Creativity and Technology in Experimentation: Fizeau's Terrestrial Determination of the Speed of Light

BY JAN FRERCKS*

Abstract

Hippolyte Fizeau was the first to measure the speed of light in 1849 over a terrestrial distance, substituting lengthy astronomical observations with a sophisticated set of apparatus. The fundamental ideas of this experiment are well known, while the practical realization achieved by Fizeau in only six months is not. I will present a close examination of this process of implementing a new experimental method, based not only on the scarce published material but also on a collection of manuscript papers, which had not been examined before, as well as my own experiences with a replication of the experiment. I will argue that Fizeau employed two substantially different ways of implementing his experimental ideas, leading to an apparatus which allows quite precise determinations if appropriate measuring methods are invented to cope with difficulties. The replication and the consideration of the scientific context suggest a new appraisal of Fizeau's results, his intentions, and the reasons for starting the experiment in the first place.

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1. Introduction

The French physicist Hippolyte Fizeau (1819–1896) is chiefly remembered for his measurement of the speed of light in 1849. This experiment has found its way into many of today's physics textbooks as the first "terrestrial" determination, placed in a line with precedent methods as Rømer's (1676) and Bradley's (1728) and continued by measurements of Cornu (1874), Michelson (1879), Perrotin (1902), and many others, leading finally to the implicit appointment of the speed of light in the new definition of the meter in 1983[1]. The importance assigned to Fizeau's measurement also stems essentially from the importance of this physical constant for modern physical theories, in particular electrodynamics and theory of relativity. Moreover Fizeau's toothed-wheel method was dominant for speed-of-light measurements together with Foucault's rotating-mirror method for almost one hundred years.

Historians of science have rarely examined Fizeau's experiment, or his entire experimental work[2]. This is regrettable because the experiment and the speed of light itself should and in fact can be appraised in terms of a contemporary context. Yet the close examination of this experiment offers more. It allows insights into the practice of a dedicated experimentalist. In particular, it shows how a general idea is transformed into experimental concepts, how these are technically realized, how theory is present in the experiment, and how the actual method of measurement has to be amended to practical difficulties.

One reason why almost none of Fizeau's experiments have been analysed in detail might be the fact that Fizeau never published comprehensive accounts of his experiments or contributions to debates about urgent theoretical questions. This to be sure is though he was well aware of the latter since many of his experiments are closely related to theoretical problems of his time.

In spite of the significance assigned to experimentalists and experiments for some years, historians are left with the question of adequate sources because major parts of the very practice of an experimentalist leave no written traces[3]. The first step to extend the source material obviously is to include unpublished papers. In the present study I will use some of Fizeau's notes which had been neglected before[4]. Moreover my main additional if less common source are my own experiences with a replication of Fizeau's experiment. I have tried to measure the speed of light myself as similar as possible to Fizeau, making use of a faithfully reconstructed apparatus[5].

I will start with an explanation of the toothed-wheel method which is the central idea of the experiment. For this I rely on Fizeau's publication at the end of his experiment in July 1849, like authors of physics textbooks generally do (section 2).

Fizeau's paper contains everything necessary to understand the ideas, and still it contains hardly anything to explain what Fizeau actually did. This is not to say that published papers tell nothing valuable about an actual experiment or even gloss it over. Nevertheless it is clear that a publication represents the long and messy experimental work in a shortened, smoothed, and even idealized way. After all a publication is intended to persuade the contemporary reader and not to provide the historian with detailed information. Finding out more about the "real actions" thus leads along the way to some insights into this process itself.

So I go back to the first trace of the experiment, the pli cacheté given to the Académie des Sciences six months before (section 3). The pli cacheté and the final publication build a kind of frame of the experiment. My main concern is what happened between January and July 1849, hoping to fit pieces of the picture into the frame. The pli cacheté allows some insights into the state of planning and implementing the apparatus which is significantly different with regards to the optical and the mechanical components of the final apparatus. This corresponds to two completely different modes of handling technology which will be worked out in sections 4 and 5 respectively. The characterization relies strongly on my experiences with the replication.

This also applies to the section which discusses practical difficulties in using the completed apparatus (section 6). These will be compared with a handwritten compilation of all of Fizeau's measurements. The somewhat surprising results of my experiment lead to a new appraisal of Fizeau's success and even of his intentions (section 7). My line of argument continues by relating Fizeau's intentions, actions, and his success to the importance assigned to the speed of light by his contemporaries (section 8). Finally I broaden the subject and try to provide some reasons why Fizeau started to measure the speed of light originally (section 9). This is done by pointing out some links to a further experiment.

2. *The publication*

Fizeau presented his experiment to the Académie des Sciences on 23 July 1849. The paper version mainly consists of an explanation of the toothed-wheel method (Fizeau 1849a). If I partly adopt Fizeau's description here, it is not only because it is concise and clear, but also because it allows us to see his publication in the light of his actual experiment later. The toothed wheel is introduced as follows:

> Lorsqu'un disque tourne dans son plan autour du centre de figure avec une grande rapidité, on peut considérer le temps employé par un point de la circonférence pour parcourir un espace angulaire très-petit, 1/1000 de la circonférence, par example.
>
> Lorsque la vitesse de rotation est assez grande, ce temps est généralement très-court; pour dix et cent tours par seconde, il est seulement de 1/10000 et 1/100000 de seconde. Si le disque est divisé à sa circonférence, à la manière des roues dentées, en intervalles égaux alternativement vides et pleins, on aura, pour la durée du passage de chaque intervalle par un même point de l'espace, les mêmes fractions très-petites (Fizeau 1849a, pp. 90–91).

Fizeau now introduces the light if still in an idealized way:

> En considérant les effets produits lorsqu'un rayon de lumière traverse les divisions d'un tel disque en mouvement, on arrive à cette conséquence, que si le rayon, après son passage, est réfléchi au moyen d'un miroir et renvoyé vers le disque, de manière qu'il le rencontre de nouveau dans le même point de l'espace, la vitesse de propagation de la lumière pourra intervenir de telle sorte, que le rayon *traversera* ou *sera intercepté* suivant la vitesse du disque et la distance à laquelle aura lieu la réflexion (Fizeau 1849a, p. 91; Fizeau's emphasis).

Hence the light point or "étoile artificielle" (Foucault 1849) normally seen by the observer placed behind the disc vanishes for a special speed of the toothed wheel. Each portion of light transmitted by a notch on its way there is blocked by the subsequent tooth which has replaced the notch during the travel time of the light portion.

The observer of course does not perceive each tooth distinctly even for a moderate speed of the toothed wheel. Instead he/she sees the transmitted part of the returning light continuously due to what can be called "*stroboscopic effect*".

> Dans les circonstances où l'expérience a été faite, la première éclipse se produit vers 12,6 tours par seconde. Pour une vitesse double, le point brille de nouveau; pour une vitesse triple, il se produit une deuxième éclipse; pour une vitesse quadruple, le point brille de nouveau, et ainsi de suite (Fizeau 1849a, p. 92).

Between extinction and reappearance, the intensity in theory varies in linear fashion with the frequency of the toothed wheel.

In order to use this effect for the actual measuring of the speed of light, one has to measure one of these frequencies. Together with the number of teeth (720 with Fizeau) and the length of the light path (8633 m with Fizeau) this leads to the speed of light by a simple calculation.

> Ces premiers essais fournissent une valeur de la vitesse de la lumière peu différente de celle qui est admise par les astronomes. La moyenne déduite des vingt-huit observations qui ont pu être faites jusqu'ici donne, pour cette valeur, 70948 lieues de 25 au degré (Fizeau 1849a, p. 92).

These "first trials", carried out between his parents' house in Suresnes and the telegraph office on Montmartre were in fact the last ones Fizeau ever made for reasons which will become clear later[6]. The value became a component of the history of the determinations of the speed of light, mostly converted in 315,300 km/s.

This short account poses more questions than it answers. What were Fizeau's intentions with this experiment? How did Fizeau manage to install a completely new method in only half a year? Why did he stop after some "first trials"? Did the apparatus fulfil the expectations concerning accuracy? How can the result be judged, compared to the then acknowledged value of the speed of light and today's figure of about 300,000 km/s? What was the relevance of the speed of light at that time? In order to tackle these questions I have to go back to the beginning of the experiment.

3. *The pli cacheté*

While Fizeau's article in the *Comptes rendus* has been the only accessible written account of his experiment for a long time, a further important source is now at hand for historians of science. It was quite usual to provide for possible priority disagreements in form of a sealed description (pli cacheté) of scientific plans handed to the Académie des Sciences. Fizeau's pli cacheté is dated 20 January 1849 with three further remarks dated 22 January. It was accepted by the Académie des Sciences as the pli cacheté No. 878 on the same day[7]. It can be seen as a semi-public paper, addressed to the public but not expected to be opened in the foreseeable future[8]. In fact it was opened only in 1982. A text analysis of such a paper is not my prevailing topic here. I rather borrow from it some starting points for the examination of the experiment itself.

To enter the content, it is striking that the text contains much more than what would be necessary for priority claims. It rather describes the state of implementation of Fizeau's endeavour. The central idea to use a rotating toothed wheel to produce a sequence of light flashes and to analyse it on its way back is explained yet very similar to his final account. The state of progress in realizing this idea is very unequal concerning the optical part and the mechanical part of the apparatus.

With regards to the optics, Fizeau is already able to present a schematic diagram of the light path, reproduced in Fig. 1. The light path is almost completely retained until the final realization. A lens focuses the light coming from a source. A glass plate reflects part of it onto the circumference of the toothed wheel. Bundled and directed by a telescope, it reaches another telescope. A mirror placed in the focus of its objective lens reflects back the light so that it hits the first telescope again. Part of the remaining light is transmitted by the glass plate and can be observed using an eyepiece. Fizeau leaves obscure how he had developed this arrangement and why he used just these devices. At the time writing the pli cacheté Fizeau is about to experiment successfully if still on a short distance of 34 m.

> J'ai cherché à vérifier ces propriétés de deux lunettes conjuguées, dans quelques expériences, et je les ai reconnues exactes, ... (Fizeau, pli cacheté)

Against this, all of the required mechanical devices were inexistent at that time. No idea is mentioned how to drive and to regulate the toothed wheel

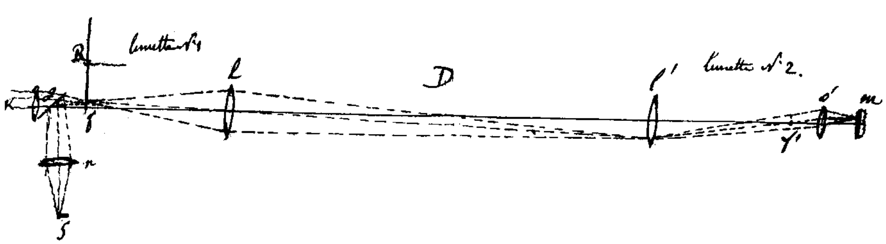


Fig. 1. Layout of the optical arrangement for Fizeau's determination of the speed of light, in his pli cacheté N° 878 given to the Académie des Sciences. By permission of the Archives de l'Académie des Sciences, Paris.

("R" in Fig. 1) or how to measure its rotational frequency. It is all the more striking that Fizeau had some precise opinions about the accuracy to be awaited of the final measured value. He envisaged a distance of 7750 m and a toothed wheel carrying 2000 teeth which shall be turned at high speed.

> Le nombre des tours pouvant certainement être porté aux 200 par seconde et plus encore avec des appareils convenables (j'ai consulté sur ce sujet des constructeurs très habiles) [;] entre 0 tours et 200 tours il y aura pour cette distance 20 extinctions et 20 réapparitions (Fizeau, pli cacheté).

This is important because if one assumes a certain absolute error in estimating the frequency of a light point extinction, the relative error of the measurement decreases with the frequency. Therefore it seems advisable to perform the measurements at the highest possible order of extinction. Reaching the 20th extinction leads to a small relative error though Fizeau allows for a considerable departure in estimating the frequency of extinction or of maximum brightness.

> Si le dernier maximum peut être observé à deux tours près (il se produit au milieu de l'intervalle compris entre dix tours) il y aura dans chaque expérience *incertitude de 1/100 seulement sur la vitesse de la lumière*. (Fizeau, pli cachété; Fizeau's emphasis).

The words "dans chaque expérience" have been added later directing to the possibility to capitalize from the opportunity to take many measurements.

I shall now discuss the way Fizeau implemented the apparatus, beginning with the optical parts.

4. *Misappropriation: The optical system*

Fizeau had all required optical devices ready in January 1849: two telescopes, an eyepiece for the observer, a small glass plate, and a lens to project the light source onto the toothed wheel. These in fact were still used in the final measuring apparatus, completed by one further lens in front of the light source while the lens "o′" in Fig. 1 has been omitted. Yet the instrumentation of an experimental idea is far from trivial and can be achieved in substantially different manners. To explain this, I will start with one device from the optical system, namely the telescope used to send back the light ("lunette No. 2" in Fig. 1).

First of all it is by no means clear why one should use an ordinary astronomical telescope to send back a light beam. In order to understand this, I will turn to my replication now.

Fizeau's telescopes still exist at the Ecole polytechnique in Palaiseau near Paris, and they served as models for the reproduction[9]. Unfortunately the mirror in the eyepiece is missing, but obviously I needed a mirror to send back the light. So – like Fizeau about 150 years before – I had to invent a mirror on my own, knowing about Fizeau's mirror only that it should be placed in the focus of the objective lens[10].

It is quite easy to realize that an ideal mirror exactly in the focus of the objective lens and both elements absolutely in line with the main optical axis allows sending back almost all the light arriving from the opposite station. Of course you never achieve the ideal situation. Therefore, in order to develop a solution which works sufficiently well, I had to understand how light is returned by a focusing optical element and a reflecting optical element. Especially I had to work out which the crucial points are. What about a slight deviation of the optical axis? Is it necessary that the mirror is absolutely perpendicular to the telescope axis? How can the mirror be placed as exact as necessary in the focal plane and what is "as exact as necessary"? It has to be kept in mind that the reflected light beam must hit a surface of 6 cm in diameter over a distance of (in my case) 6136 m.

This process took place partly in the laboratory, but mainly with pencil and paper at the desk. In fact I had to re-invent a kind of experimental theory. This theory should enable me to understand the behaviour of the reflecting system, to construct a mirror, and to adjust all items in practice. It is noteworthy that to invent a suitable mirror and to invent a useful adjustment procedure is one.

To call this kind of knowledge an "experimental theory" is justified because both words matter. On the one hand, it is a theory in the sense that it is more than knowledge inextricably bound to a special situation. A theory should enable to compare different things; it should be applicable to more than one special situation, if still adapted to each particular case[11]. This is in fact the case with the knowledge how to send back light with a telescope. I will come back to this in section 9.

On the other hand, this theory is necessarily bound to experimental practice. It only makes sense if it is *used* for some purpose. Thus it is knowledge about doing things rather than about things themselves. Especially it does not say anything general about (the nature of) light, but only how to do some thing particular with light.

Such kind of experimental theory should not be confused with a theory of an instrument. The theory needed is not simply a *theory of a telescope.* It is rather a *theory of a telescope for reflecting light*, which means a theory of a special *function* of the telescope. I became painfully aware of this when I tried to use the amply available *theories of telescopes* (Fizeau's contemporary and modern ones) which in fact turned out to be *theories of telescopes for astronomical observation.* Magnification, spherical and chromatical aberration, diaphragms, and eyepiece micrometers are important for astronomical observation, but not for my purpose. Therefore the theory of these items was of no use to me.

Thus my theory is about a technical principle rather than about a technical device. This abstract principle, which I call "*focus-reflection*", turned out to be the crucial link between the experimental demand ("Send back the light!") and the already existing astronomical telescope. In order to elucidate this interrelation of devices, procedures and experimental demands I will briefly describe my solution of this particular problem[12].

One half of a circular glass plate (the left hand side, say) was silvered, and the other side was sprinkled with some iron files. This glass plate was placed in the eyepiece of the telescope instead of the crosshair or the micrometer scale. The silver-coated surface now has to be brought into the focus of the objective lens which is done in the following manner. The observer looks through the telescope in the usual way, having his/her sight on the opposite station in the semicircular visual field on the right. If the eyepiece is now shifted until the opposite station and the iron files are seen sharply at the same time, the coated surface is in the image plane of the opposite station. This is independent of the adjustment of the eye.

One could worry if this is the correct position since the emitting telescope had made the light bundle parallel (at least in the ideal case), so the mirror telescope should be focused at infinity. However, the difference between these two positions of the mirror is only 0.1 mm, less then the attainable accuracy for the mirror position in practice. Working out this problem theoretically, one learns that in fact the geometrical representation is completely lost, and so is part of the light, but fortunately a sufficient quantity of the light nevertheless reaches the emitting telescope. And that, not a picture, is needed for the experiment.

Now the observer turns the whole telescope slightly so that the opposite station disappears behind the silvered part of the glass plate. This means, the mirror now reflects light coming from there instead of transmitting it. This trick relies on the astonishing fact that a misalignment of the telescope by even several degrees does not affect the direction of the reflected light. This of course had to be established theoretically and by some short distance trials before the construction of the mirror.

The procedure works very well. Note that it can be performed by day without a light source at the opposite station. Furthermore, since it is independent of the adjustment of the first telescope, one person alone can do it, and in particular without any communication between the stations[13].

I will now return to Fizeau. I cannot claim that my mirror looks exactly like Fizeau's. Correspondingly the adjustment procedure may not resemble Fizeau's either. But I do claim that I have found out something about his particular way of using technology for the implementation of the optical layout.

Fizeau solved one difficult experimental problem by misappropriating an astronomical telescope. The central idea was to see at once 1) the technical principle *focus-reflection* as a possible solution for the experimental demand and 2) the standard astronomical device as a possible materialization of the *focus-reflection.* The initial problem has thus been solved and in fact replaced by two other problems.

The first problem is the *focus-reflection* itself in so far as it has to be worked

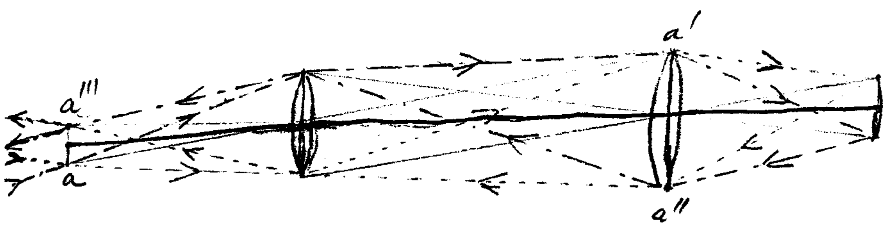


Fig. 2. One of Fizeau's sketches to work out the *focus-reflection* as the crucial link in order to use a telescope for reflecting back the light. a a''' is the plane of the toothed wheel in the focus of the left objective lens. The lens on the right (a' a'') focuses the light on a plane or concave mirror. Note that the telescopes themselves are not shown. By permission of the Musée René Sordes de Suresnes – France.

out, resulting at best in what I have called an experimental theory. Fig. 2 shows one of many sketches made by Fizeau for this. Even such a neat sketch cannot be understood by itself, since no one knows what Fizeau hoped to make clear to himself through it. Maybe he reflected upon whether to use a plane or a concave mirror. When I look at my own very similar sketches today, even I do not know what *I* wanted to make clear to *myself* with each single one. Once the system is understood the sketches seem trivial. Still there is no doubt that this kind of sketching is helpful if not necessary to make sense from using a telescope to send back light. Every single possible misalignment and maladjustment has to be mastered first of all. It must be emphasized that this kind of theorisation is an essential part of an experimentalist's work. The second problem is that the telescope has to be slightly modified: A small mirror has to be built in, a problem which is not insurmountable.

Similar strategies are obviously at play when Fizeau used another telescope to send out the light. However, this kind of misappropriation of technology is not restricted to telescopes. An example of an equivalent procedure can be found in device "g" in Fig. 1.

The necessity to place the light source apart from the observer requires introducing the light laterally into the telescope. A mirror was of course unsuitable because it would block the light completely on its way back. Simple reasoning leads to the insight that the ideal device would reflect half of the light and transmit the other half. This principle can be called a "*half-mirror*". Since a coating with such properties was not available at that time, one had to explore the natural partial reflection on the surfaces of glass plates. So what has been called a *half-mirror* has to be elaborated in order to see how it can be achieved or at least approached in practice. Applying Fresnel's formulas leads to the result that a pack of 16 plates would come closest to the ideal (instead of only one plate envisaged by Fizeau in the pli cacheté)[14]. Further reasoning shows that these plates have to be extremely thin. The latter requirement is fulfilled by cover glasses used for microscopy which were only 1/10 mm thick at that time. Again a general requirement of the experimental system ("Introduce the light!") has been achieved by the misappropriation of a device borrowed from another science, biology in this case. However, Fizeau used only two plates instead of 16, probably because of practical difficulties to be discussed in section 6 (Cornu 1874a, p. 150).

The main benefit of this kind of proceeding is that the major parts of the optical system were readily available[15]. If, for example, Fizeau used an astronomical telescope he used well-established technology and avoided problems with the actual manufacture of new instruments. The importance of this will become especially clear in comparison with the process of developing the mechanical part of the apparatus, discussed in the following section.

5. *Special manufacture: The mechanical system*

The technical implementation of the experimental demands is much more straightforward with the mechanical set-up than it was with the optical. Fizeau was wealthy enough to commission one of the best instrument-maker in Paris, Gustave Froment, with the construction of the mechanical devices. After having delivered these to Fizeau in May 1849, he built another set for the physics laboratory of the Ecole polytechnique[16]. Nothing is known about the whereabouts of Fizeau's apparatus, but the contemporary duplicate still exists at the Ecole polytechnique and served as the basis for the reproduction. The apparatus has apparently been modified slightly for its purpose in education, but Cornu's use of it in 1872 leaves no doubt that it is suitable for research (Cornu 1874a).

The construction of the toothed wheel depends significantly on its purpose in the actual measurement process. It has been stated that it is favourable to operate at a high order of extinction or reappearance. Hence the number of teeth crossing the light beam per second, i.e. the product of revolutions per second of the toothed wheel and of the number of its teeth has to be optimized. Froment's apparatus is a wheel 15.4 cm in diameter, carrying 720 teeth, each 1 mm long and only 0.35 mm wide. The whole wheel is made of 1 mm brass plate. The spokes and teeth are bevelled in order to reduce air friction.

A special motor drives the toothed wheel. A rope carrying weights is wound around an axle. The weights drive the axle by slowly winding off the rope until they reach the floor. The rotation of the axle is transmitted by a set-up gear with a ratio of transmission of 500:1.

Fizeau's motor had a brake to regulate the speed. This is missing from the device at the Ecole polytechnique and our attempts to rebuilt it according to a drawing failed[17].

The actual measuring device is a small counter, which can be attached to the toothed wheel axle, when it is tuned to a certain frequency. It is detached after a certain period of time. Obviously this time has to be measured, too, for which purpose the use of an acoustic clock beating the second seems most likely[18].

Each part of the machine has been designed and built for the particular demands of Fizeau's method, resulting in a single-purpose special apparatus. It has been crafted employing the most recent developments in machine building, for example helical teeth for continuous and smooth running[19].

So far I have described the apparatus "at rest", but the more important features are dynamical ones. Now we have to examine the motor in motion, which was achieved with the replica (shown in Fig. 3).

The limit for the weights to be suspended from the rope is given either by the stability of the gear unit or by the force needed to wind them up with the appertaining crank handle[20]. If too much force has to been exerted a disturbance of the delicate optical system is possible. I increased the weights up to 20 kg which also corresponds with Cornu's description of his experiments (Cornu 1874a, p. 142). In order to increase the torque on the driving axle, I rolled some metal foil around the axle before winding up the weights.

The free motion of the motor prepared like this has a direct bearing on the performance of a measurement, so it is interesting to know its behaviour.

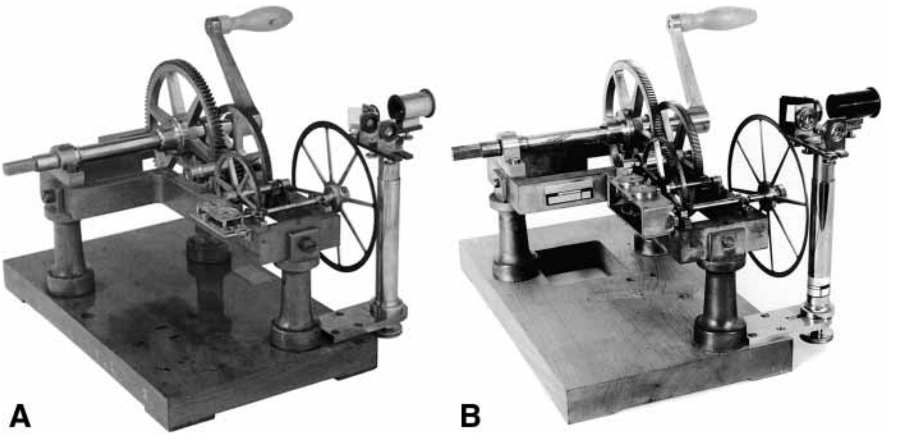


Fig. 3. Toothed wheel mounted at the motor, with crank handle, counter, half-mirror and eyepiece. Left: original device at the Ecole polytechnique; by permission of the Ecole polytechnique – Inventaire general, photo: D. Lebee. Right: rebuilt device at the Carl von Ossietzky University Oldenburg, photo: W. Golletz.

The motor left running freely accelerates the toothed wheel until it reaches its maximum frequency of 40.8 rps. For this acceleration three turns of the driving axle or 25 cm height of fall are used up[21]. Then nine turns (about 75 cm height of fall) are left while the toothed wheel turns in an astonishingly constant manner until the weights reach the floor[22]. With maximum speed, this takes 110 seconds. During this time the tuning of the frequency and the measurement itself have to be performed.

An important feature of the ensemble was found when the toothed wheel was taken off. Using a 2.7-fold smaller torque, the toothed wheel rotated at only 17.0 rps, but without toothed wheel the corresponding axle reached 120.0 rps! Thus the maximum rotation frequency is limited to a neglectable amount by friction losses in the gear unit but mainly by air friction at the toothed wheel itself.

The motor can be driven faster by use of the crank handle instead of the weights. Nevertheless I never went above 60 rps, where the motor runs very noisily and vibrates in a way, which I feared would destroy parts of it. At these frequencies taking measurements is impossible anyway since it takes both hands, one to turn the handle and the other to keep the motor and the table on the floor. On the other hand, the arrangement of the toothed wheel, the counter, and the crank handle does not allow the assistance of a helper.

These findings, technical and detailed as they may seem, allow some new appraisals of Fizeau's experiment. The maximum frequency reached by Fizeau (39.5 rps) tallies very well with mine (40.8 rps). This figure contrasts sharply with the frequency assumed to be possible in the pli cacheté. The value of 200 rps was missed by far, and Fizeau could only reach the second extinction. Strikingly, in his publication *after* his experiment it sounds much easier when he states

> Pour une vitesse double, le point brille de nouveau; pour une vitesse triple, il se produit une deuxième éclipse; *pour une vitesse quadruple, le point brille de nouveau, et ainsi suite* (Fizeau 1849a, p. 92; my emphasis).

Obviously, he does not describe the actual apparatus or his actual experiment but still only the method in principle.

If the toothed wheel itself instead of the motor mainly limits the speed of the wheel, it is worth noting that Froment indeed reduced the size of the toothed wheel against Fizeau's instruction given in the pli cacheté (the diameter from 31.8 cm to 15.4 cm; the number of teeth from 2000 to 720). But this step in the right direction was much too small. Froment certainly knew this having surely made some trials similar to mine. Furthermore he added an only 500-teeth wheel to the apparatus for the Ecole polytechnique, with which Cornu achieved a frequency 3.3 times higher than with the 720-tooth wheel, and later an even 6.6 times higher frequency with a 360-teeth wheel (Cornu 1874a, pp. 170–171).

With the benefit of hindsight, this argument can be elaborated. When Cornu performed his extensive measurements of the speed of light in 1874, he in fact reached the 20th order of extinction which Fizeau had mentioned in his pli cacheté (Cornu 1876, p. 200). This was partly due to the enlargement of the optical system which in principle was still like Fizeau's. Yet the main improvement was to make the toothed wheel much smaller, until it consisted of an at most 1/10 mm thick aluminium foil with only 150 teeth (Cornu 1876, p. 137). Cornu could turn this at 924 rps (!) after he had adopted a modified clockwork as a driving motor. Therefore among the factors, which determine the size of the toothed wheel, authority and loyalty between scientist and instrument-maker have to be taken into account as much as technical reasons in a proper sense.

We have now seen two different ways of materializing experimental requirements. The optical set-up, once understood and mastered, is very powerful. Fizeau was able to produce a light point across 8633 m instead of 7750 m designated in the pli cacheté. Against that, the mechanical set-up missed its promise. The fact that this had been specially manufactured for the demand does not necessarily imply that it is the best solution for the purpose. Still there are advantages of this latter strategy. It is noteworthy that it works at all – and even very well in the low-frequency range. Furthermore it requires only minor adaptations and its use is comparatively easy to learn[23].

6. *Lights in the mist: How to measure a physical constant*

Having discussed the optical and the mechanical part of the apparatus in some detail, I can now turn to the actual measurements. The discussion relies on Fizeau's compilation of all of his measurements in two tables (reproduced as Fig. 4 and Fig. 5; page 266 and 267). These are not to be discussed in detail here, yet some interesting inferences shall be made. I will rely occasionally on my experiment in order to illustrate the supposed experimental reality behind the data sheet.

First of all it is opportune to be reminded of the ideal measuring method: 1) The optical apparatus is adjusted until the observer can see the light point – usually at night. 2) The weight driven motor is regulated until the light disappears. 3) The counter is attached to the toothed-wheel axle for a certain time and is read afterwards. Turning now to Fizeau's tables, the margin note on Fig. 4 upper left reads:

> Dans le commencement de juin j'ai pu observer trois fois avec le disque No. 1; mais la scintillation était trop forte et l'éclat trop variable [;] j'ai pu seulement constater l'extinction et la réapparition [;] le 26 et le 30 juin et le 3 juillet les circonstances étaient très favorables [;] j'ai pu compter les tours, ...[24].

It is thus clear that the tables comprehend the whole measuring period of about five weeks.

Let us focus on June 26 as the first day when data could be produced. The first two columns show that Fizeau did not measure *the* frequency where the light point disappears, but one frequency at which it disappeared and one at which it reappeared. Hence the light point was invisible in an extended *range* of frequencies. This becomes intelligible when the fact is borne in mind that light perception requires a certain contrast in brightness between object and background. When replicating Fizeau's experiment near Oldenburg, I feared that it could fail because artificial light sources nowadays never allow for a black sky at night[25]. But problems were more banal. Background luminance was in fact a major problem, but caused by two different pieces of the apparatus itself.

First of all, the half of the light blocked by the toothed wheel yet on its way there was partly reflected into the eyepiece though the toothed wheel was blackened as far as possible with soot.

Secondly, dust and surface flaws on the double glass plate, receiving the still undiminished light directly from the light source, caused overlapping reflections. As indicated in section 4, Fizeau used two thin microscope cover glasses to introduce the light from the source into the emitting telescope. Now in microscopy a clean surface is not as necessary as in optics. However, the analysis of the principle *half-mirror* elucidates that thinness is important if one wants to use all four surfaces. Using only one side of a thicker plate meeting optical requirements would faint the light point too much.

This is because the light source is already at its limit. Fizeau has never regarded the light source as part of the apparatus and just mentioned "une lampe" (Fizeau 1849a, p. 92). However from Foucault we know that he used "Drummond's lime light", i.e. a small ball of calcium oxide made white-hot by a flame nourished with oxygen[26]. It is not certain whether he used ether (according to Foucault 1854, p. 142) or hydrogen (according to Foucault 1849) as fuel. However, in either case this was the brightest light source available at that time apart from electric arc light which was too difficult to handle for that purpose[27].

So there was no reasonable alternative to microscope cover glasses. Dust could be avoided by using brand-new plates each day but light reflections from obviously manufacturing-caused surface flaws remained. These become less disturbing if measurements are performed at twilight, but after all background luminance *never* allowed measurements of only *one* frequency of extinction. This might explain the wide variation of Fizeau's values in column 1, Fig. 5 (between "10,2" and "14,3"): These are frequencies somewhere in the *range* of extinction.

Still these measurements were taken at the best atmospherically conditions Fizeau ever had (Fig. 5, upper left):

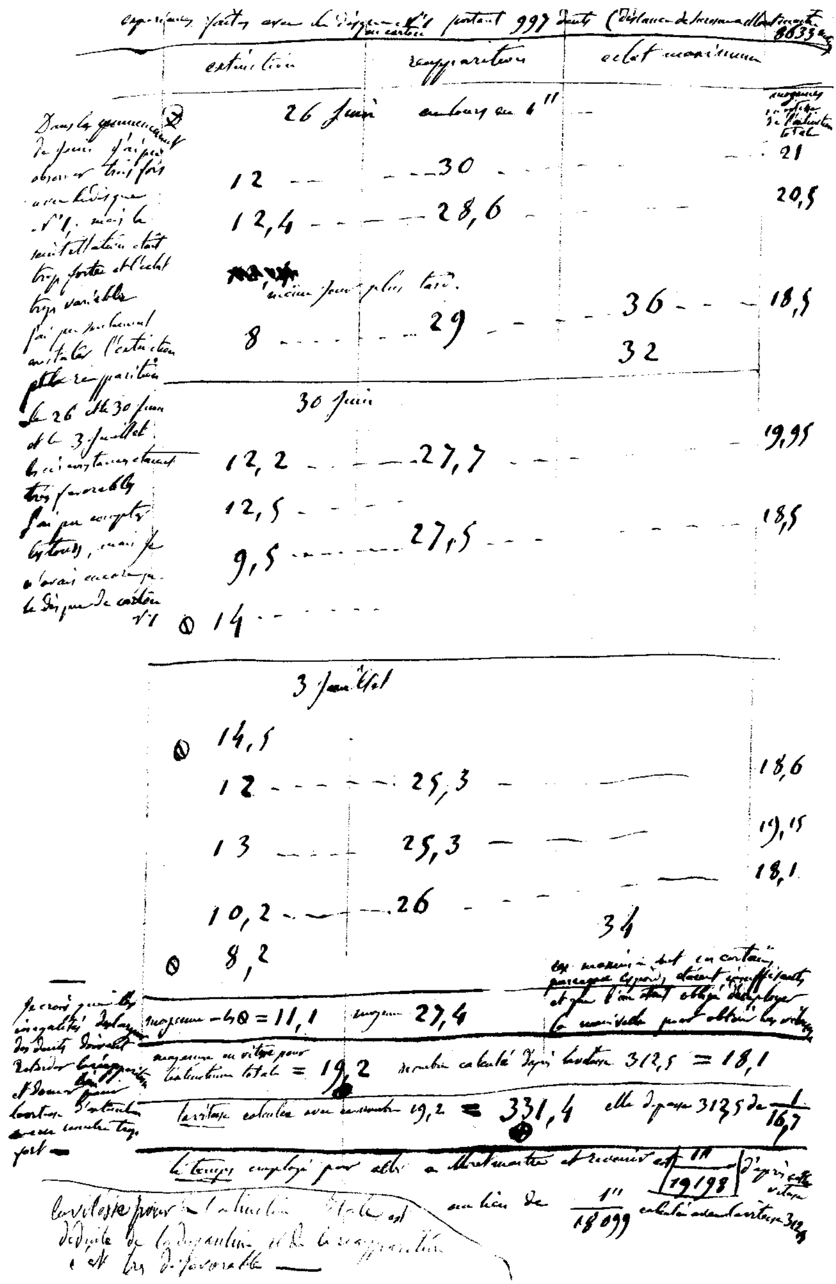
> très clair – il y a à peine de scintillation

In fact these are exactly the conditions which are necessary for good measurements. The necessity of clear air is easy to appreciate though it should be noted that a very good distance view is rare. Fizeau made a virtue of necessity when under conditions of quite bad vision he determined the frequency when the light point was visible at all (Fig. 5 left, bottom third):

> temps brumeux [;] on ne peut observer avec quelque précision que le maximum

The other requirement "à peine des scintillations" is a problem because the light coming back is not isotropic but a cone with a very small angle of aperture. Thus even a small difference in atmospheric pressure causes a slight diffraction of the light beam which is enough to deflect it from the target. It is quite normal that the light point flickers strongly, measurements cannot be restricted to the very rare times when it is stable[28].

This is different from observing stars, though otherwise the measuring


expériences faites avec le disque n°1 en carton portant 997 dents (distance de Suresnes à Montmartre 8633m)

extinction	reapparition	éclat maximum	moyenne vitesse de l'extinction totale
26 juin	environ en 1''		
	30		21
12			
12,4	28,6		20,5
8	29	36	18,5
		32	
30 juin			
12,2	27,7		19,95
12,5			18,5
9,5	27,5		
14			
3 juillet			
14,5			
12	25,3		18,6
13	25,3		19,15
			18,1
10,2	26	34	
8,2			
moyenne -40 = 11,1	moyenne 27,4		

moyenne en vitesse pour l'extinction totale = 19,2

la vitesse calculée avec ... 19,2 = 331,4 elle dépasse 313,5 de 1/16,7

le temps employé pour aller à Montmartre et revenir est 1''/19198 d'après cette vitesse au lieu de 1''/18099

Fig. 4. Compilation of Fizeau's measurements of the speed of light in summer 1849, first sheet. By permission of the Musée René Sordes de Suresnes – France.

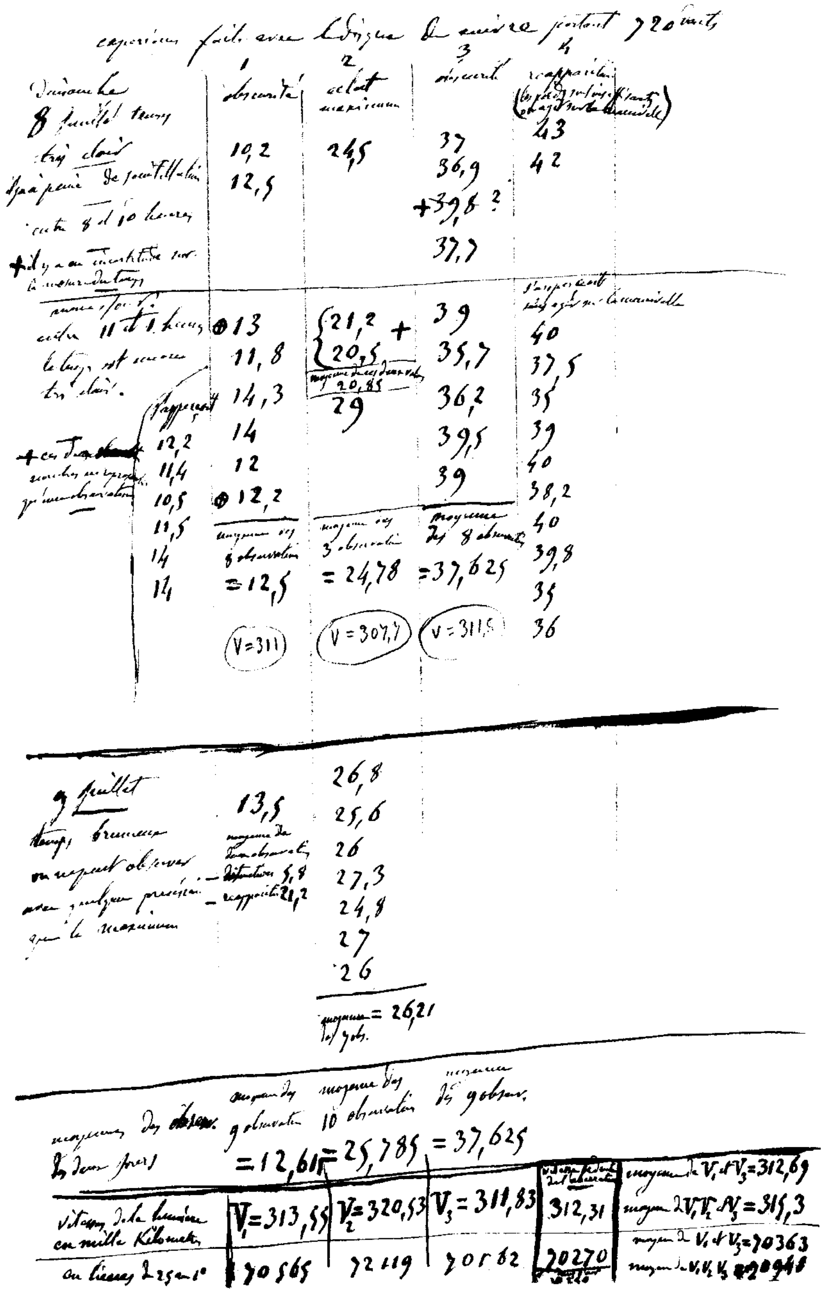


Fig. 5. Compilation of Fizeau's measurements of the speed of light in summer 1849, second sheet. By permission of the Musée René Sordes de Suresnes – France.

procedure resembles astronomical practice very much: Observing light points with telescopes at night, estimating their brightness, and determining times with the aid of a regular acoustic signal. This might lead to the mistaken assumption that cloudless nights so longed for by astronomers are also valuable for this experiment. On the contrary, and also according to my experiences, Cornu writes:

> Ainsi les nuits où le ciel est absolument découvert, nuits si précieuses pour les astronomes, ont toujours été mauvaises pour mes expériences. L'horizon était embrumé, la lumière de retour faible et onduleuse; au contraire, les nuits où le ciel restait couvert de nuages, surtout après une journée pluvieuse, ont généralement été satisfaisantes, (Cornu 1876, p. 172).

This fact might be explained by the previously mentioned refraction caused by differences in the air pressure which are more likely to occur when the temperature changes significantly between day and night, resulting in exchanges of air between ground and atmosphere. However, it takes some time until one is able to make a good guess if the weather is worth spending a night in the measuring station or not[29].

These difficulties easily explain why Fizeau produced data on only five days over more than a month. In fact, according to my experiences, he (and his apparatus) must have been prepared for measurements on each day to catch even four days with passable conditions!

So far weather problems and limitations of the mechanical devices have been stressed. This could lead to the mistaken conclusion that the experiment in Fizeau's set-up might be useful to demonstrate the practicability of the method but not to improve the knowledge of the value of the speed of light. But this is not the case. All depends on developing measuring methods suitable to cope with these difficulties, or even to profit from them.

The completed measuring equipment does not determine its application. Of course nothing else can be done meaningfully with it than to measure the speed of light, but this can be done in various ways. Fizeau started this. When he realized that the light point vanished for a range of frequencies rather than for one single frequency, he determined the limits and took the mean value. Or he tuned in to any frequency in the range and profited from the accidental distribution among a large number of trials. When the light point was hardly visible, he determined the narrow range of visibility around the maximum. When the weights were insufficient, he turned by hand and tried to keep the frequency constant by listening to the sound of the motor. There are almost no limits to invent more methods.

The difficulties mentioned here are genuinely practical ones, they cannot be deduced from the theoretically ideal method as it has been explained in section 2 and summarized at the beginning of this section. Therefore the way to deal with these difficulties must be a practical way. Sticking to the ideal method is not advisable. This is of course not to say that this way to react to difficulties is irrational. On the contrary, creating solutions for practical problems, which are neither arbitrary nor determined by the apparatus, is the art of experimentation. So in short "best" does not mean "closest to the ideal" but "fitted to unchangeable conditions". These methods lead to valuable results as will be discussed in the next section. It seems unreasonable to wait for the "golden night" with optimum conditions. Nevertheless a great deal of patience is needed anyway, more than Fizeau was willing to invest, as will be stated now.

7. *Fizeau's success*

Fizeau's value of 315,300 km/s for the speed of light drawn from "vingt-huit observations" is in moderate accordance with the now valid value of 299,800 km/s (rounded to full hundreds of km/s), it deviates by 5.2%[30]. However this might be an unwarranted comparison. To introduce a new method in order to improve the knowledge of the value of a physical constant comprises two steps. First of all you have to prove the aptitude of the new method which best can be done by replicating the current value. Later you have to diverge from this, claiming to be closer to the true value. During this second step, any assumed value must not play a role any more. Even more, from the beginning it must not have played any role. A method providing deviating values right from the start is dubious whereas a method providing values very close to the current value until the end is superfluous.

This is a complex epistemological problem, which shall not be discussed here, but it is also a practical one. For Fizeau's case, it can be reconstructed very well.

Which value should he use to check his method? The speed of light could at the time be gained from astronomical quantities, but there was no commonly accepted value of the speed of light in 1849. It significantly depended on whether someone preferred the determination of the time which light needs to travel the radius of the earth's path around the sun (observing the Jovian moon Io) or the determination of the constant of aberration (observing the apparent shift in star positions according to the direction of the observer's motion). Either of these had to be combined with the solar parallax, mostly gained from observing transits of Venus across the sun. Taking into account the most respected, i.e. the most cited determinations (Delambre's 493.2 seconds of time for the light-travel time, Struve's 20.4451 seconds of arc for the constant of aberration, and Encke's 8.5776 seconds of arc for the solar parallax) this leads to values for the speed of light of 311,100 km/s for the first method and 308,100 km/s for the second[31]. Thus Fizeau's value deviates only by 1.4% and 2.3% respectively.

However, these determinations were not the ones most esteemed by Fizeau. Among his papers, an undated enumeration of a variety of these astronomical constants and values of the speed of light according to a number of authors exists. Fizeau made up his mind in a somewhat strange way, taking the mean of the values of Delambre, Lindenau, and Struve: 312,310 km/s[32]. Without changing anything at Fizeau's experiment, its result is improved now to only 1.0% deviation.

Fizeau takes seriously his own assumption when he negotiates with himself whether to include the bad weather measurements of July 9 or not (see the bottom of Fig. 5). The number of "312,310" appears here again as the reference. He finally chose to include the mentioned values since 315,300 km/s (1.0% deviation) obviously seemed good enough to demonstrate the usefulness of his experiment. However, excluding the data of July 9 would leave only 18 values to calculate the mean from, the results of June 26, June 30, and July 3 being out of the question. Or did the mean value from these 18, namely 312,690 km/s, look simply too good to be true since it missed Fizeau's assumption by only 0.1%? Be that as it is, it is clear that Fizeau convincingly took the first step, but avoided to take the second as can be deduced from his explicit characterization of his value as

> ... peu différente de celle qui est admise par les astronomes (Fizeau 1849a, p. 92).

Note that *in public* he does not mention *which* values of *which* astronomers, obviously wanting to evade discussions about numbers at this stage. Fizeau's experiment is a demonstration of a new method, but its result is not meant to raise a discussion about the true value of the speed of light.

As a possible reason for Fizeau's reserve it could be assumed that his apparatus did not allow determinations of at least the accuracy of the astronomical methods. But this is not right. My main argument for this inference is my own experiment[33]. I used methods like Fizeau's, for example determining the limits of the extinction interval (result: 303,500 km/s; estimated maximum error: ±2%), measuring a number of accidentally distributed frequencies within the extinction interval (303,000 km/s; ±2.5%), determining the maximum (303,400 km/s; ±4%), or applying the handle to reach the second extinction (result: 296,200 km/s; ±3%). Furthermore, in a manner described in the previous section, I developed some more methods as for example determining the limits of visibility by transgressing them in small steps and making decisions whether the light point can be seen or not (result: 301,600 km/s; ±2%).

Why are my results so much "better" than Fizeau's? Surely my apparatus is not better than his. Furthermore there is no reason to believe that I am a better physicist than Fizeau was. Possibly I was luckier with the weather conditions. A strong relativist could of course object that my results are in fact not better than Fizeau's since I approached *my* assumed value to roughly 1% deviation whereas Fizeau meets *his* value with similar precision. I would not go that far, and I pertain that the apparatus does indeed allow determinations better than the astronomical values. Surely an experimentalist has always means to reconcile the empirical data to his idiosyncratic value, but why should he do this, if he wants to *change* the perceived value? My results – all below 304,000 km/s – suggest that any seriously attempted deviation from Fizeau's figure of 312,310 km/s should go at least in the right direction.

In contrast, when Léon Foucault measured the same constant with an improved rotating-mirror apparatus in 1862 (see sections 8 and 9), doubts regarding the perceived value had been uttered directing to a somewhat smaller value. This forced him to engage in the discussion and dare to announce his value of only 298,000 km/s as a seriously meant result (Foucault 1862).

With regards to Fizeau, there is only one solution: He did not *want* to establish a new value. One possible reason is easy to see. The advantages of a (physical) experiment against an (astronomical) observation are that conditions can be controlled more extensively, that experiences can be fed back into the experiment immediately, and that many measurements can be taken allowing the application of statistics. However, these advantages in principle have to be realized, and the last section gave some hints what this might mean for Fizeau's experiment. At least you need *several* days with excellent conditions, conditions which Fizeau had only once in five weeks. Cornu in 1874 patiently endured this annoying waiting every evening (Cornu 1874b, p. 1363). Like Fizeau he started measuring in June, but he continued until September, and in the end he threw away all data of the first three months (Cornu 1876, p. 172)!

The comparison with the expectations expressed in the pli cacheté shows that Fizeau's intention changed substantially during the experimentation. Possibly in the meantime he thought that he *could* not establish a better value than the astronomers, in the end however he did not *want* to. Instead he started to adopt his method for measuring the speed of electricity together with Gounelle and he continued his work with the rotating mirror (see section 9).

This is not to say that the experiment was no success. Quite the reverse, it was received enthusiastically as a demonstration how to measure the speed of light accurately in the future. Fizeau and Froment were created *chevaliers* of the *Légion d'honneur* in November 1849 and Fizeau was awarded the *Prix triennal fondé par Napoléon III* of the Institut de France on 1856 July 9 (Tobin 1993, p. 267 and Maindron 1881, p. 135, respectively). The most important success can be seen in the decision of the Académie des Sciences from 8 April 1850 to build a new device on its own costs according to Fizeau's experiment[34]. Yet this project languished for various reasons and was given up completely when the instrument-maker Froment died in 1865 (Tobin 1993, p. 267).

Still it seems strange that Fizeau stopped measuring so early. He was the first who had developed a technique to experimentally determine this important constant, so why didn't he exhaust its possibilities? Some remarks on this question shall be added in the following section.

8. *The importance of the speed of light*

The date of Fizeau's experiment belongs to a rather tranquil period concerning the development of the theory of light. It can be characterized as "normal science" in a Kuhnian sense[35]. The speed of light was accepted as a universal physical constant, in particular independent of the direction of propagation, of the wavelength, and of the distance travelled. Furthermore, its independence of the nature of the light source was so well established that Fizeau did not even mention the remarkable fact that he was the first who had determined the speed of the light of a terrestrial, artificial, and small source.

Within the then dominant wave theory, the speed of light is found as the speed of mechanical ether waves, but its precise value was obviously uninteresting. Contemporary textbooks on the wave theory did not even mention its value let alone descriptions of practical determinations or even calls for more accurate values[36]. Subsequent developments in physics which gave this constant a more important position as for example Weber's and Kohlrausch's experimental connection of the electrical and magnetic units (Weber and Kohlrausch 1856) or Maxwell's ether theory had not yet come.

Seen from a physicist's perspective, the main achievement of Fizeau's demonstration was the possibility to provide the means for a determination independent of astronomy. As Foucault wrote in December 1849:

> Non seulement M. Fizeau a prouvé la propagation successive de la lumière à la surface de la terre, mais il a créé une méthode pour mesurer sa vitesse avec une précision inespérée, et qui sans doute ne le cédera en rien aux méthodes astronomiques dont nous avons fait mention (Foucault 1849).

More far-reaching than that, the relation could be reversed if the toothed-wheel method could be made more accurate than determinations of the light-travel time, the constant of aberration, or the solar parallax. In this case, one of these could be *calculated* from the speed of light. A substitution of solar-parallax determinations, which required simultaneous observation of astronomical events from distant points on the earth, could be seen as especially desirable.

> En répétant ces observations avec des appareils mécaniquement plus parfaits, on pourra un jour, sans sortir de Paris et de sa banlieu, trouver cette parallaxe du Soleil qui, vers le milieu du siècle dernier, donna lieu à des voyages si long, si lointains, si pénibles, et à tout de dépenses (Arago 1857, vol. III, p. 418).

The solar parallax plays a central role in astronomy as it is equivalent with the distance between the earth and the sun which passes for a kind of a natural scale in the universe. Still not every astronomer was as enthusiastic as Arago. Airy for example did not even mention this method in 1857 when he discussed possibilities to determine the parallax in the future (Airy 1857). Todd regarded the physical method as reliable, but only after Foucault's measurement of 1862. In his eyes, Fizeau's attempts

> ... hardly signify more than the completion of the first great step of proving the determination to be a physical possibility (Todd 1880, p. 60).

Le Verrier had promoted Foucault's determination in order to be applied to the parallax problem (Foucault 1862). Le Verrier had reached at a somewhat higher parallax (8.95 seconds of arc) from re-calculations of the sun-earth-moon system, corresponding to a speed of light of only 295,200 km/s using Struve's constant of aberration (Le Verrier 1858). This would be and in fact was supported by a lower value of the speed of light (298,000 km/s) measured by Foucault.

Babinet praised Foucault for having provided a solution for a two hundred years old problem (Babinet 1862). Though one has to take into account the meanwhile prevailing rivalry between Fizeau and Foucault, it is striking that Babinet did not mention Fizeau's experiment at all. Le Verrier also made Cornu's large-scale experiment of 1874 possible, just before the long awaited transits of Venus. However, Cornu's experiment was not generally seen as a substitute for the great expense entailing observations of Venus on 1874 December 9[37].

To summarize, there was no need for an accurate measurement of the speed of light in 1849 and the following years from physics and at most general interest from astronomy. Before 1849, such a determination was regarded as simply impossible anyway[38]. Fizeau's experiment was seen as a piece of technology meant to direct further technological development rather than for immediate application. Since technology in general was seen as easily and endlessly improvable, one could afford to dispense with the exploitation of the already available. This kind of technological optimism is expressed by Cornu:

> On comprend sans peine que les dispositifs optique et mécanique soient susceptibles d'une précision dont la limite peut être poussée extrêmement loin.

And after having proposed a method to reduce physiological deficiencies,

> ... la méthode peut donner théoriquement une précision illimitée malgré l'intervention d'organes faillibles (Cornu 1876, p. 6).

Fizeau's measuring device thus became obsolete in the moment of its completion. I argued that this was because of Fizeau's intention and not due to technical insufficiencies. Hence one question remains. If Fizeau finally did not apply his technology for the production of a better value of the speed of light, why did he start to develop such technology at all? I can provide no full answer, but some hints on this shall be discussed in the final section.

9. How experiments begin

Sometimes it is as interesting to know how experiments begin as it is to know how they end, to paraphrase Peter Galison's well-known booktitle (Galison 1987). Historical case studies of the ends of scientific experiments, inquiries, or even controversies are valuable material for discussions about the epistemological status of scientific knowledge. As such they have been widely used in the recent years[39]. However the beginnings of experiments have been studied to a somewhat lesser extend, although they provide material for another important problem, namely the continuity of scientific practice. The question is simply: How much does what someone does depend on what he/she or others have done before?

Clearly, scientific research is creative and open-ended yet by definition and it is thus never fully determined. Still there are several factors which influence the whole of scientific practice including the very content of an experiment.

To show factors which influence the course of a research project is important because it might lead to insights into which degree science can be governed, if the outcome is predictable in any sense, and so on. Maybe they are so intertwined that one ought to avoid talking of beginnings and ends of single experiments at all.

The following final remarks on Fizeau's experiment are intended to show that close examination and someone's own practical involvement may enable one to see even a new type of continuity.

The short-lived speed-of-light experiment appears strangely isolated; it seems to oppose to be set in a context. Let us pick out just two factors: First, the aim of the experiment: I argued in section 8 that there was no urgent need for a better knowledge of the value of the speed of light. In particular it is simply irrelevant for the theory of light. Second, technical opportunity, was the experiment just the application of a well-mastered apparatus? Obviously not. It took Fizeau some reasoning and tinkering to develop the optics and some money to order the mechanics. None of the devices employed in this experiment had been used by Fizeau before[40].

Yet some links can be shown, namely to an experiment Fizeau was working on at the same time. This temporal superposition has never been regarded as anything else than a mere parallel. The experiment in question is the rotating-mirror experiment aimed to finally decide between the wave theory and the emission theory of light.

Arago had proposed this experiment in 1838, adopting the rotating-mirror idea from Charles Wheatstone (Arago 1838). Arago started to implement his idea in cooperation with Louis Bréguet in the early 1840s. When his sight diminished as a consequence of his diabetes, he allowed Fizeau and Foucault to continue his experiment (Arago 1850). These two collaborated for some time as they had done frequently before until they separated forever for unknown reasons in the beginning of 1850 (Cornu 1898, p. 11). Fizeau continued his work with Bréguet while Foucault gained Froment as a partner. Both teams succeeded in spring 1850, applying different technology for driving the rotating mirror, but using an almost identical optical layout. Foucault announced his result in favour of the wave theory on May 6, and Fizeau and Bréguet reported the same result six weeks later (Foucault 1850; Fizeau and Bréguet 1850a and 1850b).

In order to see the connections between the speed-of-light experiment and this one, I have to explain briefly the main of Arago's ideas. An electric spark produces two short light flashes simultaneously. One of the light rays passes through a water-filled tube, the other through the air (Foucault 1854). Both rays hit the surface of a rotating mirror, with a minuscule time delay between each other. According to the then dominant conception there was an unequivocal relation between the theory of light and the speed of light in optically dense media. According to the wave theory, light should travel slower by a factor of 4/3 in water whereas according to the emission theory, it should move faster in water by the same factor. Therefore to decide in which medium light travels faster was seen as an incontestable decision between the rival theories. Though everyone had been convinced of the correctness of the wave theory for some time, Arago pronounced:

> Il [le système d'expériences] tranchera *mathématiquement* (j'emploi à dessein cette expression); il tranchera mathématiquement une des questions les plus grandes et les plus débattues de la philosophie naturelle (Arago 1838, p. 954; Arago's emphasis).

The central idea of Arago's experiment was to transform a minuscule time difference into a spatial one. When the mirror is turned the light arriving on its surface first is reflected in a slightly different direction than the light arriving second. Hence observing a spatial deviation between both leads to an at least qualitative comparison of the speed of light in water and in air.

It is easy to imagine that the practical realization caused trouble. The immense speed of light necessitates a long tube and a very rapidly spinning mirror (Arago assumed necessary 14 m and 1000 rps respectively) (Tobin 1993, p. 260). However the main problem, which prevented success for Arago, was of conceptual kind. The position of the mirror when the double flash hits its surface is accidental and so is the direction of the reflection. An observer positioned at the circumference and equipped with a small telescope mostly sees nothing because it is only by rare chance that the double flash is reflected exactly in the direction of the aperture of his device. Arago proposed to install several observers armed with telescopes around the rotating mirror, but still with unsatisfactory result (Tobin 1993, p. 260).

How did Fizeau and Foucault solve this problem? The crucial idea was to reflect the light back onto the mirror, so that it is sent back towards the light source. A fixed mirror placed in a certain distance of the rotating mirror provides the reflection. Obviously the light diverges if a plane mirror is used. To prevent the inevitable light loss they introduced a lens between the light source and the rotating mirror which pointed the light source onto the fixed mirror (via the rotating mirror). This is the principle of *focus-reflection.* A continuously shining one can now replace the light source sending out short pulses. The light ray hitting the rotating mirror is then reflected continuously by the latter, and it scans the whole circumference. Most of the light is lost, only when it hits the fixed mirror it is sent back to the light source via the rotating mirror. The observer at the light source therefore perceives a rapid sequence of light flashes. When the rotating mirror turns with at least 10 or 15 rps the light appears to be continuous, which is an example of the *stroboscopic effect.* Obviously the observer cannot be placed in front of the light source, so the light must be introduced laterally by a device which reflects part of the light along the main optical axis and transmits part of the light on its way back. The ideal device would be a *half-mirror.*

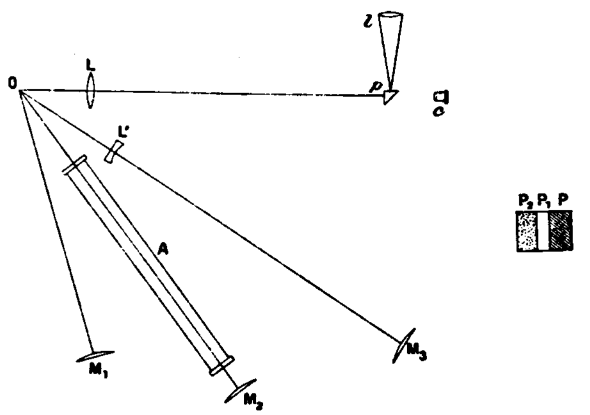


Fig. 6. The optical sytem used by Fizeau and Bréguet in 1850, taken from Mascart (1893); Photo: Niedersächsische Staats- und Universitätsbibliothek Göttingen.

This arrangement has significant advantages (see Fig. 6). The tube can be placed between the rotating mirror and the fixed mirror, which doubles the effect, because the light passes it twice. In fact *two* fixed mirrors were installed on the circumference at arbitrary positions, one for the water path through the tube, and the other for the air path without tube (either M_1 or M_3 is used). If the central mirror is driven with sufficient speed, the observer sees the light source twice. Both images are shifted in the same direction, but one more so than the other. The main advantage is of course that the observer knows where to install himself/herself, and he/she is able to observe calmly. Even several observers can judge the same event.

The overlap between the rotating-mirror experiment and the toothed-wheel experiment is now obvious in the form of these principles: *focus-reflection*, *stroboscopic effect*, and *half-mirror.* The link between both experiments is more than a mere temporal coincidence. Each of these principles occurred in both experiments and was decisive for its success. The direction of the transfer of these ideas is not clear. Was the toothed-wheel-based speed-of-light measurement a by-product of the efforts with Aragos rotating-mirror experiment? Or did the Arago-experiment wait for Fizeau's speed-of-light experiment to be crowned with success? It is even uncertain if Fizeau consciously made the transfers. Still the correspondence is undeniable.

It is important to note that both experiments are still basically different if seen from other perspectives. Let us focus, for example, on the aims of the experiments. Both deal with the speed of light, but in different ways: One experiment is important for fundamental physical theory, making use of the speed of light only as an aid. The absolute value of it is irrelevant in this respect. The other is a measurement in a proper sense. The result of the determination of this uncontested physical constant had no bearing on theory.

Similarly there is hardly any intersection with regard to technical devices. The *focus-reflection* is achieved by the lens of the emitting telescope and a freestanding mirror for the light-theory experiment, and by a telescope for the speed-of-light experiment. The *stroboscopic effect* results from a mirror covering only part of the circumference of a rotating light beam here and a toothed wheel chopping a light beam there. And finally the *half-mirror* consists of a small prism here versus two microscope cover glasses there.

10. Conclusions

Fizeau's speed-of-light experiment provides a valuable example for the implementation of a new scientific measuring technique. I pointed out that the speed of light was an uncontroversial entity at the time of Fizeau's experiment, provoking only little interest in its precise value. Since it was hardly bound to theory, it could be seen as "just" a measurement, at least from a proper science perspective. As such, one might assume that the quality of the result mostly depends on the technological state of the art. My analysis shows that things are more sophisticated. Certainly the mechanical part rely on what was possible in machine building, though talking of the "state of the art" is somewhat inappropriate with respect to a device nobody had any idea of only half a year before. However, the analysis of the optical system reveals that this part is hardly limited by instrument-makers' abilities. All single pieces were easily available standard devices. The crucial factor to create such a powerful technique was Fizeau's ingenious combination of devices from previously unrelated uses.

It turned out that this kind of technology transfer necessitates theory. Not "high theory" about some entity of nature, but a purpose-bound experimental theory, developed in order to make use of devices for some new application. I argued that building such theories is an essential part of an experimentalist's work. This has some consequences for the historiography of experiments. Historians of science have rightly begun to include remaining apparatuses as source material. But the examination of devises *besides* the examination of experimental concepts is often insufficient. Both have to be linked together. An experimental concept only *works* if it realized by appropriate devices, while devices only *make sense* when they are used for an experimental concept. This is for the scientist as much as for the historian who tries to understand the scientist's experimental practice. To achieve this, one has to uncover the inextricable links, for instance between an ordinary telescope, a sketch like the one shown in Fig. 2 and the experimental part goal "faire revenir le rayon" as expressed in the pli cacheté. Each item stays mute if examined separately. Replication is one way to (re-) construct these links, because they are necessary for a successful attempt to repeat the experiment.

This unpredictable and open-ended interaction between the behaviour of the apparatus, circumstances and the experimentalist's ideas and actions also takes place if the completed apparatus is used for measurements of the speed of light. My experiences correspond with Fizeau's annotations on his data sheets in the necessity and the possibility to invent different measuring methods. Improvements of a technique do not necessarily have to concern the material parts. There is a scope of action even with such a sophisticated, specialized set-up.

This is important for the appraisal of Fizeau's published value of the speed of light. My results obtained with the replication show that one should be cautious not to jump to conclusions and explain moderate (in our terms) results by insufficiencies of the technical equipment. Fizeau's case shows that intentions have to be taken into consideration, too. Though it might seem strange for us that Fizeau did not *want* to do what he *could* have done with his apparatus, this is the most plausible inference from the textual sources and the replication.

If principles like the *focus-reflexion* are as important for success as the experimental aims and technical devices, it is interesting how these are handed down. The demonstration of a transfer of such principles between the speed-of-light experiment and the nature-of-light experiments should be seen as a first step of such an analysis. Closely examining previous and simultaneous uses of technology by himself and others may better elucidate the role of creativity and technology in the progress of Fizeau' experimental program.

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NOTES

1. It should be noted that neither Rømer nor Bradley did in fact determine the speed of light. The astronomical phenomena discovered by these two were used for this aim only later. See Wróblewski (1985) and Sarton (1931), respectively. The new definition of the meter, anonymously announced in: *Comptes Rendus de la Conférence Générale des Poids et Mesures* (1983), p. 97, is discussed in Petley (1983). This definition fixes the speed of light to 299792458 m/s.
2. The best and most comprehensive description of Fizeau's and other's experiments related to the speed of light is Tobin (1993). The rare biographical information can be found in Coruu (1898).
3. Some contributions include Hacking (1983), Collins (1985), Shapin and Schaffer (1985),

Latour (1987), Galison (1987), Gooding et al. (1989), Pickering (1992), Pickering (1995), and Heidelberger and Steiule (1998).

4. In the following referred to as "Fizeau, notes". These belong to the *Collections du Musée René Sordes de Suresnes – France.* Some of these are reproduced in this article with kind permission of Marie Pierre Deguillaume. I thank Christine Blondel (CRHST) for her help to find these papers.
5. For discussions of this historiographical method see Pestre (1994), Voskuhl (1997), Hering (1998), and Rieß (1998).
6. A drawing of the house in Suresnes can be found in Poussin (1931, p. 112). The location of the mirror station is only vaguely indicated in Fizeau's paper as ''sur la hauteur de Montmartre" (Fizeau 1849a, p. 92), but specified by an anonymous author in the *Revue scientifique et industrielle* as a "lucarne du télégraphe de Montmartre" (Fizeau 1849b, p. 394).
7. In the following referred to as "Fizeau, pli cacheté". The pli cacheté which is entitled: "Description d'un procédé propre à constater la vitesse de propagation de la lumière" is in possession of the Archives de l'Académie des Sciences, Paris. For a general account on the practice of the deposit of plis cachetés see Berthon (1986).
8. Fizeau was right in this assumption, and accordingly he applied his ''semi-public" handwriting. Having a very nice handwriting for letters and the like, he used no particular care to the appearance of the inside of the pli cacheté. However, the text can almost completely be deciphered which is not always the case with his scribbled laboratory notes.
9. Regarding the optics, they are standard astronomical telescopes. Apart from the eyepiece they look the same. The correspondence of the diameter of the objective lens with Fizeau's indication and the special stand, which allows only horizontal observation, strongly refers to their use for the measurement of the speed of light. If they are not the originals (see section 5) it is highly probable that they are at least true copies of Fizeau's telescopes. I thank the Ecole polytechnique, especially Marie-Christine Thooris for the kind permission to examine and to measure the remaining devices. Furthermore I thank the staff of the workshops of the Oldenburg University for having built all of the required replicas, which as well resemble the originals very much as they work very well.
10. See Fizeau (1849a, p. 91). According to Cornu (1876, p. 129), Fizeau lend him a suitable device in 1874, but this has probably been built later as 1849 since it has no resemblance to some sketches among Fizeau's notes.
11. Latour (1987), for example, supports this conception of scientific theories, see in particular chapter 6.
12. Some properties of my mirror system coincide with Cornu's used in his preliminary determination of the speed of light between the Ecole polytechnique and the Mont-Valérien in 1872. Likewise some useful elucidation of the optical system could be taken from Cornu (1874a) and Cornu (1876). However, this kind of information is in no way sufficient to replace the experimentalist's own theoretical and practical work, if he/she wants to have control of the apparatus.
13. Several other replications have shown that the scientist could *not* have performed the experiment alone though the necessary helper had generally been concealed in publications. This for example applies for Joule's experiment concerning what is now called Joule-Thomson effect; see Sichau (2000). If the replication of Fizeau's experiment reveals that the alignment of the telescopes (and the actual measurements, too) can be

performed alone, this does not apply to the experiment on the whole. Many helpers were needed to install the apparatus, to organize appropriate measuring stations, to provide transport capacity, and for many preliminary experiments. The most substantial support I got from Dietmar Höttecke, Antje Kreisel, Bernhard Lange, Telsche Nielsen, Annemarie Rehahn, Sybil Rehahn, Falk Rieß, and Christian Sichau.

14. This is calculated with a refractive index of 1.52 (crown-glass in the yellow range) and with an angle of incidence of 45°.
15. There is no hint that Fizeau did astronomical observation or microscopy before, so he probably bought them from an instrument-maker or borrowed them from a friendly astronomer or biologist.
16. Letter from Froment to Fizeau, dating 1849 May 16 (Fizeau, notes). A description of the method and a drawing of the apparatus at the Ecole polytechnique can be found in Jamin (1887).
17. The drawing is printed in Arago (1857, vol. IV, p. 416). It is not very trustworthy since several details contradict to what is known from other sources, and it even contains some contradictions in itself.
18. According to the astronomer and historian of astronomy Klaus Staubermann (personal communication).
19. This has been presented and recommended in Calla (1843).
20. Against that, the driving axle is broad enough to use a rather thick rope, so that its strength is no problem.
21. This can be shortened for saving measuring time if one grasps the first toothed wheel to accelerate the motor by hand up to almost its supposed final speed.
22. Changes in the rotational speed are below the estimated measuring accuracy of 1% for time intervals of 6 seconds.
23. My laboratory notebook reveals 11 days of work concerning the mechanics (including all preliminary testing), whereas 34 days spent on the optics are documented.
24. The discque No. 1 was a temporary cardboard disc which was soon replaced by the brass disc.
25. Fizeau's parents' house, where he had installed his measuring apparatus does not exist any more and the light situation over Paris has changed significantly. Furthermore, though Oldenburg is not known to have good weather, I suppose that the difference to Paris is neglectable. This is on the assumption that temporal weather changes are much bigger than spatial differences in the mean climate. For these reasons I dispensed with replicating on the original scene and chose to experiment near my home town and in particular close to the technical facilities of the University's workshop. Nevertheless my measurements have been performed in the same season as Fizeau's. I thank the EWE AG for putting the Oldenburg water-tower to my disposal for the reflecting telescope and I thank the II. Oldenburgischer Deichband for their permission to install the measuring station on the dyke of the river Hunte in Moorhausen.
26. Thomas Drummond had developed this kind of light source for land surveying; see Drummond (1826).
27. For lack of detailed technical information and for safety reasons (at that time, hydrogen was contained in bellows made of rubber-coated silk) this part of the experiment could not be replicated. Nevertheless the main properties of an incandescent calcium-oxide ball have been determined experimentally, which enabled me to use an electric lamp with similar properties in the actual experiment.
28. A maladjustment of the mirror widens the cone of the returning light. As stated above,

this makes the light point fainter, but on the other hand it makes it more stable. A workable compromise has to be found by tinkering.

29. This makes it almost impossible to judge Fizeau's weather conditions from the reports published monthly by the Académie des Sciences (For June and July 1849: *Comptes rendus des séances de l'Académie des Sciences* 29 (1849), p. 36 and p. 192, respectively), especially since only the state of the sky at noon is documented.
30. A recalculation showed that in fact he used only 27 values, having omitted the queried value "39,8" in the third column of figure 5. The rounding-off allows interchanging values of the speed of light in vacuum and in air, the latter being about 90 km/s smaller for ordinary pressure and humidity.
31. These are taken from Delambre (1817), Struve (1844), Encke (1827), and Encke (1835).
32. It can be reconstructed that Fizeau assumed a parallax of 8.5 seconds of arc and an equatorial radius of the earth of about 6374500 m. With these data the value assigned to Delambre ("313,75") can be retraced to his light-travel time of 493 seconds of time (Delambre 1817). The same applies to Lindenau's value which surely has been taken from Lindenau (1843). Inexplicably, the value assigned to Struve ("312,42") does not fit to his well-known constant of aberration (Struve 1844).
33. The discrepancy between Fizeau's result and today's value poses the problem what shall be meant by "as similarly as possible" with regards to the replication. There are at least two options. First of all, I could try to do every single chore like Fizeau which should in the ideal case lead to Fizeau's result. Secondly, I could try to adopt Fizeau's aim, namely to measure the speed of light as well as possible with the given apparatus. This should ideally lead to a value dependent on the apparatus, on atmospherically conditions, on my abilities and on the true velocity of light, but not on Fizeau any more. The following inferences obviously rely on having chosen this latter option.
34. Announced anonymously in *Comptes rendus des séances de l'Académie des Sciences* 30 (1850), p. 413.
35. See Kuhn (1962).
36. This applies for example to Weber and Weber (1825), Airy (1831), Knochenhauer (1839), Powell (1841), Briot (1864), and Airy (1866).
37. As one for many, see Clerke (1893) for these attempts.
38. This is said, retrospectively of course, by Arago (1857, vol. IV, p. 418), Delaunay (1865, p. 437), and Cornu (1874a, p. 138).
39. Among the works listed in note 3, especially Collins, Shapin/Schaffer, Latour, and Galison, focus on how closure (of debates, experiments, and so on) is achieved in science.
40. Only the telescope to send out the light could have been the same as in the rotating-mirror experiment.