# Rhetoric, Incommensurability, and the Curious Case of Dr. Brewster

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Thank you very much to our organizers, Jen-Jeuq Yuann and Paul Hoyningen-Huene, for organizing this remarkable conference, and for inviting me to speak. And thanks to all of you for coming to hear what I have to say, on a Sunday morning. Welcome to the Church of Incommensurability.

I am a rhetorician of science and I have long thought both that rhetoricians have something of considerable exchange-value to offer other scholars of science, and that it would be nice if other scholars of science felt the same way. For the most part, they haven't. I am hopeful that this conference signals something of shift.

Incommensurability, in particular, is a matter about which rhetoricians should be able to come together with philosophers, historians, and sociologists of science. The data is primarily rhetorical. Incommensurability arises when argument fails, and argument, as Aristotle told us long ago, lies at the heart of rhetoric. Like philosophy, with which Aristotle aligned it, rhetoric has no subject matter of its own. It has, rather, a driving concern with the way all subjects are understood, framed, and discussed. As Aristotle put it, "Neither rhetoric nor dialectic is the scientific study of any one separate subject; both are faculties for providing arguments." (*Rhetoric*, 1356$^{a}$).

What I want to do today, is to bring rhetoric to bear on a putative case of incommensurability, the clash of corpuscular and undular theories of light in the 19$^{th}$ C. This case is especially interesting because it seems to offer very robust evidence of at least one type of incommensurability, the weakest type. Indeed, Jed Buchwald and George Smith hold that incommensurability is controversial largely because Kuhn relied too heavily on the Einsteinian/Newtonian divide as his prototypical case. It would have been much easier to persuade the science studies community of incommensurability, they


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argue, if he had only anchored his arguments in the dispute over the theory of light in the first part of the 19th century (Buchwald and Smith 2001, 469).

For my own part, I agree that this dispute brings precision to one fairly pivotal aspect of what we call incommensurability, associated with Kuhn's idea of the lexicon, but I have my doubts that it is the definitive case that can dissolve the controversy, in large part because I think the controversy is illusory, and, as controversies often do, this one has a life of its own largely independent of the facts of the matter. It often seems to be little more than scholars moving the goal posts around, and disagreeing on their placement, rather than on the activities of the players (the scientists).

I will be quarrelling with Buchwald and Kuhn, to a certain degree, as well as continuing an argument with Alan G. Gross, a rhetorician, someone with whom I am in close agreement on many, many things, including the importance of rhetoric for understanding science, but with whom I disagree profoundly over the "existence" of incommensurability. Alan is an incommensurability realist; I am an incommensurability constructivist. Alan finds with Buchwald and Smith that incommensurability of the Kuhnian taxonomic-lexicon sort is instrumental in breakdowns of understanding, finds 19th C optical physics as a very convincing example of its contribution to such breakdowns, and finds its most revealing manifestation in the work of David Brewster (Gross 2005; 2006, 86-87). I will be arguing, in fact, quite the reverse of Buchwald, Smith, and Gross;[1] namely, that Brewster demonstrates the *commensurability* of the 'two' 'sides' in the corpuscular/undular disputes of 19th C optical physics.

**Rhetoric**

But let's start with *rhetoric*. We all know in that in daily, desktop-dictionary language the word signals florid, empty, misleading speech.[2] Rhetoric is false or merely

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[1] And, of course, Kuhn, who endorses Buchwald's argument heartily (2000, 237)

[2] en.wiktionary.org, for instance, returns this:

1. The art of using language, especially public speaking, as a means to persuade.
2. Meaningless language with an exaggerated style intended to impress.
   "It's only so much rhetoric."

empty, but in either case it is a negative polarity, opposed to the true and the substantial. We have, in the conceptual frame for this view, a world of unadulterated fact. This world is associated with personal witnessing, and also, at one remove, with those practices that realize a public, calibrated, instrumentally governed version of witnessing—the sciences. But, parallel to this world of cold hard facts are representations of those facts. These might be pure, content-based, substantial representations, associated with the discourses of science, as well as engineering, legal testimony, some forms of journalism, documentaries, and so forth. But representations might also be adulterated, warming and softening and reshaping those facts. Various unnecessary and frivolous pieces of language might get added. Style gets added to the content. Distortions arise, some just because it is the nature of language, with its metaphors and metonyms, its colours and tones, its emotion and attitude, to twist the facts in one way or another; others, because people deliberately manipulate language to change the dispositions and actions of others, lying or misdirecting. This is the territory of *rhetoric*, in its desktop-dictionary meaning.

Both of the beliefs in this frame are, of course, well founded. It *is* in the nature of language to lead thought and conduct in various conflicting directions, to entrench and to resist and to codify and to erode beliefs. People *do* speak and write to change the dispositions and actions of others, to their detriment as well as to their benefit. These are precisely the aspects of language and symbol systems generally that rhetoric studies. Rhetoricians, however, generally don't hold that there is any degree-zero language, or even degree-zero thought, in which the structure has no effect on belief or action. You might have a putatively *objective* style. You might have an overtly *florid* style. You never have *no* style. Rhetoricians also generally hold that the meaning of language is an amalgamated product of form *and* matter, style *and* content, not one or the other. These claims should not be controversial to anyone familiar with Ordinary Language philosophy, Speech-act theory, Relevance theory, the study of pragmatics, cognitive linguistics, or any number of other contemporary approaches to language.

The usages of *rhetoric* in daily language, cluster around two basic polarities, both resting on presuppositions that style and content are separable and that style is where the

problems lie: the Satanic polarity and the Vacuous polarity. These are polarities, fields of attraction and repulsion, not discrete points, and usages often flip back and forth between them, but both are readily apparent in cases associated with incommensurability. Themes of wilful misrepresentation and of incoherent gibberish often come up in discussions of incommensurability, sometimes by scientists on either side of a putative incommensurable divide, sometimes by science-studies scholars of incommensurability. Kuhn tended to use *rhetoric* in its vacuous sense, obscuring truth and facts (e.g., 2000, 155). Feyerabend used the term word more often, more along the Satanic dimension—against Popper, for instance (1981b, 177)—but preferred to exaggerate the wilful distortion aspect of language and style with the term, *propaganda* (e.g., 1993, 16-17, 78, 118).

Turning now directly to the way rhetoricians define their discipline, I offer two quick interconnected definitions, about 2000 years apart, each of them heavily ramified:

> [Rhetoric is] the ability, in each particular case, to see the available means of persuasion.
>
> Aristotle (*Rhetoric*, $1355^{b}25$-6)

> [Rhetoric is] rooted in an essential function of language itself, a function that is wholly realistic, and is continually born anew: the use of language as symbolic *means* of *inducing cooperation* in beings that by nature respond to symbols.
>
> Kenneth Burke (1969, 43)

Rhetoric, that is, concerns itself with how it is people persuade each other of beliefs and courses of action. Aristotle is very clear, as most philosophers have not been, that rhetoric implicates the full range persuasive possibilities, chiefly featuring truth and argumentation. Among the other things to notice about his approach is that rhetoric is an analytic discipline. It is about *seeing* the available means of persuasion, not about deploying them. Burke, for his part, is clear that rhetoric does not concern something external to language, something that gets added in certain situations, but concerns rather an inevitable, ineliminable quotient in all language use (indeed, in all semiosis, including

music, images, and physical activities, like experimentation). And don't let his emphasis on *cooperation* here fool you; he is acutely aware that opposition is an equally inevitable function of language. Elsewhere, he writes of the "competition of cooperation" and the "cooperation of competition"—noting, for instance, that warfare, the ultimate act of opposition, requires cooperation on a massive scale (Burke 1969, 403)—and he is interested in the ways that agreements can be forged out of disparate, competitive positions.

I'm hopeful that by this point you can see the relevance of such a study for the issues associated with the term, *incommensurability*, to which we now turn. But please allow me to offer two more quotations to seal the deal:

> Rhetoric is, or should be, a study of misunderstanding and its remedies. (Richards 1936, 3).

> [Incommensurability is] the frequent failure of groups of scientists to understand the work of other groups. (Buchwald 1992, 40)

**Incommensurability**

I will eventually argue that *incommensurability* does not apply in any helpful way to scientific programmes, but that argument is pointless without a fair amount of semantico-rhetorical work around the term *incommensurability*, in order to get a precise-enough understanding of what it is I am arguing does not apply. I will also be resting part of that argument on taxonomic lexicons of the late-Kuhnian sort, so it may be best to start with a taxonomy of uses of our key term, *incommensurability,* in science studies. We will chart this against its mathematical origins. Euclid's definition 1 of Book 10 of the *Elements* is "Those magnitudes are said to be commensurable which are measured by the same measure, and those incommensurable which cannot have any common measure." It is a binary notion, in opposition to commensurability. Commensurability is a property of pairs of numbers like {2, 6}, which are 'measurable' (by, for instance, 3), but not of a pair like {2, π}, which has no such common measure, no common divisor, and therefore has inverse property, *incommensurability*. This originating usage is not, of course, news to anyone at this conference, but I do want you to keep that originating, sponsoring sense of

the word in mind. Please note, in particular, two characteristics of the mathematical sense:

1. The mathematical notion is inherently binary. Incommensurability is defined in full and complete opposition to commensurability.
2. The extension of this notion to theories (paradigms, frameworks) is an analogic extension.[3] It figures theories (paradigms, frameworks, research programmes) as numbers, concepts that are simple, precise, rigid; fully and completely specifiable.

Both of these characteristics are problematic for the science-studies use of the word. In particular, almost everyone in science studies uses *incommensurability* to signal a range of phenomena, not a uniform phenomenon in binary opposition to another uniform phenomenon. Nor do most science studies scholars regard theories as simple, precise, rigid objects; fully and completely specifiable. If they were, they wouldn't change, and most historians of science would be out of a job. If they were, there would be impermeable to social influences, and most sociologists of science would be out of a job. If they were, arguments about them would be unnecessary and unproductive, and most philosophers of science would be out of a job.[4]

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[3] Another explanation of this extension, which I will not pursue, but which is worth noting, is that it is synecdochic (or, in lexicographic language, meronymic). Synecdoche works by figuring the whole by the part ("all *hands* on deck," for "all *sailors* on deck;" "three hundred *head*" for "three hundred *cattle*;" "he has a new *set of wheels*" for "he has a new *automobile*"), or the part for the whole ("the *law* is here" for "a *policeman* is here;" "he *drinks* too much" for "he *drinks one particular class of fluids too much, alcoholic fluids*:" "*America* is a high-capitalist democracy" for "*The Unites States of America* is a high-capitalist democracy." On this extension, numbers are parts of theories, so theories have the properties of numbers—i.e., the fallacy of composition.

[4] "Why did [Kuhn] call it incommensurability?" Fred D'Agostino expostulated at Incommensurability50, and then paid out the rhetorical question with "I have no idea." I think I might. I no longer think, in any case, that Kuhn meant to take the term technically, from mathematics on a precise analogy (I'm much less sure about Feyerabend). There was a fairly widespread, at least among eggheads, usage of *incommensurability* that had disengaged itself from the mathematical usage, and meant something like "super incompatibility." The first usage in *Structure* is (I think—snippet view, Google) "[T]he normal-scientific tradition that emerges from a scientific revolution is not only incompatible but often actually incommensurable with that which has gone before" (Kuhn 1996, 103). In short, I think Kuhn took a non-technical term, used it rather casually, and spent the rest of his career turning it back into a quite different technical term.

Incommensurability as a range of phenomena is handled in two general ways by science scholars. On the surface, these approaches seem quite different—one is scalar, fuzzy, continuous; the other, discrete, categorical, taxonomic—but they are quite compatible. In all of their uses, for instance, they implicate comparison, translation, and intelligibility; or, rather, of incomparability, untranslatability and unintelligibility. While the feeder discipline of the *word* is mathematics, the feeder disciplines of the *concept* are much broader, prominently including anthropological linguistics and the psychology of perception, which is why there is so much talk of (mis)understanding and (mis)communication in the discussion of (in)commensurability.

Turning to the first of our science-studies usages of *incommensurability*, many scholars talk regularly of weak and strong incommensurability (Forster 2000), shallow and deep incommensurability (Hacking 1983), limited and radical incommensurability (Sankey 1994).[5] That is, they talk of incommensurability as a scalar phenomenon. The nature of terms like *weak* and *strong*, *deep* and *shallow*, *limited* and *radical*, is relative and gradual; if we formalize it somewhat, such talk generates a metaphorical incommensurometer, with gradations moving from full comparability, translatability and intelligibility to utter incomparability, untranslatability and unintelligibility.

There are two things I want you to notice in this cluster of usages. First, we immediately run afoul of the mathematical metaphor. Incommensurability in its originating field is a binary notion. For any pair of numbers, there either *is* a common divisor or there *is not* a common divisor. This yes/no, on/off sense of the Euclidean

[5] Thagard and Zhu's definition of the focal points of such a continuum is very helpful; "Two theories or conceptual schemes are strongly incommensurable if they are mutually unintelligible, so that someone operating within one conceptual scheme is incapable of comprehending the other. Weak incommensurability, however, does not imply mutual unintelligibility, but only that the two conceptual schemes cannot be translated into each other" (2002, 81. Lower yet on the incommensurometer, of course, would be full intelligibility and intertranslation; that is, commensurability. For those in need of a more technical definition for the endpoints, Hintikka's (1988, 29) can be adapted naturally; "$T_1$ and $T_2$ are (totally) incommensurable if and only if there is no question $Q_i$ in a set of relevant questions $\{Q_i\}$, $i \in q$ which is answerable on the basis of both of the two theories". The definition flips nicely for zero incommensurability (or total commensurability); $T_1$ and $T_2$ have zero incommensurability (are totally commensurable) if and only if every question $Q_i$ in a set of relevant questions $\{Q_i\}$, $i \in q$ is answerable on the basis of both of the two theories. Hintikka's definition completely formalizes the 'degree' sense of incommensurability, since, in principle, it generates a ratio for any pair of theories representing "the total information of their shared answers to the total information of the answers yielded by the two theories combined"(1998, 25). I, however, would not be first in line to try and calculate such a ratio.

concept is one of the implications of the word that made it so appealing to Kuhn, in particular, whose affection for the gestalt switch in his early writings is well known. Either you see the rabbit or you see the duck, not both. Either you see the world as a Newtonian or as an Einsteinian, not both. The word *incommensurometer* is also, we note, a kind of oxymoron, the name of a device for measuring the amount of unmeasurability. But this nominal incoherence just further highlights the ill fit of the underlying mathematical metaphor, theories (paradigms, frameworks) are numbers.

Second, as much as this scalar usage departs from the mathematical analog, it inversely approaches the anthropological-linguistic analog, different theories (paradigms, frameworks) are different languages. Miscommunication and misunderstanding, as we know, lend themselves easily to a generalized scalar account. There may be no simple unit of measure for the quantity of understanding or communication, but we all recognize that communication can be more or less successful for a variety of reasons. Communication can be partial. So can understanding. So can the related informing notions of (in)commensurability—argument, translation, comparison. Arguments can be more or less intelligible (partially intelligible). Translations can be more or less successful (partially successful). Objects can be more or less comparable. Indeed, we can go further. Comparisons are *always* partial. Partiality is their essence: they concern similarity, not identity. More to the (in)commensurability point, objects, concepts or values which may not seem comparable on the surface can often be made more comparable. Comparisons can usually be enriched, objects, ideas, and values can be brought into closer conceptual alignment, by looking deeper or focussing more closely or shifting perspective. Correspondingly, the intelligibility of arguments can be increased. Translations can be improved. These are all the products of negotiative communication, of bringing people toward shared understandings. Aristotle's focus on persuasion is largely monologic, but many other theorists—including Burke, as well as Isocrates, Nietzsche, and I.A. Richards, see persuasion as an ongoing, reciprocal activity, as fundamentally dialogic. Another prominent definition of rhetoric, in sync with a dialogic perspective and not incompatible with Aristotle's perspective, is "the function of adjusting ideas to people and people to ideas" (Bryant 1953, 413). Rhetoric deployed well

inevitably shapes ideas as it induces people, performing a kind of noetic docking procedure for concepts.

The second way of talking about (in)commensurability is taxonomic. This is, in fact, the most common way of framing (in)commensurability. Even the framings I referenced above (strong and weak, deep and shallow, etc.) are often treated as taxonomic. But it is useful to differentiate between differences in degree and differences in kind. Taxonomies signal differences in kind. There are many different taxonomies in the literature—some explicit, some implicit—and they are growing daily. In my 2005 introduction to *Rhetoric and Incommensurability*, I render them all into four categories (Harris 2005, 22):[6]

Brick-wall incommensurability

labels a situation in which communication is ridiculously impossible, where each party can only hear gibberish when the other speaks, if they hear anything at all. As William James put it, there may be universes "so unlike and incommensurable, and so inert towards one another, as never to jostle or interfere. Even now there may actually be whole universes so disparate from ours that we who know ours have no means of perceiving that they exist." (James 1911, 125).

In the science-studies literature, this species might better be called "Straw Man" incommensurability, since no-one, certainly not Kuhn or Feyerabend, advocates such a usage of *incommensurability*. It is used by opponents of incommensurability (Putnam & Davidson, for instance) to dismiss the very idea of incommensurability.

Cosmic incommensurability

labels a situation in which communication is severely hindered because of different perceptions of the 'same' phenomena, where the parties can't communicate coherently because they 'live in different worlds'. It is with cosmic flavours of incommensurability that notions of a gestalt switch usually come in, and Kuhn's talk of religious conversion. It is around this conception, too, that we find most of the Whorfian talk of different languages determining different ontologies.

[6] I also include notice of Paul Hoynigen-Huene's *meta-incommensurability*, incommensurability about the presence/possibility of incommensurability, which I treat as a special subcategory of pragmatic incommensurability, and I trace out two related usages—value incommensurability, and postmodernist incommensurability—both of which align fairly closely (without naturally subcategorizing) to pragmatic incommensurability in science studies.

I won't spend any further time on this species, except to note that I have seen no evidence that any lack of understanding this dramatic holds of rival theories or frameworks. It may well hold diachronically. Kuhn came to the notion of incommensurability trying to understand Aristotelian physics, and Hacking has a particularly good illustration contrasting Paracelsus with contemporary medical thought (1983, 70). It may even hold of highly distinct, culturally embedded epistemologies together. In such circumstances, however, we have left science behind. My own feeling is that goodwill, hardwork, and cooperation can overcome such obstacles on the cultural dimension and that hardwork and good scholarship can overcome it on the historical dimension, that we are dealing with hyper-incompatibility rather than 'true,' never-the-twain-shall-meet incommensurability.

Pragmatic incommensurability

labels a situation in which argumentation in particular is rendered very difficult because themes and practices of the contending parties are out of synch, because the parties appeal, often covertly and vaguely, to different values; inevitably, there are substantial semantic components to such situations as well, in part because the dividing line between semantics and pragmatics is highly permeable.

With pragmatic incommensurability, we are now in the realm of language—a complex realm in its own right, but somewhat more tractable than individual perception or wholesale communicative incoherence.

The critical factor about pragmatics is the centrality of context. When someone says "it's hot in here," semantically they are making a statement about the temperature. Pragmatically, in one context they could be diagnosing an overheated reactor, issuing a warning, explaining why they opened the window, delivering the punch line to a joke, … Pragmatics is less about the *meaning* of an utterance than the *point* of the meaning of an utterance between specific interlocutors in a specific context.

Pragmatic incommensurability invokes the importance of methodology and values to the operation of a claim in rival theories. This species is the most central for episodes that trigger the use of the term, incommensurability.

Semantic incommensurability

labels a situation in which communication is significantly impaired because clusters of meanings used by the parties are out of congruence. There has been plenty of action around this notion in the literature and at this conference, with a variety of claims and arguments and varieties over the years. Since it is the lowest-grade version of communicative mismatch that sports the label, *incommensurability*, I want to zero in on the variant of semantic incommensurability that Kuhn adopted in his later career, which he called "local incommensurability" (2000, 35-37).

Kuhn's local incommensurability is the lowest-grade incommensurability actively advocated in the literature, so far as I am aware. Perhaps the best known example

involves the different referents for various terms in Ptolemaic and Copernican astronomies. Under one astronomy, the thing we are standing on is a planet, the sun and the moon are not; under another astronomy, the thing we are standing on is not a planet, the sun and the moon are. Howard Sankey (1998) terms this variant, *taxonomic incommensurability*, because it most directly implicates conflicting categorizations of phenomena. So, in the Ptolemaic taxonomy, there are seven planets; the moon, the sun, Mercury, Venus, Mars, Saturn and Jupiter. There are stars, like Rigel and Sirius, and there is this thing we stand on, outside the taxonomy altogether, the earth. In the Copernican taxonomy, there are six planets, some of which are the same as in the Ptolemaic system (Mercury, Venus, Mars, Saturn and Jupiter). But there is an extra one, the earth, now incorporated into the taxonomy, and two others are placed in distinct categories; the sun is a star, along with Rigel and Sirius, and the moon is a new category altogether, a satellite. These categorizations most dramatically result in opposing truth conditions for the 'same' utterances. One can point down at the earth below one's feet and proclaim "Lo, a planet" and be making a true claim as a Copernican, a false one as Ptolemaist. One can point to the sun or the moon, utter the same clause, and be making a true claim as a Ptolemaist, a false one as a Copernican.

Staying now with taxonomic, semantic incommensurability as fully as possible (though pragmatic incommensurability cannot be entirely excluded), I would like to turn the discussion to the curious case of David Brewster, a 19th C Scottish Natural Philosopher, who intermittently and often simultaneously combined, as Stephen Shapin puts it "'the worst of all professions' [namely, being an author] with that of harassed editor [of the *Edinburgh Encyclopedia*], under-rewarded inventor [of the kaleidoscope, among other optical devices], and general factotum to Edinburgh scientific institutions" (Shapin 1984, 18).

**The curious case of David Brewster**

Brewster is important in connection with semantic incommensurability for two reasons in particular. Firstly, he was embroiled in the controversies surrounding the shift from a mostly corpuscular theory of light to an undulatory theory of light, and secondly, the optical framework he plied is regarded by Jed Buchwald, George Smith, and Alan

Gross as not only exemplifying incommensurability, but as proof positive of its existence in scientific contexts and as a clear demonstration of its scientific consequences (Buchwald 1998; Buchwald and Smith 2001; Gross 2004, 2006). Incommensurability, in Buchwald's view, drove Brewster to the margins of science.

These various arguments hang on Buchwald's analysis of Brewster's framework, which Buchwald terms "extraordinarily odd" (1989, 256), "almost complete nonsense" and "a hodgepodge" (1989, xix) —though, within a few years, Buchwald's distaste for Brewster's accomplishments had muted to just "intensely perplexing" (1992, 54). The framework was deeply incoherent, Buchwald argues, because it commingled wave and particle terminologies so promiscuously, that it drove the kindly undulationists to reluctantly disregard Brewster because they just couldn't understand him. Brewster carried [his mixing of particle and wave vocabularies] to such an extreme," Buchwald says, "that it gradually became impossible for the emerging group of wave theorists to communicate with him in any fruitful way, with the result that he was gradually pushed to the scientific periphery" (1989, 259).

Before we examine Buchwald's argument, however, I want to suggest that there is an alternate history to the gradual nudging of Brewster out of the scientific center—namely, one in which he was aggressively shoved out of the center, with his hat thrown after him, and the door slammed. Let's have a look at Brewster's career up until 1832:

> Brewster had determined the law of polarization by reflection, the so-called Brewster law. The optical community soon recognized Brewster's research. The Royal Society of London in 1815 awarded Brewster the Copley Medal for his studies of polarization, and elected him a Fellow of the Society. ... In 1819, he received the Rumford Medal from the Royal Society for his study of the interference pattern produced by polarized light. In 1830, he won another medal from the Royal Society for his discoveries of the laws of polarization by refraction and by pressure. (Chen and Barker 1992, 76-7)

He was also the earliest—and, in this "alone … [for] two decades" (Cantor 1984, 74)—advocate of Edward Young's law of interference, an important result assimilated naturally into wave optics. He was knighted in 1831. He was seen as the strongest British optical

experimentalist in the early 19th C, and a capable theorist. On this basis, he was commissioned by the new British Association for the Advancement of Science, at its inaugural meeting, "to prepare for the next Meeting a Report on the progress of Optical Science" (*Report* [1833], 52). He submitted the report at the 1832 meeting (*Report* [1833], 308-322), reading three additional optical papers at the meeting as well. Things went down hill from there.

Publicly, Brewster claimed to be—borrowing Jean-Baptiste Biot's term for particle/wave neutrality—a *rieniste* in the debate, a nothing-ist, with no theoretical commitments at all (Cantor 1984, 73). But, at the ontological level, he was deeply skeptical of the reality of the wave theory, and correspondingly sympathetic to the reality of the particle theory. Light 'really was' particulate for Brewster. He admired the explanatory reach of wave theory, but was alarmed at the requirement for "an *ether*, invisible, intangible, imponderable, inseparable from all bodies, and extending from our own eye to the remotest verge of the starry heavens" (1838b, 306). For this reason, wave theory was necessarily "defective as a *physical* representation of the phænomena of light" (1833; Brewster's emphasis). So, while his commissioned report to the British Association for the Advancement of Science repeatedly recognized the impressive descriptive successes of the wave framework and the substantial achievement of wave theorists, *and* pointed out the deficiencies of particle theory, it also highlighted several phenomena that wave theory could not handle well at all:

> Even the theory of undulations, with all its power and all its beauty, is still burthened with difficulties, and cannot claim our implicit assent. It has not yet brought under its dominion the phænomena of elliptic polarization in all its varieties, from the rectilineal polarization of transparent bodies, to the almost circular polarization of pure silver. It has not explained the singular influence of the force of double refraction over the force which polarizes reflected light; and it has great difficulties to struggle with, in accounting for certain phænomena of absorption (*Report* [1832] 1833, 318)

Brewster also strongly advocates the centrality of the "physical data" to optical studies, issuing a call for broader experimental research in optics, "a new and almost untrodden

field, which may be successfully cultivated by almost every variety of talent" (*ibid*.). One of his other papers at the meeting offered such physical data about absorption, an area of particular difficulty for wave theory ([1832] 1833a), while another analyzed experimental results concerning interference (which was a strong suit of wave theory) in terms of "*rectilineal undulations propagated across the retina*"—arguing that is for the undulations to be not in the light itself, but in the physiological perception of light ([1832] 1833b, 549). These articles could not be easily mistaken as nothing-ist observations. Nor is there a shortage of sociological factors that might help to account for the almost total rejection Brewster suffered:

> He was a Scot, his opponents principally English; there was a generation gap between them; he was an experimentalist, they were mathematically-trained theoreticians; he failed to gain a chair, whereas many of his opponents taught in universities and attracted disciples; he was an Evangelical, they were mostly broad churchmen. (Cantor 1984, 73)

Whatever the colligation of motives among his opponents, J.B. Morrell observes that "[b]etween the 1832 and 1833 meetings"—that is, after Brewster delivered his "Report on the recent Progress of Optics" and his three small articles—"the supporters of the wave theory moved into the attack" (Morell 1984, 28). They dominated the Association, and most were fervent in both their denunciation of the particle framework and their advocacy of the wave framework. William Whewell led the charge, abetted by William Hamilton, Baden Powell, George Airy, Humphrey Lloyd, and somewhat more mutedly, John Herschel. The meeting was in Cambridge and Whewell, as local secretary, gave the opening address. He took the opportunity to suggest that the time of particle/wave rivalry was over, that the wave theory was taking its place alongside "the doctrine of gravitation" in terms of scope and explanatory success, that there "nothing corresponding [to these successes] in the history of the [particle] theory," and that in several recent challenges the wave theory had come out on top (*Report* [1833] 1834, xv-xvi). At the 1832 meeting, there was a separate section devoted solely to Optics, with largely experimentalist and particle-sympathetic articles (the three by Brewster, and two by Richard Potter). At the 1833 meeting, however, the papers on optics transactions were subsumed under "Mathematics and Physics"—mathematical modelling being the strong suit of the

undulationists—and wave-theory dominated the section. There were papers by Hamilton, Lloyd, Powell, and Herschel (along with two short reports by Potter)

But the craftiest move was undoubtedly the commissioning of a report "On the state of science of Physical Optics" (*Report* [1833] 1834, 468). Brewster's report was only one year old, and they commissioned a new one to be submitted the following year, by someone his marked junior in age, station, and scientific accomplishment. It could not be missed by anyone, as Morrell notes, least of all Brewster, that this report was "deliberately intended to replace Brewster's own." This move, "[t]o call for and obtain a second report on a topic by a different author, within just two years of the first report," Morrell adds, "was unique in the early annals of the Association; and it doubtless added insult to the injury inflicted on Brewster." Nor was commission given to any old *rieniste*. The new report was assigned to the rising undulationist, Humphrey Lloyd, which "ensured that from 1834 the wave theory became the new orthodoxy of [the Association]" (Morrell 1984, 29). The report is a systematic comparison of the two frameworks, with one end, to establish the overwhelming superiority of wave theory.

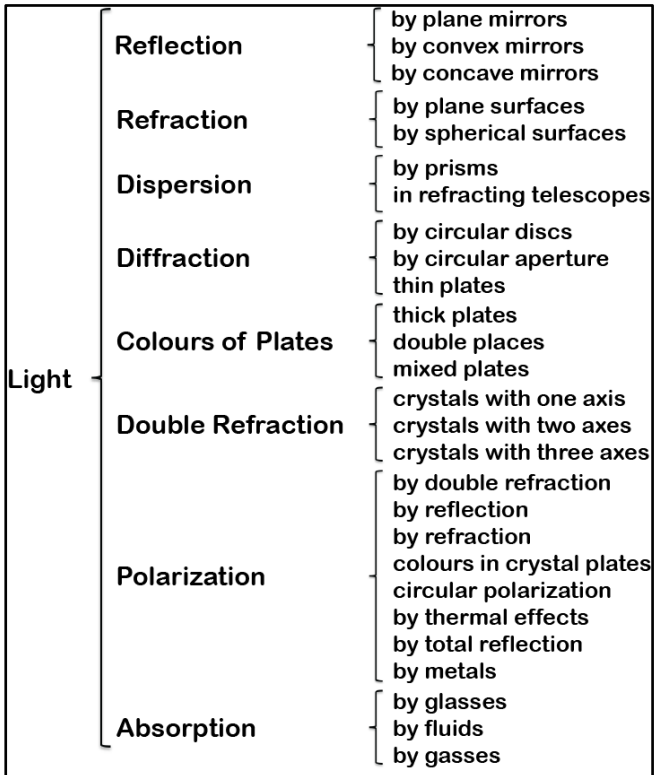


**FIGURE 1: David Brewster's taxonomy of light. Reproduced from Chen (2000, 16; Figure 2.1).**

The clearest illustration of this displacement is taxonomic. Xiang Chen has rendered the relevant taxonomies with considerable precision in his *Instrumental Traditions and Theories of Light* (2000). I have reproduced Chen's chart of Brewster's taxonomy in Figure 1, Lloyd's in Figure 2. The differences are immediately apparent, though nothing at the surface of either taxonomy declares its theoretical allegiance.

Brewster's, in particular, looks fairly *rieniste*. It is only two levels deep, and appears to be a loosely assembled catalogue of the sorts of states light might assume under different conditions—reflection, refraction, dispersion, polarization under thermal changes to the polarizing medium, …. It suggests, if not the personal interests of a curious optical philosopher, then perhaps the disciplinary interests of a curious collective. And, indeed, Brewster was fond of saying that he avoided hypotheses, establishing his taxonomy on largely chronological grounds, as new problems and observations surfaced over the long, intermittent history of optical research. But Newton, the corpuscularian, was the star of that chronicle, and the taxonomy synchs well with Newton's concerns and accomplishments. The taxonomy, too, was very popular—"the most influential one developed from the Newtonian framework," Chen calls it (2000, 15).

But even more specifically, Brewster saw his brief on the "recent progress" of optics to chiefly implicate a comparison of "the two rival theories of light," Newtonian and undulatory (*Report* 1833, 320-321), and the taxonomy was meant to "function as a ground for comparing the explanatory powers" of the rivals—not to the advantage of the wave theory (Chen 2000, 15). By Brewster's tally, the two theories were equal for his categories of Reflection, Refraction, Double Refraction, and Polarization, with the edge falling to particle theory for some aspects of Double Refraction and Polarization; Dispersion was a clear triumph for particle theory, while

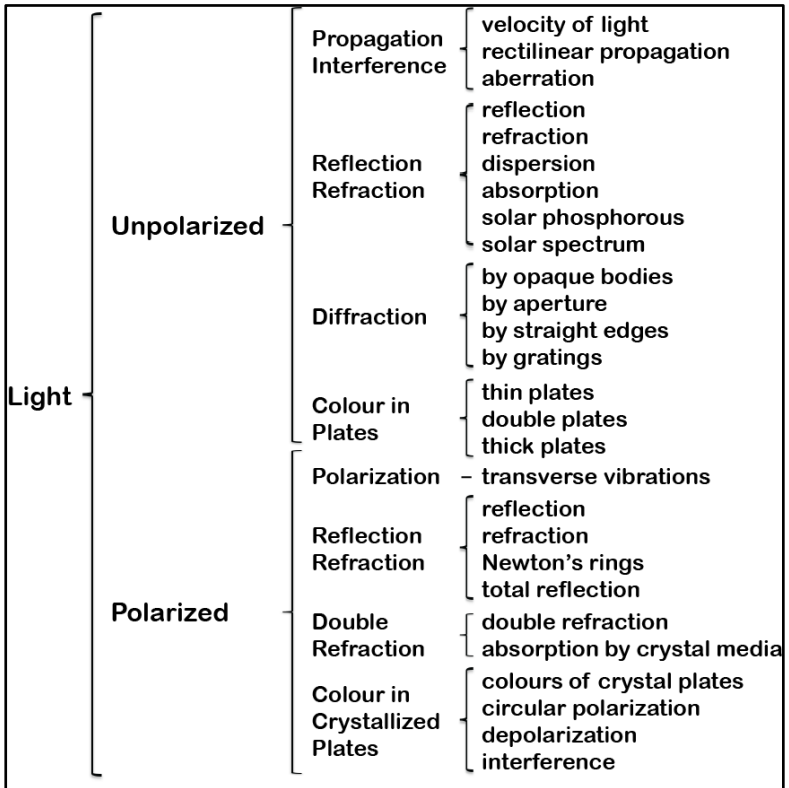


**FIGURE 2: Humphrey Lloyd's taxonomy of light. Reproduced from Chen (2000, 23; Figure 2.3).**

Diffraction and Colour of Plates fell to the wave theory; Absorption is not only more naturally handled by particle theory, in Brewster's view, it "presents a formidable difficulty to the undulatory theory" (*Report* [1832] 1833, 322). In short, Brewster's taxonomy is rhetorically constructed to represent a careful adjudication between the two rivals, and to lean its support most fully toward particle theory.

Lloyd's taxonomy looks more theoretically informed from the outset, making one initial division. Where Brewster has an assortment of eight top-level categories, Lloyd has one binary division, on the principle of polarization. All light is either polarized or unpolarized for Lloyd; any other luminous phenomena are subordinated to these categories. If the taxonomy is narrower, it is also deeper, with three levels of categorization, not Brewster's two. It is also, Lloyd quickly makes clear, much more aggressively partisan than Brewster's, without even the veneer of neutrality Brewster gave his. The taxonomy anchors and organizes the report, whose aim is unreservedly to declare the debate over and wave theory the indisputable victor. Lloyd echos Whewell in associating wave theory with gravitation, and opens his case by founding the centrality of mathematics in theory appraisal:

> Whatever be the apparent simplicity of an hypothesis, —whatever its analogy to known laws,—it is only when it admits of mathematical expression, and when its mathematical consequences can be numerically compared with established facts, that its truth can be fully and finally ascertained. Considered in this point of view, the wave-theory of light seems now to have reached a point almost, if not entirely, as advanced as that to which the theory of universal gravitation was pushed by the single-handed efforts of Newton. Varied and comprehensive classes of phænomena have been embraced in its deductions; and where its progress has been arrested, it has been owing in a great degree to the imperfections of that intricate branch of analysis by which it was to be unfolded. The principles of the theory of [corpuscular] emission, on the other hand, have, in comparatively few instances, been mathematically expressed and developed; and accordingly this theory presents but rarely those points of contact with experimental truth by which alone it can be judged. (*Report* [1834] 1835, 295-296)

The taxonomy makes this case by the way it ranks and organizes the phenomena of interest. Its very simplicity of arrangement argues in its favour, while the loose assortment of Brewster's taxonomy is proof of the theoretical poverty of particle theory. "[A] theory thus overloaded does not merit the name," Lloyd says. "It is a union of unconnected principles, which can at best be considered but as supplying the materials for a higher generalization. Its very complexity furnishes a presumption against its truth" (*Report* [1834] 1835, 296). There is, in short, no hope at all for the particle theory: "the proof of its insufficiency seems even stronger than the positive evidence in favour of the rival theory" (*Report* [1834] 1835, 297).

Lloyd organizes luminous phenomena (or, *re*-organizes them, if we take Brewster's taxonomy as his rhetorical starting point) very strategically. Polarization is the major division because it is the most revealing phenomenon for the differences between particle and wave theories of light. The differences between particle and wave approaches "reveal themselves in almost every situation that alters polarization," Buchwald says (1989, xvi); hence, the major division is between polarized and *un*-polarized light. This terminological move is not seen elsewhere in the taxonomy (we don't get diffracted and un-diffracted light, reflected and un-reflected, depolarized and un-depolarized), and it makes polarized light seem like the basic kind of light; normal, everyday, natural light is taxonomized as lacking the property of polarization.

Lloyd's taxonomy is less about light as a phenomenon, that is, or light as a collection of phenomena, and more about the contest between theories. And polarization as a focal criterion favours wave theory, all the more so in a framework that determines ultimate truth on the basis of mathematical expression. Fresnel, in particular, had recently introduced mathematical tools that made modeling polarization in wave theory very elegant, in contrast to various ad hoceries in the particle account. There is considerably more rhetorical work in Lloyd's taxonomy than this major division, however. Dispersion and Absorption, which are major categories in Brewster's taxonomy, are not just bumped to the second level, as one might expect. They are demoted further down in significance, to the new third level. Recall that both Dispersion and Absorption favour particle theory in the comparison. More yet: Lloyd pruned "all of the categories related to thermal, mechanical and chemical effects of crystallized media" (Chen 2000, 23), areas in which

Brewster conducted investigations. While conceding that such topics are "of high interest," he strikes them from "this arrangement, as being but remotely connected with the leading object of the present Report"—namely, to establish the overwhelming superiority of wave theory (*Report* [1834] 1835, 297). In particular, "these [phenomena] are as yet little understood, and … the science of light can hope to derive little aid from their examination" (*ibid*). If not exactly anti-experimental, Lloyd's attitude is certainly expressly anti-natural history, the investigation of natural phenomena for the sake of investigating natural phenomena. If optical phenomena cannot be brought into the mathematical fold of wave theory, they are not optical phenomena.

What we have in both taxonomies, in other words, is not so much representations of dispassionately sorted and assigned categories of phenomena as a set of structured rhetorical moves to advance the campaign of one side or the other. Brewster's taxonomy still has some allegiance to the natural-historian inductive mandate of collecting all phenomena that might bear on the field and its theories. Lloyd's taxonomy has a mathematico-deductive allegiance. If it can't be described mathematically, and principles can't be derived from the mathematics, it isn't really of any concern to the theory. And, to the extent that the theory determines the field—the paradigm of the field—some phenomena are ruled out of court.

To the extent that we might want to call the taxonomies incommensurable—and, frankly, I don't—it is a manufactured incommensurability. They are designed for incompatibility. Brewster's taxonomy, while motivated in part by the natural-history impulse to categorize phenomena of interest, even where they don't have a comfortable home in an established theory, is still strongly shaped to foreground phenomena that support the particle framework or embarrass the wave framework, or both. Lloyd's taxonomy is designed unapologetically, from the ground up, to expose the insufficiencies of the particle framework and promote the virtues of the wave framework and, in aggregate, to declare a triumphant conclusion to the rivalry by driving one from the field altogether and wholly occupying that field by the other, the unspoken mandate of his report in the first place. Commensurability is therefore not particularly in Brewster's interests, who regards the wave theory as a powerful fiction, and it is positively antithetical to Lloyd's interests.

The actual taxonomies of the particle/wave debate—what Buchwald calls "overt, articulated structures" (1992, 56)—do not in fact play any significant role at all in Buchwald's (or Gross's) argument. The argument, rather, is built on "scientific kinds and their associated taxonomic structure [that] often remain unarticulated" (1992, 55). Buchwald, therefore, needs to make that structure explicit before his argument can take flight, and he does a great deal of subtle and detailed investigation to explicate the unarticulated structure of Brewster's framework. Before we get there, let's quickly rehearse the notion of scientific kinds behind this approach. Several people have spoken about that notion already, most notably Paul Hoyningen-Huene's masterful account of the way that notion developed over the course of Kuhn's career and what it looks like in his unpublished final manuscript, *The Plurality of Worlds*. My account will be a faint echo of his, but, on a Sunday morning, a reminder is called for.

The taxonomies of concern are what Kuhn called in his later work, 'structured lexicons of scientific kinds.' Here is how Howard Sankey characterizes their relevance for scientific communication:

> [S]uccessful communication does not require speakers to use the same criteria in applying terms to the world. It requires only that speakers operate with "homologous lexical structures" - i.e., with a structured vocabulary incorporating the same taxonomic system (1983, p. 683). Analogously, for translation to succeed from the lexicon of one theory into another, theories need only share lexical taxonomy. If they do not, they are incommensurable. (Sankey 1998, 40)

Of crucial importance to lexical homologies between covert taxonomies is a principle that helps ensure the sanctity of the lexicon, what Kuhn called the "no-overlap principle," that:

> no two kind terms … may overlap in their referents unless they are related as species to genus. There are no dogs that are also cats, no gold rings that are also silver rings, and so on: that's what makes dogs, cats, silver, and gold each a kind. Therefore, if the members of a language community encounter a dog that's also a cat (or, more realistically, a creature like the duck-billed platypus), they cannot

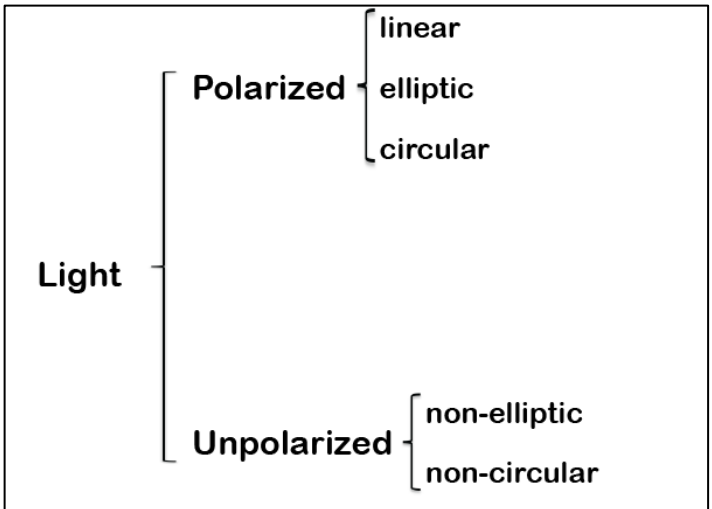


**FIGURE 3: The unarticulated taxonomy of wave theory. Adapated from Buchwald (1992, 49; Figure 2).**

> just enrich the set of category terms but must instead redesign a part of the taxonomy. (2000, 92)

So, total independence is OK. Species-to-genus complete subsumption is OK. But sort-of-a-dog, sort-of-a-cat intersection is not OK.

With this in place, we can turn to the taxonomies of interest, the covert, unarticulated taxonomies of light held by the particle theorists and the wave theorists in the early 19th C. Buchwald does incredibly subtle work to uncover these taxonomic lexicons, working not just from the British tradition that is of direct interest, but also the French tradition of Fresnel, Biot, and others. And, as everyone who has looked at scientific disputes is aware, you don't have simple uniform frameworks, but collections of theorists and experimetalists working out problems individually and in various collectives with various sympathetic or antagonistic relations to other individuals and collectives. These individual proclivities and allegiances and antipathies all shape language use. Herschel's usage may not have precisely been Whewell's usage. Whewell's usage may not have been precisely Lloyd's. Brewster's may not have been Potter's, Potter's may not have been Biot's. And so on. Discoveries were being made daily that shift these usages in various ways. Arguments were being offered that shift taxonomic alignments. This is very difficult lexicographic work. But Buchwald is convincing that the taxonomic lexicons of the particle group looked like the taxonomy in Figure 3, the wave group's looked like the one in Figure 4.

So, this is the state of taxonomies in the middle 1830s. Buchwald argues that "[these] differences run so deep that they precluded mutual understanding" (Buchwald 1989, xxii). These were not the only differences. There were also significant personal and institutional factors that added friction to the possibility of understanding. There are issues of power and status and goodwill involved. And, in disputes of this sort, it is never always clear what the phrased public responses to one another's arguments are doing. A scientist might say "X is confused," or "I don't know X could mean here" or "It is impossible to understand X on this question" and so on—not because the scientist fails to understand X, but because the scientist wants to sow doubt about X's credibility. There are, in other words, real misunderstandings, faux misunderstandings, and combinations of the two, that can make it very difficult to assert with any confidence that Y fails to understand X. But let's leave all these aside. I am happy to grant Buchwald's diagnosis that these semantic mismatches contribute to a marked gap of understanding.

In the 1989 book, *Rise of the Wave Theory*, Buchwald does not mention incommensurability. There are a few short sections on David Brewster in the the book that are worth noticing, however. In particular, it is here that Buchwald says Brewster's work in the late 1830s into the mid 1840s is "extraordinarily odd", "almost complete nonsense" and "a hodgepodge," remarking that it became impossible for the emerging group of wave theorists to communicate with him in any fruitful way, with the result that he was gradually pushed to the scientific periphery.

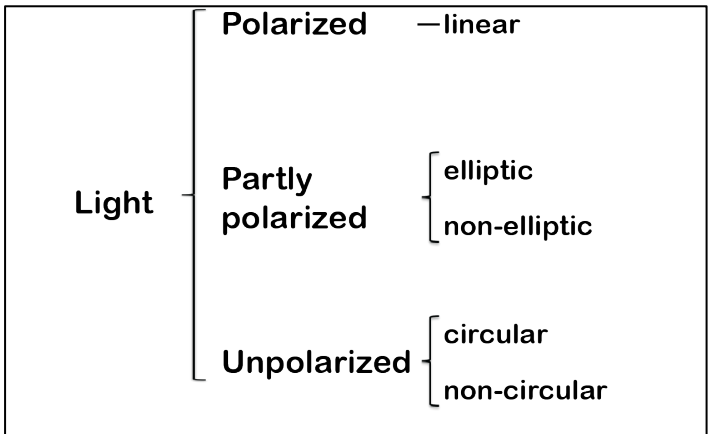


**FIGURE 4: The unarticulated taxonomy of particle theory. Adapated from Buchwald (1992, 49; Figure 2).**

It is, in fact, in this period, after the smack-down of the 1833 meeting of the British Academy for the Advancement of Science, that Brewster's work becomes most interesting. He moves into a new mode of engagement with the wave theorists. He works to blend some of their successes into the particle framework. Kenneth Burke says of Freud's work that his "theory is a dictionary, a lexicon for charting a vastly

complex and hitherto largely uncharted field. You can't refute a dictionary," Burke says. "The only profitable answer to a dictionary is another one." I am not saying that Brewster consciously adopted this strategy, but he started to shift his lexicon, particularly through

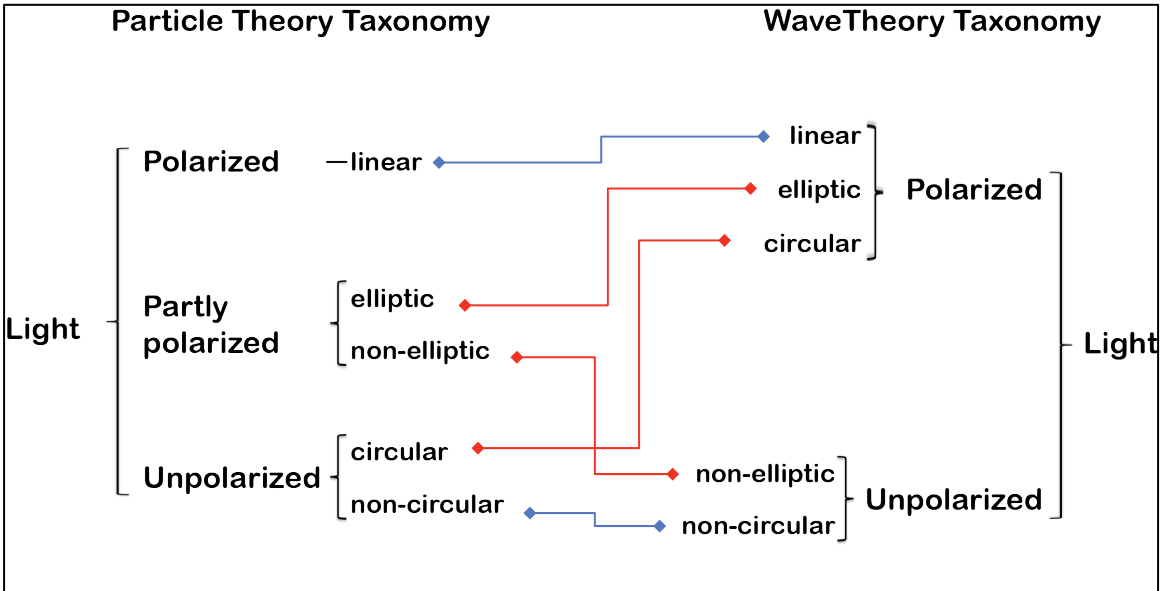


**FIGURE 5: Overlaps between the particle and wave taxonomies. Adapated from Buchwald (1992, 49; Figure 2).**

the addition of the word 'phase', in a way that could present a new dictionary. It is the implications of this dictionary that Buchwald calls nonsense and hodgepodge. Shortly after he completed the book, Buchwald wrote a highly relevant paper, on his own, and a few years after that another one with George Smith. In these papers, kind terms become the chief topic, and both of them make Brewster a poster boy for incommensurability.

Buchwald starts by plotting the two covert taxonomies against one another. (Kuhn, by the way, knew of this work and was very encouraging.) Putting the taxonomies side-by side, he makes explicit how they conflict (see Figure 5).

Only two share the same parent category, linear and non-circular. The other three sub-classes all have different parents. This shows, of course, a clear violation—several clear violations—of the no-overlap principle. In and of itself, this would provide evidence that the communicative breakdown we see between wave and particle theorists can be explained by Kuhnian local incommensurability. It's quite persuasive. I would put four caveats on this analysis, however.

First, the terms were not used equally by each camp. So that, for instance, *linear* was often just assumed. So the word *polarized*, as a default, meant 'linear polarized.' That's all it meant for the particle guys. Words like *unpolarized* were often used in a way that did not identify what sort of polarization it *wasn't,* noncircular or non-elliptic. In short, while I am quite sure Buchwald is right in his categorization, the actual usage of the words did not always reflect this scheme as closely as this neat arrangement suggests.

Second, the existence of a clear super-category in one taxonomy, the particle taxonomy—the claim that polarization was a graded phenomenon, not the binary opposition it was in wave theory—was not missed by anyone, *could not* be missed by anyone. So, some of the overlap was not only expected, but obvious, which reduces the semantic friction.

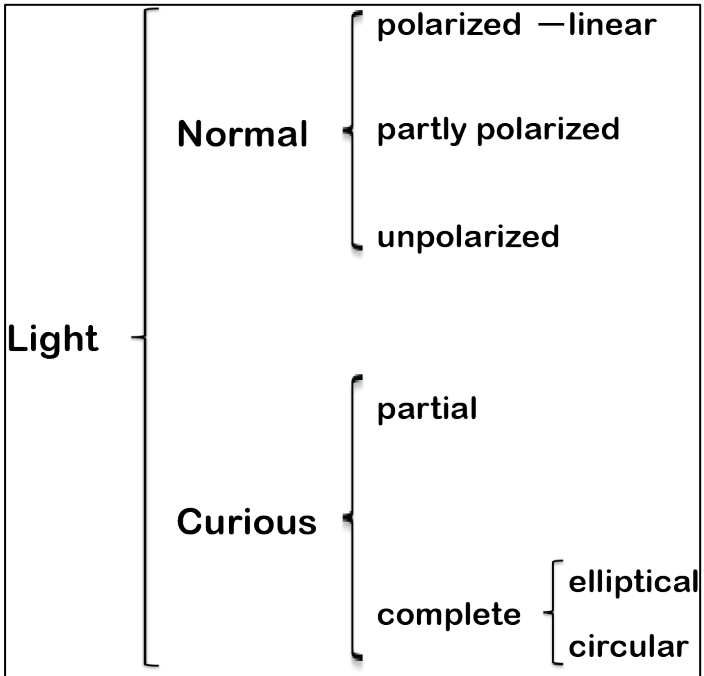

**FIGURE 6: Brewster's unarticulated taxonomy. Adapated from Buchwald (1992, 51; Figure 3).**

Third, while this precise account of semantic conflicts provides an explanation for the communicative failures, it is certainly not the only explanation, nor even the most compelling. For my money, ill-will is a more substantial factor in the failures.

And fourth, I honestly don't know what the word *incommensurability* buys us in this story. I mean this for Buchwald's analysis. But I also mean it for Kuhn's framework. There is a real semantic phenomenon at work here, and it provides an explanation for some aspects of scientific communicative blockages (and very likely other blockages as well), and Kuhn had to call it *something*. But the implications of the word incommensurability, not just in its mathematical origins, but in its very morphology, are that any sort of reconciliation is

foreclosed.[7] We know that's not what Kuhn meant, nor what Feyerabend meant. Fred quoted an article yesterday, citing the line "Kuhn is not blameless" (Weinberger 2012). No, he's not. And one of the things he's not blameless for is picking the name, *incommensurability*.

Buchwald's next move is to recreate Brewster's tacit taxonomy (see Figure 6; the last one, promise). There is at least on odd thing about this taxonomy that sets it out as different from the others that Buchwald builds, his choice of the word, *curious*, for a major (and, as we will shortly see, brittle) branch of the taxonomic tree. There is very little justification for this in Brewster's usage, and it signals an unsympathetic stance toward Brewster's work that is unbecoming in so scrupulous a historian as Buchwald. I don't quibble with his taxonomy overall, just his tactic of painting a big nose and a moustache on it before he even begins his articulation.[8] In fact, Brewster does not provide a label at all. Buchwald, as the exegete of hidden taxonomies, needs to find a label for that justified node of the tree. It's just that he might have chosen something more technical and less prejudicial.

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[7] Fred D'Agostino expresses this concern in his introduction to *Incommensurability and commensuration*—saying that *incommensurable* doesn't hold "if the modal operator 'able' is given a strong reading" (2003, x), but the suffix consistently has a strong reading (X-able pretty much always means 'able to do X'). Another troublemaker occurs at the other end of the word, another 'strong' marker, the prefix *in-*, which negates the suffix, giving us, effectively, 'unable to do X'—in the particular case, 'unable to measure to a common standard.' See Prelli (2005, 298-299) for related discussion. He opts for the suffix, *-ate*, which has the virtue of being much more neutral, it just makes adjectives without contributing any modal content at all, so that *incommensurate* really means something like 'not measured to a common standard', without any negation of the ability to do so. Indeed, incommensurate usually suggests a remedy is called for that will commensurate, say, someone's salary with her responsibilities.

[8] It's actually a bit worse than this, since Buchwald is very misleading when he claims that Brewster called this new kind of light, *curious* (1989, 51). In the attached footnote, he adds "Brewster referred to phenomena involving circular polarization as 'this curious subject' [1838, 95]; he ended his account of elliptical polarization with the words, 'for a more full account of this curious branch of the subject of polarization' (my edition does not include this phrasing, and Buchwald gives no citation). But Brewster uses the word curious 32 times in this text (*A Treatise on Optics*), only a handful of times in relation to elliptic or circular light. It is, in fact, a general approbatory term he uses for facts and laws that are both surprising in the relevant context of expectations and—almost by virtue of their challenge to expectations; remember his experimentalist affection for data—also admirable. In my very haphazard reading of the period texts (by Whewell, Lloyd, Herschel, Potter, Hamilton, and Brougham), this usage appears quite common to 19th C light scientists. Buchwald even uses the term 'non-curious' (in single-quotes) of Brewster's unarticulated taxonomy, as a synonym for normal light (1992, 51, 51n23). Brewster never uses this term. It is a strangely ungenerous move by Buchwald—though, of course, his disparaging attitude towards Brewster's theorizing is clear in *The Rise of the Wave Theory of Light*.

What is important about this set of kind terms is that they mark Brewster's attempt to amalgamate the two taxonomies above (Figures 3 & 4); or, rather, to find a way of talking about the relevant phenomena that was consonant with the vocabularies of particle theory and wave theory, since none of these taxonomies (Figures 3, 4 or 6) was explicit. The vocabularies were shaped by the behaviour of light, and the behaviour of light was revealed through experimentation, and the experimentation was conducted through instrumentation. So, in a very real sense, the taxonomies were governed largely by the instruments. The differences in the taxonomies (Figures 3 & 4) do not result from the use of different instruments, but the application of different interpretive strategies to the results of experiments utilizing those instruments. Each camp had a different set of strategies because they had a different conception of the basic phenomenon under investigation. For particle theorists, of course, the basic phenomenon was a particle. Particles travelled geometrically, in groups, called rays. So their interpretations involved the behaviour of these groups, which allowed them to see one group in the same beam behaving one way, another group behaving a different way. In absorption, for instance, some particles just get stopped by various materials (gas molecules, in particular), while others go on through. Or some of them might be bent in one direction, while others were bend in a different direction. That is why they had no trouble talking about *partial* polarization, which they saw as a subset of rays in a beam being polarized, the rest of them unpolarized, and it allowed them to talk about intensity in a simple numerical way, as the quantity of rays in a beam (or, for variations of intensity, in parts of a beam)

For wave theorists, the basic phenomenon was the wave front, moving through the ether. This conception called for a much different mathematics; and it was the power of this mathematics that was the chief selling point of the approach. Now, wave theorists could interpret many geometric behaviours quite easily—in essence, swallowing them whole into the theory, by considering a small enough section of the wave front and calculating trajectories on it. On the other hand, wave theory had access to aspects of light behaviour that the particle theorists did not, wavelength, amplitude and phase. Phase, in particular, was a highly explanatory notion with respect to polarization, and it was the property of phase that led to the overlaps between the two taxonomies depicted in Figure 5. Or, more precisely, it was the experimental demonstration of groupings of light

kinds (circular and elliptic) accommodated by the wave taxonomy, that led to the conflict, and phase was the property by which these groupings were explained.

This experimentally established grouping is what led Brewster to generate another higher-order kind in his taxonomy, preserving the established three-way polarized / partially-polarized / unpolarized division, and grafting on a new branch into which circular and elliptic light could be grouped. This branch is the one to which Buchwald gives the kind-term *curious*. "Indeed," Buchwald notes, "what compelled Brewster to generate his curious light in the first place was the existence, in [the wave framework] of something—namely, phase—that could be used to calculate how to transform elliptic into linear light, and he deliberately strove to extract some sort of meaning from the concept that could be adapted to the [particle]-scheme" (1992, 52).

Brewster incorporated a version of phase and its attendant mathematics into his *Treatise on Optics*, making his framework largely commensurable with wave theory. But the new higher order was grafted on with, and justified by, his notion of phase, which was tied very closely to the specific instrumentation that produced the behaviours he used phase. And herein lies the rub for Brewster, for Buchwald, for the dominant wave community at the time, though perhaps not for the scattered, aging, resolute particle community (Buchwald does not document this aspect, and I found no relevant commentary in my brief, narrow primary research). No one, in any case, bought what he was selling. Why not? The answers aren't very complicated, I don't think. They include all the sociological factors I mentioned earlier—generational, national, institutional—but they surely also include the artificiality of the project as well, the ad-hoc nature of Brewster's phase, which Buchwald captures eloquently:

> The intimate, indeed nearly perfect, identity between Brewster's novel category ["Curious" light] and the specific operations that fabricate and detect it makes it very difficult to assign any independent meaning to Brewster's ['phase']. This indubitably reflects the outcome of his effort to insert something into the [particle]-scheme — a simulacrum of [wave-theory] phase — whose significance cannot be divorced from its origin in the [wave-theory] without embodying it in specific instrumentalities. Because [wave-theory]-phase is a characteristic of light

> that reflection is only one way of manifesting, it can be separated from the particular devices of the day. This cannot be done with Brewster's ['phase'] because there is no way to apply it outside of the apparatus whose behaviour it was designed to specify. (Buchwald, 1992, 53)

Brewster's set of kind terms was highly artificial. In particular, his "Curious" branch was not derivable from any principles of particle theory, and it served only one purpose, to describe phenomena generated by specific arrangements of equipment. It worked, but it was ugly and unproductive. This kind of accommodation is routine in natural language, of course. When liquefied, coagulated soy bean materials entered the restaurants and supermarkets of English-speaking countries, we took the word *tofu* along with it, changing the bilabial fricative to a labio-dental fricative, but otherwise absorbing it seamlessly. It had only one purpose and did not signal its fit into any biological or culinary taxonomies. It has become productive (*tofutti*, *tofurkey*, *tofu-loaf*), but it no longer serves as a transparent kind label, as it still does in Chinese, where the component morphemes mean curdled + bean. The sort of borrowing accommodation is always available. But elegance and explanation can be lost—which scientists care far more about than regular folk, who just want a verbal handle for that new stuff.

OK, this is what I have argued, or, in any case, what I have made claims about, with various degrees of structured support. I have argued that rhetoric as a discipline can be illuminating about science, and that it is especially attuned to matters of communication, which are, if not the absolute heart of the issues people have assembled around the term *incommensurability*, then certainly the uncontested *sine qua non* of those issues. No one talks about incommensurability without pointing to breaches of communication.

I have argued that incommensurability dissolves as a problem when submerged in rhetorical understandings of symbolic action. I realize that many people at this conference do not see incommensurability as a problem. They see it as an important generative principle. I do see it as a problem, because of the implications that most researchers see correctly as following from Kuhn's formulation in *Structure*. I agree

with most of the speakers at this conference that friction is important in science—the competition of cooperation and the cooperation of competition. And I am very impressed with Kuhn's development of kind terms as a powerful account of some aspects of friction. But I don't think we need to take the unwholesome term, *incommensurability*, from him. I agree with my colleague, Carolyn Miller, that "incommensurability is an idea we can probably just do without" (2004, 502)—though I am not under the illusion that the science studies community will follow suit. I haven't proposed a dictionary myself, only complained about a page of an existing one.

And I have argued, contra Buchwald, Smith, and Gross, with Kuhn supportively in the background, that far from revealing the debilitating effects of incommensurability, Brewster in fact shows that particle and wave approaches to light could be brought together in a framework that was, taken on its own terms, quite successful. But it's own terms weren't enough. Firstly, nobody was interested in commensuration. The other particle folks, few and far between, were recalcitrant. The wave theorists had already won by that point, and gained nothing by trying to accommodate a theory they were shovelling dirt on by 1833. Secondly, and more importantly, the commensurated amalgam was too expensive. It wasn't productive, not from a lack of coherence, but from a lack of flexibility.

What Buchwald does beautifully in his argument is show how unstable and unproductive Brewster's amalgam was, and how closely operationalized it was to specific instruments. It did not generalize. This is not a problem of incommensurability.

One of the most compelling aspects of the progress of science is the semantic shifts that happen—usually not because, as in ordinary language, of fashion, error, or accident (though these factors do play a role), but because people who care very much about the way the universe works are trying really hard to get the words to fit their evolving perceptions of the way it works, with accuracy, precision, and consistency. Brewster failed on these counts. But his failure to craft a productive framework amalgamating aspects of particle and wave theories was simultaneously a success at proving how those theories were commensurable.

And, finally, let's be clear about what it means to say that two theories are commensurable; or, that they are *not in*commensurable; indeed, to say that incommensurability simply does not obtain of theories.

It means that there are significant points of contact by which scientists in one theory can take the just measure of another theory, with quite different operating principles, and make defensible judgements about its merit. And vice versa.

It does not mean that they *will* take the just measure of an opposing theory.

It does not mean that they *must* take the just measure of an opposing theory.

It does not mean that such theories can be brought into fruitful alignment.

**References available on request**