience due to motion could equally well have been due to gravitational effects. For example, a very heavy object suddenly appearing outside the room would draw objects toward it and make the tennis players feel "sucked back" in just the same way they would experience an acceleration of the ship. All acceleration and deceleration experiences can be equivalently interpreted in terms of changes in the gravitational field (that is, very heavy objects coming and going outside the ship).

ALBERT EINSTEIN, *Relativity: The Special and General Theory*, 1916, translation by Robert W. Lawson, Methuen & Co., third ed.

VII. The Apparent Incompatibility of the Law of Propagation of Light with the Principle of Relativity.

The simple law of the constancy of the velocity of light c (in vacuum) is justifiably believed by the child at school. Who would imagine that this simple law has plunged the conscientiously thoughtful physicist into the greatest intellectual difficulties? [But it has, because] this result comes into conflict with the principle of relativity. [The principle of relativity is this. According to Newtonian mechanics, uniform motion in a straight line is physically indistinguishable from rest. If we are enclosed in a box, such as a room below deck on a ship or a closed train car, then there should be no way to determine by means of experiment whether we are moving uniformly or standing still. But if the speed of light has a universal velocity c that is always the same, then measurements involving light rays could be used to determine whether we are moving or not, it would seem. For instance, place flashlights at each end of a room. Then turn them on simultaneously, and check whether the light pulses reach the middle of the room at the same time. If the room is moving, then the midpoint of the room has moved while the rays were in the air, making the distance each ray needs to cover longer or shorter. An observer in the middle of the room should therefore see one light before the other, and hence be able to detect the "forward" direction of travel.]

In view of this dilemma there appears to be nothing else for it than to abandon either the principle of relativity or the simple law of the propagation of light in vacuo. … Investigations of … the electrodynamical and optical phenomena connected with moving bodies show that experience in this domain leads conclusively to a theory of electromagnetic phenomena, of which the law of the constancy of the velocity of light in vacuo is a necessary consequence. Prominent theoretical physicists were therefore more inclined to reject the principle of relativity. …

At this juncture the theory of relativity entered the arena. As a result of an analysis of the physical conceptions of time and space, it became evident that in reality there is not the least incompatibility between the principle of relativity and the law of propagation of light, and that by systematically holding fast to both these laws a logically rigid theory could be arrived at.

VIII. On the Idea of Time in Physics.

Lightning has struck the rails on our railway embankment at two places A and B far distant from each other. I make the additional assertion that these two lightning flashes occurred simultaneously. If I ask you whether there is sense in this statement, you will answer my question with a decided "Yes." But if I now approach you with the request to explain to me the sense of the statement more precisely, you find after some consideration that the answer to this question is not so easy as it appears at first sight. …

We encounter the same difficulty with all physical statements in which the conception "simultaneous" plays a part. The concept does not exist for the physicist until he has the possibility of discovering whether or not it is fulfilled in an actual case. We thus require a definition of simultaneity such that this definition supplies us with the method by means of which, in the present case, he can decide by experiment whether or not both the lightning strokes occurred simultaneously. As long as this requirement is not satisfied, I allow myself to be deceived …, when I imagine that I am able to attach a meaning to the statement of simultaneity. …

After thinking the matter over for some time you then offer the following suggestion with which to test simultaneity. By measuring along the rails, the connecting line AB should be measured up and an observer placed at the mid-point M of the distance AB. This observer should be supplied with an arrangement (e.g. two mirrors …) which allows him visually to observe both places A and B at the same time. If the observer perceives the two flashes of lightning at the same time, then they are simultaneous.

I am very pleased with this suggestion, but for all that I cannot regard the matter as quite settled, because I feel constrained to raise the following objection:

"Your definition would certainly be right, if only I knew that the light by means of which the observer at M perceives the lightning flashes travels along the length AM with the same velocity as along the length BM. But an examination of this supposition would only be possible if we already had at our disposal the means of measuring time. It would thus appear as though we were moving here in a logical circle."

After further consideration … you declare:

"I maintain my previous definition nevertheless, because in reality it assumes absolutely nothing about light. There is only one demand to be made of the definition of simultaneity, namely, that in every real case it must supply us with an empirical decision as to whether or not the conception that has to be defined is fulfilled. That my definition satisfies this demand is indisputable.

That light requires the same time to traverse the path AM as for the path BM is in reality neither a supposition nor a hypothesis about the physical nature of light, but a stipulation which I can make of my own free will in order to arrive at a definition of simultaneity." . . .

We are thus led . . . to a definition of "time" in physics. For this purpose we suppose that clocks of identical construction are placed at the points A, B, and C of the railway line and that they are set in such a manner that the positions of their pointers are simultaneously (in the above sense) the same. Under these conditions we understand by the "time" of an event the reading (position of the hands) of that one of these clocks which is in the immediate vicinity (in space) of the event. In this manner a time-value is associated with every event which is essentially capable of observation.

This stipulation contains a further physical hypothesis, the validity of which will hardly be doubted without empirical evidence to the contrary, [namely] that all these clocks go at the same rate if they are of identical construction.

IX. The Relativity of Simultaneity.

Up to now our considerations have been referred to a particular body of reference, which we have styled a "railway embankment." We suppose a very long train travelling along the rails with the constant velocity v. People travelling in this train will with a vantage view the train as a rigid reference-body (co-ordinate system); they regard all events in reference to the train. Then every event which takes place along the line also takes place at a particular point of the train. Also the definition of simultaneity can be given relative to the train in exactly the same way as with respect to the embankment. As a natural consequence, however, the following question arises: Are two events (e.g. the two strokes of lightning A and B) which are simultaneous with reference to the railway embankment also simultaneous relatively to the train? We shall show directly that the answer must be in the negative.

When we say that the lightning strokes A and B are simultaneous with respect to be embankment, we mean: the rays of light emitted at the places A and B, where the lightning occurs, meet each other at the mid-point M of the length AB of the embankment. But the events A and B also correspond to positions A and B on the train. Let M' be the mid-point of the distance AB on the travelling train. Just when (as judged from the embankment) the flashes of lightning occur, this point M' naturally coincides with the point M but it moves . . . with the velocity v of the train. If an observer sitting in the position M' in the train did not possess this velocity, then he would remain permanently at M, and the light rays emitted by the flashes of lightning A and B would reach him simultaneously, i.e. they would meet just where he is situated. Now in reality (considered with reference to the railway embankment) he is hastening towards the beam of light coming from B, whilst he is riding on ahead of the beam of light coming from A. Hence the observer will see the beam of light emitted from B earlier than he will see that emitted from A. Observers who take the railway train as their reference-body must therefore come to the conclusion that the lightning flash B took place earlier than the lightning flash A. We thus arrive at the important result:

Events which are simultaneous with reference to the embankment are not simultaneous with respect to the train, and vice versa (relativity of simultaneity). Every reference-body (co-ordinate system) has its own particular time; unless we are told the reference-body to which the statement of time refers, there is no meaning in a statement of the time of an event.

Now before the advent of the theory of relativity it had always tacitly been assumed in physics that the statement of time had an absolute significance, i.e. that it is independent of the state of motion of the body of reference. But we have just seen that this assumption is incompatible with the most natural definition of simultaneity; if we discard this assumption, then the conflict between the law of the propagation of light in vacuo and the principle of relativity (developed in Section VII) disappears. We were led to that conflict by . . . considerations . . . which are now no longer tenable. [For we made naive assumptions about simultaneity. Our idea was to fire light pulses from opposite walls simultaneously and check whether they reach the midpoint at the same time. But now we have discovered that simultaneity must be *defined* in terms of the light rays reaching the midpoint at the same time. So "fired at the same time" and "reach the midpoint at the same time" are not two independent things whose correlation or non-correlation can be empirically tested. Furthermore, our argument that the light pulses would not reach the midpoint at the same time if the system was moving was based on the mistaken assumption that simultaneity in this system is the same thing as simultaneity in a stationary reference frame.]

X. On the Relativity of the Conception of Distance.

Let us consider two particular points on the train travelling along the embankment with the velocity v, and inquire as to their distance apart. . . . An observer in the train measures the interval by marking off his measuring-rod in a straight line (e.g. along the floor of the carriage) as many times as is necessary to take him from the one marked point to the other. Then the number which tells us how often the rod has to be laid down is the required distance.

It is a different matter when the distance has to be judged from the railway line. Here the following method suggests itself. If we call A' and B' the two points on the train whose distance apart is required, then both of these points are moving with the velocity v along the embankment. In the first place we require to determine the points A and B of the embankment which are just being passed by the two points A' and B' at a particular time t—judged from the embankment. These points A and B of the embankment can be determined by applying the definition of time given in Section VIII. The distance between these points A and B is then measured by repeated application of the measuring-rod along the embankment.

A priori it is by no means certain that this last measurement will supply us with the same result as the first. Thus the length of the train as measured from the embankment may be different from that obtained by measuring in the train itself. ...

XI. The Lorentz Transformation.

How are we to find the place and time of an event in relation to the train, when we know the place and time of the event with respect to the railway embankment? Is there a thinkable answer to this question of such a nature that the law of transmission of light in vacuo does not contradict the principle of relativity? In other words: Can we conceive of a relation between place and time of the individual events relative to both reference-bodies, such that every ray of light possesses the velocity of transmission c relative to the embankment and relative to the train? This question leads to a quite definite positive answer, and to a perfectly definite transformation law for the space-time magnitudes of an event when changing over from one body of reference to another. ... This system of equations is known as the "Lorentz transformation."

XII. The Behaviour of Measuring-Rods and Clocks in Motion.

[It follows from the above principles that a] rigid rod is ... shorter when in motion than when at rest, and the more quickly it is moving, the shorter is the rod. ... [Furthermore,] in the theory of relativity the velocity c plays the part of a limiting velocity, which can neither be reached nor exceeded by any real body.

[If,] as judged from [a reference frame taken as stationary], [a] clock is moving with the velocity v, ... the time which elapses between two strokes of the clock is not one second, but ... a somewhat larger time. As a consequence of its motion the clock goes more slowly than when at rest.

XV. General Results of the Theory.

Classical mechanics required to be modified before it could come into line with the demands of the special theory of relativity. For the main part, however, this modification affects only the laws for rapid motions, in which the velocities of matter v are not very small as compared with the velocity of light. We have experience of such rapid motions only in the case of electrons and ions; for other motions the variations from the laws of classical mechanics are too small to make themselves evident in practice.

XVI. Experience and the Special Theory of Relativity.

[Hence] this question is not easily answered[:] To what extent is the special theory of relativity supported by experience? [But some experiments confirm it.]

PART II. The General Theory of Relativity.

XVIII. Special and General Principle of Relativity.

The ... principle, which was the pivot of all our previous considerations, was the special principle of relativity, i.e. the principle of the physical relativity of all uniform motion. Let us once more analyse its meaning carefully.

... Returning to the illustration we have frequently used of the embankment and the railway carriage, we can express the fact of the motion here taking place in the following two forms, both of which are equally justifiable:

(a) The carriage is in motion relative to the embankment,

(b) The embankment is in motion relative to the carriage.

... The principle we have made use of not only maintains that we may equally well choose the carriage or the embankment as our reference-body for the description of any event. Our principle rather asserts what follows: If we formulate the general laws of nature as they are obtained from experience, by making use of

(a) the embankment as reference-body,

(b) the railway carriage as reference-body,

then these general laws of nature (e.g. the laws of mechanics or the law of the propagation of light in vacuo) have exactly the same form in both cases. [In particular, no physical experiment can decide which of the two is "really" moving.] ...

In contrast to this we wish to understand by the "general principle of relativity" the following statement: All bodies of reference ... are equivalent for the description of natural phenomena ..., whatever may be their state of motion. ... This formulation must be replaced later by a more abstract one, for reasons which will become evident at a later stage.

Since the introduction of the special principle of relativity has been justified, every intellect which strives after generalisation must feel the temptation to venture the step towards the general principle of relativity. But a simple and apparently quite reliable consideration seems to suggest that, for the present at any rate, there is little hope of success in such an attempt. Let us imagine ourselves transferred to our old friend the railway carriage, which is travelling at a uniform rate. As long as it is moving uniformly, the occupant of the carriage is not sensible of its motion. . . .

If the motion of the carriage is now changed into a non-uniform motion, as for instance by a powerful application of the brakes, then the occupant of the carriage experiences a correspondingly powerful jerk forwards. The retarded motion is manifested in the mechanical behaviour of bodies relative to the person in the railway carriage. The mechanical behaviour is different from that of [uniform motion], and for this reason it would appear to be impossible that the same mechanical laws hold relatively to the non-uniformly moving carriage, as hold with reference to the carriage when at rest or in uniform motion. [In other words, non-uniform motion can be detected by experiments even in a closed room, and hence be objectively distinguished from rest.] . . .

Because of this, we feel compelled at the present juncture to grant a kind of absolute physical reality to non-uniform motion, in opposition to the general principle of relativity. But in what follows we shall soon see that this conclusion cannot be maintained.

XIX. The Gravitational Field.

The same quality of a body manifests itself according to circumstances as "inertia" or as "weight". [The "inertial mass" of a body has to do with how much it resists being moved, or how hard you have to push to get it moving with a certain speed. The "gravitational mass" of a body has to do with with which force gravity is pulling on it, or how hard you have to push from underneath it to prevent it from falling to the ground.]

XX. The Equality of Inertial and Gravitational Mass as an Argument for the General Postulate of Relativity.

We imagine a large portion of empty space, . . . far removed from stars and other appreciable masses. . . . Let us imagine a spacious chest resembling a room with an observer inside who is equipped with apparatus. Gravitation naturally does not exist for this observer. He must fasten himself with strings to the floor, otherwise the slightest impact against the floor will cause him to rise slowly towards the ceiling of the room.

To the middle of the lid of the chest is fixed externally a hook with rope attached, and now [someone] begins pulling at this with a constant force. The chest together with the observer then begin to move "upwards" with a uniformly accelerated motion. In course of time their velocity will reach unheard-of values—provided that we are viewing all this from another reference-body which is not being pulled with a rope.

But how does the man in the chest regard the Process? The acceleration of the chest will be transmitted to him by the reaction of the floor of the chest. He must therefore take up this pressure by means of his legs if he does not wish to be laid out full length on the floor. He is then standing in the chest in exactly the same way as anyone stands in a room of a home on our earth. If he releases a body which he previously had in his land, the acceleration of the chest will no longer be transmitted to this body, and for this reason the body will approach the floor of the chest with an accelerated relative motion. The observer will further convince himself that the acceleration of the body towards the floor of the chest is always of the same magnitude, whatever kind of body he may happen to use for the experiment.

[If he is familiar with how gravity works on earth,] the man in the chest will [find that everything he observes agrees with that experience and] thus come to the conclusion that he and the chest are in a gravitational field. . . . Of course he will be puzzled for a moment as to why the chest does not fall in this gravitational field. Just then, however, he discovers the hook in the middle of the lid of the chest and the rope which is attached to it, and he consequently comes to the conclusion that the chest is suspended at rest in the gravitational field.

Ought we to smile at the man and say that he errs in his conclusion? I do not believe we ought to if we wish to remain consistent; we must rather admit that his mode of grasping the situation violates neither reason nor known mechanical laws. Even though it is being accelerated with respect to the "Galileian space" first considered, we can nevertheless regard the chest as being at rest. We have thus good grounds for extending the principle of relativity to include bodies of reference which are accelerated with respect to each other, and as a result we have gained a powerful argument for a generalised postulate of relativity.

We must note carefully that the possibility of this mode of interpretation rests on the fundamental property of the gravitational field of giving all bodies the same acceleration, or, what comes to the same thing, on the law of the equality of inertial and gravitational mass. If this natural law did not exist, the man in the accelerated chest would not be able to interpret the behaviour of the bodies around him on the supposition of a gravitational field, and he would not be justified on the grounds of experience in supposing his reference-body to be "at rest."

Suppose that the man in the chest fixes a rope to the inner side of the lid, and that he attaches a body to the free end of the rope.

The result of this will be to stretch the rope so that it will hang "vertically" downwards. If we ask for an opinion of the cause of tension in the rope, the man in the chest will say: "The suspended body experiences a downward force in the gravitational field, and this is neutralised by the tension of the rope; what determines the magnitude of the tension of the rope is the gravitational mass of the suspended body." On the other hand, an observer who is poised freely in space will interpret the condition of things thus: "The rope must perforce take part in the accelerated motion of the chest, and it transmits this motion to the body attached to it. The tension of the rope is just large enough to effect the acceleration of the body. That which determines the magnitude of the tension of the rope is the inertial mass of the body." Guided by this example, we see that our extension of the principle of relativity implies the necessity of the law of the equality of inertial and gravitational mass. ...

We can now appreciate why that argument is not convincing, which we brought forward against the general principle of relativity at the end of Section XVIII. It is certainly true that the observer in the railway carriage experiences a jerk forwards as a result of the application of the brake, and that he recognises in this the non-uniformity of motion (retardation) of the carriage. But he is compelled by nobody to refer this jerk to a "real" acceleration (retardation) of the carriage. He might also interpret his experience thus: "My body of reference (the carriage) remains permanently at rest. With reference to it, however, there exists (during the period of application of the brakes) a gravitational field which is directed forwards and which is variable with respect to time. Under the influence of this field, the embankment together with the earth moves non-uniformly in such a manner that their original velocity in the backwards direction is continuously reduced."

XXII. A Few Inferences from the General Principle of Relativity.

The considerations of Section XX show that the general principle of relativity puts us in a position to derive properties of the gravitational field in a purely theoretical manner. Let us suppose, for instance, that we know the space-time "course" for any natural process whatsoever, as regards the manner in which it takes place in the Galileian domain relative to a Galileian body of reference K. By means of purely theoretical operations (i.e. simply by calculation) we are then able to find how this known natural process appears, as seen from a reference-body K' which is accelerated relatively to K. But since a gravitational field exists with respect to this new body of reference K, our consideration also teaches us how the gravitational field influences the process studied.

... It [has always been] known that a gravitational field [and its equivalent motion as described in Section XX] influence the movement of bodies in [the same] way. [Hence] our consideration supplies us with nothing essentially new [as far as mechanical phenomena are concerned].

However, we obtain a new result of fundamental importance when we carry out the analogous consideration for a ray of light. With respect to the Galileian reference-body K, such a ray of light is transmitted rectilinearly with the velocity c. It can easily be shown that the path of the same ray of light is no longer a straight line when we consider it with reference to the accelerated chest (reference-body K'). From this we conclude, that, in general, rays of light are propagated curvilinearly in gravitational fields. In two respects this result is of great importance.

In the first place, it can be compared with the reality. ... The curvature of light rays required by the general theory of relativity is ... exceedingly small for the gravitational fields at our disposal in practice, [but on an astronomical scale it should be observable.] ... The examination of the correctness or otherwise of this deduction is a problem of the greatest importance, the ... solution of which is to be expected of astronomers. [Footnote:] The existence of the deflection of light demanded by theory was first confirmed during the solar eclipse of 29th May, 1919.

In the second place our result shows that, according to the general theory of relativity, the law of the constancy of the velocity of light in vacuo, which constitutes one of the two fundamental assumptions in the special theory of relativity and to which we have already frequently referred, cannot claim any unlimited validity. A curvature of rays of light can only take place when the velocity of propagation of light varies with position. Now we might think that as a consequence of this, the special theory of relativity and with it the whole theory of relativity would be laid in the dust. But in reality this is not the case. We can only conclude that the special theory of relativity cannot claim an unlimited domain of validity; its results hold only so long as we are able to disregard the influences of gravitational fields on the phenomena (e.g. of light). ... No fairer destiny could be allotted to any physical theory, than that it should of itself point out the way to the introduction of a more comprehensive theory, in which it lives on as a limiting case.

XXIII. Behaviour of Clocks and Measuring-Rods on a Rotating Body of Reference.

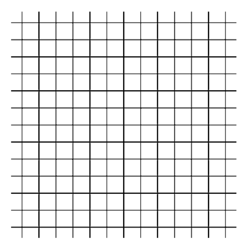
Imagine K' to be ... a plane circular disc, which rotates uniformly in its own plane about its centre. An observer who is sitting eccentrically on the disc K' is sensible of a force which acts outwards in a radial direction, and which would be interpreted as an effect of inertia (centrifugal force) by an observer who was at rest with respect to [an external, stationary] reference-body K. But the observer on the disc may regard his disc as a reference-body which is "at rest"; on the basis of the general principle of relativity he is justified in doing this. The force acting on himself, and in fact on all other bodies which are at rest relative to the disc, he regards as the effect of a gravitational field. ...

The observer performs experiments on his circular disc with clocks and measuring-rods … [but] the definition of the space co-ordinates … presents insurmountable difficulties. If the observer applies his standard measuring-rod (a rod which is short as compared with the radius of the disc) tangentially to the edge of the disc, then, as judged from [K], the length of this rod will be less than 1, since, according to Section XII, moving bodies suffer a shortening in the direction of the motion. On the other hand, the measuring-rod will not experience a shortening in length, as judged from K, if it is applied to the disc in the direction of the radius. If, then, the observer first measures the circumference of the disc with his measuring-rod and then the diameter of the disc, on dividing the one by the other, he will not obtain as quotient the familiar number $\pi = 3.14\ldots$, but a larger number, whereas of course, for a disc which is at rest with respect to K, this operation would yield π exactly. This proves that the propositions of Euclidean geometry cannot hold exactly on the rotating disc, nor in general in a gravitational field, at least if we attribute the length 1 to the rod in all positions and in every orientation. Hence the idea of a straight line also loses its meaning. We are therefore not in a position to define exactly the co-ordinates (x, y, z) relative to the disc by means of the method used in discussing the special theory, and as long as the co-ordinates and times of events have not been defined, we cannot assign an exact meaning to the natural laws in which these occur.

Thus all our previous conclusions based on general relativity would appear to be called in question. …

XXIV. Euclidean and Non-Euclidean Continuum.

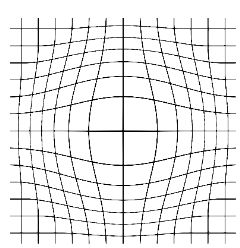
The surface of a marble table is spread out in front of me. … Let us now imagine that a large number of little rods of equal length have been made, their lengths being small compared with the dimensions of the marble slab. When I say they are of equal length, I mean that one can be laid on any other without the ends overlapping. We next lay four of these little rods on the marble slab so that they constitute a quadrilateral figure (a square), the diagonals of which are equally long. To ensure the equality of the diagonals, we make use of a little testing-rod. To this square we add similar ones, each of which has one rod in common with the first. We proceed in like manner with each of these squares until finally the whole marble slab is laid out with squares. The arrangement is such, that each side of a square belongs to two squares and each corner to four squares. …



If everything has really gone smoothly, then I say that the points of the marble slab constitute a Euclidean continuum with respect to the little rod, which has been used as a "distance" (line-interval). By choosing one corner of a square as "origin" I can characterise every other corner of a square with reference to this origin by means of two numbers. I only need state how many rods I must pass over when, starting from the origin, I proceed towards the "right" and then "upwards," in order to arrive at the corner of the square under consideration. These two numbers are then the "Cartesian co-ordinates" of this corner with reference to the "Cartesian co-ordinate system" which is determined by the arrangement of little rods. …

It is a veritable wonder that we can carry out this business without getting into the greatest difficulties. We only need to think of the following. If at any moment three squares meet at a corner, then two sides of the fourth square are already laid, and, as a consequence, the arrangement of the remaining two sides of the square is already completely determined. But I am now no longer able to adjust the quadrilateral so that its diagonals may be equal. If they are equal of their own accord, then this is an especial favour of the marble slab and of the little rods, about which I can only be thankfully surprised. …

By making use of the following modification of this abstract experiment, we recognise that there must also be cases in which the experiment would be unsuccessful. We shall suppose that the rods "expand" by in amount proportional to the increase of temperature. We heat the central part of the marble slab, but not the periphery, in which case two of our little rods can still be brought into coincidence at every position on the table. But our construction of squares must necessarily come into disorder during the heating, because the little rods on the central region of the table expand, whereas those on the outer part do not.



With reference to our little rods—defined as unit lengths—the marble slab is no longer a Euclidean continuum, and we are also no longer in the position of defining Cartesian co-ordinates directly with their aid, since the above construction can no longer be carried out. . . .

If rods of every kind (i.e. of every material) were to behave in the same way as regards the influence of temperature when they are on the variably heated marble slab, and if we had no other means of detecting the effect of temperature than the geometrical behaviour of our rods in experiments analogous to the one described above, then our best plan would be to assign the distance one to two points on the slab, provided that the ends of one of our rods could be made to coincide with these two points; for how else should we define the distance without our proceeding being in the highest measure grossly arbitrary? The method of Cartesian coordinates must then be discarded, and replaced by another which does not assume the validity of Euclidean geometry for rigid bodies.

XXV. Gaussian Co-Ordinates.

. . . Sufficiently small regions of the continuum under consideration may be regarded as Euclidean continua. For example, this . . . holds in the case of the marble slab of the table and local variation of temperature. The temperature is practically constant for a small part of the slab, and thus the geometrical behaviour of the rods is almost as it ought to be according to the rules of Euclidean geometry [in that small region]. Hence the imperfections of the construction of squares . . . do not show themselves clearly until this construction is extended over a considerable portion of the surface of the table. . . .

XXVII. The Space-Time Continuum of the General Theory of Relativity is Not a Euclidean Continuum.

In the first part of this book [i.e. in special relativity] we were able to make use of space-time co-ordinates which allowed of a simple and direct physical interpretation, and which . . . can be regarded as . . . Cartesian co-ordinates. This was possible on the basis of the law of the constancy of the velocity of light. But according to Section XXI the general theory of relativity cannot retain this law. On the contrary, we arrived at the result that according to this latter theory the velocity of light must always depend on the co-ordinates when a gravitational field is present. In connection with a specific illustration in Section XXIII, we found that the presence of a gravitational field invalidates the definition of the coordinates and the time, which led us to our objective in the special theory of relativity.

In view of the results of these considerations we are led to the conviction that, according to the general principle of relativity, the space-time continuum cannot be regarded as a Euclidean one, but that here we have the general case, corresponding to the marble slab with local variations of temperature, and with which we made acquaintance as an example of a two-dimensional continuum. Just as it was there impossible to construct a Cartesian co-ordinate system from equal rods, so here it is impossible to build up a system (reference-body) from rigid bodies and clocks, which shall be of such a nature that measuring-rods and clocks, arranged rigidly with respect to one another, shall indicate position and time directly. . . .

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§ 11.3. Reader

ANDREW EDE & LESLEY CORMACK, *A History of Science in Society: A Reader*, Broadview Press, 2007.

8.9.1. Einstein's original paper on special relativity. The technical details say the same thing as Einstein's popularisation for a general reader excerpted above. Interesting is to see how he framed the motivation of his theory for professional physicists in the introductory paragraphs.

8.9.2. Interesting for its big-picture framing of the theory, including the constructive/analytic distinction.

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§ 11.4. Philosophical context

THOMAS A. RYCKMAN, Early Philosophical Interpretations of General Relativity, *The Stanford Encyclopedia of Philosophy*, Spring 2018 Edition, Edward N. Zalta (ed.), https://plato.stanford.edu/archives/spr2018/entries/genrel-early/.

In the special theory of relativity (1905), Einstein's operational definition of the "simultaneity" of distantly separated events, whereby distant clocks are synchronized by sending and receiving light signals, is closely modeled on the operational definition of mass in Mach's Mechanics. Moreover, occasional epistemological and methodological pronouncements seem to indicate agreement with core parts of Mach's positivist doctrine of meaningfulness, e.g., "The concept does not exist for the physicist until he has the possibility of discovering whether or not it is fulfilled in an actual case" [Einstein 1917]. Thus the general theory of relativity might be seen as fully compliant with Mach's characterization of theoretical concepts as merely economical shorthand for concrete observations or operations. …

Machian influences specific to the general theory of relativity appeared even more extensive. Mach's idée fixe, that a body's inertial mass and motion result from the influence of all other surrounding masses (thus eliminating the "monstrous" Newtonian concept of absolute space), was perhaps the strongest motivation guiding Einstein's pursuit of a relativistic theory of gravity. In papers leading up to the definitive presentation of the general theory of relativity in 1916, Einstein made no secret of the fact that Mach was the inspiration for an epistemologically mandated attempt to generalize the principle of relativity. Holding, with Mach, that no observable facts could be associated with the notions of absolute acceleration or absolute inertia (i.e., resistance to acceleration), the generalization required that the laws of nature be completely independent of the state of motion of any chosen reference system. …

[Einstein's later] philosophical pronouncements increasingly took on a more realist or at least anti-positivist coloration. Lecturing at the Sorbonne in April 1922, Einstein pronounced Mach "un bon mécanicien" but "un déplorable philosophe". Increasingly, Einstein's retrospective portrayals of the genesis of general relativity centered almost entirely on the success of a strategy emphasizing mathematical aesthetics. …

Logical empiricism's philosophy of science was conceived under the guiding star of Einstein's two theories of relativity. … An overriding concern in the logical empiricist treatment of relativity theory was to draw broad lessons for scientific methodology and philosophy of science generally. … Several of the most characteristic doctrines of logical empiricist philosophy of science—the interpretation of *a priori* elements in physical theories as conventions, the treatment of the necessary role of conventions in linking theoretical concepts to observation, the insistence on observational language definition of theoretical terms—were taken to have been conclusively demonstrated by Einstein in fashioning his two theories of relativity. … [Logical empiricists were often keen to emphasise that] "it is universally known today that Einstein's general theory of relativity grew immediately out of the positivistic doctrine of space and motion."

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§ 11.5. Operationalism

P. W. BRIDGMAN, *The Logic of Modern Physics*, New York: Macmillan, 1927. Paperback reprint 1960.

"We mean by any concept nothing more than a set of operations; the concept is synonymous with the corresponding set of operations." (5) "The proper definition of a concept is not in terms of its properties but in terms of actual operations." (6)

Motivation for this: Relativity theory and quantum physics show that "when experiment is pushed into new domains, we must be prepared for new facts, of an entirely different character from those of our former experience" (2). Therefore "the physicist … recognizes no *a priori* principles which determine or limit the possibilities of new experience" (3); rather we "must use in describing and correlating nature concepts of such a character that our present experience does not exact hostages of the future" (4). "For if experience is always described in terms of experience, … we need never be embarrassed" (6–7) to find that we were talking about imagined concepts with no connection to reality.

"[A] consequence of the operational character of our concepts … is that it is quite possible, even disquietingly easy, to invent expressions or to ask questions that are meaningless. It constitutes a great advance in our critical attitude toward nature to realize that a great many of the questions that we uncritically ask are without meaning. If a specific question has meaning, it must be possible to find operations by which an answer may be given to it." (28)

P. W. BRIDGMAN, *The nature of some of our physical concepts*, New York: Philosophical Library, 1952.

"We do not know the meaning of a concept unless we can specify the operations which were used by us or our neighbour in applying the concept in any concrete situation" (7)

"Physicists do profitably employ concepts the meaning of which is not to be found in the instrumental operations of the laboratory, and which cannot be reduced to such operations without residue. Nearly all the concepts of theoretical and mathematical physics are of this character. … In fact, there is hardly any physical concept which does not enter to a certain extent into some theoretical edifice and which does not therefore possess to a certain degree a non-instrumental component." (8) "I think, however, that physicists are agreed in imposing one restriction on the freedom of such operations, namely that such operations much be capable of eventually, although perhaps indirectly, making connection with instrumental reality. Only in this way can the physicist keep his feet on the ground or achieve a satisfactory degree of precision. … Politics, philosophy and religion are full of … purely verbal concepts; it is merely that such concepts are outside the field of the physicist." (10)

§ 12. Kuhn

§ 12.1. *Discussion questions*

12.1. Why do scientists adhere to paradigms? What are the pros and cons of doing so?

Con: dogmatic, uncritical toward the core beliefs of the paradigm. Pro: enables specialisation, collaboration, structured and well-defined research tasks. Absence of paradigm = chaotic sea of facts; researchers isolated rather than engaged in cumulative enterprise; big-picture, self-contained books rather than nitty-gritty articles assuming much background knowledge; ideas associated with authors rather than objective results.

12.2. What characteristics do a theory need to become a paradigm?

Not only be a credible theory compared to rival alternative, but crucially also be able to sustain specialised research by clearly delineating problems to study and tools for tackling them. The forward-looking potential of a theory is at least as important as its established success.

12.3. What attitudes must a scientists internalise to be able to participate in "normal science"? Why?

Glorify technical puzzles; vehemently resist challenges to underlying assumptions; disregard problems that are externally rather than internally motivated. Necessary to have such "rules of the game"; otherwise science degenerates into "wild west": like a sport without rules or a country without laws—chaos reigns and there is no cumulative progress.

12.4. Several idealistic notions of science are turned on their head by Kuhn. Discuss.

Scientists are not explorers of novel frontiers, but rather spend all their time on marginal "mopping up" of well-treaded ground. Scientists do not challenge received wisdom and overthrow theories based on new facts, but rather do everything to remain loyal to the paradigm in which they were trained for as long as they possible can. Normally, scientists pay no attention to general principles of "scientific method" except as post hoc self-rationalisation.

12.5. How does the Chemical Revolution illustrate Kuhn's view of theory-change? (Including the role of crisis, and the concept of incommensurability.)

Crisis: The issue of weight gain during combustion is prima facie a problem for phlogiston theory, bit it was long dismissed as a quirk due to some secondary contamination. However, increasing use of scales and quantitative chemistry (inspired in part by the emphasis on mass in Newtonian physics) made it more of a pressing issue. Attempts at resolving it led to a proliferation of ad hoc hypotheses, none of which proved useful or fruitful beyond this particular case. Incommensurability: The old and the new systems of chemistry are rival worldviews that must be accepted or rejected as holistic units. Each can deal with all experimental facts in principle, although they may need to resort to more or less ad hoc hypotheses to do so. We must judge which system feels better overall; we cannot tabulate and quantify pros and cons and reach an objective verdict by logic alone. Lavoisier's system not only rejected specific claims of the old theory—it also rejected its way of thinking, for instance what it saw as important to explain.

12.6. Is Kuhn Kantian?

The way a paradigm determines what we see is very similar to Kant's point about the synthetic a priori lens shaping all experience. However, Kant imagined that lens as a static and universal characteristic of human nature, whereas to Kuhn it is changing from one theory to another.

12.7. Does Kuhn's sociological picture of science undermine the rationality of science?

It rules out a utopian image of scientific rationality as reducible to objective logic and data. There can still be scientific progress in a broader evolutionary sense.

12.8. How can science preserve problem-solving ability across revolutions if the new theory is incommensurable with the old? Why would scientists want to do this if all they care about is having a paradigm that can sustain normal science?

A bit of an unresolved issue in Kuhn's story, I think. But in part it can be accounted for in that normal science demands conservatism. If there was no conservatism during theory change, the credibility of this stance would be undermined.

12.9. According to Kuhn, science succeeds while other fields fail for the same reason that totalitarianism gets things done while anarchism does not. Discuss.

Agrees with Kuhn's remarks on the education, conservatism, dogmatism of scientists; the reasons for lack of progress in other fields.

12.10. A Liberal Arts and Sciences education is suited precisely for people who are not content to be doing "normal science" in Kuhn's sense. Discuss.

Normal science means accepting the "rules of the game" of the "status quo," not challenging received wisdom. It means devoting all your energy and intelligence to succeeding within the existing system, and none to trying to make a better system.

§ 12.2. *Structure of Scientific Revolutions*

THOMAS S. KUHN, *The Structure of Scientific Revolutions*, University of Chicago Press, 1962.

"II. The Route to Normal Science." Scientific works such as "Aristotle's Physica, Ptolemy's Almagest, Newton's Principia and Opticks, Franklin's Electricity, Lavoisier's Chemistry, and Lyell's Geology" are not primarily collections of facts and findings.

Rather, they “implicitly … define the legitimate problems and methods of a research field for succeeding generations of practitioners.” That is to say, such works define a paradigm—a worldview, it gives a set of concepts in terms of which the world should be interpreted, it separates primary phenomena from secondary ones, and interesting problems from subordinate ones.

“In the absence of a paradigm or some candidate for paradigm, all of the facts that could possibly pertain to the development of a given science are likely to seem equally relevant. … This sort of fact-collecting … produces a morass.”

“When the individual scientist can take a paradigm for granted, he need no longer, in his major works, attempt to build his field anew, starting from first principles and justifying the use of each concept introduced. That can be left to the writer of textbooks. Given a textbook, however, the creative scientist can begin his research where it leaves off and thus concentrate exclusively upon the subtlest and most esoteric aspects of the natural phenomena that concern his group.” “It is hard to find another criterion that so clearly proclaims a field a science”.

“III. The Nature of Normal Science.” Normal research is determined by and loyal to the reigning paradigm. “No part of the aim of normal science is to call forth new sorts of phenomena; indeed those that will not fit the box are often not seen at all. Nor do scientists normally aim to invent new theories, and they are often intolerant of those invented by others. Instead, normal-scientific research is directed to the articulation of those phenomena and theories that the paradigm already supplies.”

“Paradigms gain their status because they are more successful than their competitors in solving a few problems that the group of practitioners has come to recognize as acute. To be more successful is not, however, to be either completely successful with a single problem or notably successful with any large number. The success of a paradigm—whether Aristotle’s analysis of motion, Ptolemy’s computations of planetary position, Lavoisier’s application of the balance, or Maxwell’s mathematization of the electromagnetic field—is at the start largely a promise of success discoverable in selected and still incomplete examples. Normal science consists in the actualization of that promise.”

“Few people who are not actually practitioners of a mature science realize how much mop-up work of this sort a paradigm leaves to be done or quite how fascinating such work can prove in the execution. … Mopping-up operations are what engage most scientists throughout their careers.”

By contrast, “Scientists … generally regard … the use of existing theory to predict factual information of intrinsic value … as hack work to be relegated to engineers or technicians. At no time do very many of them appear in significant scientific journals.”

“[It would be a mistake for someone] who reads a science text [to] take the applications to be the evidence for the theory, the reasons why it ought to be believed. … Science students accept theories on the authority of teacher and text, not because of evidence. What alternatives have they, or what competence? The applications given in texts are not there as evidence but because learning them is part of learning the paradigm at the base of current practice. If applications were set forth as evidence, then the very failure of texts to suggest alternative interpretations or to discuss problems for which scientists have failed to produce paradigm solutions would convict their authors of extreme bias.”

“IV. Normal Science as Puzzle-solving.” “The most striking feature of … normal research problems … is how little they aim to produce major novelties, conceptual or phenomenal.” “But if the aim of normal science is not major substantive novelties—if failure to come near the anticipated result is usually failure as a scientist—then why are these problems undertaken at all? … Though its outcome can be anticipated, … the way to achieve that outcome remains very much in doubt. Bringing a normal research problem to a conclusion is achieving the anticipated in a new way, and it requires the solution of all sorts of complex instrumental, conceptual, and mathematical puzzles. The man who succeeds proves himself an expert puzzle-solver, and the challenge of the puzzle is an important part of what usually drives him on.”

“It is no criterion of goodness in a puzzle that its outcome be intrinsically interesting or important. On the contrary, the really pressing problems, e.g., a cure for cancer or the design of a lasting peace, are often not puzzles at all, largely because they may not have any solution. Consider the jigsaw puzzle whose pieces are selected at random from each of two different puzzle boxes. Since that problem is likely to defy (though it might not) even the most ingenious of men, it cannot serve as a test of skill in solution. In any usual sense it is not a puzzle at all. Though intrinsic value is no criterion for a puzzle, the assured existence of a solution is.”

“One of the things a scientific community acquires with a paradigm is a criterion for choosing problems that, while the paradigm is taken for granted, can be assumed to have solutions. To a great extent these are the only problems that the community will admit as scientific or encourage its members to undertake. Other problems, including many that had previously been standard, are rejected as metaphysical, as the concern of another discipline, or sometimes as just too problematic to be worth the time.”

“The scientific enterprise as a whole does from time to time prove useful, open up new territory, display order, and test long-accepted belief. Nevertheless, the individual engaged on a normal research problem is almost never doing any one of these things. … His motivation is of a rather different sort. What … challenges him is the conviction that, if only he is skilful enough, he will succeed in solving a puzzle that no one before has solved or solved so well. Many of the greatest scientific minds have devoted all of their professional attention to demanding puzzles of this sort. On most occasions any particular field of specialization offers nothing else to do, a fact that makes it no less fascinating to the proper sort of addict."

"V. The Priority of Paradigms." Commitment to a paradigm is prior to commitment to rules and assumptions regarding what constitutes legitimate scientific method and subject matter, etc. Such rules and assumptions can be abstracted from a paradigm to some extent but they are not needed for normal science (where the model problems and solutions of the paradigm provide sufficient guidance). They are, however, very important when a paradigm is called into question.

"VI. Anomaly and the Emergence of Scientific Discoveries." "Normal science does not aim at novelties of fact or theory and, when successful, finds none. New and unsuspected phenomena are, however, repeatedly uncovered by scientific research, and radical new theories have again and again been invented by scientists. If this characteristic of science is to be reconciled with what has already been said, then research under a paradigm must be a particularly effective way of inducing paradigm change. That is what fundamental novelties of fact and theory do. Produced inadvertently by a game played under one set of rules, their assimilation requires the elaboration of another set. ... [This process] commences with the awareness of anomaly, i.e., with the recognition that nature has somehow violated the paradigm-induced expectations that govern normal science. ... And it closes only when the paradigm theory has been adjusted so that the anomalous has become the expected."

"VII. Crisis and the Emergence of Scientific Theories." "The emergence of new theories is generally preceded by a period of pronounced professional insecurity ... generated by the persistent failure of the puzzles of normal science to come out as they should." Theories are often "anticipated during a period when there was no crisis in the corresponding science; and in the absence of crisis those anticipations had been ignored." "Philosophers of science have repeatedly demonstrated that more than one theoretical construction can always be placed upon a given collection of data. History of science indicates that ... it is not even very difficult to invent such alternates. But that invention of alternates is just what scientists seldom undertake. ... So long as the tools a paradigm supplies continue to prove capable of solving the problems it defines, science moves fastest and penetrates most deeply through confident employment of those tools. The reason is clear. As in manufacture so in science—retooling is an extravagance to be reserved for the occasion that demands it."

Example of a crisis preceding the chemical revolution: "Lavoisier ... was ... much concerned to explain the gain in weight that most bodies experience when burned or roasted, and that again is a problem with a long prehistory. At least a few Islamic chemists had known that some metals gain weight when roasted. In the seventeenth century several investigators had concluded from this same fact that a roasted metal takes up some ingredient from the atmosphere. But in the seventeenth century that conclusion seemed unnecessary to most chemists. If chemical reactions could alter the volume, color, and texture of the ingredients, why should they not alter weight as well? Weight was not always taken to be the measure of quantity of matter. Besides, weight-gain on roasting remained an isolated phenomenon. Most natural bodies (e.g., wood) lose weight on roasting as the phlogiston theory was later to say they should. During the eighteenth century, however, these initially adequate responses to the problem of weight-gain became increasingly difficult to maintain. Partly because the balance was increasingly used as a standard chemical tool and partly because the development of pneumatic chemistry made it possible and desirable to retain the gaseous products of reactions, chemists discovered more and more cases in which weight-gain accompanied roasting. Simultaneously, the gradual assimilation of Newton's gravitational theory led chemists to insist that gain in weight must mean gain in quantity of matter. Those conclusions did not result in rejection of the phlogiston theory, for that theory could be adjusted in many ways. Perhaps phlogiston had negative weight, or perhaps fire particles or something else entered the roasted body as phlogiston left it. There were other explanations besides. But if the problem of weight-gain did not lead to rejection, it did lead to an increasing number of special studies in which this problem bulked large. One of them, "On phlogiston considered as a substance with weight and [analyzed] in terms of the weight changes it produces in bodies with which it unites," was read to the French Academy early in 1772, the year which closed with Lavoisier's delivery of his famous sealed note to the Academy's Secretary. Before that note was written a problem that had been at the edge of the chemist's consciousness for many years had become an outstanding unsolved puzzle. Many different versions of the phlogiston theory were being elaborated to meet it. ... Increasingly, the research it guided resembled that conducted under the competing schools of the pre-paradigm period, another typical effect of crisis."

"VIII. The Response to Crisis." "[The] object [of normal science] is to solve a puzzle for whose very existence the validity of the paradigm must be assumed. Failure to achieve a solution discredits only the scientist and not the theory. ... The proverb applies: 'It is a poor carpenter who blames his tools'." Only after repeated failures of this kind "scientists may conclude that no solution will be forthcoming in the present state of their field."

"Confronted with anomaly or with crisis, scientists take a different attitude towards existing paradigms, and the nature of their research changes accordingly. The proliferation of competing articulations, the willingness to try anything, ... the recourse to philosophy and to debate over fundamentals, all these are symptoms of a transition from normal to extraordinary scientific research."

"Science's reorientation by paradigm change, [can be] described ... as 'picking up the other end of the stick', a process that involves handling the same bundle of data as before, but placing them in a new system of relations with one another by giving them a different framework."

The purpose of extraordinary research is to establish a new paradigm, since "once it has achieved the status of a paradigm, a scientific theory is declared invalid only if an alternate candidate is available to take its place." "The act of judgment that leads scientists to reject a previously accepted theory is always based upon more than a comparison of that theory with the world. The decision to reject one paradigm is always simultaneously the decision to accept another, and the judgment leading to that decision involves the comparison of both paradigms with nature *and* with each other." "To reject one paradigm without simultaneously substituting another is to reject science itself."

"IX. The Nature and Necessity of Scientific Revolutions." "Paradigm choice can never be unequivocally settled by logic and experiment alone." "Each group uses its own paradigm to argue in that paradigm's defence. ... [This can] provide a clear exhibit of what scientific practice will be like for those who adopt the new view. ... That exhibit can be immensely persuasive, often compellingly so [but] it cannot be made logically or even probabilistically compelling for those who refuse to step into the circle."

"The reception of a new paradigm often necessitates a redefinition of the corresponding science. Some old problems may be relegated to another science or declared entirely 'unscientific'. Others that were previously non-existent or trivial may, with a new paradigm, become the very archetypes of significant scientific achievement. And as the problems change, so, often, does the standard that distinguishes a real scientific solution from a mere metaphysical speculation, word game, or mathematical play."

"Einsteinian dynamics and the older dynamical equations that descend from Newton's Principia . . . are fundamentally incompatible . . . : Einstein's theory can be accepted only with the recognition that Newton's was wrong."

Many object to this view as follows. "Relativistic dynamics cannot have shown Newtonian dynamics to be wrong, for Newtonian dynamics is still used with great success by most engineers and, in selected applications, by many physicists. Furthermore, the propriety of this use of the older theory can be proved from the very theory that has, in other applications, replaced it. Einstein's theory can be used to show that predictions from Newton's equations will be as good as our measuring instruments in all applications that satisfy a small number of restrictive conditions. For example, if Newtonian theory is to provide a good approximate solution, the relative velocities of the bodies considered must be small compared with the velocity of light. Subject to this condition and a few others, Newtonian theory seems to be derivable from Einsteinian, of which it is therefore a special case."

"[And] no theory can possibly conflict with one of its special cases. If Einsteinian science seems to make Newtonian dynamics wrong, that is only because some Newtonians were so incautious as to claim that Newtonian theory yielded entirely precise results or that it was valid at very high relative velocities. Since they could not have had any evidence for such claims, they betrayed the standards of science when they made them. In so far as Newtonian theory was ever a truly scientific theory supported by valid evidence, it still is. Only extravagant claims for the theory—claims that were never properly parts of science—can have been shown by Einstein to be wrong. Purged of these merely human extravagances, Newtonian theory has never been challenged and cannot be."

"But to save theories in this way, their range of application must be restricted to those phenomena and to that precision of observation with which the experimental evidence in hand already deals. . . . Such a limitation prohibits the scientist from claiming to speak 'scientifically' about any phenomenon not already observed. . . . These prohibitions are logically unexceptionable. But the result of accepting them would be the end of the research through which science may develop further."

"More important, . . . Can Newtonian dynamics really be derived from relativistic dynamics? . . . To prove the adequacy of Newtonian dynamics as a special case [of Einstein's theory], we must add to [the latter] additional statements . . . restricting the range of the parameters and variables [such as making velocities much less than the speed of light]. [The resulting] set of statements is then manipulated to yield a new set . . . which is identical in form with Newton's laws of motion, the law of gravity, and so on. Apparently Newtonian dynamics has been derived from Einsteinian, subject to a few limiting conditions."

"Yet the derivation is spurious. . . . Though the [law obtained in this way] are a special case of the laws of relativistic mechanics, they are not Newton's Laws. . . . The variables and parameters that in the Einsteinian [theory] represented spatial position, time, mass, etc., still occur in the [Newtonian special case]; and they there still represent Einsteinian space, time, and mass. But the physical referents of these Einsteinian concepts are by no means identical with those of the Newtonian concepts that bear the same name. [For example,] Newtonian mass is conserved; Einsteinian is convertible with energy."

"X. Revolutions as Changes of World View." "To the Aristotelians ... the swinging body [of a pendulum] was simply falling with difficulty. Constrained by the chain, it could achieve rest at its low point only after tortuous motion and considerable time. Galileo, on the other hand, [saw] a body that almost succeeded in repeating the same motion over and over again ad infinitum. And having seen that much, Galileo observed other properties of the pendulum as well and constructed many of the most significant and original parts of his new dynamics around them." "The duck-rabbit shows that two men with the same retinal impressions can see different things; the inverting lenses show that two men with different retinal impressions can see the same thing." "The scientist who looks at a swinging stone can have no experience that is in principle more elementary than seeing a pendulum. The alternative is not some hypothetical 'fixed' vision, but vision through another paradigm."

"As a result of discovering oxygen, Lavoisier saw nature differently. And in the absence of some recourse to that hypothetical fixed nature that he 'saw differently', the principle of economy will urge us to say that after discovering oxygen Lavoisier worked in a different world."

"Crises … are terminated, not by deliberation and interpretation, but by a relatively sudden and unstructured event like the gestalt switch. Scientists then often speak of the 'scales falling from the eyes' or of the 'lightning flash' that 'inundates' a previously obscure puzzle, enabling its components to be seen in a new way that for the first time permits its solution. On other occasions the relevant illumination comes in sleep. No ordinary sense of the term 'interpretation' fits these flashes of intuition through which a new paradigm is born. Though such intuitions depend upon the experience, both anomalous and congruent, gained with the old paradigm, they are not logically or piecemeal linked to particular items of that experience as an interpretation would be. Instead, they gather up large portions of that experience and transform them to the rather different bundle of experience."

"XII. The Resolution of Revolutions." "The transition between competing paradigms cannot be made a step at a time, forced by logic and experience." But this "is not to say that no arguments are relevant or that scientists cannot be persuaded to change their minds." "Probably the single most prevalent claim advanced by the proponents of a new paradigm is that they can solve the problems that have led the old one to a crisis." Other arguments include successful novel predictions and "appeal to the individual's sense of the appropriate or the aesthetic." "But paradigm debates are not really about relative problem solving ability. ... Instead, the issue is which paradigm should in the future guide research. ... The man who embraces a new paradigm at an early stage must often do so in defiance of the evidence provided by problem-solving. He must, that is, have faith that the new paradigm will succeed with the many large problems that confront it, knowing only that the older paradigm has failed with a few."

"The proponents of competing paradigms will often disagree about the list of problems that any candidate for paradigm must resolve. Their standards or their definitions of science are not the same. Must a theory of motion explain the cause of the attractive forces between particles of matter or may it simply note the existence of such forces? Newton's dynamics was widely rejected because, unlike both Aristotle's and Descartes's theories, it implied the latter answer to the question. When Newton's theory had been accepted, a question was therefore banished from science. That question, however, was one that general relativity may proudly claim to have solved. Or again, as disseminated in the nineteenth century, Lavoisier's chemical theory inhibited chemists from asking why the metals were so much alike, a question that phlogistic chemistry had both asked and answered. The transition to Lavoisier's paradigm had, like the transition to Newton's, meant a loss not only of a permissible question but of an achieved solution."

"XIII. Progress through Revolutions." "Why should the enterprise sketched above move steadily ahead in ways that, say, art, political theory, or philosophy does not?"

"Scientific progress is not different in kind from progress in other fields, but the absence at most times of competing schools that question each other's aims and standards makes the progress of a normal-scientific community far easier to see. … Once the reception of a common paradigm has freed the scientific community from the need constantly to re-examine its first principles, the members of that community can concentrate exclusively upon the subtlest and most esoteric of the phenomena that concern it. Inevitably, that does increase both the effectiveness and the efficiency with which the group as a whole solves new problems."

"[The] insulation of mature scientific communities from the demands of the laity and of everyday life [is] unparalleled. … There are no other professional communities in which individual creative work is so exclusively addressed to and evaluated by other members of the profession. Just because he is working only for an audience of colleagues, an audience that shares his own values and beliefs, the scientist can take a single set of standards for granted. He need not worry about what some other group or school will think and can therefore dispose of one problem and get on to the next more quickly than those who work for a more heterodox group. Even more important, the insulation of the scientific community from society permits the individual scientist to concentrate his attention upon problems that he has good reason to believe he will be able to solve. Unlike the engineer, and many doctors, and most theologians, the scientist need not choose problems because they urgently need solution and without regard for the tools available to solve them."

"The student in [non-science] disciplines is constantly made aware of the immense variety of problems that the members of his future group have, in the course of time, attempted to solve. Even more important, he has constantly before him a number of competing and incommensurable solutions to these problems, solutions that he must ultimately evaluate for himself. Contrast this situation with that in at least the contemporary natural sciences. In these fields the student relies mainly on textbooks