Synthese (2021) 198 (Suppl 25):S6115–S6141
https://doi.org/10.1007/s11229-019-02500-6



# Realism, perspectivism, and disagreement in science

Michela Massimi[1]

Received: 29 October 2018 / Accepted: 29 November 2019 / Published online: 20 December 2019


**Abstract**
This paper attends to two main tasks. First, I introduce the notion of perspectival disagreement in science. Second, I relate perspectival disagreement in science to the broader issue of realism about science: how to maintain realist ontological commitments in the face of perspectival disagreement among scientists? I argue that often enough perspectival disagreement is not at the level of the scientific knowledge claims but rather of the methodological and justificatory principles. I introduce and clarify the notion of 'agreeing-whilst-perspectivally-disagreeing' with an episode from the history of modern physics: namely, how we came to agree about the electric charge as a minimal natural unit despite different scientific perspectives and associated data-to-phenomena inferences available for it in the period 1897–1906.

**Keywords** Realism · Perspectivism · Disagreement · Electric charge · Data-to-phenomena inferences · M. Planck · J.J. Thomson

## 1 Introduction: realism and the history of the electron

Scientific disagreement has been a battleground for realism in science. Is not disagreement among scientists a powerful argument against realism? Thomas Kuhn put forward this point in *The Structure of Scientific Revolutions*, in the context of the controversy surrounding Lavoisier's discovery of oxygen:

> Though, undoubtedly correct, the sentence, "Oxygen was discovered", misleads by suggesting that discovering something is a single simple act assimilable to our usual (and also questionable) concept of seeing… any attempt to date the discovery must inevitably be arbitrary because discovering a new sort of phenomenon is necessarily a complex event, one which involves recognizing both *that* something is and *what* it is. Note, for example, that if oxygen were dephlogisticated

✉ Michela Massimi
michela.massimi@ed.ac.uk

1 Chair of Philosophy of Science, School of Philosophy, Psychology and Language Sciences, The University of Edinburgh, Dugald Stewart Building, 3 Charles Street, Edinburgh EH8 9AD, UK

air for us, we should insist without hesitation that Priestley had discovered it, though we would still not know quite when. (Kuhn 1962/1996, p. 55)

Scientists often disagree both *that* something is and about *what* it is. This kind of scientific disagreement is of great interest to historians of science, who might want to establish *who* really discovered some entity—e.g. whether it was Joseph Priestley rather than Antoine Lavoisier who discovered what we now call 'oxygen';[1] or, whether it was George J. Stoney or J.J. Thomson who really discovered the electron, given that in his Nobel Prize speech Thomson was still calling his entity a 'corpuscle'.[2] But, historiographical debates aside, disagreement *that* something is and about *what* it is also raises pressing questions for philosophers with realist leanings. How are we to spell out the realist commitment in cases where scientists disagree about the nature of the entity? What is it like to be a *realist* in the face of scientific disagreement?

This paper takes some steps towards answering this question by looking at the case of the electric charge. What is to be said about the electric charge? Has there ever been any disagreement about it? As it happens, at the turn of the last century, there was a disagreement about the nature of the electron as the bearer of the electric charge. And there were also different views about the electric charge and the reasons why it is a 'natural unit'. Digging (briefly for limits of space here) into the history of this scientific disagreement around 1897–1906 is instructive for two different reasons. First, it helps elucidate the nature of disagreement. This was rooted not in scientists accepting or denying pieces of evidence, but rather in the way in which pieces of evidence, or, better, data, were embedded in different *scientific perspectives* and used for inferring a variety of phenomena, from which the electric charge could in turn be inferred. Second, a brief foray into the history of the electric charge can help us understand the exact nature of the realist commitment that is compatible with what I call 'perspectival disagreement'.

In brief, this paper elucidates how it is possible to be realist about the electric charge—understood as a minimal natural unit, not as a property—while also perspectivally disagreeing about how data-to-phenomena inferences provide evidence for it. My goal in what follows is to explain how this realist commitment was borne out of data-to-phenomena inferences that were perspectival every inch of the way. Or, better, I aim to explain how it is possible to be realist *that* there exists a natural unit of charge and about *what* it is, despite perspectival disagreements concerning how to reach this conclusion.

To be clear: I will be defending neither traditional scientific realism nor property realism. The realism I defend here is not downstream of any prior commitment to a true (final? God's-eye?) theory of the electron (with charge as one of its theoretical posits). Nor is my realist commitment akin to some version of property realism. For I do not take the electric charge as a Lewisian natural property in a Humean mosaic of sparse natural properties. Instead, I take electric charge as a feature involved in a number of robust *phenomena* in Bogen and Woodward's sense (1988): from the bending of cathode rays in the presence of an electric or magnetic field, to the electrolysis of water; from metal conductivity to blackbody radiation. It is here argued

[1] See Chang (2012, 2015), and Kusch (2015) for a recent debate.

[2] For this historiographical debate, see, for example, Falconer (2001) and Achinstein (2001).

that our realist commitment to the electric charge is borne out of data-to-phenomena inferences that are thoroughgoingly perspectival (building on Massimi 2007, 2011). Data are perspectival in being harvested within well-defined scientific perspectives, and the inferences from the data to the phenomena are themselves perspectival in that the evidential line of reasoning "If data D, then conclude that phenomenon P" is also embedded in a well-defined scientific perspective.

By 'scientific perspective' I mean (as in Massimi 2018b, p. 152) the actual—historically and intellectually situated—scientific practice of a real scientific community at a given historical time. Scientific practice should here be understood to include: (1) the body of *scientific knowledge claims* advanced by the scientific community at the time; (2) the experimental, theoretical, and technological resources available to the scientific community at the time to *reliably* make those scientific knowledge claims; and (3) second-order (methodological-epistemic) principles that can *justify* the scientific knowledge claims advanced. Scientific perspectives so understood are different from Kuhn's scientific paradigms. Nor is perspectival disagreement to be conflated with Kuhn's 'working in a new world' type of scenarios.[3]

Scientists endorsing different scientific perspectives produce different *perspectival representations* of the same phenomenon (e.g. electric charge), which cannot be equated with Kuhnian *Gestalt* switches because perspectives do not mould perspectival facts; they do not act as cookie-cutters in the worldly dough. Scientific perspectives deliver perspectival knowledge of phenomena; they do not deliver perspectival facts.[4] Different scientific perspectives privilege different kinds of data, e.g. depending on whether one is studying water electrolysis, black-body radiation, or electromagnetic induction, as we shall see below. And they use those data to provide evidence for the phenomenon at stake within the technological, experimental, and theoretical resources available to each. Thus, the ensuing data-to-phenomena inferences are thoroughly perspectival. The key question then becomes: how do we come to agree *that* something is and *what* it is, in spite of our (thoroughgoingly perspectival) ways of knowing? My main upshot is to show in historical detail how pluralism about ways of knowing is not necessarily a hurdle to realist commitments in science, *pace* Kuhn's influential por-

[3] See for a discussion of Kuhn (1993, 2000), Hacking (1993) and Massimi (2015a).

[4] I have discussed elsewhere (Massimi 2015b) possible semantic ways of reading and understanding Giere's (2006) own version of scientific perspectivism, as well as the semantic argument behind the late Kuhn's own view (Massimi 2015a). Other varieties of perspectivism focus more directly on the semantic nature of knowledge claims. For example, Teller (2011, 2018, 2020) has argued for a kind of perspectivism that focuses on what he calls "semantic alter-egos"—namely, false precise statements (e.g. "John's height is six feet precisely") that can get transformed into true yet imprecise ones (e.g. "John's height is six feet close enough"), see Teller (2011, p. 469). A ground-clearing remark is in order here. There are different possible ways of thinking about what does the heavy-lifting when it comes to perspectival knowledge. One way is to focus on the role of main theoretical terms and how perspectival meaning-change might affect possible reference continuity and associated realist commitments. Another way (the one I am pursuing here) stresses the epistemic commitments that agents are willing to take when engaging with perspectival data-to-phenomena inferences. There is no denying that any knowledge claim comes with a semantic component. But the difference between the two approaches concerns what is doing the work for the 'agreeing-whilst-perspectivally-disagreeing': i.e. reference-continuity for main theoretical terms versus truth-conducive conditionals-supporting inferences. The two do not have to be construed as antithetical to one another. On the contrary, they just place a different emphasis on the process of 'agreeing-whilst-perspectivally-disagreeing'.

trait of how disagreement plays out in the realist arena. On the contrary, perspectival disagreement can deliver on realism in a non-lucky way.

Section 2 tackles the nature of perspectival disagreement in science before illustrating it at work with the historical case study. Section 3 presents three scientific perspectives that at the turn of the last century jointly shaped our realist view about the electric charge: J.J. Thomson's *Faraday–Maxwell* perspective (Sect. 3.1); Grotthuss' and Helmholtz's *electrochemical* perspective (Sect. 3.2); and Max Planck's *quantum* perspective (Sect. 3.3). In Sect. 4, I return to the discussion of ontological commitment and clarify what kind of realism is compatible with perspectival disagreement, and I draw some concluding remarks in Sect. 5. But before I embark on this journey, let me explain and motivate the choice of this particular case study.

Why engage with the history of the electric charge? While it is very attractive to engage with the history of long-defunct entities (be they caloric, phlogiston, the ether, or whatever else) to challenge the received wisdom about realism in science, the history of the electric charge does not lend itself naturally to such an exercise. The electric charge has had an enviably successful track record. The ether—postulated in the Queries added to Newton's *Opticks*—was abandoned at the turn of the twentieth century. Caloric—still visible in Dalton's drawings of atoms at the start of the nineteenth century—was dismissed in the first half of the nineteenth century. Electrical and magnetic fluids, widely used in the early days by Ampère and others, have similarly gone. But the electric charge, whose history is surprisingly entangled with that of the ether, thermal phenomena, and electrical fluids, has survived to the present day. It is the history of this survival around 1897–1906 that I want to briefly recount in what follows, not for the sake of history as such, but for the sake of clarifying where our realist commitment originates from and the role of perspectival disagreement in it.

In the literature, attention has been paid to the electron more than to the electric charge itself as a minimal natural unit. In the late nineteenth century, the electron was first regarded as the minimal unit of electric charge (by George J. Stoney in the 1870s); then as a structural element of an elastic ether (by George Fitzgerald and Joseph Larmor); and then as a corpuscle (by J.J. Thomson in 1897), just to mention a few examples. In the early twentieth century, the scientific representations of the electron similarly underwent major changes with the discovery of the electron spin (by Pauli, Kronig, Uhlenbeck, and Goudsmith in 1924–1925), and Dirac's relativistic equation for the electron in 1926. Unsurprisingly, historians and philosophers have been fascinated with the history of the electron as a case study to better understand the evolution of our scientific representations (see, for example, Arabatzis's excellent 2006 book). Three main philosophical stances can be found in the literature.

*1. Entity realism and scientific realism.* Some philosophers have argued that the realist commitment to the electron is unscathed by historically changing representations. The burden of such realism is usually carried by the causal theory of reference (the typical semantic weapon in the realist arsenal). If the reference of a theoretical term is fixed not by a description (which might come and go with our changing representations) but by its role in a causal baptism, reference continuity (and realist commitment) is secured (think of Hacking: "if you can spray electrons, they are real", based on Putnam's causal theory of reference). Scientific realists go further and argue as Anjan Chakravartty has for example done that "Entities are capable of these relations because of the properties they have, and properties and relations are precisely what theories describe, so by asking the realist to believe only in the existence of certain entities but not further aspects of theories, the entity realist (ER) asks too much. …Although theories regarding the nature of an entity might change over time, one might carry one's knowledge of its existence throughout… indeed, starting with Thomson, Robert Millikan, Ernest Rutherford and throughout the twentieth century, a long line of experimentalists interacted with the same entity….Perhaps not all of these experimenters had sufficient causal knowledge of the electron to satisfy ER—not all manipulated it, for instance—but no doubt many met the required standard" (Chakravartty 2007, pp. 31–32).

*2. Property realism.* Other philosophers have gone selective in their realist commitment. For example, Bain and Norton (2001, pp. 454–461) have argued that the history of the electron is not one of pessimistic but optimistic induction because the sequence of theories from Thomson to Millikan, Bohr, Pauli, and Dirac highlighted a "growing core of historically stable properties of the electron": from the electric charge to the mass, from the angular momentum to the spin. Realist commitment to properties goes hand in hand with the reliability of experimental methods in detecting and overdetermining the measured values of the relevant properties.

*3. Coherent holism.* A less sanguine realist commitment to the electron can be found in Norwood Russell Hanson[5] and Henry Margenau,[6] both of whom warned against the limit of applying classical mechanical images to quantum entities. What is it that we are committed to when we claim to have very accurate knowledge of the electron, if the electron is neither a particle nor a wave? By contrast with entity realism, scientific realism and property realism, coherent holism takes a more decisive anti-realist stance. On this view, not only is any attempt at producing images or representations of the electron fraught with difficulties. But it is also an idle exercise, for it naïvely (and mistakenly) assumes that those representations carry the burden of our realist commitments. A somewhat similar approach can be found in a contemporary trend in epistemology that places non-factive understanding (rather than knowledge) centre-stage.[7] The price to pay for this move, however, is to replace knowledge (which implies justified true beliefs) with non-factive understanding.[8]

Where does this discussion leave us? Of the three philosophical views here briefly sketched, the third concedes too much to holism and non-facticity, in my view. It is one thing to claim with Hanson and Margenau that the electron is subject to a "radical unpicturability". It is another thing to conclude that therefore our different representations of the electron are all equally non-factive. There is a sense in which granularity matters in science. For it allows us to ascertain *what* J.J. Thomson got right, and *what* he got wrong about what he called the 'corpuscle'. It is the granularity of scientific knowledge claims, our ability to assess each and every of them as either

[5] See Hanson (1958/1972, pp. 123–125).

[6] See Margenau (1950, pp. 321–322).

[7] Elgin (2017), for example, defends a kind of non-truth-conducive non-factive understanding about scientific practice. And in this context, she gives the example of the positron (Elgin 2017, p. 221).

[8] Non-factive holistic understanding denies the granularity of scientific knowledge claims (whereby, say, J.J. Thomson's knowledge claim about the charge-to-mass ratio was factive and true, but his knowledge claim about electrons being scattered in the atoms like plums in a pudding was non-factive and false).

true or false that enables scientists across different perspectives to retain or withdraw individual knowledge claims that might either continue to serve them well or fail to do so over time. Thus, while I agree that understanding is an important feature of scientific inquiry, it would be interesting to explore how non-factive understanding may complement truth-conducive knowledge (rather than taking the former as antithetical to the latter). What is it like to gain a *better* understanding of the electric charge, for example?

At the other hand of the spectrum, *realism* (be it entity realism or scientific realism) has traditionally faced a problem when dealing with the history of science. The fact that our representations of the electron (and the electric charge) have changed over time cannot easily be brushed aside in the name of the causal theory of reference. Entity realists have a point here against scientific realism. Realist commitments do not necessarily require full-blown theories. The kind of commitment to the electric charge delivered by Millikan's oil-drop experiment is not necessarily the same kind of commitment delivered by, say, Dirac's relativistic equation of the electron. What is missing in *scientific realism* is once again the ability to tease out *upfront* (i.e. not with wisdom of hindsight) which knowledge claims are true, and which are false, when a theory as a whole gets superseded.

This leaves us with property realism as the most attractive philosophical view out of these three. There is a lot to like in the idea of a "growing core of historically stable properties of the electron": it provides us with a metaphysical tether, without buying into any inflated metaphysics. It accounts for the historical evolution of our representations without sacrificing realist commitments. However, property realism seems to displace the bump in the carpet. For the thorny question "What is *really* an electron?" becomes the (equally thorny) question "What is *really* the electric charge?", as a property that has grown stable in the cluster of properties we tend to identify the electron with.[9] Did scientists ever disagree about the electric charge? Before I give the historical details that belie intuitions about the metaphysics of ready-made natural properties in Sect. 3, let us get clear about the nature of scientific disagreement at play whenever there is a plurality of scientific perspectives.

## 2 Perspectival disagreement in science

The vast recent literature on peer disagreement (cf. Feldman and Warfield 2010 for an overview) has concentrated on cases where 'peers' who have access to the same evidence and share the same ability to reason might nonetheless disagree. Most of the debate has revolved around the question of what is rational to do in the circumstances. The two main views—conciliatory and steadfast theorists—have argued, respectively, for the need to revise one's own beliefs in the light of the peer disagreement; or to remain steadfast in one's own belief despite peer disagreement (see Christensen and Lackey 2013 for an extensive treatment). Perspectivism has featured in this debate in

[9] In what follows, I shall exclusively concentrate my attention to the question of what the *electric charge* is. I will not attempt to address the broader question of what the *electron* is (as a natural kind) because it would require a separate, detailed discussion about natural kinds. Such discussion will have to wait for another occasion.

the effort to link rationality to an individual's point of view, namely what would be rational for an individual to believe given available evidence, principles of logic, and possible defeaters and undercutters. Kvanvig (2013), for example, offers an interesting defence of a fully perspectivalist account, whereby rationality is perspectival in the sense that whether a behaviour is considered rational depends on the "egocentric point of view of the individual in question" (p. 224).

In what follows, I do not connect perspectivism directly with what is rational to believe, or with rationality in general. I am interested instead in how epistemic agents, or, better, epistemic communities, who might endorse different scientific perspectives (as defined in Sect. 1) might disagree in a distinctive way. The kind of perspectival disagreement at play in *bona fide* scientific scenarios is often enough *justificatory*, and not *factual*. It does not concern how informed each interlocutor is, whether or not they have access to the same data and pieces of evidence. Often enough, genuine scientific disagreement arises despite peers having access to the same data and empirical observations. It does not concern how competent each interlocutor is either; whether they each follow rules of logic and probabilistic reasoning. Presumably, they all do insofar as their activities can be described as *bona fide scientific* (as opposed to unscientific, pseudo-scientific, propagandistic or similar other cases which I will not discuss). Thus, the kind of perspectival disagreement that I am here focussing on is disagreement between interlocutors who can legitimately claim the same competence. It is justificatory in nature because, despite access to the same evidence and same competence in the rules of logic and probabilistic reasoning, scientists may nonetheless disagree in the way they come to justify their respective beliefs.[10]

Epistemic communities working in different scientific perspectives might disagree at different levels, and for different reasons. Sometimes the disagreement affects the very *scientific knowledge claims*[11] that they respectively endorse. Other times, they disagree in that they might adopt different experimental, theoretical, and technological resources to *reliably* make those scientific knowledge claims. And, on yet other occasions, they disagree by subscribing to different second-order (methodological-epistemic) principles to *justify* the scientific knowledge claims. Perspectival disagreement so understood is at a distance from Kuhnian incommensurability because it is a piecemeal multi-level process. It is not a matter of all-or-nothing, 'living in a new world', or *Gestalt* switches. For perspectival disagreement envisages the possibility that two or more epistemic communities from different scientific perspectives[12] might agree about either the *reliable* experimental procedures or the

10 I am here taking my cue from perspectivalist accounts of justification in epistemology such as Sosa (1991) and Rosenberg (2002).

11 A clarification about the terminology here. It might seem tautologous to talk about reliability and justification when talking about "scientific *knowledge* claims" for the very notion of *knowledge* as justified true belief presupposes both. Please note, the expression is used here to stress the "*claims* of knowledge" advanced by different scientists in different historical epochs and across different scientific perspectives. In some cases they did amount to genuine knowledge, in other cases they did not because the belief in question, although justified and sometimes even reliably generated, was nonetheless false (think of George Fitzgerald's belief that material bodies would contract as a way of making sense of the Michelson–Morley experiment; or Priestley's belief that phlogiston was released in combustion).

12 I am here assuming "different scientific perspectives" that are nonetheless sufficiently close in historical terms for the inter-conversational disagreement to take place. A medieval alchemist and a twentieth-century

*justificatory* principles, while also disagreeing on which *scientific knowledge claims* these in turn support. Let us briefly look at two famous examples to illustrate this point.

Consider the Michelson–Morley experiment in 1887 giving null evidence for the phenomenon of the ether drag. Michelson and Morley devised an experiment with an instrument called the interferometer to detect whether the Earth was indeed moving through a subtle invisible substance called the ether. But no evidence for it was found. While no one doubted the *reliability* of the experiment, in 1889 George Fitzgerald took it as a springboard for accommodating this piece of negative evidence with the then accepted Fresnel–Lorentz theory of the luminiferous ether, assuming that the negative experimental result must have been due to some kind of contraction.[13]

By contrast, a few years later, Einstein took it as evidence for the conclusion that the ether of Newtonian origin did not exist. The disagreement between Fitzgerald's still classical perspective and Einstein's new relativistic perspective can be regarded as an example of perspectival disagreement at the level of scientific knowledge claims effected by adopting different second-order (methodological-epistemic) *justificatory* principles on the same data.[14] Einstein's commitment to what became known as the principle of relativity and the light principle led him to interpret the data from the Michelson–Morley experiment as evidence for the knowledge claim that the ether does not exist and that simultaneity is relative to inertial reference frames. Without these two principles, Fitzgerald in 1889 interpreted the Michelson–Morley data as licensing a different kind of knowledge claim (which eventually proved wrong).

On other occasions, the perspectival disagreement at the level of scientific knowledge claims isn't effected by a divergence in (second-order) justificatory principles, but instead by a divergence in what one counts as *reliable* experimental (or theoretical) procedure. For example, while both Priestley and Lavoisier undoubtedly were committed to simplicity[15] as a justificatory principle for their respective scientific knowledge claims, they disagreed on the *reliability* of gravimetric methods as a way

---

Footnote 12 continued
chemist would be too far away from each other historically to be able to even enter into any such disagreement, so I won't consider this kind of historically far-fetched perspectival scenarios in what follows. Instead, my analysis applies to Kuhnian scenarios of historically contiguous epistemic communities, and it goes beyond Kuhn in considering disagreement among various scientific perspectives at play at the same historical time (not just one paradigm after another).

[13] See Brown (2001) for a historical and philosophical analysis of this famous episode in the history of relativity theory.

[14] A clarification on this point is in order. Although of course Einstein took these principles as well-established general claims about the world, they play a second-order methodological role in laying the foundations of what might be called the 'Einsteinian perspective', to echo Giere (2006). In other words, with these principles in place, it becomes then possible to derive consequences such as relativity of simultaneity, time dilation and length contraction as reliable first-order knowledge claims.

[15] On a popular historiographic view, phlogiston theory was disproved because it could not explain why calcinated metals were heavier (rather than lighter) than the original metals, as one would expect if calcination involved the release of phlogiston. That is the way Lavoisier himself presented his own view as achieving "greater simplicity" than his rivals (see Lavoisier 1799, p. 271): "Are the heat and light, which are disengaged during the different species of combustion, furnished by the burning body, or by the oxygen which combines in all these operations?… it belongs to those who make suppositions to prove them; and doubtless a doctrine, which without suppositions explains the phenomena as well, and as naturally, as theirs does by supposition, had at least the advantage of greater simplicity." But Joseph Priestley, too, appealed to a principle of simplicity. See, for example, Priestley's discussion of how it would be highly unphilosophical

of measuring gases as evidence for or against phlogiston.[16] Hence, their disagreement on whether phlogiston had in fact been released in combustion (rather than oxygen being combined with carbon in charcoal) was due to a divergence in what each of them counted as a reliable experimental procedure to assess the evidence in their own respective scientific perspectives.

Disagreement either at the second-order level of justificatory principles or at the first-order level of what counts as a reliable procedure results in disagreement at the level of the scientific knowledge claims. What typically happens in these situations is that one claim of knowledge proves true (e.g. Einstein's and Lavoisier's) and the rival proves false (e.g. Fitzgerald's and Priestley's). Such verdicts typically depend on whether either the second-order *justificatory* principles (e.g. the principle of relativity) or the first-order *reliability* of the procedure (e.g. gravimetric methods) is in turn truth-conducive. When assessed from the point of view of our current scientific perspective, which has 'retained' Einstein's principles and Lavoisier's gravimetric methods (but withdrawn Fitzgerald's and Priestley's rival principles and methods), it is easy to make this kind of judgement.

Obviously, as historians of science have repeatedly stressed, the hard question concerns how to reach such verdicts *at the time* when perspectival disagreements first arose and took place.[17] It goes beyond the scope and remit of the present paper to address this specific and important challenge. But at least the benefit of a perspectivalist account should already be evident. Perspectivism locates the disagreement about scientific knowledge claims at the meta-level (first- or second-order) concerning the reliability or justification of the procedures for generating scientific knowledge claims. It does not make any of the two disagreeing interlocutors/epistemic communities less rational than the other, for they can equally and legitimately claim to be competent and well informed. It does not dissolve disagreement either. But it resolves it eventually. I cannot attend in this paper to the task of offering any systematic analysis of how such resolution happens in historical terms. Let me instead concentrate on a somewhat easier scenario.

Consider the reverse case of perspectival disagreement where two epistemic agents this time agree on scientific knowledge claims while disagreeing either at the second-order level of justificatory principles, or at the first-order level of what counts as a reliable procedure. The underlying question here is how to *agree* whilst perspectivally disagreeing. Or, in other words, how it is possible for different epistemic agents/communities to reach the same conclusion, perspectival differences notwithstanding. These are the cases that I am interested in exploring in what follows. What has to be said about them? Two preliminary remarks are in order.

First, the temptation to dismiss such cases as trivial and epistemically uninteresting is misguided. One might be tempted to think: "if epistemic agents agree in the end, where is the problem?". No matter how they reached agreement, the fact that they did agree might seem to show that there is no problem about disagreeing in this

Footnote 15 continued
to conclude that there must be more than one cause for the phenomenon of matter's resistance different from repulsive power in *Disquisitions* (1782, vol. 1, p. 17).

[16] For the importance of gravimetric methods in Lavoisier, see Gough (1988, pp. 17–20); and Bensaude-Vincent (1992).

[17] See Chang (2012) for a discussion of the Lavoisier–Priestley controversy.

case. However, this would be a hasty conclusion to draw. For an explanation of how they could agree whilst perspectivally disagreeing is still required. Agreement between epistemic agents who endorse different scientific perspectives cannot accrue by chance and luck: namely, they cannot just *happen* to agree *regardless of* how perspectivally different their justificatory principles or reliable procedures for the generations of such knowledge claims were. Agreeing whilst perspectivally disagreeing has to satisfy an epistemological anti-luck requirement for scientific knowledge claims (to echo here Pritchard's 2005 on anti-luck epistemology).[18]

Second, agreeing whilst perspectivally disagreeing has to satisfy also a non-self-defeating requirement. Perspectivism would become moot if it proved to be the case that in the end there is an overarching perspective (some kind of Ur-perspective) defining rationality and to which every single scientific perspective is reducible. Thus, the burden is on perspectivalists to articulate an account of scientific disagreement that does not ultimately collapse into a non-perspectivalist account.

In the following sections, I take some preliminary steps towards articulating an account of perspectival disagreement in science that satisfies both the anti-luck and the non-self-defeating requirements. Such an account relies on there being inferential patterns that allow scientists to *agree* on what there is and about what it is, even if their data-to-phenomena inferences are perspectival at the second-order justificatory level. I argue that all that is required for this 'agreeing whilst perspectivally disagreeing' to happen in a non-lucky and non-self-defeating way is that epistemic agents are *willing to engage* in truth-conducive conditionals-supporting inferences licensed by a historically identified group of rather diverse phenomena across different perspectives. No incommensurability or incompatibility arises here. Our veridically and justifiably asserting that there is an electric charge $e$ as a fundamental natural unit is the outcome of truth-conducive conditionals-supporting inferences licensed by a historically identified group of rather diverse phenomena.

Key to my argument is that our coming to agree about the electric charge is not a matter of winners or losers; of one scientific perspective prevailing over another, or imposing itself on others. It is instead a matter of science being a fundamentally social and cooperative inquiry, where progress takes place not *in spite of* but *thanks to* a plurality of scientific perspectives. Scientific progress is ultimately the story of our coming to agree whilst perspectivally disagreeing. I see three main elements at play in this story: (a) scientists willingness to engage in certain kinds of inferences; (b) the conditionals-supporting nature of those inferences; and (c) their being truth-conducive. Let me say something here on (a) and I shall return in more detail on (b) and (c) in Sect. 4.

Scientific knowledge is not given to us on a silver plate, under the view presented here. Nor is it gained by accidentally peeking behind the veil of Maya and catching a glimpse of an underlying metaphysical reality. Scientific knowledge is *our* historical-social achievement. It is something scientific communities effect over time, within the

[18] As Pritchard has argued, knowledge as justified true belief cannot accrue by chance and luck, as in the 'fake barn' scenario where someone sees what seems to be a barn and forms the true justified belief that there is a barn and it turns out that it was just a lucky guess: for what the person has seen is in fact a painted façade of a barn hiding a real barn in the back. Similarly, our agreeing that there is the electric charge cannot be a lucky guess for it to count as knowledge.

historical boundaries afforded by their scientific perspectives and in continuous dialogue with other scientific perspectives. The willingness to engage with other epistemic agents occupying different scientific perspectives (synchronically and diachronically) is, then, key to perspectivism as a pluralist view about ways of knowing. Without this component, perspectivism could be easily conflated with a version of epistemic relativism;[19] or, with some kind of epistemic solipsism (with each community trapped in its own perspective and unable to step outside it).

The willingness to engage is broadly and loosely understood here along the lines of Brandom's inferentialism (1998, p. 389), where in "calling what someone has 'knowledge' one is doing three things: *attributing a commitment* that is capable of serving both as premise and as conclusion of inferences relating it to other commitments, *attributing entitlement* to that commitment, and *undertaking that same commitment* oneself. Doing this is adopting a complex, essentially *socially* articulated stance or position in the game of giving or asking for reasons."

To be more precise, I see our coming to unanimously agree *that* something is and about *what* it is as the outcome of conditionals-supporting inferences. In the historical example I examine in Sect. 3, the social game of giving or asking for reasons becomes the game of considering a number of phenomena (let us call them $P_1$, $P_2$, $P_3$) that *at the time* had been historically identified via a plurality of data-to-phenomena inferences. Such phenomena included the electrolysis of water; the bending of cathode rays; and the black-body radiation, among others. Each of these modally robust phenomena was evinced from data via methods, experimental and theoretical techniques, modelling practices that were all genuinely diverse and perspectival. Planck was not working on electrochemistry; nor was Thomson studying the quantum of action.

Consider, for example, the two phenomena of water electrolysis ($P_1$) and electromagnetic induction ($P_2$). The former takes place at the level of molecules for the chemical electrolytes. The latter occurs at the level of the interaction between magnetism and electricity and affects the very nature of electricity, on which wildly diverging views existed throughout the late eighteenth and early nineteenth century—from Galvani's animal electricity to Volta's metallic electricity; from Ampère's electrical fluids to Faraday's tubes. One of the challenges for scientists like J.J. Thomson at the end of the nineteenth century was to try and reconcile the continuous field-theoretical nature of phenomenon ($P_2$), with the discrete corpuscular-like nature of phenomenon ($P_1$). The models Thomson used to this effect were mainly heuristic or exploratory. They did not represent the target system (the electric charge) either as seen from the perspective of electrochemistry or as seen from the perspective of electromagnetism, with ensuing inconsistent property ascription. Their role was instead to enable J.J. Thomson and other scientists at the time, who were willing to engage in this social game of giving and asking for reasons, to make inferences about the relevant phenomena and their identified lawlike dependencies. But what kind of inferences are here at play? And what lawlike dependencies? Before I can continue with

[19] After all, perspectivism shares with relativism the rejection of the monist idea of a single neutral standpoint and it equally embraces a kind of epistemic pluralism, so it might be tempting to read it along the lines of a kind of relativism (see Ashton 2020 for this line of argument).

my philosophical account, a closer (albeit brief) look at the historical details of this fascinating episode is in order.

In Sect. 3, I illustrate how the realist commitment to the electric charge originated from a number of perspectival inferences around 1897–1906. These inferences involved three main scientific perspectives of the time: the Faraday–Maxwell field-theoretical perspective, in which J.J. Thomson was working (Sect. 3.1); the electrochemical perspective, to which Grotthuss and Helmholtz (among many others) contributed (Sect. 3.2); and the emerging quantum perspective championed by Max Planck (Sect. 3.3). Evidence for the electric charge appeared, independently, in each of these three different perspectives, no matter how diverse the data and the phenomena were in each case. At no point was the reliability of each procedure called into question by practitioners of another one. Thus, the perspectival disagreement here at stake is instead at the second-order level of justificatory principles. Our realist commitment to the electric charge as a minimal natural unit came out of the fruitful interactions among these three scientific perspectives; out of the willingness of their main epistemic agents to engage with one another via a series of truth-conducive conditionals-supporting inferences, whose nature I tease out in Sect. 4.

## 3 The rise of the natural unit of electric charge ca. 1897–1906

### 3.1 J.J. Thomson's Faraday–Maxwell perspective

In 1906, J.J. Thomson was awarded the Nobel Prize for his "theoretical and experimental investigations on the conduction of electricity by gases".[20] The Prize caption did not mention the electron as such because—as is well known among historians of physics—Thomson's pioneering experiments with cathode rays in 1897–8 did not lead him to the conclusion that 'the electron exists' (he did not use the term 'electron' even in his Nobel Prize speech, but rather 'corpuscle'). The Presentation Speech by J.P. Klason, President of the Royal Swedish Academy of Sciences, is particularly telling when read in conjunction with Thomson's own acceptance speech. Klason mentioned Thomson's work with H.A. Wilson (building on C.T.R. Wilson's method) concerning the discharge of electricity through gases, and presented Thomson as following in the footsteps of Maxwell and Faraday, especially Faraday's 1834 discovery of the law of electrolysis, which had shown

> that every atom carries an electric charge as large as that of the atom of hydrogen gas, or else a simple multiple of it corresponding to the chemical valency of the atom. It was, then, natural to speak, with the immortal Helmholtz, of an elementary charge or, as it is also called, an atom of electricity, as the quantity of electricity inherent in an atom of hydrogen gas in its chemical combinations. Faraday's law may be expressed thus, that a gram of hydrogen, or a quantity equivalent thereto of some other chemical element, carries an electric charge of $28{,}950 \times 10^{10}$ electrostatic units. Now if we only knew how many hydrogen

[20] The Nobel Prize in Physics 1906. NobelPrize.org. Nobel Media AB 2018. Mon. 1 Oct 2018. https://www.nobelprize.org/prizes/physics/1906/summary/.

atoms there are in a gram, we could calculate how large a charge there is in every hydrogen atom.[21]

Interestingly enough, having just presented J.J. Thomson as the scientist who "by devious methods" was able to answer this puzzle, Klason added (almost as a caveat to the rationale for the Prize) that "even if Thomson has not actually beheld the atoms, he has nevertheless achieved work commensurable therewith, by having directly observed the quantity of electricity carried by each atom. … These small particles are called electrons and have been made the object of very thoroughgoing researches on the part of a large number of investigators, foremost of whom are Lenard, last year's Nobel Prize winner in Physics, and J.J. Thomson."[22] The qualification "even if Thomson has not actually beheld the atoms" is important and telling at the same time. For the fact that Thomson did not refer to his particles as 'electrons' was not just a terminological matter: he did not quite see them as genuine particles having inertial mass,[23] and believed that there were positive and negative electric charges whose field-theoretical behaviour was captured by what elsewhere he had called a 'Faraday tube' (more on it here below).

But today we do not recall J.J. Thomson for his beliefs about the Faraday tubes. He has gone down in history as the discoverer of the electron. And for good reasons too, thanks to his precise experiments on cathode rays: by exhausting vacuum tubes so that very little air was left (which could interfere by acting as an electric conductor), Thomson was able to determine the velocity $v$ of cathode rays, which he found to be 1/3 the velocity of light. And, using classical laws of electrostatics and magnetism, he could measure the displacement of the cathode rays in the presence of an electric field. From these experiments, he was able to establish that there must have been a charge-to-mass ratio ($e/m$, or better $m/e$ as Thomson still referred to it in 1897) at work in the modally robust phenomenon of the bending of cathode rays.

The charge-to-mass value was found to be *stable* under a range of changes in background conditions (to use Jim Woodward's terminology): it was found to be independent of the velocity, independent of the kind of metal used for the electrodes, and independent of the gas used in the tube.[24] Most interestingly, under an additional range of interventions, the same lawlike dependency between charge and mass was observed

[21] Presentation Speech by Professor J.P. Klason, President of the Royal Swedish Academy of Sciences, on December 10, 1906. In https://www.nobelprize.org/prizes/physics/1906/ceremony-speech/, accessed 1 October 2018.

[22] Ibid.

[23] In the rest of the Presentation Speech, Klason remarks that "From experiments carried out by Kaufmann regarding the velocity of ß-rays from radium, Thomson concluded that the negative electrons do not possess any real, but only an apparent, mass due to their electric charge," and referred to more recent work by Thomson "in the present year (1906)" that "seem[s] to intimate that only about a thousandth part of the material is apparent and due to electric forces". Ibid.

[24] Thomson run a series of experiments to test whether the electrostatic deflection was proportional to the electric intensity of the rays. He used air, hydrogen, carbonic acid as different gases, and as cathode he used different materials from aluminium to platinum from which he concluded that "the value of $m/e$ is independent of the nature of the gas, and that its value $10^{-7}$ is very small compared with the value $10^{-4}$, which is the smallest value of this quantity previously known, and which is the value for the hydrogen ion in electrolysis" Thomson (1897, p. 310).

to hold *stably* across a number of other phenomena at different scales, including water electrolysis in chemistry on which I return in the next Section.

Since the beginning of his career at the Cavendish Laboratory in Cambridge in the 1880s (and still visibly in the 1893 book *Notes on Recent Researchers in Electricity and Magnetism*), Thomson's research on electric discharge in gases took place within a well-defined scientific perspective, popular in Cambridge at the turn of the twentieth century: I am going to call it the *Faraday–Maxwell perspective*.[25] This was primarily concerned with electromagnetism: namely, the interconversion of electricity and magnetism observed by Ørsted in Denmark and Faraday in England in the 1820s, and to explain which Maxwell in the early 1860s produced mechanical models of the ether (the so-called 'honeycomb model of the ether'). This scientific perspective centred on the field-theoretical analysis of the electromagnetic field. One of the models employed in the Faraday–Maxwell perspective still at the time of J.J. Thomson was the so-called 'Faraday tube', which physically conceived of "tubes of electric force, or rather of electrostatic induction, … stretching from positive to negative electricity".[26]

Faraday tubes were a visual semi-classical way of conceiving of the electric field as a collection of ethereal vortex tubes, carrying electrostatic induction, and with negative and positive charges at each end. Thomson toyed with the model of Faraday tubes in 1891 as they allowed him to reconcile the discrete nature of electricity emerging from electrochemical experiments with Maxwell's electromagnetic field. Atoms of opposite electric charge connected by a Faraday tube could serve to represent molecules of electrolytes—polarized with the passage of electric current as in Grotthuss' chain model of electrolysis (see Fig. 1 below).

Yet, the *Faraday–Maxwell perspective* gave a perspectival representation of the electric charge which was in stark contrast with the one emerging from the electrochemical perspective (as we shall see in the next Section). Already in his seminal article, Thomson (1891, pp. 149–150) had made it clear that Faraday tubes were not just an expedient to visualize mathematical equations, but they had instead "real physical existence" and that the contraction and elongation of such tubes and their motion through the electric field could explain the passage of electricity through metal, liquid, and gases, the production of current, magnetic force, and electromagnetic induction. Twelve years later, and just three years before the Nobel Prize, Thomson returned to the topic in the Silliman Lectures delivered in May 1903 at Yale University (Thomson 1904). These lectures provide an instructive example of his considered and longstanding ontological commitment to the electric charge at the dawn of the new century (just at the time when Planck was ushering in the quantum revolution). Four elements of Thomson's treatment of the electric charge in the Silliman Lectures are worth briefly underlining:

[25] See Falconer (1987, 2001) for an excellent historical account.

[26] Thomson (1893, p. 2). See Smith (2001) for an excellent historical account of Thomson's experiments and intellectual background in 1897–1898. Faraday's tubes were a way of referring to what we would now call 'electric flux' as a measure of the electric field strength, with the two charges (positive and negative) at the two ends of it.

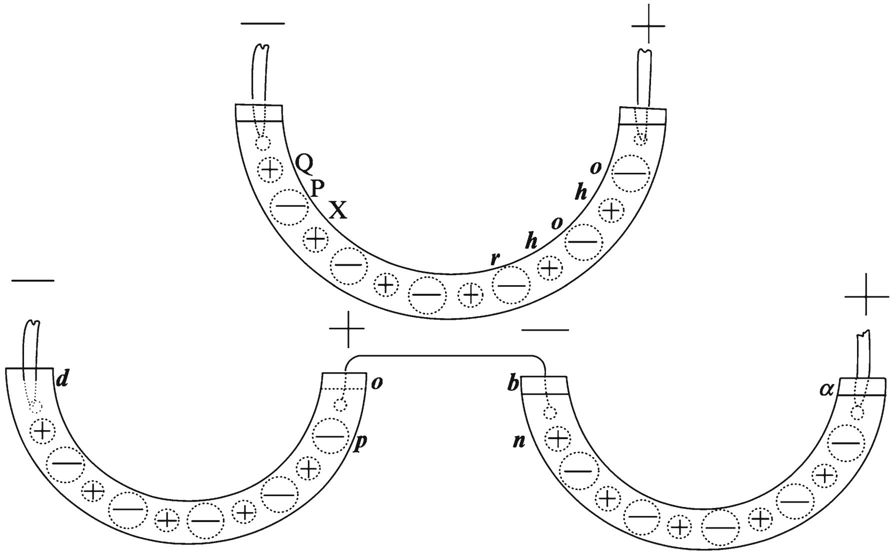


**Fig. 1** Grotthuss' (1806) chain model of water electrolysis represented polarized molecules of water with the passage of electricity as forming a chain inside the electrolyte (with hydrogen having positive charge and oxygen negative charge). Reproduced with permission from Chang (2012, *Is Water $H_2O$?,* p. 84)

(a) *Thomson's treatment of the electric charge in 1903 is still deeply rooted in the nineteenth-century Faraday–Maxwell tradition of lines of force and mechanical ether models for electromagnetic induction.*[27]
Thomson refers once again to the Faraday tube as a "tube of force" or a tubular surface marking the boundaries of lines of force so that "if we follow the lines back to the positively electrified surface from which they start and forward on to the negatively electrified surface on which they end, we can prove that the positive charge enclosed by the tube at its origin is equal to the negative charge enclosed by it at its end" (ibid., p. 14). In this way Thomson explained the old ideas of positive and negative electricity with "each unit of positive electricity in the field … as the origin and each unit of negative electricity as the termination of a Faraday tube" (ibid., p. 15).[28]

(b) *The boundary between Thomson's corpuscles and Faraday tube is a lot more subtle than it might seem.*
The mass of the Faraday tube is nothing but the mass of the bound ether, or, as Thomson puts it, "the mass of ether imprisoned by a Faraday tube" (ibid., p. 39). The term 'corpuscle' is introduced to refer to "the negatively electrified

[27] Thomson (1904, p. 5) remarks right at the outset how "Fluids were mathematical fictions intended merely to give a local habitation to the attraction and repulsion existing between electrified bodies," and how "Faraday materialized the lines of force and endowed them with physical properties so as to explain the phenomena of the electric field" (ibid., p. 9).

[28] What Maxwell had called electric displacement was nothing but a certain number of Faraday tubes per unit area, and the motion of Faraday tubes was hypothesized to explain the production of a magnetic field as per electromagnetic induction and Ampère's law.

particles in the atom", or, better, "those small negatively electrified particles whose properties we have been discussing. On this view of the constitution of matter, part of the mass of any body would be the mass of the ether dragged along by the Faraday tubes stretching across the atom between the positively and negatively electrified constituents" (p. 50). Thus, Thomson's corpuscle is effectively nothing but a "concentration of the lines of force on the small negative bodies" so that "practically the whole of the bound ether is localised around these bodies, the amount depending only on their size and charge" (ibid., p. 52).

(c) *The electric charge is presented as a natural unit and its atomicity is explained in terms of Faraday tubes.*[29] However, by contrast with the electrochemical perspective, the line of reasoning leading to Thomson's conclusion that the electric charge is somehow atomistic does not rely on electrolysis but on Wilson's experiments on the conductivity of the vapour obtained from metallic salts (the so-called 'electron vapour theory').[30]

(d) *An explanation of Röntgen rays is given in classical terms of corpuscles* as Faraday tubes and with no reference to the quantum hypothesis and electrons losing part of their quantized energy as in Planck's contemporary treatment of the topic (Sect. 3.3).

Starting with data about cathode rays and Wilson's electron vapour experiments, building on Faraday's lines of force and Maxwell's honeycomb model of the ether in the 1860s, Thomson resorted to a field-theoretical model that made it possible to infer what *might happen, under the supposition that the Faraday tubes were stretched and broken*. For this conditionals-supporting inference to be truth-conducive, it had to be the case that other perspectival data-to-phenomena inferences could be brought to bear on it across a network of inferences that eventually guided epistemic agents through the garden of forking paths to the correct identification of the electric charge. This was indeed what happened in this story.

### 3.2 Grotthuss' and Helmholtz's electrochemical perspective

The Faraday–Maxwell field-theoretical perspective on electromagnetism was at some distance from what I call the '*electrochemical perspective*'. In 1874 G. Johnstone Stoney used Faraday's law of electrolysis to conclude that in the phenomenon of water electrolysis "For each chemical bond which is ruptured within an electrolyte a certain quantity of electricity traverses the electrolyte which is the same in all cases"

[29] "Hithertho we have been dealing chiefly with the properties of the lines of force, with their tension, the mass of the ether they carry along with them, and with the propagation of the electric disturbances along them; in this chapter we shall discuss the nature of the charges of electricity which forms the beginning and ends of these lines. We shall show that there are strong reasons for supposing that these charges have what may be called an atomic structure; each charge being built up of a number of finite individual charges, all equal to each other. … if this view of the structure of electricity is correct, each extremity of the Faraday tube will be the place from which a constant fixed number of tubes start or at which they arrive" (Thomson 1904, p. 71).

[30] "Wilson found that the saturation current through the salt vapour was just equal to the current which if it passed through an aqueous solution of the salt would electrolyse in one second the same amount of salt as was fed per second in the hot air. … Thus whether we study the conduction of electricity through liquids or through gases, we are led to the conception of a natural unit or atom of electricity" (ibid., p. 83).

(Stoney 1874/1894). Stoney introduced the term 'electron' to describe this minimal quantity of electricity carried by the hydrogen ion in electrolysis.[31] In 1881 Helmholtz in Germany championed the hypothesis that elementary substances were composed of what he called "atoms of electricity"[32] (or 'ions', as Lorentz later called them), motivated and justified precisely by chemical studies of electrolysis going back to the German chemist Theodor von Grotthuss, who in (1806) had published his model for water electrolysis (Fig. 1).

The atoms of electricity were regarded here as the minimum quantity carried by electrolytes (or, better, by the hydrogen atoms) when molecules decomposed with the passage of electricity. Helmholtz's argument originated from Faraday's laws of electrolysis, which established that the chemical valency (i.e. electrical charge of hydrogen atoms, or, better, of what we now know to be valence electrons) was a fundamental unit not amenable to being further divided. Helmholtz's reasoning for taking the electric charge as a physical unit (and in Britain, George J. Stoney's analogous reasoning) was entirely chemical, rooted in the well-known tradition of eighteenth- and nineteenth-century electrolytical experiments. What made $e$ a minimal unit under this perspective was the fact that it was the charge corresponding to chemical valence 1. Thus, a different data-to-phenomena inference was at play in this scientific perspective, one that fed into the indicative conditional

> (E.1) If hydrogen and oxygen molecules form a Grotthuss' chain, hydrogen molecules are released at the negative electrode.

By physically conceiving of a minimal electrical unit for the ions of electrolytes, Grotthuss' model could be used to explore what might happen in the well-known and well-observed phenomenon whereby water molecules decompose with the passage of electricity with oxygen at one end and hydrogen at the opposite end. The antecedent of this conditional supposed point-like 'ions' or atoms of electricity whose chemical valence 1 was at the time associated with the hydrogen atom in the water molecule. Bringing this kind of information to bear on J.J. Thomson's perspective proved key in this story. Because as Thomson himself recounted in his Nobel Prize speech, it became apparent that the phenomenon $E/M$ of the hydrogen atom (known from electrolysis) and the phenomenon $e/m$ emerging from cathode rays within the Faraday–Maxwell perspective were at odds with one another: a numerical discrepancy of the order $e/m = 1700\ E/M$ emerged. This discrepancy led Thomson to his breakthrough:

[31] In a 1874 talk presented at the British Association meeting in Belfast and entitled "On the Physical Units of Nature", Stoney presented this minimal quantity of electricity as "one of the three physical units, the absolute amounts of which are furnished to us by Nature, and which may be the basis of a complete body of systematic units in which there shall be nothing arbitrary" (Stoney 1874/1894, p. 418). But Stoney believed that these electrons within each molecule or chemical atom were "waved about in a luminiferous ether" and that in this motion through the ether the spectrum of each gas originated.

[32] "The most startling result of Faraday's law is perhaps this. If we accept the hypothesis that the elementary substances are composed of atoms, we cannot avoid concluding that electricity also, positive as well as negative, is divided into definite elementary portions which behave like atoms of electricity. As long as it moves about on the electrolytic liquid each ion remains united with its electric equivalent or equivalents. At the surface of the electrodes decomposition can take place if there is sufficient electromotive force, and then the ions give off their electric charges and become electrically neutral" (Helmholtz quoted in Stoney 1874/1894, p. 419).

> We have already stated that the value of $e$ found by the preceding method [i.e. Wilson's][33] agrees well with the value $E$ which has long been approximately known. Townsend has used a method in which the value $e/E$ is directly measured, and has shown in this way also that $e$ equal to $E$. Hence since $e/m = 1700E/M$, we have $M = 1700\ m$, i.e. the mass of a corpuscle is only about 1/1700 part of the mass of the hydrogen atom (Thomson 1906, p. 153).

The equivalence between the value $e$ found by Thomson within the Faraday-Maxwell perspective and the value $E$ for hydrogen atoms in the electrochemical perspective suggested only one possible outcome: the mass of Thomson's corpuscle must have been much smaller than the mass of the hydrogen atom.

But the perspectival inferences that led to the electric charge were not confined to phenomena about water electrolysis, X-ray gas diffusion, charged droplets, and cathode rays bending. On the other side of the Channel, German physicists were laying the foundations of a new scientific perspective that was soon bound to influence the final chapter of this story.

### 3.3 Max Planck's quantum perspective: the electric charge as a 'natural unit'

In the Preface to the Second Edition of *The Theory of Heat Radiation* (1906/1913, p. vii), Planck announced that his measured value for $e$ ($4.69 \cdot 10^{-10}$) laid in between the values of Perrin and Millikan. More importantly, he presented the idea of an "elementary quanta of electricity" as the single most important new piece of evidence in support of his hypothesis of the quantum of action:

> Recent advances in physical research have, on the whole, been favorable to the special theory outlined in this book, in particular to the hypothesis of an elementary quantity of action. … Probably the most direct support for the fundamental idea of the hypothesis of quanta is supplied by the values of the elementary quanta of matter and electricity derived from it. When, twelve years ago, I made my first calculation of the value of the elementary electric charge and found it to be $4.69 \cdot 10^{-10}$ electrostatic units, the value of this quantity deduced by J.J. Thomson from his ingenious experiments on the condensation of water vapour on gas ions, namely $6.5 \cdot 10^{-10}$ was quite generally regarded as the most reliable value. This value exceeds the one given by me by 38 per cent. Meanwhile the experimental methods, improved in an admirable way by the labors of E. Rutherford, E. Regener, J. Perrin, E.A. Millikan, The Svedberg and others, have without exception decided in favour of the value deduced from the theory of radiation which lies between the values of Perrin and Millikan.

[33] The equivalence between $e$ and $E$ was established thanks to the work of C.T.R Wilson on cloud formation, which made in turn possible H.A. Wilson's measurement of charged droplets, as well as by Townsend's (1899) measurement of the charges of gas ions generated by X-rays. As the historian of science George E. Smith points out, Townsend's experiment was "predicated on Maxwell's diffusion theory. …Townsend inferred a magnitude for $Ne$, where $N$ is the number of molecules per cubic centimetre under standard conditions. The uniformity of this magnitude for ions of different gases and its close correspondence to the value $NE$ from electrolysis (where $E$ is the charge per hydrogen atom), then allowed Townsend to conclude, *independently of any specific value of* e *or* $N$, that the charge per ion, when generated by X-rays, is the same as the charge on the hydrogen atom in electrolysis" (Smith 2001, pp. 74–75).

> To the two mutually independent confirmations mentioned, there has been added, as a further strong support of the hypothesis of quanta, the heat theorem which has been in the meantime announced by W. Nernst, and which seems to point unmistakably to the fact that, not only the processes of radiation, but also the molecular processes take place in accordance with certain elementary quanta of a definite magnitude.

With these words, Planck established a tradition that proved very influential and with far-reaching philosophical consequences. The idea that there must be an elementary electric charge corroborated Planck's hypothesis of quanta and showed how the applicability of the hypothesis extended well beyond the radiation of the black body, and right into the nature of matter and electricity. And there was no better evidence for this conclusion than to identify $e$ as a physical constant (along the lines of Planck's constant $h$) and present the experiments of Thomson, Rutherford, Perrin, and Millikan (among others) as all equally dealing with the same task: to measure as precisely as possible the value for the elementary charge $e$.[34] Property realism has its roots in Planck's crafted presentation of the elementary charge $e$ in the Preface to the Second Edition of *The Theory of Heat Radiation*.

In Chapter 4 of the book, unsurprisingly, Planck returns to the hypothesis of quanta and the temperature of black-body radiation originating from a system of stationary oscillators and embarks on what in my view is an illuminating journey on the nature of physical constants. For just after introducing Planck's constant as

$$h = \frac{ac_2^4}{48\pi\alpha c}$$

he goes on to the kinetic theory of gases and discusses how to estimate the number of hydrogen molecules contained in 1 $cm^3$ of an ideal gas at 0 °C and 1 atm. And from the mean kinetic energy of translatory motion of a molecule at the absolute temperature $T = 1$ he concludes that the "elementary quantity of electricity or the free charge of a monovalent ion or electron" $e$ in electrostatic unit is equal to $4.67 \cdot 10^{-10}$, adding that "the degree of approximation to which these numbers represent the corresponding physical constants depends only on the accuracy of the measurements of the two radiation constants $a$ and $c_2$" (ibid., p. 173).[35] In a single stroke Planck effectively established:

1. The theoretical equivalence between the "free charge of a monovalent ion or electron" with the "elementary quantity of electricity" (where it is worth stressing

---

[34] That Planck was keen to find a connection between his constant $h$ and other physical constants was revealed by Max Klein in a letter to Ehrenfest of July 6, 1905, at a time where the existence of an elementary charge quantum $e$ was nothing more than a conjecture. As reported by Klein, Planck was keen to find a "bridge" between his quantum hypothesis $h$ and the experimentally found values for $e$. See Gerald Holton (1973), footnote 19, p. 176.

[35] The Boltzmann constant $a$ features in the Stefan–Boltzmann law for the radiant energy of a black body, which relates the volume and intensity of black-body radiation to the fourth power of the absolute temperature. Planck took its numerical value from Kurlbaum's original measurements, although Kurlbaum's results were soon rectified and improved by a series of measurements performed by others, including W.H. Westphal in 1912.

Planck's ambiguous use of the double terminology of Lorentz's 'ions' as interchangeable with Stoney's 'electron').[36]
2. The further identification of the "elementary quantity of electricity" *e* with a "physical constant" among others in the context of black-body radiation.
3. And the accuracy in the values of the physical constant *e* depending on the refined measurements of radiation constants.

The ambiguity in the terminology ion/electron is symptomatic, in my view, of Planck's intentional disengagement with the ontological debate at the time about the nature of *e* (and of atoms more generally).[37] In Planck's hands, the electric charge proved instrumental to establishing the validity and universal applicability of the quantum hypothesis. And, in return, what up to that point had been just a hypothesis—the "ion hypothesis", as Paul Drude still called it his Preface to the *Theory of Optics*[38]—had become in Planck's hands a 'natural unit'.

The electron gas theory of the German physicist Paul Drude was an important influence for Planck, who cited him (Planck 1906/1913, p. 179; see Kaiser 2001 on which I draw here). Interestingly enough, Drude himself was not working on Planck's quantum hypothesis but on metal optics, and how to explain a range of phenomena such as dispersion of light and optical reflection from metal surfaces within Maxwell's electromagnetic theory. Building on van't Hoff's kinetic theory of osmotic pressure and Nernst's theory of concentration cells in electrolysis, Drude patterned electrical conductivity in metals on the model of the kinetic theory of gases, and used Boltzmann's equipartition theorem with the universal constant *a* to establish that "If a metal is now immersed in an electrolyte in the case of 'temperature-equilibrium', the free electrons in the metal would have the same kinetic energy as the ions in the electrolyte" (Drude quoted in Kaiser 2001, p. 259). He was also able to derive the Wiedemann–Franz law, which establishes that the ratio between thermal conductivity *k* and electrical conductivity *c* is proportional to the temperature *T* (and to the squared ratio between *a* and the electric charge *e* of the free electron bouncing off inside the metal).

Planck did not speculate on the nature of the minimal unit of electricity. He was far more interested in identifying *e* as a physical constant among others emerging from his quantum hypothesis; and in establishing with accuracy the measured values of various inter-related physical constants. In so doing, he was laying the foundations of a very influential view about 'natural units'. What makes some units natural, according to Planck, are two features that ever since we have tended to identify with physical constants: namely, their objectivity and necessity.

(1) Physical constants are *objective*. Planck maintained that their holding does not depend on us qua epistemic agents: it is not meant to cater to our epistemic needs, or to our research interests. Physical constants are thus set aside from metrological considerations that typically apply to other units of measure, for

[36] See Arabatzis (2006, p. 79) for a historical reconstruction of Lorentz's 'ions' vs 'electrons' as they were called by Stoney, Larmor, and Zeeman.

[37] I refer the reader to the excellent historical reconstruction of this episode by Arabatzis (2006, ch. 4).

[38] Drude (1902) contributed to the electron gas theory and his experiments are mentioned by Planck as additional evidence for the hypothesis of the quantum of electricity.

there is no conventional element affecting their validity here and everywhere else.

(2) Physical constants are *necessary*. They are natural because they are part of the fabric of nature: they exist and would have existed even if humankind had not existed (or had not developed our particular scientific history). The naturalness of these constants (and their necessity) is tied to laws of nature, according to Planck. Their "natural significance" is retained as long as the relevant laws "remain valid; they therefore must be found always the same, when measured by the most widely differing intelligences according to the most widely differing methods" (Planck 1906/1913, p. 175).[39]

The introduction of the "elementary quantity of electricity" *e* in this context signals, then, an important shift in the debate at that time about the nature of electric charge. It signals that ontological discussions about the bearer do not matter, because the fundamental unit is not the electron (or hydrogen atom or gas ion or corpuscle or whatever one wants to call it), but the electric charge. And electric charge is a physical constant (among others), whose naturalness is not at the mercy of arbitrary metrological decisions of particular epistemic communities at particular historical times. It is entrenched instead in its featuring in laws of nature, whose validity holds always and everywhere.

## 4 Truth-conducive conditionals-supporting inferences

Let us take stock from this brief foray into the history of electric charge around 1897–1906, and go back to the kind of perspectival disagreement I presented in Sect. 2, namely to those cases where we *agree* on the scientific knowledge claim (e.g. that *there is* an electric charge and about *what it is*) whilst also perspectivally disagreeing on the justificatory principles adopted to reach such a conclusion (e.g. Grotthuss' chain model vs. Thomson's Faraday tubes vs. Planck's quantum hypothesis). The reliability of the procedures at stake in each phenomenon was never called into question: there was no disagreement about the phenomenon of hydrogen being released at the negative electrode in electrolysis; or about Wilson's and Townsend's experimental methods for measuring *e*/E; or about Planck's procedure for estimating *e*. But the methodological-epistemic justificatory principles for the conclusion that 'there is an electric charge as a minimal natural unit' were noticeably different in each case and distinctively perspectival in the way described above.

The underlying question raised in Sect. 2 was: how is it possible to *agree* whilst perspectivally disagreeing? Or how it is possible for different epistemic agents to reach

[39] "All the systems of units which have hitherto been employed … owe their origin to the coincidence of accidental circumstances, inasmuch as the choice of the units lying at the base of every system has been made, not according to general points of view which would necessarily retain their importance for all places and all times, but essentially with reference to the special needs of our terrestrial civilization. … In contrast with this it might be of interest to note that, with the aid of the two constants *h* and *k* which appear in the universal law of radiation, we have the means of establishing units of length, mass, time, and temperature, which are independent of special bodies or substances, which necessarily retain their significance for all times and for all environments, terrestrial and human or otherwise, and which may therefore be described as 'natural units'" (1906/1913, pp. 173–174).

the same conclusion (e.g. *that* something is and *what is*), perspectival differences notwithstanding?

Two requirements were identified, namely agreement between epistemic agents (and more broadly communities) who endorse different scientific perspectives should neither accrue by luck, nor be self-defeating in assuming an overarching perspective to which every scientific perspective is ultimately reducible. At the end of Sect. 2, I mentioned the inferential patterns—their (b) conditionals-supporting nature and (c) their being truth-conducive—that ultimately explain how and why scientists do come to agree whilst perspectivally disagreeing. It is now time to clarify these two points in light of the historical episode just discussed. Consider the difference between the aforementioned indicative conditional

> (E.1) If hydrogen and oxygen molecules form a Grotthuss's chain, hydrogen molecules are released at the negative electrode

and the subjunctive conditional (let us denote the subjunctive with A rather than E)

> (A.1) Were electrodes immersed in water, hydrogen molecules would be released at the negative electrode.

Despite prima facie similarities, the two conditionals conceal a crucial difference behind the syntactical difference between the present tense 'are released' and 'would be released'. Although the consequent is the same in both cases, namely the observable phenomenon $P_1$, the subjunctive mode in (A.1) conveys the objective possibility of $P_1$ occurring, were the antecedent condition to hold. But the indicative conditional (E.1) conveys instead an implicit (unpronounced) epistemic possibility concerning $P_1$, assuming the antecedent holds.

In other words, the subjunctive mode (A.1) speaks to the modal robustness of phenomenon $P_1$ under the antecedent's holding—hydrogen's being released at the negative electrode were electrodes immersed in water—its objective possibility being grounded on the lawlike causal dependency between quantity of electricity and electrochemical decomposability established by Faraday's law. Phenomenon $P_1$ is modally robust under a wide range of changes in the background conditions of the experimental set-up (along Woodward's interventionist view): hydrogen would still be released if electrodes were immersed in water, regardless of the nature of the liquid or the metal used for the electrodes, for example.

By contrast, the indicative mode (E.1) speaks to our epistemic attitudes when we judge whether $P_1$ is likely to occur in the hypothetical scenario described by Grotthuss' model in the antecedent. This is the realm of perspectival models as heuristic models (see Massimi 2018a). Consider now the following chain of indicative-conditionals-supporting inferences:

> (E.1) If hydrogen and oxygen molecules form a Grotthuss's chain, hydrogen molecules are released at the negative electrode.
> (E.2) If ether vortices move as in Maxwell's honeycomb model, electric current is displaced.
> (E.3) If a Faraday tube of electrostatic induction is stretched and broken, free atoms of electricity are produced (be it in metal, gases, or liquid electrolytes).

(E.4) If free atoms of electricity in metals are conceived along van't Hoff's kinetic theory of osmotic pressure (as Paul Drude did), dispersion of light and reflection of metal surfaces ensue.
(E.5) If carriers of metallic conductivity are conceived along the model of Drude's electron gases, the phenomenon of blackbody radiation can be calculated.
(E.6) If the monovalent hydrogen ion is conceived along the lines of Planck's quantum hypothesis, the quantum of electricity (measured from the radiation constants $a$ and $c^2$) is equal to $4.67 \times 10^{-10}$ in electrostatic units.

The inferential chain (E.1)–(E.6) eventually allowed physicists around 1897–1906 to conclude that something was (electric charge) and what it was (a quantum of electricity with a well-defined measurable value). 'Electric charge' as a fundamental unit of nature is not *just* an itemised list of phenomena $P_1, P_2, P_3, \ldots$ from electrochemical decomposition to electromagnetic induction, from metal conductivity to radiant heat.

What is needed in addition is a *set of instructions* for epistemic agents —working across different scientific perspectives and willing to engage with one another—to reliably make informed decisions about how to proceed forward, what conclusions to draw, what tentative conclusions to discard, what further novel inferences to explore and probe about these phenomena and new ones too. These instructions take the form of conditionals-supporting inferences like (E.1)–(E.6), for example. Models are involved at different points in these inferences. Some of the models are perspectival, as already indicated. But perspectival models are only a subset of a much larger and varied family of scientific models routinely used to make these inferences, including phenomenological models such as Drude's electron gas one, and theoretical models too, such as Planck's theory of blackbody radiation.

In the example at stake, the instructions encoded by these conditionals-supporting inferences required comparing the phenomenon $P_1$ of *E/M* in the hydrogen molecules emerging from Grotthuss' and Helmholtz's work on electrolysis with the phenomenon $P_3$ of *e/m* measured by Thomson's experiments on cathode rays as per (E.2), and noticing a numerical discrepancy between the two.

To resolve this numerical discrepancy, a new round of data-to-phenomena inferences was required that this time involved *forking subjunctive conditionals*. The indicative conditional (E.3) opened up a forking inferential path, so to speak:

(A.3.a) Were *e* bigger than *E, e/m* would be much bigger than *E/M;*
*Or,*
(A.3.b) Were *m* much smaller than *M, e/m* would be much bigger than *E/M.*

And as new data became available (i.e. using Wilson's technique of weighing water droplets around negative charges, as well as J.S.E. Townsend's experiments on the charge of ions produced by X-rays), the choice could reliably be made in favour of (A.3.b). This led Thomson to conclude that his corpuscle had a mass much smaller than the hydrogen atom: it was indeed the first sub-atomic particle to be identified.

From there the step to the next further inference that there *is* a quantum of electricity was a short one. A further round of data-to-phenomena inferences was required, this time involving the comparison of J.J. Thomson measured value for *e* (as per A.3.b) with Planck's measured value derived from his theory of black-body radiation (via E.4–E.6).

And again, the discrepancy between the two opened up yet another inferential forking path of new subjunctive conditionals:

(A.6.a) Were *e* a semi-classical quantity, its value would be derived from the laws of classical electrodynamics;
*Or*
(A.6.b) Were *e* a quantum of electricity, its value would be derived from the laws of black-body radiation.

Further measurement obtained by Rutherford, Regener, Perrin, Millkan among others, led to more accurate estimates of the value for *e* and settled the choice for Planck's (A.6.b) eventually. In Rorty's language, one might say that we all stand on Planck's grid today in taking Planck's constant *e* as a minimal natural unit. Electricity got quantized alongside with the blackbody radiation.

What we have learned about the physical constant *e* and a quick look at a relevant episode in its long history is that our realist commitment to it is the outcome of conditionals-supporting inferential chains enabled by lawlike dependencies at work in each and every of these different phenomena. Some of these lawlike dependencies are causal in nature, as when one observes cathode rays bending by increasing the strength of the electric or magnetic field. Others are non-causal in nature, e.g. the relation between half-integral spin and Fermi–Dirac statistics.[40] Ultimately, it is the *lawlike dependencies* in phenomena, the way they enter into forking subjunctive conditionals, and how we go about choosing which forking path to take on the basis of available pieces of evidence at particular historical times that underpins the *truth-conducive* nature of conditionals-supporting inferences—no matter the epistemic nature of the indicative conditionals.

Historically, the identification of the *relevant* lawlike dependencies in the phenomenon of cathode rays constituted the main hurdle. J.J. Thomson's experiments in 1897 and his ability to reliably settle for (A.3.b) gained him the Nobel Prize, no matter how mistaken his beliefs about corpuscles in Faraday tubes were. Truth-conducive conditionals-supporting inferences—reiterated and enabled by an ever-growing number of perspectival data-to-phenomena inferences—reliably lead epistemic agents to agree *that* something is and *what it is* in spite of, or better *thanks to* the perspectival variety of methodological-epistemic justificatory principles.

## 5 Concluding remarks: *agreeing* whilst perspectivally disagreeing

Our realist commitment to the electric charge was not the result of any lucky stumbling into natural properties hidden behind the veil of Maya. Much as we all stand on Planck's grid today in taking *e* as a physical constant, a closer look at the twists and turns in its history around 1897–1906 should act as a reminder (and a cautionary tale) not to conflate Planck's own manifesto in the Preface to the Second Edition of *The Theory*

[40] The Pauli principle does not tell a causal story as to why half-integral spin particles cannot be in the same dynamic state, but it simply imposes a constraint to the type of state (antisymmetric) that an ensemble of such particles is allowed to be in. There are similarities here with Lange's (2017) account of non-causal explanations "by constraint", which for reason of space I cannot pursue.

*of Heat Radiation* with a metaphysical guide as to what there is, any more than we would nowadays take Newton's *Principia* as a metaphysical guide to what there is.

Our realist commitment to the electric charge emerged by proceeding through the inferential garden of forking paths, so to speak, where at each junction novel data were brought in and determined which path to take and which one not to take, which subjunctive conditional expresses a genuine lawlike dependency and which one does not. That is how real historical communities over time learn how to navigate the space of what is objectively possible: i.e., by comparing a plurality of modally robust phenomena so as to make more and more refined inferences on what might be the case at every twist and turn. This procedure is entirely fallibilist, anti-foundationalist, and revisable. It does not start from building-blocks (properties, dispositions, truthmakers, or Humean mosaics). It takes seriously the situated nature of our scientific knowledge, our starting always *from somewhere* (rather than from *nowhere*), in the form of model-based inferential reasoning that places centre-stage epistemic indicative conditionals. It is truth-conducive in giving and asking for reasons as to why some paths are taken and others are not along the way.

Perspectivism is not a backward-looking reflection on the practice of science and its being historically and culturally situated. It is also, first and foremost, a forward-looking commitment to engage across scientific perspectives and retain knowledge claims that have served us well across multiple perspectives. It is a perfect illustration of how scientific knowledge grows from a perspectival point of view: how epistemic communities come to agree, and how successful our perspectival data-to-phenomena inferences can be in delivering knowledge 'from within' the boundaries of what is historically conceivable at any point in time. Realist commitments are to be found aplenty along the paths we have taken walking in the historical garden of inferential forking paths.

**Acknowledgements** I thank audiences at the Integrated HPS conference in Hannover, the Institute for Cross-Disciplinary Engagement at Dartmouth College, the XIII International Ontology Congress at the University of San Sebastian for feedback on earlier versions of this paper. Thanks to Theo Arabatzis, Jed Buchwald, Marcelo Gleiser, Richard Healey, Peter Lewis, Tim Maudlin, George Smith for constructive questions on various points of the material here covered. Special thanks to Marc Lange for reading a draft of this paper and providing very insightful comments. I am very grateful to Maria Baghramian and Finnur Dellsen for inviting me to the conference on *Scientific Disagreement*, University of Dublin, and for organising this special issue. This article is part of a project that has received funding from the European Research Council (ERC) under the European Union's Horizon 2020 research and innovation programme (Grant agreement European Consolidator Grant H2020-ERC-2014-CoG 647272 *Perspectival Realism. Science, Knowledge, and Truth from a Human Vantage Point*).

## References


Achinstein, P. (2001). Who really discovered the electron? In J. Z. Buchwald & A. Warwick (Eds.), *Histories of the electron* (pp. 403–424). Cambridge, MA: MIT Press.

Arabatzis, T. (2006). *Representing electrons. A biographical approach to theoretical entities*. Chicago: University of Chicago Press.

Ashton, N. (2020). Scientific perspectives, feminist standpoints and non-silly relativism. In A. Cretu & M. Massimi (Eds.), *Knowledge from a human point of view* (pp. 71–86). Berlin: Springer.

Bain, J., & Norton, J. (2001). What should philosophers of science learn from the history of the electron? In J. Z. Buchwald & A. Warwick (Eds.), *Histories of the electron* (pp. 451–466). Cambridge, MA: MIT Press.

Bensaude-Vincent, B. (1992). The balance: Between chemistry and politics. *The Eighteenth Century, 33,* 217–237.

Bogen, J., & Woodward, J. (1988). Saving the phenomena. *Philosophical Review, 97,* 303–352.

Brandom, R. B. (1998). Insights and blindspots of reliabilism. *The Monist, 81,* 371–392.

Brown, H. (2001). The origins of length contraction: I. The Fitzgerald–Lorentz deformation hypothesis. *American Journal of Physics, 69,* 1044.

Chakravartty, A. (2007). *A metaphysics for scientific realism*. Cambridge: Cambridge University Press.

Chang, H. (2012). *Is water $H_2O$? Evidence, realism and pluralism*. Berlin: Springer.

Chang, H. (2015). The chemical revolution revisited. *Studies in History and Philosophy of Science, 49,* 91–98.

Christensen, D., & Lackey, J. (Eds.). (2013). *The epistemology of disagreement. New essays*. Oxford: Oxford University Press.

Drude, P. (1902). *The theory of optics* (C. Riborg Mann, & R. Millikan, Trans. from German). New York: Longmans, Green and Co.

Elgin, C. Z. (2017). *True enough*. Oxford: Oxford University Press.

Falconer, I. (1987). Corpuscles, electrons and cathode rays: J.J. Thomson and the 'discovery of the electron'. *British Journal for the History of Science, 20,* 241–276.

Falconer, I. (2001). Corpuscles to electrons. In J. Buchwald & A. Warwick (Eds.), *Histories of the electron: The birth of microphysics* (pp. 77–100). Cambridge, MA: MIT Press.

Feldman, R., & Warfield, T. A. (Eds.). (2010). *Disagreement*. Oxford: Oxford University Press.

Giere, R. (2006). *Scientific perspectivism*. Chicago: University of Chicago Press.

Gough, J. B. (1988). Lavoisier and the fulfilment of the Stahlian revolution. *Osiris, 4,* 15–33.

Grotthuss, C. J. D. (1806). Memoir upon the decomposition of water, and of the bodies which it holds in solution, by means of galvanic electricity. *Philosophical Magazine, 25,* 330–339.

Hacking, I. (1993). Working in a new world: The taxonomic solution. In P. Horwich (Ed.), *World changes. Thomas Kuhn and the nature of science* (pp. 275–310). Cambridge, MA: MIT Press.

Hanson, N. R. (1958/1972). *Patterns of discovery*. Cambridge: Cambridge University Press.

Holton, G. (1973). *Thematic origins of scientific thought*. Cambridge, MA: Harvard University Press.

Kaiser, W. (2001). Electron gas theory of metals: Free electrons in bulk matter. In J. Buchwald & A. Warwick (Eds.), *Histories of the electron* (pp. 255–304). Cambridge, MA: MIT Press.

Kuhn, T. S. (1962/1996). *The structure of scientific revolutions* (3rd ed.). Chicago: University of Chicago Press.

Kuhn, T. S. (1993). Afterwards. In P. Horwich (Ed.), *World changes. Thomas Kuhn and the nature of science* (pp. 311–339). Cambridge, MA: MIT Press.

Kuhn, T. S. (2000). *The road since structure. Philosophical essays, 1970-993, with an autobiographical interview*. Chicago: University of Chicago Press.

Kusch, M. (2015). Scientific pluralism and the chemical revolution. *Studies in History and Philosophy of Science Part, 49,* 69–79.

Kvanvig, J. (2013). Perspectivalism and reflective ascent. In D. Christensen & J. Lackey (Eds.), *The epistemology of disagreement. New essays*. Oxford: Oxford University Press.

Lange, M. (2017). *Because without cause*. Oxford: Oxford University Press.

Lavoisier, A. (1799). *Elements of chemistry* (4th ed.) (Robert Kerr, Trans.), Philadelphia.

Margenau, H. (1950). *The nature of physical reality*. New York: McGraw Hill.

Massimi, M. (2007). Saving unobservable phenomena. *British Journal for the Philosophy of Science., 58,* 235–262.

Massimi, M. (2011). From data to phenomena: A Kantian stance. *Synthese, 182,* 101–116.

Massimi, M. (2015a). Working in a new world: Kuhn, constructivism and mind-dependence. *Studies in History and Philosophy of Science, 50,* 83–89.
Massimi, M. (2015b). Walking the line: Kuhn between realism and relativism. In A. Bokulich & W. Devlin (Eds.), *Boston studies in the philosophy of science. Kuhn's structure of scientific revolutions: 50 years on*. Berlin: Springer.
Massimi, M. (2018a). Perspectival modeling. *Philosophy of Science, 85,* 335–359.
Massimi, M. (2018b). A *perspectivalist* better best system account of lawhood. In L. Patton & W. Ott (Eds.), *Laws of nature* (pp. 139–157). Oxford: Oxford University Press.
Planck, M. (1906/1913). *The theory of heat radiation* (Engl. Trans.). Dover Publications.
Priestley, J. (1782). *Disquisitions relating to matter and spirit* (2nd ed., Vol. 2). Birmingham: Pearson and Rollason.
Pritchard, D. (2005). *Epistemic luck*. Oxford: Oxford University Press.
Rosenberg, J. (2002). *Thinking about knowing*. Oxford: Oxford University Press.
Smith, G. E. (2001). J.J. Thomson and the electron, 1897–1899. In J. Z. Buchwald & A. Warwick (Eds.), *Histories of the electron* (pp. 21–76). Cambridge, MA: MIT Press.
Sosa, E. (1991). *Knowledge in perspective. Selected essays in epistemology*. Cambridge: Cambridge University Press.
Stoney, G. J. (1874/1894). Of the 'electron', or atom of electricity. *The London, Edinburgh, and Dublin Philosophical Magazine and Journal of Science, 5*(38), 418–420.
Teller, P. (2011). Two models of truth. *Analysis, 71,* 465–472.
Teller, P. (2018). Making worlds with symbols. *Synthese*. https://doi.org/10.1007/s11229-018-1811-y.
Teller, P. (2020). What is perspectivism, and does it count as realism? In M. Massimi & C. D. McCoy (Eds.), *Understanding perspectivism. Scientific challenges and methodological prospects*. Abingdon: Routledge.
Thomson, J. J. (1891). On the illustration of the properties of the electric field by means of tubes of electrostatic induction. *Philosophical Magazine, 31*(190), 149–171.
Thomson, J. J. (1893). *Notes on recent researchers in electricity and magnetism*. Oxford: Clarendon Press.
Thomson, J. J. (1897). Cathode rays. *Philosophical Magazine*, *44*, 293–316.
Thomson, J. J. (1904). *Electricity and matter. Silliman lectures*. New York: Charles Scribner's Sons.
Thomson, J. J. (1906). *Carriers of negative electricity. Nobel lecture.*
Townsend, J. S. E. (1899). The diffusion of ions into gases. *Philosophical Transactions of the Royal Society A, 193,* 129–158.