Review of: "Objectivity and Honesty in Science: The case of Light Interference Phenomena"

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Potential competing interests: No potential competing interests to declare.

General comment:

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In order to facilitate a proper interpretation of my review, note that I was trained a physicist and now became a lecturer in history and philosophy of sciences in Aix-Marseille Université. And that I am currently working on a commented translation of Thomas Young's papers published in the philosophical transactions of the Royal Society from 1800 to 1804.

This being said, I obviously was very excited by the title of the article at stake. As not only the question of objectivity, honesty, and actually all human factors are an obvious factor of the development and dissemination of science. But also because I consider myself as relatively well aware of Thomas Young's works, but had never investigated this aspect of the character. Such that I was very eager to collect new information on him, and also very persuaded that his case would be a very relevant one for addressing the problems of "honesty" and "objectivity" in science. For I knew that already his early published papers - that I am working on - are filled with quotations and endorsements of other people's theories. But also with public withdrawals of some his own early declarations, with apologies to the audience, even when these withdrawals were not necessarily justified (see the long controverse with Hunter and Home on the accommodation of the eye (Young 1794, 1796, 1800); the modification of his primary colours on the basis of Wollaston writings (Young 1802); or the withdrawal of his own ether distribution hypothesis (Young 1804)).

Adding to these considerations the considerable amount of public and private controversies Young got involved into (such as those with Brougham and Fresnel mentioned by the author, or others of the theory of sound with Smith, Robinson and Gough) certainly made an ideal character for the illustration of the problem of scientific 'objectivity' and 'honesty' that was at stake. And I was sure that plenty of material was at hand to the author here.

And indeed, the author collected material; mainly from letters that were compiled in Young's and Fresnel's collected works (Young 1855, Fresnel 1866) and within their respective biographies. Yet, the major problem of this text, according to me, is that erudition and accumulation of details do not produce meaning by simple juxtaposition. For it is precisely the author's job to bind together and give meaning to historical, philosophical, sociological and scientific material; such that it even seems to me that this is precisely what the expertise of the historian of science is all about.

Therefore, unfortunately, and with all due respect to the author, despite the proliferation of historical information - some of

it incorrect by the way, or poorly targeted - and despite the interest of the question posed and the potential richness of the example of the Young-Fresnel case used to illustrate it, one can't help but feel a sense of awkwardness and incompleteness when reading this text.

Note that the field of the investigation, that is apparently situated at the border between psychology and sociology of sciences, is very exciting. But that it is also very slippery and demanding. Such that it certainly requires a very rigorous methodology in order to curb some natural enthusiasm and excitation that could lead one to some caricatural analysis of the supposed “psychology” and “social interactions” of the characters at stake. For there is a very high risk to interpret their writings and behaviors in such a biased way that we would only project our own psychological traits and social experiences onto them. And my impression is that such methodology is lacking here.

Now on a more formal level, my feeling is that the historico-scientific context of the text was excessively developed – and, paradoxically, quite clumsily too, for want of space. That contextualization seems all the more excessive than a considerable number of authors have presented this context (Young’s or Fresnel’s life, the content and arguments of projectiles and vibrations theories at the beginning of the 19th century…) much more rigorously and more clearly before the author. Such that one cannot help feeling that the author would better reduce the very long introductive part, and focus on the heart of her investigation: that is the question of the role of honesty and objectivity in the debate and acceptance of scientific ideas. A question which at least seemed central to me as I went through the title and abstract of the text. But which ends up being underexploited.

This for three reasons I believe:

-First, and quite evidently, because these two concepts (objectivity and honesty), certainly central to the investigation, are never frontally questioned and not even defined.

-Second, I suspect, because as noted by a former reviewer, the text “bears traces of haste, especially towards the end”. A comment that the author herself justified by the fact that she probably “did not take enough care to write the end of the paper. May be lassitude. The paper seemed too long and I wondered whether it would really be interesting.” My bet is then that a drastic reduction of the introductory part would not only reduce the feeling of lassitude and excessive length expressed by the author herself. But would also offer an occasion to develop much deeper the potentially interesting matters that are announced by her abstract and still lay hidden in the abridged end of the present text.

-Third, and finally, because the present text seems to be based on the naïve assumption that a mere juxtaposition of “facts”, without a leading issue, and without proper analysis, would by itself turn into an elaborate reflection on the history of science. Such that the very form of the article conveys the erroneous idea, according to me, that the investigation started indeed with the first quotations offered by the authors (page 6), and ends with the last ones offered just before conclusion (page 20). Unfortunately, this does not stand, I think. Neither in academic, nor in any kind of investigation. As long as a precise and rigorous issue (‘problématique’) has not been set and justified, and as long as no precise solution to this issue has been anticipated and explicited, that will serve as the main lead for the investigation, the investigation progresses senseless: that is both without direction and without meaning. And the incredibly long list of questions that the

authors suggests will be addressed by her text (page 5, lines 6-10) constitutes by no means that precise and rigorous issue which should be the bone structure of a proper research article.

That is to say that, to my opinion, the present text would require a deep and thorough restructuration in order to reach some of the goals it pretends to aim at in its abstract.

I hope that — since I'm researching on Thomas Young myself – I won't be suspected of “dishonesty”, or “lack of objectivity” in my analysis of Carole Nahum's article. All the more so as I do not know the author, that the community of the history of optics is so narrow that I am glad to find someone sharing my own concerns, and that I was delighted by the perspective opened by her article, as I said. And it is indeed as a proof of good will, and of a sincere desire not to condemn the substantial work that has been produced here, but to open a debate as constructive as possible for the future of this text that I will now deliver a series of more local comments, located by the page and line number of the passage concerned in version 1 of the article. Therefore, please believe that my comments have absolutely no intention to hurt or upset the author. But is just the truthful, though obviously subjective, result of my analysis of this text.

Local comments:

Abstract.

p. 1. it seems to me that the author's starting point can simply be summed up by the plain statement that scientists are human. As plain as it is, that is a fact that is deserved to be stated, and which consequences deserve to be analyzed. Yet in that second paragraph of the abstract (Lines 4-8.), this statement seems exaggeratedly biased by the emphasis on the negative qualities of human beings exclusively. As if they alone deserved to be considered as playing a role in science. And as if they could be considered as a coherent category, without any discernment between the long list of human flaws accumulated by the author. I think this is quite a biased start, which already announces the urgency for the author to focus her investigation and stick to some concepts only (or psychological qualities in that case) whose role she could hope to analyze in science, rather than mixing all up.

Introduction

p. 2. footnote 4 should refer to "attacks" on Young rather than to his own articles. Cite Brougham's articles in the Edinburgh review (footnotes 44-46), or a critical analysis of these texts (e.g. Geoffrey Cantor, *Henry Brougham and the Scottish methodological tradition*, 1971)

p. 2 line 7. Assertion “these particles can travel because of attractive and repulsive forces one exerts on to the others’’ just seems confuse or mistaken to me. Instead, one could write : "These particles can travel freely in right lines and at constant speed, because of inertia. And their trajectory is sometimes bent because of attractive and repulsive forces that the corpuscules of material bodies exert on these particles.”

p. 2. footnote 5 should not refer to the Royal Academy of science, unless it is moved earlier in the sentence (after “at this time”). If maintained here it should refer to Fresnel's memoir or to any early letter from Arago to Young.

p.2. footnote 7. I don't see the point of the first part of footnote 7. A reference to the 'programme de l'enseignement...' only would seem more than enough and more relevant to me.

p. 2-4 : Figure 1. I am not sure Figure 1 is of any use for the improvement of the text.

p. 4: footnote 11: why not citing the primary source? i.e. John Herschel himself.

p. 4. Last line, just before Figure 2. One should write “experimentum crucis’’. Plus, from such vague sentence, I don't see what experiment the author is talking about and although my job is precisely to know about such experiments. Which is just an examle that indicates that the text is often far too implicit for the general audience it is aiming at. Furthermore in that case, if it is Foucault's comparison of velocities of light in water and air that is at stake, Foucault’s own text should be cited directly and explicitly instead of those secondary sources (Foucault, *Thèse de physique. Sur les vitesses relatives de la lumière dans l’air et dans l’eau, présentée à la Faculté des sciences de Paris, pour obtenir le grade de docteur ès-sciences physiques*, Paris, Bachelier, 1853.)

And, most of all, the experiment should be dated correctly 1850 (and not 1862!)

p. 5, line 1 : “their works” the author certainly means “the compilations of their respective works”. As “their works” were obviously published long before.

p. 5. footonote 14 should include the reference to both miscellanea of Young's and Fresnel's works (which are presently cited for one in footnote 14 and for the second in the first part of footnote 15)

p.5 line 2; “but this is another story” the sentence is so elusive that it does not make any sense. And footnote 15 does not clarify the implicit idea in the author's mind at all. I suggest the author be much more explicit in her statements.

p. 5, line 6: “correspondence fructuous”. The order of those two words should probably be reversed, if I undersatnd the sentence properly.

p. 5. Lines 3-11. I am afraid that a long list of questions never meets the requirements of a precisely established issue (“problématique”), which is necessary to any serious investigation. My feeling is that the author should take the time to focus and sharpen here the precise problem she really intends to tackle before proceeding any further.

Scientific context

(As mentioned earlier, this context is both incomplete, partial and too long. I suggest the author relies on more references to the very good books that were written on that context and moves on faster to the core of her investigation.)

p.5. I do not see the use of footnote 16.

p. 5, lines 5 and 4 from the bottom. “forces, expressed in terms of the square of their distances”. I don’t think this

statement is useful. And I think it is not true; as I far as I know, from one author to the next a whole set of distance dependences were proposed. For instance, Newton's force for refraction is a constant (Principia) and Brewster's law for inflexion is as 1/r.

p. 5, line 4 from the bottom. “cause of their movement”. I insist that the forces studied in the projectiles theories of that time are not the cause of the movement of projectiles (once they are ejected, they move inertially) but the cause of the modification of that movement. In those theories, light particles move because of inertia. And they are refracted, reflected... because of forces.

p. 5. footnote 17 is not relevant since the quoted book (Sabra), though it is excellent, stops with the study case of Newton and the author is referring to what happened after him. To that respect the author should most certainly read an cite Geoffrey Cantor, 'Optics after Newton', 1983. And I do actually think that the reading of this book and of other writings of this ingenious historian would provide the author with very much relevant information that might temper some assertations of that text.

p. 5, last line : “still remained unexplained: inflexion of light also called diffraction, double refraction and colours of thin plates”. I would not acknowledge this statement, as Newton gave a very eloquent emissionist interpretation of all these phenomena in his Opticks.

p. 5, last line : “and even why did light..”. I don’t think the sentence is grammatically correct. Try : "as well as the reason why..."

p.6, lines 4-6. ‘’But as sound propagates through the air, as water waves”. I think the reference to 'water waves' is not useful here and rather confusing. Since one of the main confusions to avoid here – I think – is that sound waves are longitudinal, water waves transverse, and light waves at that time were always thought to be longitudinal, and not transverse as we think is obvious today. Therefore, I think that for pedagogical reason, the author should stick to the analogy with sound, that was indeed the most important one in Young’s mind.

p.6. footnote 24 : I don't think the comment of Chappert's position is relevant and I beleive that the citation of his work (as well as that of Sabra's) would be enough here.

p. 6. footnote 25 : A primary reference to Euler's work (Nova theoria lucis and colorum, 1746) or to Hakfoort's monography dedicated to "Optics in the age of Euler", 1995, would seem pretty relevant here.

p. 6. Lines 13-15. ‘’Despite these arguments against the emission theory, the wave system was not even thought of at the beginning of the nineteenth century. Thomas Young appeared to be the first physicist to have raised once again the controversy after he had made his experiments.’’

This sentence should probably be tempered by a reference to the short but not empty list of vibrationnists mentionned by Cantor in by "Optics after Newton". And by the remark that – as far as I know - no historic analysis leads to the conclusion that Young came to the vibration hypothesis through experiments. Not optical ones at least. Such that there are even serious doubts that he even made the ones he is so famous for (and that he proposed long after the first developments of

his theory). On that matter, see for instanceWorrall, “Thomas Young and the ‘refutation’ of Newtonian optics: a case-study in the interaction of philosophy of science and history of science’’, 1976.Or Kipnis “history of the principle of interference of light”, 1991.

Therefore I think this long statement is a hasty declaration that stands on nothing but an excessively empirist idea of the logic of scientific discovery.

p. 6. Second half of the page. From Hauy’s to Arago’s opinion included. It is not clear why those specific authors' opinions on physics is notified. And why not that of Young and Fresnel's only. In any case, the mere juxtaposition of these truncated opinions lacks sense, I believe. And they should be synthesized and commented in order to explicit the role that they may play in the following parts of the article. Or simply removed if it happens that they will not play a significant role later.

Thomas Young’s reputation in Great Britain.

(Note that this is the part my own research is more or less focused on)

p.7 lines 1-2: I fear that the author underestimated the fact that these questions were famously addressed quite many times before (see Cantor, Kipnis op. cit., just as two examples). As such it is regrettable that those previous analyses, which actually may present some common arguments with the present ones - and some very different ones too - are not cited.

p. 7, line 9. “He also invented the ‘Eriometer’ for measuring the average size of small particles” should be probably be turned into : “He also invented the ‘Eriometer’ for measuring the average diameter of wool and silk fibres; and was incidentally applied to the estimation of the average size of small particles” for sake of exactitude.

p. 7, line 14. “second paper”. I guess the author wanted to mean “FIRST paper”.

p.7. line 8 from bottom: “In his second paper, he explained that color perception requires only the ability to discern between blue, green, and red.’’

I am formal that this theory is presented in his THIRD and FOURTH papers published in the Journal of the Royal Society. Besides that, Young wrote a whole lot of papers in other journals at the time; which makes the author's statement not only inexact, but also vague.

On top that, the series of three primary colours listed here was never the one defended by Young, neither in november 1801 (red, yellow, blue), nor in july 1802 (red, blue, purple), nor again in 1807 and 1817. Such that I fear that the “red, blue, green” suggested by the author is a pure retrospective projection of her own modern interpretation of that question.

p. 7. Line 5 from bottom. ‘’Young became professor of Natural Philosophy at Cambridge University between 1796 and 1799’’.

This is wrong. Young was a student in medicine at Gottingen in 1796 and still a student in 1799 at Cambridge, as the

author will actually concede later on.

p.7, footnote 33 : the reference to the compilation of Young's lectures at the RI is mistaken and should be *A Course of Lectures in Natural Philosophy and Mechanical Arts*, vol. I et II, London, Savage, 1807.

p. 8, footnote 36: for sake of brievity I suggest limiting footnote 36 to the reference to Hecht books, and to the first sentence only.

p.8 first line below fig.3. “Between 1801 and 1804, he published three papers related to sound, fluid and light in the Philosophical Transactions’’

This sentence clearly lacks precision. First, what does "related to fluid" mean?

And second Why starting 1801? If one wants to be exact, the article onsound is published in 1800. An article on the accommodation of the eye published in 1801. And then three other articles on light are published in 1802 and one in 1804.

p. 8 footnote 37 should obviously refer to Young's paper on sound. And footnote 38 is probably useless

p. 8. 4th line below fig 3. ‘’He was intrigued by the different colours the bubbles of soap’’

Here, I would be very interested in finding a reference proving that Young's interest was triggered by the colours of soap bubbles, For I don't know any. Instead it is usually said that the analogy came to him through the consideration of the different tones produced by organ pipes (see Young 1800 p.128 for instance)

p.9, lines 3-4. As confirmed by the author's footnote 43, Brougham’s political career arose many years after he attacked Young's theory. Such that "at this time " (line 3) is pretty confusing and may bias the reader’s judgement.

p. 10, lines 1-4. As mentioned earlier, the Brougham-Young exchange was studied many times. For instance, Cantor wrote a lengthy and beautiful article on that topic (uncited here) that is clearly giving much more historical and epistemological insight on that question than the author does here. (read ‘’Henry Brougham and the Scottish methodological tradition’’, 1971).

p. 10, lines 2 from bottom. this statement is correct and contradicts the earlier claim that Young was a professor at Cambridge

p.12; line 7. Write ‘’A fortiori’’ with a ‘t’.

p.12, line 12 from bottom. I don’t know any ‘Snalus’. Although Willebrord "Snell", sometimes called ‘’Snellius’’ gave his name to the law of refraction.

p. 13. Line 14 from bottom. “Snalus” again.

p. 13. Lines 1-6 from bottom. This whole paragraph sums up one flaw, I think, of this article.

I actually do believe that matters of “objectivity”, intellectual “honesty”, “supremacy” and “pre-eminence” are of the most importance in the building and diffusion of scientific knowledge. But what the author develops i this paragraph is – according to me - her mere opinion. And it absolutely fails to convince me because, although I probably share her opinion, her dissertation is nothing like a philosophical, historical or sociological argumentation to me. Indeed, it is unfortunately not enough to be right: One has to defend his right opinion with due arguments - whether one is a scientist (like the characters of the article) or a historian of science (as the author assumes to be).

Therefore I am asking: Would the author have the least historical evidence to support that claim developed through this whole last paragraph of the section, for instance? And if the answer is yes, then I think she should urgently show it.

Malus price

Young’s comments about Laplace’s works

(nothing much to say about these two sections that mostly juxtapose extracts of letters as factual examples that now need to be synthesized into a proper philosophical of historical argument)

p. 15, line 11 from bottom. “Young clearly preferred geometrical methods with sketches rather than algebra and calculations”

That is absolutely right! Cite for instance is “essay on cycloidal curves” which very explicit on that point.

p.16. conclusive paragraph of this section. I fear that the previous juxtaposition of chosen extracts of letters from Young lacks any analysis and most of all, lacks the point of view of the French authors this section is pretending to talk about. Obviously, one might ask wether Young's single opinion on what he thinks of others, and what he believes the others think of him gives a truthful image of what they actually do think.

Augustin fresnel’s opportunity to study optics

(As I am not an expert of Fresnel’s life, I won’t comment this section)

The relationships between Young and Fresnel

p. 17, lines 6-7. “as if he wanted to act alone”.

This is just an example of the many psychological assertions of this article. Indeed psychology of science and of sciences can be a very interesting and exciting, though extremely tricky, discipline. But here the psychological statements seem all to arise more from the mere psychological intuitions of the author. Which I would not dare minimize. But psychology is not only empathy and intuition. It is an academic discipline based on concepts and methods. Therefore, as the author invokes a lot of psychological arguments I believe it would be good to turn to the basics of the methodology of Psychology, in order to sharpen a future and more grounded version of those arguments.

p. 17, line 9. “delate” should be “delete”

p. 19, lines 7-4 from bottom. these conclusions seem quite hasty to me and weakly founded. All the more than these important questions have been investigated with very deep attention by a large set of historians of science

Arago's thought

(this section is certainly too brief. But is it really useful?)

p. 20, line 5. "around 1830".

Cite ARAGO, F., « Système d'Expériences à l'aide duquel la théorie de l'Émission et celle des Ondes seront soumises à des épreuves décisives », Comptes Rendus des Séances de l'Académie des Sciences 7, Paris : Bachelier, 1838, p. 954-965.

p. 20. footnote 106 is elusive and not useful I think.

Conclusion.

According to me, the conclusion is mainly a fictionnal narrative that is disguised as a historical one. For if a whole set of examples were given before, no argumentation was built to coherently sustain such conclusion.

p. 20, line 1 of the section "I intended to demonstrate".

I would ay that this text is more "illustrative" that "demonstrative" for now. But I hope the author will find the way to make it demonstrative.

p. 20, lines 2-4 of the section.

This statement on Young is absolutely unclear to me. For a thorough and historical case study of the supposed evidence of Young's experiment, one may see John Worrall, 1976 (op. cit.)

p. 20, line 8 of the section. "this broke his career".

Some very serious authors would temper this statement (see Cantor, 1983 and Kipnis, 1991 for instance)

p. 20. Line 9 of section. "might have been a revenge".

This opinion was discarded a long time ago. I do not think the author serves her argument properly as she neglects the opposite or complementary arguments that have been developed on some of the cases she studies.

p. 20. Line 9-10. ''did Young break Brougham's scientific career with his critics and his lack of diplomacy''

Although Young's lack of diplomacy is undeniable, this is a question that was not treated in the article, I think. And as I know no clear evidence on that point, besides the author's intuition, I suggest that it should be removed or connected to some reference.

p. 21, line 3; "thirty years later". It happened in 1850 exactly (see Foucault, 1853, op. cit.)