Measuring Speed of Light: Why ? Speed of what?

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ABSTRACT. This paper is based on our attempt to repeat, with undergraduate students, the 1850 and 1862 speed–of–light measurements by Foucault with the rotating mirror technique. "Good questions" arise naturally and lead to a deep discussion about the motivations for specific measurements in Science and a critique of the so–called—"crucial" experiments which are supposed to decide between opposing theories, for example, the wave-particle dilemma from the XVIIth to XIXth centuries, and the new role of the speed of light after 1862. A discussion follows, related to pedagogical, experimental, theoretical and epistemological points of view.

Introduction

During the year 1838, Arago claimed:

> It (the experimental setting) will decide *mathematically* (I use deliberately this expression), it will decide, once and for all, one of the most important and most debated questions of the natural Philosophy . (Arago, 1838)

Which such important question?

> Later, he declared:

> By repeating those observations with more mechanically perfect instruments, it will become possible one day, without leaving Paris and its neighbourhood, to get this *solar parallax* which, around the middle of last century, gave rise to such long, such distant, such difficult journeys and to so many expenses. (Arago, 1857)

Why the "solar parallax"?

Arago's first statement led to the *qualitative* comparison of the speed-of-light in air and in water by Léon Foucault in 1850 (Foucault, 1850) — not an accurate measurement of it, which was not a major concern at the time. A "crucial" experiment rejecting *definitively* the "emission" or "particle" theory in favour of the wave model as it was believed at the time? However, particles will come back fifty years later with the photon! Speed of light, speed of *what*? And *why*? What was "crucial"? What was wrong?

The second statement is related to the 1862-*accurate* measurement in air, again by Foucault and with the same — however improved — rotating mirror device (Foucault, 1862). *Why now*? The concern is new, issued from an explicit demand from Astronomy in view to determine more accurately the astronomical unit.

The starting point of the present discussion lies on our trial to repeat with undergraduate students — in connection with History of Science teaching — the 1850 and 1862 speed-of-light measurements by Foucault with the rotating mirror technique: "good questions" arise then naturally and lead to a deep discussion about "particles" and "wave" models of light from XVIIth to mid-XIXth centuries, about the motivations for specific measurements in science, and to a criticism of the — as-said — "crucial" experiments supposed to *decide* between opposite theories. The contrasted functions of the *measurement* will emerge from the comparison of both experiments. Pedagogical, experimental, theoretical and epistemological points of views will take place in the debate.

The 1850 Speed of Light Experiment. Waves or Particles?

In 1850, which was this question, among the "most important and most debated questions of the natural Philosophy"? Let us quote Foucault himself in his report: "Is the upper image less displaced than the lower one? Does it appear on its left? *Light is a body.* Does the contrary take place? Does it appear on the right? *Light is a wave*". And further, his conclusion: "The final conclusion of this work consists in declaring that the emission system is *incompatible* with the real nature of facts" (Foucault, 1850).

Thus, the left- or right position of an image was supposed to *decide* definitively — a "crucial" experiment — between two types of light theories: the wave model, and the "particle" or, as said at the time, the "emission" model: light is a wave *or* light is a body. Was it fully reasonable? We would stress nowadays the fact that the experiment could, at best, falsify one of the models; besides, it is just what Foucault seems to tell us in his conclusion (last quote, above), a conclusion more restrictive than the previous statement. It is very easy *nowadays* to observe that the final conclusion of 1850 did not prevent a come back of the "particles" fifty years later with the photon. But the photon — a *quantum* particle within a dualistic model — has got nothing to do with the classical particles and it is completely unthinkable at that time. Let us then examine the things in the conceptual framework of 1850: a classical "particle" or "emission" model following the principles of Newtonian Mechanics; and a wave model, a classical one too — i.e. with a "light medium", a "luminiferous ether" supporting and propagating the light vibrations — a wave model which was meeting with a string of considerable successes from the beginning of the century.

FALSIFYING THE "EMISSION" MODEL OF LIGHT

The emission model in use at that time was, in fact, affected by a "superimposed hypothesis" which does not belong necessarily to any "particle model" of light: it was indeed believed, by an analogy with reflection, and following the right thread of Descartes, that the tangential component of the velocity at the air-water separation (i.e. parallel to the water surface) should be conserved when penetrating into water through the refraction process. A logical consequence of the refraction law is then that the light velocity should be *larger* in water than in air. Indeed, the conservation of the tangential component *and* a refraction angle smaller than the incident one *imply* mathematically a larger velocity in water (a simple matter of elementary geometry in a right-angled triangle).

Two supplementary arguments, also lying on analogies, came in addition: for Descartes — close to the future wave model —, similarly to the fact that the sound velocity is larger in dense materials than in air, the propagation of the "luminous phenomenon" through "a tendency to motion" without any transport of matter, should take place "more easily" in a dense transparent medium than in air. For Newton, the "light particles" were supposed to undergo some gravitational attraction from the water in the direction normal to the surface of separation while, in agreement with Descartes, there is no reason for such an attraction in the parallel direction: no force, consequently no modification of the *velocity* in this direction. An irony of History will be that the argument of Descartes, which had been conceived in a pre-wave context, will be ruined by the wave theory of Huyghens half a century later (Huyghens, 1690): the wave model, with a propagation through wave fronts, *and* a refraction angle smaller than the incident one *imply* mathematically a smaller velocity in water than in air. Irony again in the fact that this argument, whose Descartes in the father, will go on in use, implicitly or explicitly, in the emission context and in Newton's filiation until the middle of the XIXth century.

In 1850, Foucault performs the experiment: the velocity of light is *smaller* in water. The "emission" model is rejected. In fact, *logically* only the "superimposed hypothesis" should be rejected: the sole "crucial" result in this experiment — viewed with our modern eyes — is that no theory, whatever its nature, will never be allowed to claim that the propagation of light is faster in water than in air! Couldn't people see it at that time? Interpreting the experimental result in terms of particles that would *slow down* when passing through the "piece of cloth" representing the water surface (according to Descartes's picture) was in no way forbidden. However, nobody tells this, nobody does. The lack of any theoretical model for a particle following such a behaviour (which will be obeyed, later, by the photon), and the strength acquired at that time by the wave model, will prevent from any other considerations: Foucault's result is *de facto* taken as a supplementary support for the wave model. And, what a support! *apparently* entirely lying upon an experiment.

THE DIFFICULTIES OF THE WAVE MODEL OF LIGHT

However, the wave model was not without suffering difficulties: the lack of detection of any motion through the "ether" in the first-order optical experiments during the first half of the century was *already* an irremediable defect. But Fresnel's empirical model — two ethers — and his famous "partial driving" formula relative to moving fluids, accounted miraculously for the facts. This formula was incomprehensible and auto-contradictory (Hoffmann, 1985, chap. 4), but it was also a stroke of genius. It will be experimentally verified by Fizeau and it will appear later in first-order agreement with special Relativity. Thus, at that time, there are "working things", though not understood, and people go on postponing to future times a full understanding, without questioning at all the successful wave model.

Besides, we would stress here another defect of the wave model which, *logically*, might have been noticed at the time of our discussion, i.e. mid- XIXth century. Let us consider the apparent variations of the period of Jupiter's satellite Io, listed by Roemer to prove the finite velocity of light (Roemer, 1676), and which gave rise to one of the astronomical methods to determine the velocity of light. It has been recently emphasized that the "Roemer effect" is related to the Doppler-Fizeau effect, in some way a *generalized* Doppler-Fizeau effect (Shae, 1998; Bailhache, 2002; Eisenstaedt, 2002, p. 31). Imagine you are in motion relatively to a time-periodic object — for instance a modern revolving light on a vehicle — then you measure a *modified* period of revolution of this light (and this has nothing to do with the frequency of the emitted light itself). This is Roemer's discovery, with Io in place of our revolving light. The difference with the strict Dopler-Fizeau effect — as discovered in the middle of the XIXth century — is that, in the Roemer or revolving light cases, the observed time-periodic object and the "vehicle" used for transmission of information (the light) are distinct, while in the standard Dopler-Fizeau effect (with either sound or light), the observed periodic phenomenon is the time-periodicity of the transmitted wave *itself*. However, it is sufficient, in order to observe a modified period — a "Roemer effect" — that the information be transmitted at a finite velocity, *whatever the vehicle*, either waves or particles. Now, our point is that, strictly speaking, the analysis is *different* according to the chosen model.

In the case of a classical wave model (with an ether), the analysis is exactly the same as for the sound Doppler Effect (Bailhache, 2002): the observed deviation is depending not only on the velocity of light c, but moreover on the respective velocities of the observer and of Io, v_{obs} and v_{source}, *relatively to the ether*. For instance, the analysis in order to yield the c value is different according to either Jupiter, or the Earth, or the Sun, is supposed fixed in ether. You have to know every velocity! Now, at the time of Foucault's experiment, from the evidence of the stars proper motion and the failure of the first-order experiments to detect the Earth motion in ether, nobody no longer knows which is our speed relatively to the ether, nor

"where" the ether stands. Of course, the differences are second order effects, thus small. But this is a matter of principle: there is a shocking asymmetry in the formula and in the results, depending on either the source, or the observer is supposed at rest. You *need* explicitly the velocities relatively to the ether. Strictly speaking, in the middle of the XIXth century, the Roemer effect has become non-interpretable within the wave model and *the whole set of concepts and tools that would allow to realize this is available*.

On the contrary, the *emission model* does not suppose any intermediate medium. Thus, any asymmetry between the source and the observer does not appear: let the Earth be at rest, or Jupiter, or the stars, let any "absolute" system of reference *exist or not*, the result for the velocity of light will be strictly the same. From that point of view, *the emission model appears much more satisfactory and in agreement with the principle of Relativity*. Moreover, remember Roemer himself explaining how the cumulated time-delays over six months yield the time required by the light to cross the Earth's orbit. This is his sole result, since he does not conclude to an explicit value for the speed of light. Now, it is easily seen that this important result is fully valid within the emission model; on the contrary, it remains correct within the classical wave model *only* if Jupiter is supposed at rest relatively to the ether. Roemer, of course, does not consider the problem: he is not supposed in 1676 to refer to a wave theory which has not yet been formulated. But wasn't Huygens himself — the "father" of the wave theory of light published in 1690 — implicitly reasoning within an emission model when he deduced a *c*-value from Roemer's result ? Indeed, he only combined Roemer's result (i.e., the admitted 22 mn to cross the orbit) with the Earth-Sun distance estimated in 1672 by Cassini and Richer, from which the speed of light (Huygens, 1690-2000, p. 57). A similar remark can be made about the measurement by Bradley using the stellar aberration (Bradley, 1728) which was easily interpreted within the emission model, while the wave interpretation will suffer increasing difficulties along the XIXth century due to the apparent "driving" of the ether by moving bodies. In both cases, only the special theory of Relativity will completely solve the problem.

Thus, in 1850, the wave theory of light was not without raising strong fundamental difficulties. On another hand, the emission model, though unable to account for diffraction and interference phenomena, was nevertheless much more in agreement with the Galilean principle of Relativity. At the time, Optics is not yet identified with Electromagnetism; however, the conflict between Mechanics and Electromagnetism, which will become sharper and sharper during the second half of the century, is already potentially present as a conflict between wave Optics and Mechanics: Galilean invariance versus non-additivity of the velocity of light. People, at least part of them, are quite aware of those difficulties. However, there is such a strong wind blowing in favour the successful model — much stronger that the "ether-wind" — that the emission model is rejected as a consequence of Foucault's result without any further discussion.

VELOCITY OF LIGHT VERSUS CONCEPTS OF LIGHT

Typically in the case of light, measuring a velocity has been inseparable of the very concept of "luminous phenomenon". When you are trying to measure a velocity, you implicitly suppose that *something* is to be measured, that *something* is propagating, whatever the time of flight. But, this has not always been the case. The idea of an instantaneous effect was dominant during Antiquity and Middle Ages, with two exceptions: Empedocle of Agrigento during Greek Antiquity; and, at the X-XIth centuries, in the Middle-East, Ibn al-Haytham (al-Hazen in Latin texts) who left considerable works in Optics: light is considered as a "substantial matter", the propagation of which — occurring through light rays — is clearly distinguished from vision and *requires time* "even if this is hidden to our senses". However,

the debate will remain at a conceptual level, since it appears no *practical* difference between such fast a phenomenon and an instant one.

The debate will resume at the XVIIth century, among many difficulties and ambiguities. The first trial was Galileo's one, trying to measure directly the velocity of light with two distant lights as reported in the *Discorsi* (Galileo, 1638-1995, p. 37-39). Having tried over a distance of one mile, with a negative result, he very wisely concludes (speaking through Salviati): "if the appearance of the opposite light is not instantaneous, at least it is extremely fast, nearly immediate". However, the same Salviati had beforehand argued that, "if the result would remain negative at a distance of 3 miles (i.e. 6 miles for the whole return path), then it should be concluded with certainty that the propagation of light is instantaneous". But the experiment was not performed! As A. Wroblewski observed — with modern eyes —, admitting that Galileo could detect a delay of, say, one tenth of a second, the one-mile experiment proved only that the velocity of light is, roughly, larger than 30 km/s (Wroblewski, 1985), and, for the hypothetical 3-miles experiment, larger than 90 km/s. However, such is not Salviati's conclusion! The calculation would have been, of course, within Galileo's intellectual ability, but the problem was not a matter of calculation. The problem laid in the fact that, at the time, *such a fantastic velocity could not ever be conceived as different from infinite*. Half a century later, reporting Roemer's observations, Huygens will have to stress strongly the point that *we have to* accept a velocity "more than six hundred times larger than the sound one" (Huygens, 1690-2000, p. 57). Clearly, this has been *extremely* difficult.

Again, the scholarly ambiguities of Descartes illustrate how high the difficulties were: a propagation of light "within an instant": a very short moment? or an instantaneous phenomenon? In the parable of the blind man and the stick, in the Dioptrics, he seems to conclude in favour of an instantaneous propagation (a duration that would be independent of the distance); and again in the example of lunar eclipses: a "time of flight" from Moon to Earth lower than one hour seems to him *inconceivable*, and since such a duration appears contradictory to astronomical observations (Sun, Earth and Moon along a straight line), the conclusion of an instantaneous propagation seems to follow logically. However, the same Descartes will declare elsewhere, in a letter to Mersenne: "I declare contradictory that an infinite velocity may occur in Nature" (Costabel, 1978). This should prevent us to declare "absurd" his assertion according to which the propagation of light should be "easier" in water than in air: a velocity larger than infinity? Certainly not.

Let us quote another ambiguity of Descartes, in the famous text on reflection and refraction in the Dioptrics. His conception of the luminous phenomenon is much closer to the waves of the future Huygens than to any "emission" model: "an inclination to motion", "some type of motion, or very sudden and sharp action which passes towards our eyes" and "*without any material thing passing from the objects up to our eyes*". However, in order to explain the refraction phenomenon, and lacking of a clear model (which will be brought later only by Huygens), he refers at length to the model of the ball and the web in which a very thin web is supposed to take place at the boundary between both mediums — typically a "particle" model. Just here, the idea of the conservation of "the determination for motion", in the sole direction *parallel* to the boundary layer, is introduced, just as for reflection. However, a light ray deviates in the direction opposite to a real ball when entering the water, the light ray coming *closer* to the normal to the surface. Thus, the hypothesis implies a *larger* velocity for the light in water than in air, contrary to the ball. Then, the "ball and web" model is suddenly abandoned, with the sole explanation that "light is not like a ball, but …". And an analogy with the interaction of balls with materials is then called for, in order to justify an "easier motion" in water (and other dense transparent materials) than in air. An analogy which, though lying on several firm grounds, finally leads to an error.

Finally, Descartes' model is a pre-wave model, but affected by a "superimposed hypothesis" which is itself justified *only* from a "classical particle" point of view. Here lies its very ambiguity: certainly there would be some anachronism to speak of well-defined "wave" or "particle" models about Descartes. We should also underline that, according to P. Costabel, Descartes was extremely cautious about the concepts of time and velocity: an example (above) is its use of the word "easier" rather than "faster" for the propagation of light in water.

Newton himself, half a century later, was not so strongly in favour of "particles of light" than his "descendants". At least, a mixture of both concepts appears in his works: vibrations of the "medium" or "ether waves" are induced when the light particles are passing trough (Rosmorduc, 1977). Not, of course, any prefiguration of the xx[th] century dualism — a quite different matter — but a sign that, at Descartes and yet Newton times, the wave and particle concepts are not fully elaborated and remain more or less intricate. At least, the full formulation of the wave theory by Huygens, and a wide diffusion of Newton's *Principia* will be necessary in order to get two well established and conflicting theories of light. But we are then practically at the beginning of the xviii[th] century. Let us put the emphasis on the word "conflicting": with, on one side, an "ether" supporting light vibrations without any transport of matter, and, on the other side, an "emission" of classical particles obeying Newtonian Mechanics, both theories are now conflicting. But *only* now.

And such is the situation that Arago and Foucault will inherit one and half century later. That is why Foucault's result could be considered at the time as "crucial" and falsifying the emission model, but only the *current* emission model: the crucial character of a result can, at best, be relative to its time.

The 1862 Speed of Light Measurement and the Astronomical Unit: A Virtual Jump into Space.

Now, let us come now to Arago's second statement:

> By repeating those observations with more mechanically perfect instruments, it will become possible one day, without leaving Paris and its neighbourhood, to get this *solar parallax* which, around the middle of last century, gave rise to such long, such distant, such difficult journeys and to so many expenses.

Why the solar parallax? Why not in 1850?

Contrary to the controversy about the nature of light, the accurate value of the speed of light was not a major concern in 1850: it was not yet related to Electromagnetism, and, of course, far to be considered as a fundamental constant. The first terrestrial measurement by Fizeau in 1849, using a toothed-wheel technique, yielded a value (312000-315000 km/s) roughly in agreement with the astronomical determinations (308000-312000 km/s) i.e. Roemer or Bradley's methods, Jupiter's satellites or stellar aberration respectively. This was considered at the time as sufficiently satisfactory: see, for example, Jan Frerks' detailed report about his replica of the 1849 measurement (Frerks, 1849). However, it appeared clearly (cf. Arago's statement above) that the laboratory devices for terrestrial measurements could be more rapidly improved than the telescope technology (to which a main contribution will also be brought soon by Foucault himself). Indeed, following his 1850 air-water qualitative experiment, Foucault announced a further absolute measurement in air. But it will be performed only in 1862. Why now?

THE ASTRONOMICAL UNIT PROBLEM

Meanwhile, a new concern had appeared, and it came from Astronomy. The accurate value of the Earth-Sun mean distance — i.e. the astronomical unit — was considered by Airy, in 1857, as "the worthiest problem of Astronomy". Until the 1850's, the Earth-Sun distance was deduced only from *parallax* methods *which were depending on the value of the Earth's radius*: such methods were based on the fact that an astronomical event appears slightly differently when observed from either of two distant points of view on the Earth. The distance of a "nearby" planet (i.e. when nearest to the Earth) was at first determined, from which its distance to the Sun was deduced from Kepler's third law. Two methods were in use: the position of Mars relatively to the remote stars, first performed by Cassini and Richer between Cayenne and Paris in 1672; or the transits of Venus before the Sun, first proposed by Halley in 1716, which occur only rarely (1761-1769, 1874-1882 and 2004-2012!) and which gave rise during the XVIIIth century to the "long and distant journeys" above mentioned by Arago; in this last method, the apparent path of the planet on the solar disc is different, depending on the latitude of the observation site, from which different transit times. The accuracy of the Earth-Sun distance was, of course, depending on the accuracy to which the distance between both points on the Earth was known, i.e. in the final analysis, on the accuracy of the Earth's shape and radius. It is significant that the result was always given by the astronomers in terms of the "solar parallax", the angle subtended by the Earth's radius as viewed from the centre of the Sun: indeed, this angle could be deduced from pure astronomical angular measurements from two distant points on the Earth. Turning to the Earth-Sun distance required furthermore to introduce the value of the Earth's radius, which was mainly not an astronomical problem but a geodesic one left to land surveyors. The emphasis should be put on *the importance of the astronomical unit*: its knowledge allows indeed the transposition of the planets parallax method onto a higher scale: the "stellar parallax" of nearby stars is obtained trough two sightings of a star at a six months interval. The distance of the star is then deduced from the knowledge of the size of the Earth's orbit, i.e. twice the astronomical unit. This last distance is consequently the *length standard* for all measurements in the universe. Thus, it is easily understood that knowing the astronomical unit independently of the Earth's radius and shape — too small and imperfectly defined — would be a highly significant issue for Astronomy.

Now, the point is that any terrestrial measurement of the velocity of light yields such an independent determination of the astronomical unit. This is precisely the meaning of the above statement of Arago: there would be no longer any need of remote expeditions, only local astronomical sightings and a laboratory velocity measurement. Let us take, for example, the determination of the velocity of light from the stellar aberration (Bradley, 1728) : the aberration angle (in radians) is just the ratio of the orbital velocity of the Earth to the velocity of light (roughly 10^{-4}). But the process can be reversed: knowing independently the speed of light will yield the Earth's orbital velocity, from which the astronomical unit! Similarly, the method of Jupiter's satellites yields directly the time taken by the light to cross the Earth's orbit: knowing independently the velocity of light will give the length of this orbit. This mix of astronomical and terrestrial measurements will be called later "the physicists method" by contrast to the pure "astronomical method".

FOUCAULT'S 1862 MEASUREMENT

The explicit demand came from the french astronomer Le Verrier, during the last of the 1850's years. His monumental work on the mutual perturbations between planets led him to the conclusion that the astronomical unit, deduced either from purely astronomical

determinations or from Fizeau's velocity of light, was overestimated by several percent. He asked Foucault — currently physicist at the Observatory — to perform an *accurate* measurement of the light velocity: his rotating mirror driven with a steam turbine — a small Cagniard-Latour siren — had proved its superiority during the qualitative air-water experiment of 1850. Foucault will resume his 1850-device and bring to it important improvements: the 4-meters basis is extended to 20 m (folded in "Z" on his table!); the value of the speed of rotation, which was obtained in 1850 by tuning the sound of the siren to a 400 Hz tuning fork, is now obtained from a stroboscopic technique with the help of a clock mechanism; and the steam-drive of the turbine is changed to a pressured air one, thanks to an organ bellows— a large hand-driven wooden case called a "mannequin" — designed by his friend, the famous organ builder Cavaillé-Coll. Finally, thanks to an accurate regulator, a pressure stability of 0.2 mm of water is obtained for a pressure difference of 30 cm of water between the input and the output of the turbine. The 1862-measurement will give right completely to Le Verrier: Foucault obtains 298 000 km/s for the speed of light, with a somewhat underestimated uncertainty of 500 km/s (Foucault, 1862; Tobin, 2003, chap. 13).

IMPORTANCE OF FOUCAULT'S MEASUREMENT

Thus, the astronomical unit is henceforth determined from the velocity of light. The title of Foucault's report at the French Academy of Sciences is, in this respect, quite significant: "Experimental determination of the velocity of light; the *solar parallax*". The title might however look somewhat ambiguous since Foucault's result, combined to the stellar aberration — an angle measured in the sky — yields *directly* the Earth-Sun distance, not the solar parallax, and this is quite clear in the paper. Why, then, does he go on speaking of this "solar parallax", a quantity which requires, following a reverse calculation, to combine this distance with the Earth's radius? Isn't the solar parallax definitively useless? The reason is that the problem of the Earth-Sun distance was so deeply identified, for two centuries, with the solar parallax that Foucault, in order to emphasize the immense importance of his result, feels the need to use the language of the astronomers. Furthermore, converting his result to the solar parallax was necessary for comparison with the *dominant* purely astronomical methods (though, of course, the reverse way would be legitimate as well). The "physicist method" will now be in competition with the "astronomical method" for the distances in the universe. That is why a qualified judgment is passed nowadays on the purely scientific interest of the remote expeditions organized for the Venus transits in 1874 and 1882: if a similar accuracy for both methods could yet be expected by the astronomers in 1874, then in 1882, following improvements in physical measurements (Cornu, 1876), the non-scientific, i.e. the geo-political motivations were probably dominant in this time of booming colonial expansion.

Finally, we emphasize the major contribution of Foucault' measurement to the History of mankind: liberating for the first time the distances in the universe from the terrestrial circumstances, Foucault performs *a giant virtual jump in space*, a jump that will be only equalled, one century later, by the first spatial explorations, those last ones quite real.

FURTHER DEVELOPMENTS

The "physicist method" will be improved at first in 1872 and 1876 by Cornu who returned to Fizeau's toothed-wheel technique and obtained 288500 and 300400 km/s respectively for the speed of light (Cornu, 1874; 1876). As regards the rotating mirror technique, Michelson will obtain 299990 km/s in 1879 (those new measurements were contemporary of the above-mentioned expeditions of 1874 and 1882 for the Venus transits). In 1926, following a series of improvements and using a 35 km basis and an octagonal rotating mirror, Michelson will

obtain 299796 +/- 4 km/s, a value quite close to the modern one (299792.458 km/s) and which will remain the reference during several decades, thus paying a final and well-deserved tribute to Léon Foucault. It would be out of our scope to discuss here the new role of the speed of light — from the beginning of the XXth century — as a *universal constant* in connection with the propagation of electromagnetic waves and special relativity. However we shall observe that, among the modern ways which have been used to determine the speed of light (or, equivalently the length standard, the metre) during the last half-century, some important ones are related to "historical methods" : the Shoran technique (radar pulses) is related to the original trial by Galileo and his two lights; the modulated light detector technique, a kind of electronic stroboscopic technique, is related to Fizeau's toothed wheel; and the Doppler-Fizeau method to Jupiter's satellites, the former method initiated by Roemer.

Conclusion

Thus, in 1850 and in 1862: two experiments about the speed of light, only one physicist — Léon Foucault — only one technique, the rotating mirror device. But with quite different motivations. The 1850-experiment aimed at an epistemological level: to decide between two conflicting theories. As shown in our discussion, the "crucial" character of the conclusion — the emission theory "definitively" rejected — should, in any eventuality, be understood *relatively* to the time and to the dominant ideas of those days. On the contrary, the 1862 one is an accurate *metrological* experiment. Thus, during this short time interval, the speed of light has undergone a deep change in its status: from being a qualitative criterion for a theory of the "luminous phenomenon", it becomes finally part of a length standard — independent of the figure of the Earth — in order to survey the universe. What a jump!

A parallel can be drawn between those mid-XIXth century speed of light measurements and the torsion balances of Coulomb and Cavendish, half a century earlier: for both physicists, only one torsion balance technique, only one underlying theoretical law (the inverse-square law), but contrasted motivations: a fundamental one for Coulomb in 1785 — to establish, as far as possible, the inverse square law for Electrostatics; on the contrary, accepting the law for Gravitation (as fully justified by the motion of planets), Cavendish performs in 1798, with his gravitational balance, a wonderful experiment of Metrology yielding, through the Earth mass, a mass standard for weighing the whole solar system and, beyond, the universe (Lauginie, 2003).

An experiment is never performed for itself, but in order to answer a scientific, epistemological or even social questioning, in a well dated conceptual framework. The inferred conclusions are not — as shown with Foucault's 1850 experiment — independent of this framework.

References

Arago, F.:1838, 'Sur un système d'expériences à l'aide duquel la théorie de l'émission et celle des ondes seront soumises à des épreuves décisives,' *C.R. hebd. Acad. Sci.*, 954-965.

Arago, F. : 1857, In : *Astronomie populaire en 4 vol.*, vol. 4, Gide, Paris, p. 418.

Bailhache, P.: 2002, 'Un effet Doppler-Fizeau méconnu: Roemer et la vitesse de la lumière', *Revue d'Histoire des Sciences* **55**, 411-430.

Bradley, J.: 1728, *Phil. Trans. Roy. Soc. London* **35**, 637.

Cornu, A.: 1874, 'Détermination de la vitesse de la lumière et de la parallaxe du Soleil', *Journal de l'École polytechnique,* **44,** 133-180.

Cornu, A.: 1876, 'Mémoire sur la détermination de la vitesse de la lumière entre l'Observatoire et Montléry', *Annales de l'Observatoire de Paris (Mémoires)* **XIII**, A, 115.

Costabel, P.: 1978, 'La propagation de la lumière sans transport de matière de Descartes à Huygens' In : R. Taton (ed.), *Roemer et la vitesse de la lumière*, Vrin, Paris, pp. 83-91.

Eisenstaedt, J.: 2002, 'La vitesse de la lumière et la Physique classique', In: *Einstein et la Relativité générale* (chap. 1), CNRS-Editions, Paris.

Fizeau, H.: 1849, *C.R. hebd. Acd. Sci.* **29**, 90-94 & 132.

Foucault, L.: 1851. 'Sur les vitesses de la lumière dans l'air et dans l'eau', *Ann. de Chim. et de Phys.* **3ʳᵈ series XLI**, 129-164.

Foucault, L.: 1862, *C.R. hebd. Acad. Sci.* **55**, 792-796.

Frercks, J.: 2000, 'Creativity and Technology in Experimentation: Fizeau's Terrestrial Determination of the Speed of Light', *Centaurus* **42**, 249-287.

Galileo, G.: 1638-1995, 'Première journée', In: Clavelin, M. (ed.), *Discours concernant deux sciences nouvelles,* PUF, Paris, pp. 38-39.

Hoffmann, B.: 1985, *Histoire d'une grande idée, la Relativité*, Pour la Science-Belin, Paris.

Huygens, C.: 1690-2000, *Traité de la Lumière*, M. Blay (ed.), Dunod, Paris.

Lauginie, P.: 2003, 'La Pesée de la Terre', In: *La Gravitation*, Dossier Pour la Science, Belin, Paris, pp. 18-21.

Lauginie, P.: 2003, 'Cavendish and Coulomb Torsion Balances: Only one Technique, only one Law; two balances, two Aims, two Styles, two Destinies'. In: *7ᵗʰ IHPST Conference Proceedings, Winnipeg*, pp. 504-517.

Rashed, R.: 1978, 'Lumière et vision : l'application des mathématiques dans l'optique d'Ibn al-Haytham'. In: René Taton (ed.), *Roemer et la vitesse de la lumière*, Vrin, Paris, pp. 19-44,.

Roemer, O.: 1676, 'Démonstration touchant le mouvement de la lumière trouvée par M. Römer de l'Académie Royale des Sciences', *Journal des Sçavans, 7ᵗʰ Dec. 1676,* 233-236.

Rosmorduc, J.: 1977, 'L'idée d'une structure de la lumière dans l'histoire de la physique', *Cahiers d'Histoire et de philosophie des Sciences, CNRS* **2**, p. 62.

Tobin, W.: 2003, *The Life and Science of Léon Foucault,* Cambridge University Press, Cambridge.

Shae, J.-H.: 1998, 'Ole Roemer, the speed of light, the apparent period of Io, the Doppler effect, and the dynamics of the Earth and Jupiter', *Am. Journal of Physics* **66**, 561-569.

Wroblewski, A.: 1985, 'De Mora Luminis : A spectacle in two acts with a prologue and an epilogue', *Am. Journal of Physics* **53**, 620-630.