Text drawn from draft of November 26, 2025
June 18, 2026

Chapter 17

Gettier-Like Cases are Successes of Scientific Progress

1. Introduction

The Gettier cases reviewed in the preceding chapter have attracted considerable attention in the epistemology literature. They have, as far as I know, essentially no presence in the literature in philosophy of science. That may be, we might suppose, because standard examples of Gettier cases involve fanciful scenarios that are remote from actual science and are artfully contrived to make a specific point. Even in the fictional worlds of their invented scenarios, they are by construction extremely rare. They require *two* unlikely occurrences to fit together in just the way needed. First, an otherwise reliable mode of justification for some result must fail to operate as intended; and, second, some further unlikely coincidence allows the result still to be true. Even that is not enough. An essential part of the scenario is that there is a disembodied, omniscient narrator who just declares the truth for our convenience.

These considerations may lead to the expectation that Gettier cases are so rare as to be irrelevant to actual scientific practice. This chapter will show otherwise. Gettier-like cases, it turns out, are pervasive in the history of science; and their results are routinely celebrated as scientific successes, even when their Gettier-like character is recognized.

Two aspects of Gettier cases do not readily translate into real scientific cases. To see how these Gettier-like cases arise in the history of science, Section 2 reviews surrogates for these aspects. First, in place of the omniscient narrator are the judgments of a later stage of the science as to which of the results of the earlier stage are true or false. Second, there is no simple analog in the sciences for success through unanticipated luck. In its place is that success came even though the scientist was using methods that are found in later investigations to be unreliable.

Section 3 reports that Gettier-like cases are not just possible in science, but pervasive. It notes further how the variability often arising in methods used in them follows from improvements in our understanding of prevailing facts. It also reports that the presence of these cases has not been an occasion for alarm in science, even when their Gettier-like character has been discovered.

Section 4 collects seven cases in which important and celebrated discoveries in science turned out to be Gettier-like. The examples recounted are: Bradley's discovery of stellar aberration, Dalton's discovery of atomic theory, Carnot's discovery of the founding principles of thermodynamics, Thomson's discovery of the electron, Bohr's discovery of the quantum mechanics of atoms, Einstein's discovery of the general theory of relativity and Hubble's determination of the Hubble law. In each case, the Gettier-like result is not just preserved as the science progresses. They are routinely celebrated as major achievements in the foundations of a science.

Section 5 concludes by comparing the reception of these cases in the epistemology and science literatures. That the original discovery might not conform with an epistemologist's definition of knowledge is immaterial to the scientists. What matters is that the result was discovered, that we can secure an evidential foundation for it and that the original discoverer does deserve credit.

2. The Character of Gettier-Like Cases in Science

2.1 A Surrogate for the Omniscient Narrator

The first difficulty in identifying Gettier cases in science is that we have no omniscient narrator to tell us when such a case has arisen. The dynamics of scientific progress supplies a suitable surrogate. We will never have an absolute affirmation of the truth of core propositions in science. As science progresses, each stage can identify what it considers to be the errors of the earlier stages and which new results are to be accepted as its truths. Gettier-like cases will be those in which an earlier stage is, informally, lucky in its determination of a truth, as judged by the next stage.

2.2 Method Replaces Luck

Luck, or more precisely, *unanticipated* luck is the key fact in a Gettier case.[1] The difficulty in scientific examples is to determine when some truth has been achieved by

---

[1] Is there such a thing as *anticipated* luck? A beachcomber, sifting the sand each day, does not know, on any particular day, whether luck will turn up some valuable. However, the

unanticipated luck. In the contrived examples from ordinary life, we have a default expectation on the range of possibilities in the fictional Gettier scenario. If we are willing to distribute chances over them, we can then determine whether some particular instance has a small chance and thus can be lucky. It is plausible from default assumptions about how I might behave that my visit to the town square (in the previous chapter) could reasonably happen over a wide time interval. It does seem unlikely and thus lucky that my visit coincides precisely with noon. It is unanticipated luck since I have no reason to think that the precise timing of my visit has special import.

It is, however, quite unclear that a similar concept of luck can be implemented in the cases discussed below in the history of science. Niels Bohr, for example, used a collection of ideas from 19th century electrodynamics, applied inconsistently, to arrive at some key results of the coming quantum theory of the atom. He also used these same ideas to derive many more results inconsistent with the coming theory. We can loosely assign chances to the different times for my visit to the town square. Over what possibilities do we assign chances so that Bohr's successes are lucky?

A better way to identify Gettier cases in the history of science, is supplied by de Grefte's (2023, p. 533) reliabilist characterization above of Gettier cases. He wrote: "…it is a matter of luck that the method one used to form one's belief produced a true belief." The key idea for me is that luck is defined in terms of what the *method* can properly deliver. We can discern the methods used by a scientist and we can determine retrospectively whether the scientist had any basis for expecting those methods to provide results that are important to the next stage of science.

A Gettier-like case in science in one in which a method that turns out in retrospect to be ill-adapted to the science nonetheless proves to deliver a truth, as judged by the next stage. Among the many parts of the method is one that just happens to work by the retrospective judgment of a later stage of the science. That is, some component of the method used replicates something that works according to the later science. The scientists of the time cannot know of the future validation of this component, since they do not know the future science. Call it an

---

beachcomber anticipates that if the sifting is carried out repeatedly, day after day, luck will eventually come their way.

"unanticipated saving factor." It is the distinctive feature of Gettier-like cases in science. They are, informally speaking, the lucky occurrence that makes the case Gettier-like.

2.3 Actual Performance Replaces Possibility

In the Gettier literature, it is routine to make judgments of luck by means of possible world semantics. A success is lucky if the same success would not be realized in some suitably defined nearby possible world. I am loath to use this approach to assess the adaptation of the methods. My concern over judgments concerning possible worlds is their malleability. We need to determine which possible worlds are closer to the actual and which are farther away; and we need to determine what it is for the conditions and methods of the actual world to be realized in them. My suspicion is that there is sufficient elasticity in the complications of these determinations that a creative philosopher could manipulate them to get whichever result happens to suit their purposes.

Here the fact that the examples are real and not fictional stories gives us a better way to proceed. In each case, we can identify the methods actually used by the scientists; and we can determine their prospects. In the Gettier-like cases, we will see that the methods used routinely returned many results deemed false in the retrospective view of later stages. Thus, we have no good basis for expecting these methods to return a result amenable to the later stage. Considerations of luck have been replaced in the analysis by considerations of the actual performance of the method. There is no need to entertain fragile counterfactuals as to how the method would perform in some imaginary possible world. We consult the actual history.

3. Gettier-like Cases are Common

These general reflections open the possibility of Gettier-like cases in scientific progress. It does not establish their frequency. When I began looking for them in the history of science, I was astonished at how often they appear. Gettier cases are *pervasive* in the history of science. The following section recounts briefly a selection of them from a range of sciences with whose history I have some comfort. The examples selected are notable for their importance in the history of science and for their subsequent celebration as major discoveries in science. The examples are: Bradley's discovery of stellar aberration, Dalton's discovery of atomic theory, Carnot's discovery of the founding principles of thermodynamics, Thomson's discovery of the electron, Bohr's discovery of the quantum mechanics of atoms, Einstein's discovery of the general theory of relativity and Hubble's determination of the Hubble law.

3.1 Variability of Methods

We will find that the methods used in the historical examples change over time. This is an expectation of the material theory of induction. Central to the methods are judgements of inductive support. These judgments are in turn dependent on the facts that prevail in each domain. Thus, the scientists' assessments of these relations of support depends in turn on their assessments of which are the facts of their domains. Since these last assessments are fallible, it will turn out commonly that methods that they deem reliable are not so. That variability is what is responsible for the prevalence of Gettier-like cases in the progress of science.

3.2 There is No Cause for Alarm

The sense of alarm surrounding Gettier cases in the epistemology literature is misplaced in the context of science. The worst we can say of the Gettier-like cases in the history science is that the evidential case made for their results was, at the time of their discovery, imperfect. They were still discoveries, clearly important to subsequent developments in their science. For this they are celebrated by later scientists. Whether the scientists originally making the discoveries knew them in conformity with some epistemologist's definition of knowledge is immaterial. What matters is the discovery.

Here, I do not intend a skeptical moral about discovery in science. Rather, I intend an optimistic moral. These cases show how science can proceed successfully, in spite of the fallibility of its methods. We see scientists proceeding responsibly with the best methods then available, as they should, and that later work is able to identify the errors and correct them. In all the cases considered below, the Gettier-like successes do not result from pure happenstance. Rather, unknown to scientists at the time, there is something right about the methods they are using. What they cannot then distinguish are which are those parts and which are not. That is a retrospective judgment. In the case of Bohr's model of the atom, for example, his key results of the stability and energetic discreteness of his electron states was properly a reflection of the stability and discreteness of the observed atomic spectra. Bohr would have recognized this at the time, but he would not have been able to identify just which of his remaining results were erroneous, for his methods did not allow it.

4. Gettier Cases in the History of Science

In order to make it possible to recall a range of Gettier cases in the history of science, I have given only abbreviated accounts of each.

4.1 Bradley's Discovery of Stellar Aberration

James Bradley (1729) reported his discovery of stellar aberration to the Royal Society. According to it, the apparent angular position of a star is altered slightly as a result of the motion of the Earth. In the simplest case, in which the star is located perpendicular to the Earth's motion, we subtract vectorially[2] the Earth's velocity v from the velocity of the light c and find the angular displacement in the direction of the Earth's motion to be $\alpha = v/c$. We saw in discussion of this result in Chapter 11, Section 10, that the general formula is $\tan \alpha = \sin \theta (v/c) / [1+(v/c) \cos \theta]$, where θ is the angular elevation of the star in the direction of motion of the Earth. For Bradley's case of $v << c$, this general formula reduces to the one Bradley reported, $\sin \alpha / \sin \theta = v/c$.

Bradley's discovery, as we saw in Chapter 11, was a celebrated result and one of the few experimental results that Einstein reported as decisive for his discovery of special relativity. It is, on our best present understanding, a truth. However, Bradley's method was flawed. For he derived the result by Newtonian addition of velocities. Famously, according to special relativity, this method fails for motions close to or at the speed of light. Under the Newtonian rule, the motion of the Earth has to be added vectorially to the speed of a light signal to produce an altered speed for the light. A postulate of special relativity is that the speed of light remains unchanged, even if measured from a moving Earth. Special relativity offers a different rule for the composition of velocities in place of the Newtonian rule of vector addition that preserves the constancy of the velocity of light.

Bradley's result survived, however, because of an unanticipated saving factor. The relativistic[3] and Newtonian rules for velocity addition give different results for the composed speeds, but the *same* result for the angular deflection under the small v/c conditions of aberration.

---

[2] To use the vector concept, not introduced until the following century.

[3] The relativistic rule yields $\tan \alpha = (\sin \theta (v/c) / [1+(v/c) \cos \theta])\sqrt{1 - v^2/c^2}$. The correction to the Newtonian result is a term second order in (v/c) and thus negligible for stellar aberration.

There is an intermediate stage in the history of astronomy in which the Gettier-like character of Bradley's discovery was explicitly recognized. Bradley's Newtonian addition of velocities applied as long we could conceive of light as the rapid propagation of Newtonian light corpuscles. In the nineteenth century, this Newtonian theory of light was replaced by a wave theory. The Newtonian addition of velocities could no longer be applied in a straightforward way to the direction of wave propagation and it became quite unclear as to how the effect of stellar aberration was to be recovered. Arthur Berry, writing a history of astronomy at the end of the nineteenth century, expressed puzzlement over Bradley's success. His account captured rather perfectly its Gettier-like character with the verbiage I have emphasized below in his remarks (1898, p. 265):

> The curious inference may be drawn that, if the more correct modern notions of the nature of light had prevailed in Bradley's time, it must have been very much more difficult, if not impracticable, for him to have thought of his explanation of the stellar motions which he was studying and *thus an erroneous theory led to a most important discovery*.

The difficulty for the wave theory had been resolved in electrodynamic theory, though apparently not to Berry's satisfaction, by H. A. Lorentz' 1895 theorem of corresponding states. The mathematics of that theorem was then incorporated by Einstein into his special theory of relativity. Stellar aberration is, we now recognize, a relativistic effect, arising from the relativity of simultaneity.

4.2 Dalton's Discovery of Atomic Theory

John Dalton's (1808, 1810) *New System of Chemical Philosophy* is celebrated as the beginning of modern atomic theory in chemistry. Dalton proposed facts that we still believe lie at the foundations of chemistry: that each element is composed of identical atoms of that element; and that the atoms combine to form the familiar compounds of ordinary chemistry, such as water, muriatic acid, ammonia and so on.

Our concern is with the methods that Dalton used to arrive at his results. What is clear is that whatever methods he was using, they were quite unreliable and produced false results throughout his work. Part I of Dalton's *System* (1808) was concerned with heat and the physical properties of bodies formed from atoms. He determined (Chapter II) that the atoms of gases consist of particles enveloped in atmospheres of heat, where the latter was conceived as an element, caloric, following Lavoisier's system of elements. The enveloped cores are spherical and thus he could characterize a gas as arranged as a neat pile of shot. He used this static model to account for the elastic behavior of gases under changes of pressure and temperature, in direct conflict with the kinetic theory of heat that emerged a few decades later.

Dalton's treatment of the chemical properties of material in Part II (1810) consisted largely of extensive reports of chemical experiments and what could be inferred from them. What little he could infer of the details of the atomic constitutions of these chemicals was, again, largely incorrect. He did not recognize that elemental hydrogen, oxygen, nitrogen and so on consist of diatomic molecules. He had no developed theory of chemical bonding to account for how compounds formed. In its absence, he determined that water is a compounding of *one* atom of hydrogen with *one* atom of oxygen. In modern language, that amounts to the formula HO and not the familiar H_2O.

Methods dependent on this factually erroneous portrayal of materials are quite unreliable. A specific example are the rules he developed in Part I, p. 214, to determine the ratios of atoms in each compound. Those rules were responsible for the determination of water as HO. They enjoin us to use the simplest ratios possible; and lead directly to the one-to-one ratio of hydrogen to oxygen.[4]

With methods so unreliable, there can be no expectation that Dalton could secure the credit for introducing modern atomism to chemistry. My sense is that the key factor lay in his timing. The idea that all matter is atomic has been proposed routinely since antiquity. All such proposals were ill-fated prior to Lavoisier's epochal determination of the elements in his *Elements of Chemistry* (1790). After it, success is assured for the first chemist to propose an atomic constitution for the chemical element, no matter how flawed their other results may be.

4.3 Carnot's Discovery of the Foundations of Thermodynamics

Sadi Carnot's (1824) *Reflections on the Motive Power of Fire*, is, in my view, one of the greatest works of science of all eras. Its pre-eminence lies in it not just laying the foundations of the new science of thermodynamics, but in introducing a novel form of theorizing that is

---

[4] See Norton (2024, Ch. 11) for an account of the half century of work needed to establish the correct atomic weights and molecular formulae.

distinctive of thermodynamics.[5] He recognized that it is possible to have a general theory of efficiency covering heat engines of all types, not just steam engines, and no matter their operating fluids or other details. The maximum capacity of these heat engines to produce motive power for each unit of heat supplied was determined solely by the temperatures of the hot source from which it drew heat and cold sink to which it discharged heat. That maximum capacity was realized by reversible operations, that is, ones that could run equally in the forward or the reverse directions. The key insights leading to the later formulation of the second law of thermodynamics are routinely and correctly attributed to Carnot's work.

Nonetheless, Carnot's successes are a Gettier-like case. For Carnot's analysis was carried out within the caloric theory of heat, using methods that routinely led to incorrect results. In the later thermodynamic theory, a heat engine *converts* the energy of heat into work energy. In Carnot's analysis, caloric, that is heat, is a conserved substance. There is no conversion of heat to work. It is solely the *transfer* of heat from hot to cold that engenders motive power. It is then inevitable that much of Carnot's analysis produced results that we now think incorrect. Most notably, because he assumed that caloric is conserved, the quantity of heat drawn from the heat source must match the quantity discharged to the heat sink.

How did Carnot achieve his successes using an understanding of heat engines that is so at variance with the modern view? Here are the saving factors he might not have foreseen. The general thermodynamic principles that were carried forward to modern thermodynamics prove to be insensitive to the particular assumptions that Carnot made erroneously about heat and heat engines. A quite specific one of his erroneous assumptions made his successes possible. For, if we assume that heat engines convert heat energy into work, it would be natural to conclude that an engine that converts heat fully into work is the most efficient. There would be no point in including a heat sink for waste heat in the analysis of the idealized, most efficient engine. Modern thermodynamics recognizes that no heat engine can operate without a colder heat sink to which to discharge waste heat. It is a necessity of its operation and, without it, an analysis of the style of Carnot cannot be carried out. Carnot's assumption about the conservation of caloric, however, requires the heat sink and made it a fixture in the analyses of later thermodynamicists.

---

[5] For an analysis of how he was able to achieve this feat, see Norton (2022).

4.4 Thomson’s Discovery of the Electron

J. J. Thomson (1897) reported the results of his experiments on cathode rays. They were, he announced, beams of negatively charged particles. This announcement is now celebrated as the discovery of the electron, the first particle of the many to come in modern particle physics. In his paper, Thomson sought to determine whether these cathode rays are waves propagating in the ether, as Hertz, Lenard and others had supposed; or whether they are streams of charged particles. Thomson found that the rays are deflected by electric and magnetic fields exactly as would a stream of particles, if each particle carried a negative electric charge. The deflections allowed him to determine a unique mass to charge ratio that is characteristic of the particles that would soon come to be called “electrons.”

The case against wave propagation specifically was laid out elsewhere in Thomson’s writings. (For a more detailed narrative of the evidential arguments deciding between waves and particles, see Norton, 2021, Ch. 9.) The decisive consideration was that cathode rays are deflected by a homogeneous magnetic field. There was no mechanism in the theory of waves in the ether that would allow this. Charged particles, however, are deflected by such a homogenous field by the Lorentz force law.

Thomson’s methods were those dictated by late 19th century electrodynamics. Using them, he made his case well. What he could not foresee was that the quantum theory of the early 20th century would find cathode rays to be the propagation of a quantum electron wave. His case for electrons as localized particles and not waves collapsed. A quantum electron wave can be deflected by a homogeneous magnetic field. Thomson’s 19th century methods were ill-adapted to the quantum physics of the 20th century.

However, in spite of the failings of his methods, enough of his conclusions for his particles survived. The unanticipated saving factor is that the deflections he attributed to the behavior of particles were recovered in the quantum theory by Ehrenfest’s theorem. It showed that these are the motions of a quantum wave, if the quantum wave is sufficiently localized in space. That then allowed the mass and charge Thomson had determined for his particles to become the corresponding parameters of the quantum electron.

4.5 Bohr’s Discovery of the Quantum Character of Atoms

Niels Bohr’s (1913) “On the Constitution of Atoms and Molecules” depicts hydrogen and other atoms as consisting of positively charged nuclei, surrounded by one or more electrons in stable, discretely spaced energy states. The electrons can transition up and down between these stable energy states by absorbing or emitting the energy difference between the states in electromagnetic waves. These results provided the basis for the quantum theory of atoms that built upon Bohr's findings.

Even though the results just described are foundational in the later quantum theory of atoms, Bohr's methods were quite ill-adapted to the new quantum theory. His analysis employed the methods and results of 19th century electrodynamics. Bohr's electrons were not the quantum waves that surround the nucleus of an atom in diffuse clouds. His were still the localized particles of Thomson and they orbited in circular or elliptical orbits. Bohr's electrons jumped instantaneously between their stable energy states. Their transitions were not governed by the Schrödinger evolution of the yet to be developed quantum electrodynamics. The electromagnetic energy absorbed and emitted by Bohr's electrons were in the form of classical waves, not as the light quanta Einstein had introduced in 1905 that became the foundation of the quantum treatment of electromagnetic systems.

Worse, Bohr's 19th century methods were employed inconsistently. The classical theory insisted that an orbiting charge could not adopt a stable state, but would spiral into the atomic nucleus. Bohr posited stability. The classical theory allowed that an electron could adopt a continuous set of energy states in its orbiting the atomic nucleus. On the strength of the evidence of discrete atomic spectra, Bohr inferred otherwise.

In sum, Bohr's methods were unreliable when judged by the later quantum theory. Most of his results were incorrect. But within the compendium of false results lay a few that became the foundation of the new quantum theory of atoms. These were the ones that reflected the characteristics of the atomic spectra that formed the empirical basis of his theory: the stability and energetic discreteness of his electron states reflected the stability and discreteness of the observed atomic spectra. The unanticipated saving factor is that just these were the results that would survive into the new science to come.

4.6 Einstein's Discovery of the General Theory of Relativity

For over seven years, from 1907 to 1915, Einstein expended at times exhaustive efforts to generalize his special theory of relativity. The result was his general theory of relativity. It was celebrated then and still today as our most successful and most profound relativistic theory of gravity. Private notes, correspondence and intermediate publications have provided us extensive documentation of the stages of Einstein's thinking and the methods he used. On his later summary, his methods were controlled by three principles: the idea that the relativity of motion was to be extended to acceleration; Mach's principle, that the inertia of a body results from an interaction with the other masses of the universe; and the principle of equivalence, which asserted the identity of gravity and acceleration.

What makes Einstein's discovery Gettier-like is that two of these ideas, so foundational to his discovery, prove to be incompatible with his final theory. His theory does not generalize the relativity of motion to acceleration; and his rewriting of the generalized principle as a principle of general covariance was shown by Kretschmann in 1917 to be vacuous. Mach's principle also failed in the theory. Einstein disowned it for its dependence on an ontology of interacting masses at variance with the notion of unified field theory that dominated his later thinking. In so far as Einstein's methods were controlled by these principles, we could not now expect them to lead him successfully to his final theory. Yet he did arrive at the theory.[6]

In retrospect, I believe his success depended at least in part on his insistence on further conditions that are sufficiently unremarkable as to elude inclusion in celebratory histories, but are, in retrospect, powerful guides toward the final theory. I have explored some of them in Norton (2020). During the crucial period of the discovery of the theory, Einstein could not have anticipated that these were the saving factors and that the principles he prized would fail in the theory.

4.7 Hubble's Discovery of the Hubble Law

Hubble's (1929) celebrated discovery of the recession of the galaxies was recounted in Chapter 10 here. Its main result is a linear relationship between the velocity of galactic recession and the distance to the galaxy. In retrospect, his results were a mix of successes and failures. The linearity of the relation survives and carries his name. Hubble's determination of the constant of proportionality, 500 km/sec/megaparsec, proved to be 6 or 7 times greater than the later value of roughly 70 km/sec/megaparsec.

---

[6] For my early recounting of Einstein's discovery, based on private calculations in his Zurich notebook, see Norton (1984). For more details of the incompatibility of these principles with his theory, see Norton (1993).

The analysis in Chapter 10 recounts where Hubble's methods failed in the determination of the constant. The difficulty was that he did not know that there is more than one type of Cepheid variable star that formed the basis of his estimates of the distanced to the galaxies. He had presumed the wrong type in estimating distances to nearby galaxies and this compromised all this remaining estimates. The saving factor that Hubble could not anticipate was a result of later investigations. There is only one other type of Cepheid to be considered. Accommodating it into the analysis merely requires a recalibration of Hubble's distance estimates by a constant factor. The correction altered the constant, but not the celebrated linearity of Hubble's law.

5. Conclusion

It is interesting to reflect on the different receptions received by Gettier cases in epistemology and by Gettier-like cases in science. In the first, Gettier cases are alarming. They are taken to reveal a profound flaw in a central and widely used definition of knowledge as justified, true belief. The challenge, which has been met poorly, is to find a way to defend this definition.

In science, Gettier-like cases are regarded as benign and even inevitable. They are recognized as cases of scientists proceeding responsibly with the best methods then available. What matters is the fact discovered, even if it was by methods that are in retrospect judged to be flawed. For such flaws can be identified and corrected and the discovery can be given a more secure foundation. What does not matter is whether the original discovery conformed with some epistemologist's definition of knowledge. Progress in science is, at any moment imperfect. Over time these imperfections can be corrected, so that our science rests on ever more secure foundations. This is science progressing responsibly within the bounds of fallibilism.

References

Berry, Arthur (1898) *A Short History of Astronomy*. London: John Murray.

Bohr, Niels (1913) "I. On the Constitution of Atoms and Molecules," *Philosophical Magazine* Series 6, **26**, No. 151, pp. 1-25.

Bradley, James (1729). "An account of a new discovered motion of the fixed stars." *Philosophical Transactions of the Royal Society*, **35**, pp. 637–661.

Carnot, Sadi (1824). *Réflexions sur la Puissance Motrice du Feu*. Paris: Bachelier. Trans., *Reflections on the Motive Power of Heat*. Wiley. 1897.

Dalton, John (1808) *A New System of Chemical Philosophy*. Part I. Manchester: R. Bickerstaff.

Dalton, John (1810) *A New System of Chemical Philosophy*. Part II. Manchester: R. Bickerstaff.

de Grefte, Job (2023) "Knowledge as Justified True Belief" *Erkenntnis*, **88**, pp. 531–549.

Hubble, Edwin (1929) "A Relation between Distance and Radial Velocity among Extra-Galactic Nebulae." *Proceedings of the National Academy of Sciences*, **15**, pp.168–73.

Lavoisier, Antoine-Laurent (1790) *Elements of Chemistry.* Trans. Robert Kerr. Edinburgh: William Creech.

Norton, John D. (1984) "How Einstein Found His Field Equations: 1912-1915," *Historical Studies in the Physical Sciences*, **14,** pp. 253-315.

Norton, John D. (1993) "General Covariance and the Foundations of General Relativity: Eight Decades of Dispute," *Reports on Progress in Physics*, **56**, pp.791-858.

Norton, John D. (2020) "Einstein's Conflicting Heuristics: The Discovery of General Relativity," pp. 17-48 in *Thinking about Space and Time*: *100 Years of Applying and Interpreting General Relativity*. *Einstein Studies*, Volume 15. C. Beisbart, T. Sauer, C. Wüthrich (eds). Cham, Switzerland: Birhäuser/Springer Nature.

Norton, John D. (2021) *The Material Theory of Induction*. BSPS*Open*/University of Calgary Press.

Norton, John D. (2022) "How Analogy Helped Create the New Science of Thermodynamics," *Synthese*. **200**, article 269, pp. 1-42.

Norton, John D. (2024) *The Large-Scale Structure of Inductive Inference.* BSPS*Open*/University of Calgary Press.

Thomson, Joseph J. (1897) "Cathode Rays," *Philosophical Magazine,* **44,** pp. 293–316.