*Being charitable to scientific controversies*

***On the Demonstrativity of Newton’s Experimentum Crucis***

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*I. On Contexualizing*

There are at least two different approaches to the rational reconstruction of a position. The first can be characterized by what we call a *universal principle of charity*: its strategy is to reconstruct positions in accordance with some, typically tacit, interpretative commitment in order to endorse or dispute the claims made, reveal their strengths or weaknesses, etc. The principle of charity that this strategy relies upon places the emphasis of interpretation on the truth content and rationality of the author's beliefs, and as such, it diminishes the relevance of their context. Contextual approaches, on the other hand, while methodologically more diverse, share a common perspective: utterances are made in a given context, which bears on their meaning. Diversity arises from the fact that contexts for interpretation can be construed in various ways, and therefore various meanings can be reconstructed on various planes, depending on the methodological commitments of the historian. Here we are going to argue for a *via media* characterized by a *local principle of charity*, that pays due attention to what is rational, thus preserving charitability, in the context in which specific utterances are made.

The relevance of contextual approaches to questions of meaning is frequently disputed. Although they are acknowledged as heuristically fruitful in constructing historical narratives, their capacity to contribute to questions of theoretical content and methodology is contested. A critique of this sort assumes a sharp division between rational and contextual reconstructions as exemplified by the following quote: “To say that everything in science is local and contextual implicitly downplays the importance of understanding the arguments that scientists use, and have used, to support their hypotheses or experimental results in any manner that breaches locality. ... In our view history of science is not just about causes, whether these be social, cultural, or a complex mix of the two. It is also about understanding.”[[1]](#endnote-1) The quest for understanding detached from the “local and contextual” is exactly what we mean by an approach relying on a universal principle of charity. If contextual details are minimized, there is indeed hardly anything to rely on but truth conditions and rationality – implying that there is nothing in them either that is local and contextual. When developing interpretations along these lines, interpreters rarely expound on what role the concepts of truth and rationality play in their approach, but instead lament “a general abandonment of rationalist interpretations”.[[2]](#endnote-2) This could easily lead to charges that they apply their own intuitive standards of truth and rationality, without having judged carefully the usefulness, legitimacy and productivity of their approach.

It is hard to doubt that some awareness of context is methodologically unavoidable. Looking at the material the historian works with without having some clue of its pragmatics, one can only find a bunch of locutionary acts. Lacking knowledge of context of any sort entails the impossibility of forming the appropriate expectations one must have in order to understand the utterances as belonging to a particular discourse*.* Someone lacking sufficient background knowledge could mistake the chronicle of the Wannsee conference for a fiction, or Tolstoy's *War and Peace* for a historical report.

Similarly for the historian of science: Without knowing that the utterances in question belong to the cultural and institutional context of science the utterances cannot be understood as belonging to *that* kind of activity. Without this, the interpretive work cannot even begin; one could even say that science itself cannot be understood. This finds acknowledgement in Howard Stein who suggests that one should read treatises “in the way one ought to read a scientific treatise (indeed, in the way one ought to read any sort of exposition)” and “not in the way one reads a computer program or a legal contract (looking for possible ‘bugs’ or ‘loopholes’), but with the aim of understanding the view being propounded.”[[3]](#endnote-3) One must be aware of the kind of text one is reading in order to understand it. The question is thus not whether we need context in order to interpret utterances, but rather *how much and what kind of context* it is that we must rely on.

Without knowing that the scientific enterprise has certain aims and that pursuing them conforms to certain norms, it is hardly possible to form an adequate understanding of a scientific theory. On closer scrutiny it turns out that visions about the aim of science, as well as the norms of pursuing it, are themselves local and subject to change. The enterprise has a different character if its aim in forming theories is thought to be uncompromised truth or empirical adequacy, or if it is focused on instrumentality, the knowledge of God, causal explanations or just producing mechanical models.[[4]](#endnote-4)

One may believe that even more details are necessary to understand a position correctly. Arguably, a charitable interpretation requires some knowledge about the more *direct context* in which utterances are made. One must know what moves can be made within the cultural-institutional context that science provides by those engaged in scientific practice, and one must be aware what a given move amounts to in this context. Contextualizing can focus on detailed knowledge about the instrumental and discursive contexts in which knowledge is produced in order to understand how evidence is produced and used in theory building, on the legitimate moves one can make in a controversy, on the kind of inferences that can be drawn, and on how controversies between conflicting theories are settled etc.

Let us further agree that, in order to be charitable, interpretation and understanding must presuppose some rationality on the part of the agents. But rationality is a notoriously vague concept and requires some fine-tuning before put to use, and tuning can be done in different ways. If the interpreter is committed to finding the maximum of truth and coherence in someone's beliefs behind his utterances, then the interpreter’s expectations will be tuned accordingly. When interpreting all of the written documents produced by a scientist as expressions of a unified system of thought, one necessarily creates for oneself the problem of utterances that seem to contradict each other – since it is already presumed that all utterances contribute to the same consistent system. And therefore, one has to find ways to explain them away. This results in treating separate loci as if they testified to synchronically held beliefs which can yield the exclusion of inconsistencies, possibly attributed to distorting factors or momentary loss of sobriety.

Even more importantly, this focus makes the interpreter less sensitive to inconsistencies, disregarding them as insignificant noise, and it can make him believe that the coherence of the reconstruction is *evidence* for the coherence of the utterances. As Howard Stein justifies his approach: “It is quite true that I cite later works to reinforce evidence, or to explicate a principle, drawn from earlier ones […]. But I have done so only when there seemed to me good evidence that the views expressed in the later passages agree with those in the earlier ones”.[[5]](#endnote-5) Looking at utterances through this lens runs the risk of increasing coherence by shifting one’s attention away from those things that could diminish it. If one focuses on finding maximal truth and rationality in a corpus of sentences, then it is not surprising to find in them the coherence of theoretical content.

But it is not necessary to stick to the idea of maximal individual coherence in order to produce charitable interpretations. In order to do so one needs first to decide on questions of pragmatics: what is the protagonist doing in the passages under scrutiny. Sociological interpretations can achieve this, but they usually see utterances made in an epistemic context as attempting to reach certain social goals, often downplaying the role of epistemic content. Therefore their charitability can be questioned. But going sociological is not the only option. As we will illustrate, charitability can be preserved in context-sensitive interpretations, by focusing on the argumentative role of utterances in reaching epistemic goals.

The interpreter's expectations, if tuned by this context-sensitive principle of charity, will be different. In addition focusing on coherence in beliefs, this principle also gives a further orientation for the reconstruction by advising one to take seriously the various commitments an author makes in argumentative situations responding to challenges. The utterances in this light have specific functions in the argumentative discourse, and the focus is on these functions and commitments. These commitments, such as accepting a premise, rejecting another speaker’s theory, etc., are possibly inconsistent with the set of beliefs the author should have on the basis of coherence-maximizing reconstructions. The charitability of our contextual reconstruction consists in the maximizing of the argumentative force that can be ascribed to a given utterance.

*II. A Tedious Dispute*

In the following we contrast the two models of rational reconstruction by examining an important debate in the Early Modern period, the controversy that followed Newton’s first publication on “A New Theory of Light and Colours”. As an example of a coherence-maximizing approach, we will discuss Howard Stein’s unpublished manuscripts on the debate, but without considering the historiographical controversy about them. We agree with Stein on many points, including his observation that his papers and reactions have turned on “fairly deep issues of interpretation – issues that concern, in part, exactly what Newton’s theory itself asserts, but that concern also how *any* account of a scientific theory ought to be read.”[[6]](#endnote-6) We, however, believe that the methodology of his reconstruction, which basically posits the coherence of Newton’s beliefs throughout the period in question, is less than ideally suited to reconstructing the controversy – and thus to the interpretation of Newton’s position, let alone to that of others. In contrast, we will illustrate the differences, at times striking, in the commitments Newton makes in various letters throughout the debate. This we will do with attention to the arguments put forward concerning the scope and demonstrativity of the famous *experimentum crucis* (crucial experiment). Then we turn to the evaluation of the reconstructions.

Historians have long recognized that “there is no short and final answer” to the question of what the *experimentum crucis* is supposed to prove.[[7]](#endnote-7) In Newton’s first publication the term surfaces at the end of a series of considerations designed to explain the elongated image of the spectrum and preceding this conclusion: “And so the true cause of the length of that Image was detected to be no other, then that Light consists of Rays differently refrangible, which, without any respect to a difference in their incidence, were, according to their degrees of refrangibility, transmitted towards divers parts of the wall.”[[8]](#endnote-8)

The *experimentum crucis,* together with the status of Newton’s theory, was a recurring theme in the debates following the publication, and it was challenged on various counts by his antagonists. The best known of these challenges is probably Hooke’s. He could still read the famous passage in Newton’s letter to the Royal Society, later deleted by Oldenburg, where, after demonstrating the different refrangibility of light rays with the *experimentum crucis*, Newton stated that “a Mathematician may determin all the Phaenomena of colours that can be caused by refractions, & that by computing or demonstrating after what manner & how much those refractions doe separate or mingle the rays in which severall colours are originally inherent; *I suppose the Science of Colours will be granted Mathematicall & as certain as any part of Opticks*.”[[9]](#endnote-9) Hooke explicitly challenged the epistemic status of Newton’s theory: “I cannot think it [Newton’s theory] to be the only hypothesis; not soe certain as mathematicall Demonstrations.”[[10]](#endnote-10)

In his reply to Hooke, Newton discussed in more detail both the design of the *experimentum crucis* and its demonstrativity. Regarding the latter, he wrote:

“In the last place I should take notice of a casuall expression wch intimates a greater certainty in these things then I ever promised, viz: The certainty of *Mathematicall Demonstrations*. I said indeed that the *Science of Colours was Mathematicall & as certain as any other part of Optiques*; but who knows not that Optiques & many other Mathematicall Sciences depend as well on Physicall Principles as on Mathematicall Demonstrations: And the absolute certainty of a Science cannot exceed the certainty of its Principles. Now the evidence by wch I asserted the Propositions of colours is in the next words expressed to be from *Experiments* & so but *Physicall*: whence the Propositions themselves can be esteemed no more then *Physicall Principles* of a Science. And if those Principles be such that on them a Mathematician may determin all the Phenomena of colours that can be caused by refractions, & that by computing or demonstrating after what manner & how much those refractions doe separate or mingle the rays in wch severall colours are originally inherent; I suppose the *Science of Colours* will be granted *Mathematicall* & as certain as any part of *Optiques.* And that this may be done I have good reason to believe, because ever since I became first acquainted with these Principles, I have with constant success in the events made use of them for this purpose.”[[11]](#endnote-11)

This can be considered a *clarification* of the position; as Stein says, “Newton is thus *demonstrably* not prevaricating in what he says to Hooke, but pointing accurately to what he had said, and explaining what he meant by it. For Newton, the ‘certainty’ of mixed mathematics rests upon experiments, and is thus ‘physical’—not ‘mathematical’—certainty.”[[12]](#endnote-12) Starting from “Physicall Principles” and using “Mathematicall Demonstrations” suggests that while the premises derived “from experiments” are contingently (and not necessarily) true, the reasoning nevertheless has a form that *if* these premises are true, the conclusion must necessarily be true. So the certainty of science is limited by the certainty of its principles, but not by the rigour of the reasoning leading to a conclusion from the premises. We suggest reading this passage, on the one hand, as *narrowing* and *weakening* the commitment the original claim makes to the certainty of the premises, and, on the other hand, as maintaining the demonstrativity of the inference process. That it is not just a clarification is also indicated by the *inductive* ‘hedging’ at the end of the paragraph, supporting the claim that the science of colors *is* mathematical, and providing an additional argument for the correctness of the physical principles.

Anthony Lucas challenged exactly this commitment to the status of the inference process in a later phase of the debate.In 1676, in a letter to Newton through Oldenburg, when investigating the *experimentum crucis*, he claims that „drawing the proof of this new Theory into a close Syllogisticall methode”[[13]](#endnote-13) shows that the experiment *does not* *provide* demonstrative proof. In Lucas’ reconstruction Newton’s major premise states that “Rays differently colourd suffer an unequall refraction at an equall incidence”. But the “maine difficulty lyes in the Minor, viz *that this unequall refraction necessarily implys an unequall refrangibility in rays differently coloured*”. The conclusion is that “rays differently colourd have an unequall refrangibility, even at an equall incidence on the prisme”[[14]](#endnote-14). As the “Minor is neither evident in it selfe ... nor evidenced from the *Experimentum crucis* ...,” Lucas continues, “I conceive not how according to the received lawes of Logick (which require a strict connection between proof and thing proved) this experiment can be esteemed a demonstrative proof”.

Now why would Lucas reconstruct the *Experimentum crucis* in this manner? Here we have to address three issues, namely the scope of the experiment, the question of whether “refrangibility” is meant to be a property, and Newton’s notion of demonstrativity. Regarding the first, in the original letter by Newton there is no mention of colours when describing the crucial experiment. But in his reply to the challenge by Hooke, in a short section titled “That ye Experimentum crucis is such,” Newton wrote that “ye designe of it is to show that rays of divers colours considered apart do at equall incidences suffer unequall refractions, without being split, rarefied, or any ways dilated.”[[15]](#endnote-15) This commitment, connecting refrangibility with colour, is at odds with the earlier one in the original publication[[16]](#endnote-16). Lucas, when reconstructing Newton’s “demonstration”, took up this commitment. This move appears to be justified: it postdates Newton’s original letter, and thus it suggests that this was indeed Newton’s position after the first round of criticisms. It is also noteworthy that Newton composed a number of drafts when responding to Hooke and gradually improved and refined his arguments, copying significant portions of the earlier drafts. In one such draft the issue is discussed *without* connecting refrangibility to the colours of the rays, so the stronger commitment in the published reply to Hooke can hardly be seen as an involuntary sloppiness on Newton’s part.[[17]](#endnote-17)

Concerning the second issue, the statusof refrangibility, Stein believes that when Newton, in an earlier letter to Lucas, states that he *demonstrated* different refrangibility by the *Experimentum crucis,*

"this claim will be found to satisfy even the stringent standard of Karl Popper, who argues that experiments can never provide warrant for the typical propositions of science, because the latter are *universal:* they cannot be established by a finite amount of evidence; but they can be *refuted* by a *single* counterexample. We have here to do, however, with an *existential* statement: that *there are* kinds of light that differ in refrangibility; and—as I have remarked in MMN—Newton’s *Experimentum Crucis* actually *produces* (examples of) such kinds, and *exhibits* their difference of refrangibility. The very unusual language Newton uses in this connection, then, testifies to his acute sensitivity to the methodological principle involved."[[18]](#endnote-18)

But this appears not only to be in conflict with the general understanding of Newton’s work on optics, that is, discovering a new (universal) property of light,[[19]](#endnote-19) but is also problematic if we take Newton’s response to Lucas where he states that each ray has an “internal disposition” to be refracted in a certain way.[[20]](#endnote-20) Again, the contextual reading suggests that the commitments Newton made in the debate are stronger than the ones suggested by Stein: they were understood in this stronger way, and this understanding was not corrected by Newton.

Regarding the third issue: how then did Newton respond to Lucas’ renewed challenge to the demonstrativity of his experiments? What did demonstrativity mean for him? As Stein writes, “In the case of the *Experimentum crucis,* it is quite reasonable to speak of … a ‘deduction’ – not, of course, in the strict logical acceptation of the term that is now unusual (and one certainly licensed by common usage).”[[21]](#endnote-21) *If,* however, Newton’s notion was *not* equivalent to the strict logical notion, and common usage licensed a weaker sense, it is hard to see why he did not simply disambiguate this in his response to Lucas, stating that by demonstrativity he did not mean a mathematical or logical demonstration. Yet Newton never rejected Lucas’ supposition that by “demonstration” a syllogism is meant, and other loci also suggest that, contrary to Stein’s comment, demonstration belonged to the realm of mathematical or logical inference-making in this period of Newton’s life.[[22]](#endnote-22)

On all three of the above counts, the analysis of Newton’s local commitments at various stages of the debate differ from the coherence-centered reconstruction that Stein provides.[[23]](#endnote-23) Subtle inconsistencies appear again and again, and some of these are far from insignificant. As we have seen in his letter to Hooke, Newton’s account of exactly what the *experimentum crucis* proves, and of how it proves what it proves, appears to link colour and refrangibility. In the following comment to Lucas his theory of colours is decoupled from that of light:

“[The Question of different refrangibility which I bring] ye Experimentum Crucis to decide, is not, as I sayd whither rays differently coloured are differently refrangible, but only whether some rays be more refrangible yn others…. If you consider this you wil see yt while you have laboured to oppose different refrangibility, there’s not one of your objections wch concerns it except ye third where ye Experiment succeeds otherwise than you have reported it. The rest are only against analogy between refrangibility & colour, for *if I would say yt rays differently refrangible have no appropriate colours, all your objections would cease*.”[[24]](#endnote-24)

Here Newton claims that with the help of the *experimentum crucis* he can derive the notion of different refrangibility, without referring to colours. So far so good. Yet, he also states that Lucas’ objections would “cease” *if* Newton would maintain that “rays differently refrangible have no appropriate colours”. This appears to fly in the face of one of his strongest commitments made earlier. When preparing his optical lectures, before the onset of the controversy, Newton stated that

“*The relation between the properties of refractions and those of colors is certainly so great that they cannot be explained separately*. Whoever wishes to investigate either one properly must necessarily investigate the other. Moreover, if I were not discussing refractions, my investigation of them would not then be responsible for my undertaking to explain colors; nevertheless the generation of colors includes so much geometry, and the understanding of colors is supported by so much evidence, that for their sake I can attempt to extend the bounds of mathematics somewhat…Thus although *colors may belong to physics, the science of them must nevertheless be considered mathematical, insofar as they are treated by mathematical reasoning.*”[[25]](#endnote-25)

It is hard to see the move from a “mathematical” science of colors to conditionally giving up the claim that differently refrangible rays have appropriate colors as an insignificant shift in position. It is also difficult to maintain that Newton’s position on demonstration by experiment did not show significant variation. Before Lucas’ attack on the *experimentum crucis* Newton still maintained that his experiment was demonstrative: “Now if this demonstration [of the *experimentum crucis*] be good, there needs no further examination of ye thing; if not good the fault of it is to be shewn, for ye only way to examin a demonstrated proposition is to examin ye demonstration.”[[26]](#endnote-26) In the 1717 English edition of the *Opticks*, however, Newton wrote the following: “And although the arguing from Experiments and Observations by Induction be no Demonstration of general Conclusions; yet it is the best way of arguing which the Nature of Things admits of, and may be looked upon as so much the stronger, by how much the Induction is more general.”[[27]](#endnote-27)

*III. Newton-bashing vs. Newton-mashing*

If one searches for coherence in these loci, the textual differences will be minimized in the process of reconstruction. Most of these will be relegated to the status of “bugs” or “loopholes”, especially as many of the quotations above have been suppressed and not published – because either Oldenburg or Newton did not see them fit. We emphasize instead the possibility of finding local cues that can be used *both* to rationalize *and* contextualize the specific utterances. In our cursory overview it was possible to link some of the commitments to the challenges raised in different phases of the debate. Such a reading is sensitive to the argumentative context of the individual letters. Instead of creating overarching coherence, it is tuned to detect subtle changes in the positions by exploiting locally the topical potential available.[[28]](#endnote-28)

Although we believe that there is a strong case to be made with respect to the specific controversy that Newton’s position changed substantially during the debate, our aim here was not to decide on a question of who is right and who is wrong. We sketched and contrasted two possible reconstructions in order to substantiate the claim that it *is* possible to provide both types of reconstructions.[[29]](#endnote-29) Any controversy offers itself to both readings, yielding at times significantly different appreciations of actors, and some of the substantial differences in the episode’s historiography can be accounted for by the differences in the analytical frameworks.

Reconstructions showing these inconsistencies are often scoffed at and portrayed as uncharitable – in the present case study, the phrase “Newton-bashing” has even been coined.[[30]](#endnote-30) Our present guide, Stein, notes that “Newton writes with a precision that exceeds that of most readers (which is, of course, not to say that he writes with the precision of a computer program); and that he has suffered from imprecise readers (who impute to him confusions based on their own misreadings).”[[31]](#endnote-31) Stein’s approach is applauded by many as a “refusal to let antecedent biases obscure the intention of an author,” and it has been considered exemplary, since “Howard makes a strong case that Hooke and Huygens, and incidentally some modern historians of optics, failed for doctrinal reasons to appreciate Newton’s compact but clear discriminations.”[[32]](#endnote-32)

Refusing “antecedent biases” in favour of charity to the “intention of an author” is typically a virtue associated with decontextualizing approaches. From this angle, contextual approaches, by not accentuating individual coherence, are prone to be seen as displaying antecedent bias, and not charitable as such. We strongly believe that this is not the most charitable reading of contextual approaches. Stein does not appear to follow his own principle of charity when evaluating Newton’s readers. This is even more problematic and lamentable in the case of contemporary readers.

The lack of charity towards Newton’s antagonists is clear if we consider the procedure Stein follows in his reconstruction. To reconstruct Newton’s position, he often makes use of the replies by Newton to challenges raised by antagonists in the controversy. Sometimes reconstructing a position is possible only when challenges were made and responses were formulated. To blame an antagonist for not comprehending the protagonist’s position begs the question. It is most natural to see the challenge of the antagonist as triggering the response, and the historian often needs this response in order to reconstruct the position. Once done, the antagonist can easily slip into the role of an “imprecise reader”. Exploring this process makes clear that something is potentially lost: this type of reconstruction is *not* aimed at doing justice to the role arguments and counter-arguments play, yet these elicit the very responses that are indispensable for the reconstruction. The contribution of contemporaries to the process of knowledge production is obscured by the focus on reconstructing a maximally coherent view of *any one* of the actors in a controversy.

In our view, however, this lack of charity affects not only the role and contribution of the antagonists, but even the understanding of the protagonist in question. Let us return to the controversy and highlight a point where we are in unison with Stein, who, arguing against a number of historians, states that “there is a rather convincing array of evidence that Newton held in principle to the revisability of conclusions based upon experiment – and therefore of all conclusions in physics – at the time of his first paper”,[[33]](#endnote-33) and that “Newton rejected any decisive *a priori* commitment to a special mode of scientific explanation.”[[34]](#endnote-34) In our reconstruction Newton does just this: refining and modifying his commitments, and in the aftermath of the debate even giving up his notion of the demonstrativity of experiments. In Stein’s reconstruction, however, revisability is held ‘in principle’, but seen much less in practice. Unless the changes in position are made overtly (rare for Newton in this period), decontextualising reconstructions are prone to overlook them. Therefore the aim of understanding Newton as maximally coherent results in not understanding him on certain counts. As Stein notes on “Newton’s repeated references to the *demonstrative* character of the *Experimentum Crucis”:*

That is a very unusual term for Newton to apply to an experiment (or to a series of experiments either): there may possibly be other instances, but I myself am not aware of any. Newton’s characteristic term for the evidentiary contribution of experiments that support a proposition is *proof—*a word he never, so far as I am aware, uses in connection with mathematical argumentation. For the latter he reserves the word “demonstration”; and, as I have said, I know of no other instance in which he uses that term for an experiment.[[35]](#endnote-35)

As our reconstruction shows, it is plausible to believe that criticism plays a *constitutive role* in shaping the commitments of an author, helping him to arrive at the precise formulation of concepts, the probative force of experiments, etc. Our reading can explain what appears as an enigma for Stein: the debates after Newton’s first publication could indicate that in order successfully to convince his peers of his discovery, he had to change his claims as to what his experiment proved and how strongly it proved his theory.[[36]](#endnote-36)

Being charitable to the whole controversy can also contribute to an understanding of Newton’s remarkable methodological development. We see that the late Newton was much more cautious with respect to the methodological norms he employed, as the weaker claims he made on reasoning based on experiments and observations show. In our view, being charitable to others in these controversies does not diminish his contributions to both science and methodology. We also believe that our analysis, focusing on the local argumentative context and not on an individual’s system of beliefs as closed under the principle of maximal coherence, *is* a charitable reconstruction that employs a notion of rationality. Such an analysis is inclined towards a picture of science as made up of individuals characterized by bounded rationality, not of exceedingly precise Newtons and imprecise readers (whether contemporaries or analysts), and it reconstructs the rational element of the development of science in a way that emphasizes the communal process of debating results.

The contextual approach outlined here occupies a middle ground between mainstream history and sociology of science, bracketing questions of rationality, and individual coherence-maximizing, rationality-centered approaches. It can satisfy those who believe that science is an epistemically privileged endeavor and that its epistemic content should not be neglected when reconstructing the scientists’ positions. It can also satisfy those who hold that it is naive to believe that the immediate context, e.g. the challenges to a theory, the expectations of the author about his audience, etc., does not affect the position a scientist takes. Although we have only shown its capacity to analyze a direct controversy, given that it is hard to think about any philosophical and scientific text as detached from an argumentative context, this approach has the potential to be a general guide for interpretation.

Bibliography

Arthur, Richard 1990. Foils for Newton: Comments on Howard Stein. In: *Philosophical Perspectives on Newtonian Science*, edited by P. Bricker and R. I. G. Hughes. Cambridge, Mass.: MIT Press, 49-56.

Bechtel, William 2006. *Discovering Cell Mechanisms: The Creation of Modern Cell Biology*. Cambridge: Cambridge University Press.

Bechtel, William 2008. *Mental Mechanisms: Philosophical Perspectives on Cognitive Neuroscience*. London: Routledge.

Buchwald, Jed, and Allan Franklin. 2005. Introduction: Beyond Disunity and Historicism. In *Wrong for the Right Reasons*, edited by J. Buchwald and A. Franklin. Dordrecht: Springer.

Craver, Carl F. 2007. *Explaining the Brain*. Oxford: Oxford University Press.

Dear, Peter. 1995. *Discipline & experience: the mathematical way in the scientific revolution*, *Science and its conceptual foundations*. Chicago: University of Chicago Press.

Ducheyne, Steffen. 2005. Newton's notion and practice of unification. *Studies in History and Philosophy of Science* 36 (1): 61-78.

Eemeren, Frans H. van, and Rob Grootendorst. 2004. *A systematic theory of argumentation*. Cambridge: Cambridge University Press.

Friesen, John. 2006. New Trends in Newtonian Scholarship: Benefits and Pitfalls. *Annals of Science* 63 (1): 111-17.

Kutrovátz, Gábor. 2008. Rhetoric of Science, Pragma-dialectics, and Science Studies. In *Controversy and Confrontation: Relating Controversy Analysis with Argumentation Theory*, edited by F. v. Eemeren and B. Garssen. Amsterdam: John Benjamins, 231-247.

Lohne, Johannes August. 1968. Experimentum crucis. *Notes and Records of the Royal Society* 23:169-199.

Malament, David B., ed. 2002. *Reading Natural Philosophy: Essays in the History and Philosophy of Science* Peru, Ill.: Open Court.

Newton, Isaac. 1671-72. New Theory about Light and Colors. *Philosophical Transactions* (80): 3075-3087.

———. 1952. *Opticks or a Treatise of the Reflections, Refractions, Inflections & Colours of Light*. London: Dover Publications.

———. 1984. *The optical papers of Isaac Newton*. Edited by A. E. Shapiro. Vol. I. Cambridge: Cambridge University Press.

Raftopoulos, Athanassios. 1999. Newton's Experimental Proofs as Eliminative Reasoning. *Erkenntnis* 50: 95-125.

Schaffer, Simon. 1989. Glass works: Newton's prisms and the uses of experiment. In *The uses of experiment: Studies in the natural sciences*, edited by D. P. Gooding, Trevor; Schaffer, Simon. Cambridge: Cambridge UP.

Shapiro, Alan E. 1980. The Evolving Structure of Newton's Theory of White Light and Colour. *ISIS* 71 (257): 211-235.

———. 1996. The Gradual Acceptance of Newton's Theory of Light and Color, 1672-1727. *Perspectives on Science* 4 (1): 59-140.

Stein, Howard. 1990. On Locke, "the Great Huygenius, and the Incomparable Mr. Newton". In *Philosophical Perspectives on Newtonian Science*, edited by P. Bricker and R. I. G. Hughes. Cambridge, Mass.: MIT Press.

———. On Metaphysics and Method in Newton. Manuscript [MMN]

———. Further Considerations on Newton’s Methods. Manuscript [FCNM]

Turnbull, H. W., ed. 1959. *The correspondence of Isaac Newton I. 1661-1675*. Cambridge: Cambridge University Press.

———, ed. 1960. *The correspondence of Isaac Newton II. 1676-1687*. Cambridge: Cambridge University Press.

Worrall, John. 2000. The Scope, Limits, and Distinctiveness of 'Deduction from the Phenomena': Some Lessons from Newton's 'Demonstrations' in Optics. *Brit. J. Phil. Sci.* 51 (1): 45-80.

Zemplén, G. Á. 2008. Scientific controversies and the pragma-dialectical model: Analysing a case study from the 1670s, the published part of the Newton-Lucas correspondence. In *Controversy and Confrontation: Relating Controversy Analysis with Argumentation Theory*, edited by F. v. Eemeren and B. Garssen. Amsterdam: John Benjamins, 249-273.

1. Buchwald and Franklin 2005 p. 1. [↑](#endnote-ref-1)
2. Friesen 2006 p. 111. [↑](#endnote-ref-2)
3. Stein, *Further Considerations on Newton’s Methods* (further FCNM) p. 44. With FCNM and MMN two manuscript papers by Howard Stein are designated. These have been widely circulated among scholars and cited in both articles and books. Although in a manuscript form, they have had an impact on the field and are publicly accessible:

   <http://www.strangebeautiful.com/other-texts/stein-further-consider-meth-newton.pdf>

   [http://www.strangebeautiful.com/other-texts/stein-metaphys-meth-newt3on.pdf](http://www.strangebeautiful.com/other-texts/stein-metaphys-meth-newton.pdf) [↑](#endnote-ref-3)
4. This is not just a concern for 17th-century natural philosophy. See the recent debates about mechanistic explanations in biology, both for the current practice of the discipline and for the understanding of its past (Bechtel 2006, 2008; Craver 2007). [↑](#endnote-ref-4)
5. FCNM p. 73. [↑](#endnote-ref-5)
6. FCNM p. 39. [↑](#endnote-ref-6)
7. Lohne 1968 p. 170. [↑](#endnote-ref-7)
8. Newton 1671-72 p. 3079. [↑](#endnote-ref-8)
9. Turnbull 1959 p. 187. Oldenburg also deleted the following passage from a later letter (21 September 1672): “To comply wth your intimation … I drew up a series of such Expts on designe to reduce ye Theory of colours to Propositions & prove each Proposition from one or more of those Expts by the assistance of common notions set down in the form of Definitions & Axioms in imitation of the Method by wch Mathematitians are wont to prove their doctrines” (Turnbull 1959 p. 237). [↑](#endnote-ref-9)
10. Turnbull 1959 p. 113. [↑](#endnote-ref-10)
11. Turnbull 1959 p. 187-188. [↑](#endnote-ref-11)
12. FCNM p. 6. [↑](#endnote-ref-12)
13. Turnbull 1960 p. 104. [↑](#endnote-ref-13)
14. This is a syllogism Barbara. It has to be noted that a correct reconstruction has the premises the other way round. The major term, the predicate of the conclusion, is predicated of the middle term. The middle term (unequal refraction) is predicated of the minor term, the subject of the conclusion. The middle term suffers in this reconstruction. Lucas calls the proposition predicating unequal refrangibility of unequal refraction the minor term, which makes that predicating unequal refraction of differently colored ray(s) the major term. But to conclude from these two premises that R is (predicated of) C, R has to be the major and C the minor term. To save Lucas’s distinction, we left the names as they were (or as they were transcribed from the manuscript). [↑](#endnote-ref-14)
15. Turnbull 1959 p. 187. [↑](#endnote-ref-15)
16. This inconsistency was stressed e.g. in Schaffer 1989 p. 86. Note that Stein claims that in the period in question, Newton’s position remained constant: “two distinct properties of the rays of light have been identified [by Newton], their refrangibility and their colorific disposition; the *Experimentum crucis* has shown light to be difform in point of the first of these; further experiments show that this is strictly parallel to a second difformity, with respect to the second.” (FCNM p. 42) And also: “note well: the doctrine of colors does not *follow from* the crucial experiment, but it does *rest upon* the result of the latter” (FCNM p. 4) This fits Newton’s own claim (Turnbull 1960 p. 256), yet our reading shows that it is justified to see his commitments as *wavering*. [↑](#endnote-ref-16)
17. In the earlier, unpublished draft (Add MS 3970, 437v) Newton clearly separates the issue of colour from that of refrangibility. As he starts the rather lengthy entry, “That ye experimentum crucis is such: hitherto I have considered the nature of colours shewing which are originall & wch compounded, & that whitenesse is the most compounded of all others. The principall thing wch now remaineth to be examined is the force of my experiment wth the perforated [wedge] boards”. So clearly the question of colours is strictly separated from the experimentum crucis, as it also is in the following whole folio sheet disambiguating the experiment from a number of others. Discussing the irregularities he writes (438r) “And amongst other irregularities if the first Prism had spread & dissipated every ray into an indefinite number of diverging parts, the second should in like manner have spread & dissipated every one of those parts into a further indefinite number, whereby the image would have been still more dilated; contrary to ye event. And this ought to have been so, because those linear diverging parts depend not on one another for the manner of their refraction, but are every one of them as truly & completely rays, as the whole was before its incidence; as may appeare by intercepting them severally.” This detailed discussion without any reference to colour makes it even more surprising that he made the much bolder claim about colour and refrangibility in the published letter, but the possibility that it was a “slip of the tongue” is meager. [↑](#endnote-ref-17)
18. FCNM p. 19. [↑](#endnote-ref-18)
19. Stein himself states this: “There are three points of contrast between Newton and Locke that I want here to call attention to (with two more to come later): First, the qualities Locke calls *primary* and *original* constitute a fixed list; it is clear that Newton's usage is more flexible, since the qualities he attributes to rays of light as primary and original are ones that have been discovered by his experimental investigation itself (and therefore it is reasonable to suppose that further investigations may discover new ‘primary qualities’).” Stein 1990 p. 29. For us it is enigmatic to claim that attributing a new primary qualities to light can be done via a non-universal statement. [↑](#endnote-ref-19)
20. Turnbull 1960 p. 256. [↑](#endnote-ref-20)
21. FCNM p. 7. [↑](#endnote-ref-21)
22. See e.g. the usage close to eliminative resoning: “You know the proper Method for inquiring after the properties of things is to deduce them from Experiments. And I told you that the Theory wch I propounded was evinced to me, *not by inferring tis thus because not otherwise,* that is not by deducing it onely from a confutation of contrary suppositions, but *by deriving it from Experiments concluding positively & directly.*” (Turnbull 1959: 9). It is reasonable to suggest that Newton at this time believed that the inference was as strong as a logical / mathematical one. See also (Dear 1995, esp. Ch 8), and the influence of Barrow on the early Newton. [↑](#endnote-ref-22)
23. To provide a further example, by contrasting his answers to Lucas with the previously cited one to Hooke, we see that to claim that rays have different *refrangibility* (i.e. it is an internal property of the rays) Newton had to rely on a premise *not* derived from experiments, as the letter to Hooke would suggest, but on a methodological norm. Stating that “nothing can be more absurd” than “to tell me yt an effect may be varied by unvaried causes” (Turnbull 1960 p. 256) is at best a rule about inference-making, but it is not derivable from the experiments Newton describes. [↑](#endnote-ref-23)
24. Turnbull 1960 pp. 257-8. [↑](#endnote-ref-24)
25. Newton 1984 p. 87. [↑](#endnote-ref-25)
26. Turnbull 1960 p. 80. [↑](#endnote-ref-26)
27. Newton 1952 p. 404. We do not trace or discus the development here (with Turnbull 1959, p. 164, p. 209 as key early appearances of the view last presented). [↑](#endnote-ref-27)
28. The approach we use in the paper shows strong parallels with the pragma-dialectical model, which investigates “exactly which obligations are created by (explicitly or implicitly) performing certain speech acts in a specific context of an argumentative discourse or text.” (Eemeren and Grootendorst 2004 p. 54). For the applicability of the model in history of science and science studies see Kutrovátz 2008. [↑](#endnote-ref-28)
29. For this reason we tried to keep to a minimum references to the debate in historiography, exemplified by papers like (Shapiro 1980, 1996; Schaffer 1989), or the debate on the methodology including works like (Worrall 2000; Ducheyne 2005; Raftopoulos 1999). [↑](#endnote-ref-29)
30. Arthur 1990 p. 55. [↑](#endnote-ref-30)
31. FCNM p. 40. [↑](#endnote-ref-31)
32. Malament 2002 p. 4. [↑](#endnote-ref-32)
33. FCNM p. 8. [↑](#endnote-ref-33)
34. FCNM p. 37. [↑](#endnote-ref-34)
35. FCNM p. 8. [↑](#endnote-ref-35)
36. To Stein, Lucas’ objections are just impertinent (FCNM p. 18). As such, suppressing much of the correspondence with Lucas is also explainable in this way. On the interesting differences concerning the appreciations of Lucas, see Zemplén 2008. [↑](#endnote-ref-36)