L'ÉVÉNEMENT ASTRONOMIQUE DU SIÈCLE ? HISTOIRE SOCIALE DES PASSAGES DE VENUS, 1874-1882

Numéro spécial des
Cahiers François Viète

Textes rassemblés par David AUBIN

TABLE DES MATIERES

Cahiers François Viète, 11-12, 2007, p.15-40.

SENSATIONAL DIFFERENCES: THE CASE OF THE TRANSIT OF VENUS

Jimena CANALES

Résumé

Au dix-neuvième siècle, astronomes, physiologistes et psychologues expérimentaux remarquent que des individus différents mesurent différemment le temps d'événements simultanés. Pire, ces « différences sensationnelles », parfois appelées équations personnelles, s'accroissent de manière considérables dans les mesures astronomiques, dans la perception du mouvement et dans la mesure des phénomènes rapides à l'instar de celle de la vitesse de la lumière. On s'inquiète de l'apparition de ces différences dans les sciences bien au-delà de la mesure du temps et de la simultanéité. On les confronte en effet dans la perception des longueurs, des angles, des couleurs et des sons. Contrairement aux erreurs aléatoires bien connues, elles persistent même lorsque les observations sont répétées et moyennées. Cet article examine plusieurs tentatives faites par les scientifiques dans le but de vaincre ces différences dans la détermination de la plus importante constante en mécanique céleste : la parallaxe solaire. On s'attardera sur les célèbres observations du passage de Vénus de 1874 et sur les mesures ultérieures de la vitesse de la lumière, en retraçant certains changements fondamentaux dans l'établissement d'une science basées sur la mesure.

Abstract

In the nineteenth century, astronomers, physiologists and experimental psychologists noticed that different individuals timed simultaneous phenomena differently. These small "sensational differences," sometimes referred to as personal equations, augmented considerably in astronomical measurements, in perceptions of movement, and in measurements of rapid phenomena, such as the speed of light. Alarmingly, these differences affected science beyond determinations of time and simultaneity. They appeared in elementary perceptions of lengths, angles, colour, and sound. Unlike well-known random errors, they persisted even when observations were repeated and averaged. This paper examines various attempts by scientists to overcome these differences in the determination of the most important constant of celestial mechanics: the solar parallax. It takes us from the famous transit of Venus observations of 1874 to later speed-of-light measurements, tracking fundamental changes in the establishment of measurement-based science.

Introduction

Individual differences in determining the precise moment of apparent contact between two celestial bodies affected one the most important constant of nineteenth-century science: the solar parallax[5]. The solar parallax was used by astronomers to determine the speed of light and the distance from the earth to the sun. At a time when all measurement standards, from the meter to the second, were debated, it promised to be a natural standard from which all others could be deduced—if only the problem of individual differences could be overcome.

The transit of Venus across the sun, a rare astronomical event occurring only twice in approximately every hundred years, was seen as the best opportunity for ending decades of debate surrounding the value of the solar parallax. To determine it, astronomers needed to time the precise moment of Venus's apparent contact with the sun in 1874 and 1882, but timing this moment proved to be extremely difficult. A mysterious "black drop" which appeared between the edges of Venus and of the sun and individual differences in the observation of the phenomenon brought disrepute to traditional methods. To combat these difficulties, the astronomer Jules Janssen devised a controversial new instrument, the "photographic revolver" that photographed Venus at regular intervals of approximately one second[6]. Another solution came from physicists who rivalled the astronomers' dominance in precision measurements by deducing the solar parallax from physical measurements of the speed of light. Yet other astronomers relied on drawings and well-trained observers.

Controversies surrounding the determination of the moment of Venus's apparent contact were part of much larger philosophical debates about the nature of observation, experiment, and the boundary between humans and machines. The merits of different methods in astronomy, astronomy's relation to physics, and the nature of space and time, were all at

[5] This account of the transit of Venus is a continuation of Jimena Canales (2002) "Photogenic Venus: The 'Cinematographic Turn' and Its Alternatives in Late Nineteenth-Century Science", *Isis*, 93, p. 585-613.

[6] For a partial account of photography and the transits during this period although not mentioning Janssen's photographic revolver, see John Lankford (1987) "Photography and the Nineteenth-Century Transits of Venus", *Technology and Culture* 28, p. 648-657.

stake in these debates[7]. The new space emerging from these controversies was characterized by a decline in the faith of geometry and (non-standardized, non-reproducible) photography, and by the growing realization of the importance of alternative elements needed for establishing scientific truths, namely: power and authority; skill and discipline; standardization, mechanical reproducibility and theatricality.

Soon after its invention, Janssen's photographic apparatus was modified and moved into other areas of science and culture, most famously to the physiological laboratory of Étienne-Jules Marey and then to the studio of the Lumière brothers, where it was gradually transformed into what would later be called the cinematographic camera. Although the Marey and Lumière instruments differed markedly from Janssen's original one, the applications of the revolver to the study of living beings as well as its inverse use for synthesizing images were vaunted by Janssen as proof of its bedazzling ability to create assent in visual matters. From the moment Janssen pointed his revolver toward Venus (1874) to the time when he starred in one of the first films to be shown publicly (1895), the device passed through a painful gestation intimately tied to the debate on how to eliminate differing observations.

My account of individual differences ends with the appearance of the cinematographic method (1895), by reference to which certain scientists, astronomers and amateurs sought to solve the problem. When the cinematographic camera was divided into separate instruments (one for shooting and one for projecting), the topography of science was altered dramatically, differing radically from what had characterized it for the preceding fifty years. The relation of science to individuality, art, media, spectacle and its place in public and private spheres would never be the same. This new configuration emerged clearly in the famous confrontation between Henri Bergson and Albert Einstein (1922) on the Theory of Relativity, where the problem of individual differences in perceptions of time and simultaneity was finally brushed aside from the physical sciences[8].

---

7 Political aspects of transits of Venus are explored in Simon Werrett's contribution to this issue of the *Cahiers François Viète*, and in David Aubin (2004) "Un passage de Vénus en politique", *La Recherche Hors série*, 15, p. 85-89.

8 Jimena Canales (2005) "Einstein, Bergson, and the Experiment that Failed: Intellectual Cooperation at the League of Nations, " *Modern Language Notes*, 120, p. 1168-1191.

Contact

In the eighteenth century the British astronomer Edmond Halley claimed that the solar parallax could be determined by combining simple Euclidean triangulations with direct observations of Venus's apparent contact with the sun. But a mysterious "black drop" which appeared between Venus and the sun and individual differences in its observation soon brought disrepute to traditional astronomical methods.

When nineteenth-century astronomers reviewed previous transit observations their conclusion was appalling: different people saw different things. After the transits of 1761 and 1769, astronomers came to doubt the very possibility of timing celestial contacts precisely[9]. Quite apart from the larger political and juridical consequences of disagreement, the immediate problems were insurmountable: if the solar parallax remained closer to the current value, astronomers would have to posit the existence of an unlikely ninth planet; and if the differences in observations were caused by the Venusian atmosphere, then the possibility that Venus was a world like the earth would have to be seriously considered.

In 1863 the astronomer Charles-Eugène Delaunay inaugurated the debate in France with an article designed to point out the "embarrassment" of previous observations. According to Delaunay, the "black drop" that mysteriously appeared between the edges of Venus and of the sun, combined with the problem of irradiation and personal errors in observations, all contributed to the astronomers' "embarrassment in trying to determine the precise instant of contact" and caused an alarming "defectiveness of observations"[10].

In an article that appeared in the popular journal *La Nature* Wilfrid de Fonvielle described "that which was seen" in the transits of 1761 and 1769. He found discordant results even when astronomers observed side-by-side and with the same instruments. He suggested that the discrepancies were mainly due to the "black drop," which he described as "a mysterious object with very strange variations." Other astronomers similarly reported

---

[9] On eighteenth-century transits, see Harry Woolf (1959) *The Transits of Venus: A Study of Eighteenth-Century Science* (Princeton: Princeton Univ. Press).

[10] Charles-Eugène Delaunay (1874) "Notice sur la distance du soleil à la terre, extrait de l'Annuaire pour l'an 1866, publié par le Bureau des Longitudes", in Institut de France, *Recueil de mémoires, rapports et documents relatifs à l'observation du passage de Vénus sur le Soleil* (Paris : Firmin Didot), 43, 97-98. The "goutte noire" was alternatively called the "ligament noir."

that "the transit of 1761 was totally fettered" by the "black drop" phenomenon, yielding "all sorts of discordant results." Not only did observers disagree about what they saw, Faye complained, but "after a whole century of discussions, astronomers still have not been able to agree on the physical circumstances of the phenomenon, and on the true meaning of the important observations of 1769"[11].

Even before the problem of individual differences leaked to the general public, governments across the world became concerned. In 1869 Napoléon III's minister of public instruction, Victor Duruy, addressed a letter to the Académie des sciences requesting "scientific missionaries" to go to the end of the world in 1874 "to rid observations from the causes of error which so strangely affected those of 1769." Despite the "sorry state of the country's finances," the French government was able to amass an impressive amount of money and resources designed to overcome all obstacles. The problem, the astronomer Hervé Faye explained, should be solved "no matter the cost"[12].

Discipline

To address the problem of divergent observations, the full authority of the Commission for the Transit of Venus of the Académie des sciences was thrown behind official methods and instruments. The Commission, headed by the chemist Jean-Baptiste Dumas, opted to rely chiefly on well-trained observers and specific photographic instruments. In particular, it

---

[11] Faye (1874) "Le prochain passage de Vénus", art. cit., p. 365 ; Hervé Faye (1869) "Sur les passages de Vénus et la parallaxe du soleil", *CRAS* 68, p. 42; Wilfrid de Fonvielle (1874) "Les derniers passages de Vénus", *La Nature* 2, 43, p. 257.

[12] Ministère de l'instruction publique (1869) "Lettre", *CRAS* 68, 205. The meeting at the academy was recounted in *Les Mondes* 19 (1869), 212-213. The Government initially gave 300,000 francs and in 1875 provided an additional 125,000 francs. The expenses, however, continued to mount, and in 1876 a "projet de loi" demanded 25,000 francs more from the Chambre des députés. For cost issues see *Journal officiel de la République Française*, 27 juillet et 1er septembre 1872, 14 septembre 1876 et 27 mars 1876. See also Hervé Faye (1874) "Association Française pour l'Avancement des Sciences, congrès de Lille, conférences publiques: Le prochain passage de Vénus sur le Soleil", *Revue scientifique* 14, p. 361. For an account of the French preparation work, see Aubin (2004) "Un passage de Vénus", art. cit.

sponsored the work of Charles Wolf who had already addressed the problem of individual discrepancies in meridian transit observations[13].

After a century of disagreements, Wolf and his collaborator Charles André claimed they had found in 1869 "on which side truth lay". Observers saw differently, they concluded, because their telescopes distorted the phenomena they were pointed at, not because of physiological differences. Wolf and André denied the existence of a physiological irradiation of the eye, which some had suggested could alter the estimation of time. They complained that it was "useless to give the name of a purely subjective phenomenon to a group of real phenomena linked to known causes". In this respect, they differed most noticeably from Faye, who had claimed that "the determinations [of astronomical phenomena] are complicated by a new personal error, varying from one observer to the next, and from one moment to the next for the same observer"[14].

Wolf and André claimed to have solved the "black drop" mystery by using an apparatus that artificially reproduced transits of Venus. A few years earlier Wolf had designed a similar machine to measure the different times at which observers reacted to an artificial star crossing the wires of a meridian transit instrument. With it, he obtained the observer's "psychological time" by subtracting the "real" time of contact from the total time. His machine was frequently used to educate observers so as to reduce and stabilize their reaction times, which the Swiss astronomer Adolph Hirsch had claimed were dangerously unpredictable[15].

Wolf and André's transit machine was based on the same principles as the artificial transit star machine. From the Paris Observatory, Wolf aimed a telescope at the library of the Senate in the Palais du Luxembourg (at a distance of 1,300 meters), where André operated a number of lamps and screens imitating Venus and the sun. When Wolf saw an "apparent" contact, he immediately pressed a telegraph key that sent the signal back to

---

[13] On Wolf, see Jimena Canales (2001) "The Single Eye: Reevaluating Ancien Régime Science", *History of Science*, 39, p. 71-94.

[14] Charles Wolf and Charles André (1874) "Recherches sur les apparences singulières qui ont souvent accompagné l'observation des contacts de Mercure et de Vénus avec le bord du Soleil", in Institut de France, *Recueil de mémoires,* op. cit., 125, 30. Their initial work was reported in *Les Mondes* 19 (1869), p. 174 and 361 and also in *Revue scientifique* 8 (1871), 575.

[15] For Hirsch, see Jimena Canales (2001) "Exit the Frog, Enter the Human: Astronomy, Physiology and Experimental Psychology in the Nineteenth Century" *British Journal for the History of Science*, 34, p. 173-197.

the Senate and compared it to the time of the "real" contact. From these experiments Wolf and André concluded that the "black drop" disappeared when an aberration-free lens was used (such as those made by Léon Foucault) and when the instrument was aimed properly. Contradicting those who believed that the "black drop" was an inherent astronomical or physiological phenomenon, they insisted that it was an illusion due mainly to defective telescopes and faulty aiming. They recommended that observers practice with moving targets and test for a personal equation that could be factored into the final result. According to them, the "black drop" was no impediment to observation and, with the right instruments and training, observers could almost see the geometric contact expected by Halley.

Wolf and André vindicated observational methods that were being profoundly criticized by Faye and others. Furthermore, they concluded that there was no further point in investigating the physical aspects of the problem that fascinated Faye, Janssen, and the great popularizer Camille Flammarion. For Wolf, the existence of the "black drop" was nothing more than a "scientific prejudice." In deep irony he remarked: "The fable of an animal in the moon is still true"[16].

While Wolf denied that there was either an astronomical or physiological source for the "black drop," a residual problem persisted. In an article published in the *Revue scientifique*, he cautiously admitted that "this, nonetheless, is not to say that under these conditions observers will note exactly the same time, or experience the contact in the same way". In fact, experience showed that observers still did not time the contact in the same way, which proved that "the contact of two discs is never a purely geometrical phenomenon." Thus, he argued, observers should still be compared against each other "in order to determine their personal equations". The Paris Observatory director, Urbain Le Verrier, backed Wolf's suggestion, insisting to send on future expeditions "only those observers who have been compared amongst themselves"[17].

According to Edmond Dubois, who wrote a popular account of the work surrounding the transit, Wolf and André's conclusions showed that "almost constant and VERY SIGNIFICANT differences persist between differ-

---

[16] Charles Wolf (1872) "Le passage de Vénus sur le soleil en 1874", *Revue scientifique* 9, 1009.

[17] Urbain Le Verrier (1869) "Sur les passages de Vénus et la parallaxe du soleil", *CRAS* 68, 49 ; and Wolf (1872), 1010.

ent observers, especially in estimating the time of […] contacts"[18]. Rodolphe Radau discussed their work in the *Revue des deux Mondes*: "Nevertheless, there is a constant difference between the estimation of the moment of contacts by two observers—a difference due to physiological causes." In the end, the commission was unable to muster full trust in the training machine, and it included photography as part of its effort to bypass the recalcitrant problem of individual differences in observation. Even Wolf and André, who were not photography's first advocates, were convinced of its usefulness[19].

Photography

Disillusioned by the transitory nature of the "fleeting instants of calm which the English astronomers call a *glimpse*," Faye continued to promote "the simple yet fecund idea of suppressing the observer and of replacing his eye and brain with a sensitive plaque connected to an electrical telegraph"—an idea that he had sponsored and implemented decades before[20]. Interested in both the physical and physiological aspects of the problem, he suggested that astronomers couple their observations with "a detailed account of the physical phenomena and include drawings." More important, he urged them to use photography, where "everything is automatic." He recommended that multiple photographs be taken in the same plate at one-second intervals by advancing it with "the simple movement of a handle" to expose its different parts in succession[21].

---

[18] Edmond Dubois (1873) *Les Passages de Vénus sur le disque solaire* (Paris : Gauthier-Villars), 154.

[19] Rodolphe Radau (1874), "Le passage de Vénus du 9 décembre 1874", *Revue des deux mondes,* 3e période, tome 1, 446 ; Wolf and André (1869).

[20] Hervé Faye (1870) "Sur l'observation photographique des passages de Vénus et sur un appareil de M. Laussedat", *CRAS* 70 (1870), 541. Italics in the original. Hervé Faye (1874) "Rapport sur le rôle de la photographie dans l'observation du passage de Vénus", *Recueil de mémoires, rapports et documents*, op. cit., 228. On "mechanical objectivity" see Peter Galison and Lorraine Daston (1992), "The Image of Objectivity", *Representations* 40, 81-128.

[21] Hervé Faye (1869) "Sur les passages de Vénus et la parallaxe du Soleil," *CRAS* 68 (1869), 71-72. See also *Les Mondes* 19 (1869), 42-44 et 85-86 ; Faye (1874) "Le prochain passage", 366. Other advocates of photography were Warren de la Rue, Simon Newcomb, and Friedrich Paschen. For a letter describing Faye's

With photography, Faye claimed, “the observer does not intervene with his nervous agitations, anxieties, worries, his impatience, and the illusions of his senses and nervous system.” Only by “completely suppressing the observer”—as photography purportedly did—could astronomers have access to nature itself: “[With photography] it is nature itself that appears under your eyes.” Faye’s dream was realized in part by a number of people who contributed to the design of the photographic instruments for the transit and ran test measurements on the plates[22]. The overwhelming conclusion at the time was that photography would be better than other methods, such as heliometry, because they did not involve the observer’s personal errors: “Photography,” even Wolf concluded “is safe from this cause of error”[23].

Revolver

To Janssen’s way of thinking there was something particularly photogenic about Venus. The “missionary of the Bureau de longitudes”, long considered the expert in “transient phenomena”, Janssen published a note in the *Comptes rendus* explaining his new procedure for observing the transit. His method—soon baptized “photographic revolver”—closely followed Faye’s idea of photographing in a single plate sequences separated by a second[24]. Not only would this apparatus ostensibly suppress the observer, as Faye dreamed, but it also would permit the study of the physical circum-

---

phototelegraphic experiments see Hervé Faye, Paris, 31 octobre 1861, Preußischer Staatsbibliothek zu Berlin , Sammlung Darmstädter J 1846(6) Faye, 3.

[22] Faye (1874) “Le prochain passage,” 366 (“observer does not intervene,” “nature itself”) ; Faye (1870) “Sur l’observation photographique”, 543 (“suppressing the observer”).

[23] Heliometry is a method used for the accurate measurement of angles with a divided object-glass micrometer. Charles Wolf and Antoine Yvon-Villarceau (1874) “Rapport sur les mesures micrométriques directes à faire pour l’observation du passage de Vénus,” Institut de France, *Recueil de mémoires,* op. cit., p. 339.

[24] Hervé Faye (1870) “Sur l’expédition de M. Janssen”, *CRAS* 71, p. 819-822, on p. 821 ; Jules Janssen (1873) “Passage de Vénus: Méthode pour obtenir photographiquement l’instant des contacts, avec les circonstances physiques qu’ils présentent”, *CRAS* 76, p. 677-679. On Janssen’s revolver, see also Françoise Launay et Peter D. Hingley (2005) “Jules Janssen’s ‘Révolver photographie’ and its British Derivative, ‘The Janssen Slide’”, *Journal for the History of Astronomy* 26, p. 57-79.

stances surrounding the contacts, circumstances whose very existence Wolf and André had cast into doubt by focusing on telescopic aberration and unskilled observers.

Aiming his revolver at Wolf and André's artificial planets, Janssen "hoped that the photographic images [would] be free […] of phenomena which so horribly complicate the optical observation of contacts." Yet despite these successes, he was unable to convince his colleagues of the revolver's merits. The state-sponsored and official effort for determining the solar parallax did not adopt the device as its main instrument.

Views on the merits of photography soon diverged. When Faye presided over the commission before Dumas took over, he and Janssen advocated the use of "a photographic instrument based on the same principles as that of the English, whose long experience had taught them the best methods." But after Faye left the group (allegedly because he "had other things to do"), "many members from the section of astronomy stopped going to the meetings"[25]. Now headed by Dumas, the commission eventually settled on metallic daguerreotypes instead of using collodion, the process chosen by most other nations from which paper prints could be made. In the opinion of the commission, the "conditions of inflexibility and invariability [of metal daguerreotypes] not offered by either paper or glass" outweighed the fact that they were not easily reproducible or comparable to other nations' images.

Although Janssen disagreed with the official prescriptions with respect to photography and his revolver, "a discipline spirit" compelled him to follow prescribed methods on his expedition to Japan[26]. Nevertheless, in addition to the officially sanctioned instruments, he brought along delinquent ones, including his controversial revolver.

---

[25] Jules Janssen to the ministre de l'Instruction publique (6 April 1876), p. 5, Archives nationales [hereafter AN], F[17] 2928-2; and Jean Baptiste Dumas and Elie de Beaumont to the ministre de l'Instruction publique (3 February 1873, AN, F[17] 2928-1. After Delaunay's death in 1872, Faye became president of the commission. He explained the reasons for his departure in Faye to the Director of the *Revue* (3 April 1873), Archives de l'Académie des Sciences de Paris [hereafter AAS], Carton 1645, Folder: Faye.

[26] Janssen to the ministre de l'Instruction publique (6 April 1876), AN, F[17] 2928-2.

Controversy

Despite the government's best efforts and hefty expenses, the controversy on methods and instruments surrounding the "true" value of the solar parallax persisted well after 1874[27]. Even before the Navy ships set sail to the faraway stations of Saint-Paul and Campbell islands, Beijing, New Caledonia, Vietnam, and Japan, optimism for the commission's methods was fading. The *mécanicien* Montagne, who at the last moment refused to participate in the expedition, argued that the astronomers knew they could not obtain the desired precision. He demanded a "public discussion" in order to "eliminate all the doubts and all the errors." Criticisms from more powerful quarters were silenced. "The *Comptes rendus* said nothing" of protests made by the physicists Hippolyte Fizeau and Alfred Cornu against Janssen, nor of Le Verrier's objections raised against the commission during a meeting at the Académie des sciences right before the ships left. During this meeting Janssen finally showed his photographic results, and Fizeau protested the secrecy that had veiled them to that point: "Why had Janssen not communicated the result of his photographic research to the Commission of the Transit of Venus, of which he is a member?"[28] Soon Fizeau and Cornu would distance themselves further from Janssen's photographic methods—and to some extent from photography itself—by advocating the determination of the solar parallax through measurements of the speed of light.

To complicate matters further, Faye's and Janssen's worst fears were realized when it became evident, after the transit, that the different cameras used had produced photographs so different that their results were impossible to compare. In short, the same singularities that had plagued the transit of Venus observations in the previous century had reappeared, and scientists were unable to determine the "real" instant of the planet's apparent contact with the sun. However, the academy did not give up: a sous-

---

[27] On debates about the value of the solar parallax after 1874, see Richard Staley's contribution to this issue of the *Cahiers.* See also Steven J. Dick, Wayne Orchiston, and Tom Love (1998) "Simon Newcomb, William Harkness, and the Nineteenth-Century Transit of Venus Expeditions", *Journal for the History of Astronomy* 29, p. 232-255, on p. 248.

[28] Montagne fils to the ministre de l'Instruction publique (29 January 1874), (26 February 1874), and (9 April 1874), AAS, Carton 1645, Folder: Passages de Vénus, demandes de Missions; see also "Académie des sciences—6 juillet 1874", *Revue scientifique* 4 (1874), p. 45 and "Académie des Sciences: Séance du 6 juillet", *Le Moniteur scientifique* 16 (1874), p. 774-775.

commission was created to deduce the parallax from measurements taken off photographs—again with discouraging results. Victor Puiseux, a mathematician who had worked at the Bureau des longitudes, challenged the photographic results, considering them inferior to those obtained from direct observations[29]. In the end Fizeau and Cornu, who were both by this point deeply invested in the determination of the solar parallax through measurements of the speed of light, gave up, announcing that the feared personal equation reappeared in efforts to measure photographs[30].

In mathematics, the results of the transit opened up debates pertaining to the relationship of geometry to the physical world. Before the transit most scientists would have agreed with Flammarion's remark that "Geometry has justified its name by gaining possession of the terrestrial globe"[31]. But in the case of the transits, traditional geometric methods proved unsuccessful. According to Faye, the problem of differing observations arose because "[astronomers] had reasoned too much like mathematicians." Even Wolf, who came close to vindicating Halley's proposed "geometrical" methods with the objectives of Foucault, grew to accept that "the contact of two discs is never a purely geometrical phenomenon." And Puiseux, the expert in mathematical astronomy who challenged the transit's photographic results, explained how, "in reality" the contact of Venus with the sun does not occur with "the geometric simplicity that had been supposed"[32].

---

[29] Almost eight hundred photographs had to be measured. Four machines to measure the plates were made by Brunner between November 1874 and the end of 1875. At first Cornu, Alfred Angot, Mercadier, and Baille operated them. See Victor Puiseux "Remarques sur les observations du passage de Vénus du 8 décembre 1874" (27 March 1880), AAS, Carton 1646, Folder: 1882 Passage de Vénus; and Puiseux to Académie des Sciences (31 May 1880), AAS, Carton 1647, Folder: 1882 notes diverses.

[30] *Documents relatifs aux mesures des épreuves photographiques,* 3 vols., vol. 3, Institut de France (1882) *Recueil de mémoires, rapports et documents relatifs à l'observation du passage de Vénus sur le Soleil*, extrait du tome III, 3e partie (Paris: Gauthier-Villars).

[31] Flammarion's defense of geometry was based on its importance in longitude determinations: Flammarion (1874) "Le prochain passage de Vénus", art. cit., p. 388.

[32] Hervé Faye (1874) "Association Française pour l'Avancement des Sciences, congrès de Lille, conférences publiques: Le prochain passage de Vénus sur le Soleil", *Revue scientifique* 14, p. 361 ; Charles Wolf (1872) "Le passage de Vénus

Timing the moment of contact between these two celestial bodies proved so difficult that some scientists, including the Belgian psychophysicist Joseph Delboeuf, were led to believe in an essential discontinuity in perceptions: "One knows that, in the observations of the transit of Venus over the sun, astronomers are frustrated by a phenomenon know as the black drop." Like two drops of water that "once they are sufficiently close to one another get brusquely confused," Venus's apparent contact with the sun had happened just as suddenly. Delboeuf explained how Venus seemed suspended when it was at the verge of an apparent contact, and then "tout à coup" it seemed to "precipitate" towards the sun, "in a manner which made it impossible to know the precise instant"[33].

Controversy

To add urgency to the matter, the next transit of Venus (1882) was rapidly approaching—the last until the years 2004 and 2012. The ministre de l'instruction publique criticized how in 1874 "without prior agreement" nations had "acted in an independent and personal manner." Once again Faye spoke out, expressing the hope "that the experience acquired at such a high price in 1874, should be useful in 1882, and that, this time, all civilized nations would unite their efforts in a common plan." Accepting the need for international cooperation, in 1881 a Conférence Internationale du Passage de Vénus was held, with Dumas presiding[34].

During the meeting, scientists from around the world acknowledged that the transit of 1874 had greatly damaged the prestige of astronomers: "the scientific public was amazed to see that after seven years, there were only partial and few publications on the results of the observations of

---

sur le soleil en 1874", *Revue scientifique* 9, 1010 ; Victor Puiseux (1881) "Académie des sciences de Paris: Séance du 7 mars 1881", *Revue Scientifique* 1, p. 378.

[33] Joseph Delboeuf (1878) "La loi psychophysique et le nouveau livre de Fechner," *Revue philosophique de la France et de l'étranger, dirigée par Th. Ribot*, 5, 134.

[34] Faye (1874) "Association Française", art. cit., p. 366; Ministère de l'Instruction publique (1881) *Conférence internationale du passage de Vénus* (Paris: Imprimerie Nationale), "Première séance: Mercredi 5 Octobre 1881", p. 4. The proceedings were reprinted in Anon. (1882) "La Conférence internationale du passage de Vénus", *Revue scientifique* 29, p. 42-46. The results of the 1882 transit appeared in Anatole Bouquet de la Grye, "Le passage de Vénus sur le soleil en 1882", *Mémoires de la Académie des Sciences de l'Institut de France,* 48 (1905).

1874[35]." Some attendees believed that "separate and hurried publications" on the upcoming transit should be prohibited and urged astronomers to "defer these until everyone had agreed." Through restraint, argued Wilhelm Förster, director of the Berlin Observatory, "the authority of astronomers would increase." Dumas also advocated a common publication to safeguard the "dignity of each country," and Förster was clearly being too frank when, thinking about the relation between the astronomers and the government, he said: "Scientific liberty can be restrained a little, in order to assure a definitive result useful to the Governments who have a special right to it after having given extraordinary means."

Dumas argued that cooperation was "nothing extraordinary" but a "natural consequence of scientific evolution." "Before," he continued, "science progressed by the effort of isolated observers; later, the need for cooperation between savants of a same nation was felt, creating academies and national learned societies. Today, that is not enough, and one feels at all times the need for international gatherings of savants." By now almost every astronomer recognized, with Faye and Janssen, that in planning the 1874 transit expeditions they should have "agreed on the type of instruments and adopted everywhere the same dimensions in order to render observations more comparable"[36].

The attendees of the Conférence asked, "Should we continue to employ photography, and to what degree?", leading astronomers to inquire about the nature of observation. Wolf found Faye's dream of "eliminating the observer" absurd. Observers, he claimed, would always be needed for obtaining "absolute and authentic knowledge." While Janssen thought that the difference between the photographic plate and the human retina proved to the superiority of the former, most astronomers disagreed. In fact, for Wolf, the eye's superiority resided in its stability across time. While different cameras and photographic methods produced different results (for example, collodion versus gelatin and bromide), "the human eye, on the contrary, is an organ which remains the same, and the observations of the eye are, at all times, comparable amongst themselves". Similarly, for Förster, the superiority of direct observation consisted in that, while instantaneous photography recorded only an instant, a good observer did a more valuable

---

35 Ministère de l'Instruction publique (1881) *Conférence internationale,* op. cit., p. 10 (quoting Adolph Hirsch).

36 Ibid., p. 4, 11, 17 and 26-27.

job by averaging over all instances[37]. Förster found the probable error of photography versus direct micrometer measurements five times as large. Besides the uncertainties posed by the photographs themselves, he complained about the "considerable" work needed to measure them—a problem that was later solved in Paris by the incorporation of female labour in the observatory[38].

Despite some dissent, the Conférence's overwhelming conclusion was that direct observations of Venus were better than photographic ones. These discouraging results "led the French commission to limit the use of photography," and harking back to older methods that were once discredited, they recommended that observers "accompany their notes with a drawing"[39]. By 1882 almost everybody agreed that non-photographic observations were better. This position was most forcefully articulated by Förster, who called for the total elimination of photography for the 1882 transit, and by Ernest Mouchez, who had led one of the French expeditions in 1874 and "agreed completely"[40].

Cinematography

In light of the commission's delayed results, the French government started to change its strategy. While it continued to fund the commission even as it exceeded its budget, it also started funding alternative methods, such as Janssen's revolver. Paradoxically, as the commission moved farther and farther away from photography, Janssen, his revolver, and his calls for a physical astronomy based in large part on photographic methods were becoming immensely popular—outside of the academy.

When Janssen returned from his expedition, the President of the Republic Patrice de Mac-Mahon greeted him "warmly," and shortly thereafter the Meudon Observatory was created for him by decree[41]. Following a plea from Faye on his behalf, the President also gave Janssen almost 40,000

---

[37] Charles Wolf (1886) "Sur la comparaison des résultats de l'observation astronomique directe aves ceux de l'inscription photographique", *CRAS*, 102, p. 477 ; Ministère de l'Instruction Publique (1881) *Conférence internationale*, op. cit..

[38] Charlotte Bigg (2000) "Photography and the labor history of astronomy: the *Carte du Ciel*", in *Acta Historia Astronomiae,* 9, p. 90-106.

[39] Ibid., p. 30 (drawings), 4 and 6.

[40] Ibid., p. 7-8.

[41] On the foundation of the Meudon Observatory, see Le Gars in this issue.

francs to cover his outstanding expedition expenses and the instruments he had constructed for his personal use, including the revolver.

Two years after the first transit, Janssen claimed that the revolver "was now definitely introduced in science[42]." In 1882 Marey transformed Janssen's revolver into a "*fusil photographique*," producing his famous images of flying birds. Moreover, by arranging the images on a phenakistiscope and giving them the illusion of movement, he realized Janssen's dream of obtaining both "analysis" and "synthesis." Soon afterward chronophotographs were projected onto a screen. In fact, one of the first films ever to be shown publicly was a "movie" of Janssen himself at a conference of the Société Française de Photographie. Janssen's apparatus was an essential part of a new, emerging, and highly contested evidentiary regime that through chronophotography and its "inverse" (phenakistiscope or projection) ostensibly eliminated individual differences in the observation of moving phenomena.

In the wake of Janssen's chronophotographic successes, photography was eventually reconsidered as a tool for astronomical observation. Only five years after the Conférence Internationale condemned it, Janssen started to campaign for the Carte du Ciel, a project for cataloguing stars with photography. Ironically, it was initially led by Mouchez, now director of the Paris Observatory, and one of photography's early critics. Some years later Janssen preached his victory to an audience of photographers by reference to the wars of religion:

> I will gladly say that you belong to […] the triumphant church. But there was also amongst you a militant church, a church of catacombs, which the majority of you did not even know. And right now, your church triumphs as the Christian church has triumphed against Constantine[43].

---

[42] Janssen to Ministre de l'Instruction Publique (6 April 1876), AN, F^{17} 2928-2.

[43] Jules Janssen (1930) "En l'honneur de la photographie: Discours prononcé au Banquet annuel de la Société Française de la photographie, juin 1888," *Oeuvres scientifiques recueillies et publiées par Henri Dehérain* (Paris : Société d'Éditions Géographiques, Maritimes et Coloniales). The project of the Carte du ciel lasted until 1970, when the gargantuan effort of photographing the heavens was finally put to an end.

Janssen's public successes, however, contrasted starkly with his standing in the eyes of the commission. For most scientists, his contributions—although spectacular—were illusory. His colleagues reproached him for taking matters into his own hands during his transit of Venus expedition to Japan, ignoring his official instructions. This resulted in the division of his mission into two separate expeditions, one to Kobe and the other to Nagasaki—and also to his boldly incurring extra expenses that exceeded his allocated funds[44]. Needing money to get home—and expecting to find allies in Paris—Janssen telegraphed Dumas asking for help. Dumas's response, however, made it clear that the Academy would rather leave Janssen stranded in Japan than extend further financial aid. He was left with no option but to use his own funds to come back.

Physics

The debate over chronophotography's claims to truth and the value of the solar parallax quickly moved beyond the confines of astronomy and entered into the domain of physics. In light of astronomy's failure to establish a single, reputable value for the solar parallax, a new role for physics emerged with respect to precision measurements—in particular, regarding the determination of the speed of light.

While terrestrial measurements of the speed of light could be used to determine the solar parallax, for years this method seemed untrustworthy. Only astronomical measurements would "immediately convince the spirit" of the true parallax value[45]. Traditionally, astronomical methods were used to determine physical constants—not the other way around. In the seventeenth century, Ole Rømer, the Danish astronomer who worked at the Paris Observatory, calculated the speed of light from current astronomical determinations of the solar parallax. Centuries later, astronomy lost this preeminence. *Physicists* determined astronomical constants. In 1875 the attendees of the evening lectures of the British Royal Institution were alerted to the "inverted" roles of physics and astronomy: "Now the progress of science requires an inverse march; the exact value of the velocity of light permits, by the inverted calculus, the computation of the mean distance of the sun or

---

44 Janssen's trip to Japan and his expenses are documented in his notebook for 1874, Bibliothèque de l'Institut de France, Ms. 4128. For a description of his plea for help see Janssen to Ministre de l'Instruction Publique (6 April 1876), AN, F^{17} 2928-2.

45 Delaunay (1863) "Notice sur la distance du soleil," art. cit., p. 94.

the sun's parallax, that is to say, the same element which is directly given by the transit of Venus."[46] Alfred Cornu, of the École Polytechnique, explained the new order in the introduction to the "Détermination nouvelle de la vitesse de la lumière": "Today *astronomy reverses those roles* and demands from the progress of Optics the value of this constant." He stressed how these experiments were directly related to the problems plaguing the transit of Venus expeditions: "These experiments have a truly current importance since they permit us to determine with exactitude the value of the solar parallax, which astronomers of all nations are demanding from the next transit of Venus across the sun at the price of costly voyages, both difficult and risky."[47] Physical determinations of the speed of light, he argued, were a simpler, cheaper, and surer way of determining the solar parallax.[48]

Le Verrier was essential in effecting this transformation. Seeing that his own value for the solar parallax coincided neatly with the one Foucault derived for the speed of light, he called on physicists for help, and moved forcefully to support first Foucault and then Cornu.[49] Le Verrier's strategy was unprecedented in scientific circles. Physical determinations of the solar parallax or the speed of light, he insisted, should no longer be considered inferior to astronomical ones. Even Foucault's value for the speed of light, ironically, "found favor among astronomers" at a time when it "was not accepted by most physicists"[50].

---

[46] For an account of Cornu's second determinations from the Paris Observatory to Montlhéry (ordered by the *Conseil* of the Paris Observatory on the proposal of Le Verrier) see Cornu, "New Determinations of the Velocity of Light," published by the Royal Institution of Great Britain on 7 May 1875.

[47] Cornu, "Détermination nouvelle de la vitesse de la lumière," 133, Hippolyte Fizeau, *Comptes rendus des séances de l'Académie des Sciences* 75 (22 July 1872), Le Verrier, "Sur les masses des planètes et la parallaxe du soleil." Italics mine. Cornu repeated Fizeau's (1849) and Foucault's (1862) experiments on the speed of light. While Fizeau's results centered around 315,000 km/second, Foucault's gave 298,000 km/second. Cornu's results were 298,500 km/second. Cornu, "Détermination nouvelle de la vitesse de la lumière," 139.

[48] Though Cornu presented his results on the speed of light before the 1874 transit, five Navy ships laden with ten kilograms of silver smeared on photographic plates nonetheless sailed off.

[49] Cornu (1874) "Détermination nouvelle", op. cit., p. 138.

[50] LeVerrier to Jurien de la Gravière, Paris, 24 July 1873, AN, F17 3726, Folder: Passage de Vénus (1867-1882, particulierèment 1873-1875), p. 1; and Cornu, "Détermination nouvelle de la vitesse de la lumière," 139. After the presen-

Shifts

Despite the hope of these astronomers, it soon became evident that physical determinations of the speed of light were not immune from the problem of individual differences. Observers criticized Fizeau's "toothed wheel" experiments for including an element of reaction. Timing the moment of the appearance of a light signal was as difficult as rapidly reacting to any other stimuli (such as Venus), introducing a time of reaction and individual differences in the final result.

To eliminate these problems, physicists revived lensless microscopic technologies pioneered in the eighteenth century by Ernst Chladni (1756-1827). By placing fine sand on vibrating plates and observing the patterns into which the sand settled, he was able to visualize previously unseen vibrations. In France, Jules Antoine Lissajous continued this type of experiments. He used moving mirrors to study the infinitely small vibrations of a tuning fork. Aiming a ray of light at a small mirror placed on a diapason that reflected vibrations on a screen, he produced the figures named after him.

Foucault turned to these methods in order to avoid rapid reactions and created a "rotating mirror" system. While most scientists believed that avoiding reactions was a radical improvement, others claimed that a personal equation still appeared in the measurement of the images displaced by

---

tation of Cornu's work to the *Académie des Sciences*, Le Verrier insisted on sending it to the transit of Venus commission. Le Verrier, Paris, 21 May 1875, AN, F17 2928-1, Folder C: Commission de l'Académie des Sciences, Travaux, Préparations, etc. (documents supporting Cornu's experiments on the speed of light); Note for *Ministre de l'Instruction Publique*, 15 May 1875, AN, F17 2928-1, Folder C: Commission de l'Académie des Sciences, Travaux, Préparations, etc., (asking whether Cornu's new petition for funds to determine the speed of light should fall within the budget for the transit of Venus); and Cornu, Courtenay (Loiret), 15 Mar. 1875, AN, F17 2928-1, Folder C: Commission de l'Académie des Sciences, Travaux, Préparations, etc., p. 1-2 (asking for money for his experiments and saying that "since the *main interest in the direct determination of the speed of light is due to the computation of the solar parallax*, we were forced to finish the instruments, do the experiments, and publish the definite result before the middle of December 1874"; he also described how his value "conformed to the results of M. Le Verrier." Italics mine). Cornu to *Ministre de l'Instruction Publique*, 5 February 1875, AN, F17 2928-1, Folder C: Commission de l'Académie des Sciences, Travaux, Préparations, etc. (Cornu, supported by Fizeau and Le Verrier, asked for money to pay Louis Bréguet, member of the renowned family of clockmakers, for his services).

the rotating mirror. In the end, Foucault was unable to eliminate this source of error.[51] His experiments to determine the speed of light were still at the mercy of “errors of observation” because he used a micrometer “comparable in every way to the […] micrometers used for astronomical observations[52].”

Intent to outdo astronomers’ claim to precision, Cornu improved on Fizeau’s “toothed wheel” and Foucault’s “rotating mirror” system by experimenting on the speed of sensory transmission and on the time taken by the mind to perceive and to react. He knew that small, sensational differences caused errors that amounted to thousands of kilometers per second, and he tried to eliminate them. Yet Cornu was unable to solve these problems. “Unfortunately,” he explained, “almost all of these delays are part of the domain of nervous phenomena, that is to say essentially variable, following external circumstances.” His conclusion was cautious: “with respect to having eliminated them completely, there is no demonstration which can provide this certitude” [53].

When Albert A. Michelson undertook his speed of light experiments (in 1878) he placed his bets on Foucault’s methods. Michelson, who studied with Cornu in Paris, perfected the rotating mirror method by using a mirror that “could execute 128 turns per second,” and increasing the distance to 500 feet. With these changes he increased the deflection “about twenty times that obtained by Foucault”[54]. His result showed a “remarkable

[51] On measurements on the speed of light see Xiang Chen (2000) *Instrumental Traditions and Theories of Light* (Dordrecht: Kluwer). Chen divides instrumental traditions in the nineteenth century into two paradigms, “visual” and “geometric”. He places Foucault’s experiments within the “geometric tradition”. Fizeau’s experiments, in contrast, appear in the “visual tradition”. Chen is mistaken in his claim that Foucault’s “measurements no longer depended upon the physiological and psychological status of the observer”. In fact, they also took into consideration observation errors. Instead of opposing toothed wheel experiments (Fizeau) as a “visual” paradigm that contrasted with the rotating mirrors (Foucault) of a “geometrical” kind, I see both experimental systems as offering different solutions to the problem of individual differences.

[52] Léon Foucault (1862) “Détermination expérimentale de la vitesse de la lumière; description des appareils”, *CRAS* 55, p. 792-796, on p. 794.

[53] Alfred Cornu (1876) “Détermination de la vitesse de la lumière d’après des expériences exécutées en 1874 entre l’observatoire et Montlhéry”, *Annales de l’Observatoire de Paris —Mémoires,* 13, p. 103 and 112.

[54] Albert A. Michelson (1880) “Experimental Determination of the Velocity of Light”, *Proceedings of the American Association for the Advancement of Sci-*

coincidence…with that obtained by Cornu, by the method of the toothed wheel"[55]. Even before the second transit approached, Michelson's work was a strong contender among alternative determinations of the value of the solar parallax.[56]

Light

The debate on the personal equation pushed these precision experiments in yet another direction: interferometry. Justin-Mirande René Benoît (1844-1922), director of the Bureau International de poids et measures, remarked on the mysterious nature of the personal equation; how it varied according to the "momentary disposition of the observer," and how it was "not easy to understand what this phenomenon depends on[57]." Benoît found hope on the interferometric methods employed by Jamin and Fizeau and redirected the work of the Bureau international des poids et measures in this direction[58]. He sponsored Michelson's work who first perfected his methods in collaboration with the Bureau, as well as that of Alfred Pérot and Charles Fabry who followed on Fizeau's and Michelson's steps by building another interferometer to determine the length of the standard meter[59].

---

ence, 28, p. 24. He then further increased the speed to 250 turns per second and the distance to 2000 feet.

55 Ibid., p. 160. Cornu's work as interpreted by Helmert d'Aix gave 299990 kilometers per second for the velocity of light, and Michelson's gave 299940 kilometers per second.

56 See Richard Staley's article.

57 J.-René Benoît (1900) "De la précision dans la détermination des longueurs en métrologie", in *Rapports présentés au Congrès International de Physique réuni à Paris en 1900*, ed. Ch.-Éd. Guillaume and L. Poincaré (Paris: Gauthier-Villars), p. 61 and 62.

58 Jules Jamin, physics professor at the École polytechnique for almost 30 years, adapted interometric effects for building a refractometer; In 1864 Fizeau turned to interferometry for his work on metrology. With this method he discovered minute dilations to a millionth part of a millimeter that are now named after him.

59 Ibid., p. 69 and 70. On the history of French interferometry, see Charlotte Bigg (2002) "Behind the Lines. Spectroscopic Entreprises in Early Twentieth Century Europe", Ph.D. thesis (University of Cambridge).

A few years later, Michelson famously used interferometry to test the effect of the ether on the speed of light. His interferometric methods are usually considered to be some of the most precise ever—indeed earning him the Nobel Prize for physics in 1907. What has been ignored in the historiography of modern physics is that, from the 1874 transit of Venus onward, the question of the speed of light and its relation to the ether moved to center stage, a process that started with the work of Foucault and was continued by others—mainly Michelson, who took on Fizeau's and Cornu's "unfinished business" [60]. Michelson's speed-of-light experiments were done to eliminate the element of reaction, individual differences in observation, and in direct response to the photographic and cinematographic methods exemplified by Faye and Janssen.

Standards

Late nineteenth-century speed-of-light measurements were scientists' main weapon for overcoming the main philosophical and scientific crisis of the century: the problem of standards. Especially after the failure to deduce the length of the meter from the circumference of the earth, finding an alternate standard would save scientists from the problems of conventionalism—or, even worse, nominalism. This problem was urgent at the level of celestial mechanics (solar parallax, aberration constant, masses and diameters of the planets, perihelion of mercury, sunspots and the nature of the sun, etc.) and at the table-top level of metrology (the length of the meter, the second, etc.).

---

[60] Michelson (1880) "Experimental Determination of the Velocity of Light", art. cit., p. 124. His later experiment was described in Albert A. Michelson (1881) "The Relative Motion of the Earth and the Luminiferous Ether", *The American Journal of Science,* 22, p. 120-129. For the "unfinished business" see J. B. Gough (1970-1986) "Armand-Hippolyte-Louis Fizeau", in *Dictionary of Scientific Biography*, ed. Charles Couston Gillispie (New York: Scribner's), 5, p. 16-20, on p. 20. For an account of Michelson's work and its relation to the solar parallax and the transits of Venus see Octave Callandreau (1881) "Histoire abrégée des déterminations de la parallaxe solaire", *Revue scientifique* 2, 39-43 and Staley's article in this issue of the *Cahiers.* For Foucault and Fizeau's early work on the ether, see Paul Acloque (1984) "Hippolyte Fizeau et le mouvement de la Terre: Une tentative méconnue", *CRAS — La Vie des sciences* 1, p. 145–58; Pierre Costabel (1984) "L. Foucault et H. Fizeau: exploitation d'une information nouvelle", in ibid., p. 235-249.

Seduced by the authority of standards derived from nature, Flammarion hoped that once they determined the solar parallax astronomers would have "the meter of the *système du monde*." Similarly, Cornu, similarly, hoped to contribute to this problem of "capital importance," since the solar parallax would "define the absolute dimensions of the solar system" [61]. As Faye put it, the solar parallax was "the key to the architecture of the heavens" and an ultimate "touchstone, a precise verification of the theories of celestial mechanics"[62].

But the inconclusive results of the transit of Venus expeditions only exacerbated mathematical, philosophical, and scientific debates over absolute standards. Fonvielle, who had alerted the general public to the discordances of the previous transits of Venus, commented cynically on the host of solar parallax values that had resulted from the British, American, French, German, and Russian expeditions:

> There are as many great nations as there are distances from the sun to the earth. It is terribly irritating that each nation cannot have its own special planet for its own individual use and is obliged to prosaically receive heat from that banal celestial body which illuminates all the others[63].

The failure to determine an absolute standard of measurement from observations of the transit of Venus forced scientists to reevaluate their claims to absolute truth. Yet old habits die hard: some now attached their hopes to Fizeau's and Michelson's new attempts to base measurement standards on wavelengths; and others, like Fonvielle, advocated using the speed

---

[61] Cornu (1874) "Détermination nouvelle", art. cit., p. 138; Camille Flammarion (1874) "Le prochain passage de Vénus et la mesure des distances inaccessibles", *La Nature,* 2, p. 386-391, on p. 387. Part of this work was repr. in Alfred Cornu (1873) "Détermination nouvelle de la vitesse de la lumière", *CRAS* 76, 338-342. For work on standards see esp. the essays by Ken Alder, Graeme Gooday, and Simon Schaffer in M. Norton Wise, ed. (1995) *The Values of Precision* (Princeton: Princeton Univ. Press).

[62] Faye (1874) "Association Française", p. 367-368. The problem of standards was articulated in Charles Wolf (1870) "La figure de la terre, soirées scientifiques de la Sorbonne", *Revue scientifique* 7 (1870), 226-234. Later, with respect to nominalism, I am thinking of the work of Edouard Le Roy, and, for conventionalism, Poincaré.

[63] Wilfrid de Fonvielle (1875) *Le mètre international définitif* (Paris: Masson), p. 140.

of light as an absolute standard of measurement—a dream that was not realized until Einstein's 1905 paper and later interpretations of the Michelson-Morley experiments appeared.

The director of the Bureau International de poids et measures explained how in the fin-de-siècle scientists were very close to reaching this old ideal. Using interferometric methods, Benoît explained, the metric unit could be compared to the length of light waves produced under fixed conditions:

> In this way, it is interesting to note, how the primitive conception of relating the unit of length to a natural, constant reference (or one that is reproducible at will in an identical form) which was considered for a longtime chimerical, has returned in the fin-de-siècle.

Acknowledging that basing standards of measurements on wavelengths was perhaps not as grandiose as the Jacobin's attempt to base the meter on the circumference of the world, Benoît argued that this method "in the end is not so different" [64].

Conclusion

Although techniques for determining the moment of contact which were located between scientific and popular cultures, such as chronophotography, became more successful than ever after the transit of Venus expeditions, critics remained. Influential criticisms were launched by the philosopher Henri Bergson, who described the age-old scientific practice of using static, sequential images to illustrate movement through time and dubbed it the "cinematographic method." Referring not merely to the cinematographic camera, but to the proclivity of the human mind for arranging temporal images spatially, he criticized its restrictiveness, and urged scientists to "set the cinematographical method aside" and search instead for a "second kind of knowledge"[65]. In a discussion where Bergson emphasized the constructed nature of our knowledge of physical phenomena, the mathematician Louis Couturat raised the counterexample of the transit: "An eclipse, or even better, the transit of Venus across the sun," he argued, was proof

---

[64] Benoît (1900) "De la précision", art. cit., p. 69.

[65] Henri Bergson (1991) "L'Évolution créatrice," in *Oeuvres* (Paris: Presses Universitaires de France, p. 784.

that some physical phenomena were highly precise and delimited events. Bergson disagreed: "C'est l'astronome qui [avec la méthode cinématographique] cueille cette position de l'astre sur la continuité de la courbe qu'il décrit"[66]. It was not only Venus's form that was elusive, but all forms: "there is no form, since form is immobile and reality is movement. What is real is the continual *change of* form: *form is only a snapshot view of transition*"[67]. Bergson's views had important repercussions for both science and philosophy of science. The renowned philosopher William James, for example, claimed that Bergson had compelled him to "give up the logic, fairly, squarely and irrevocably"[68].

This sentiment was prevalent in many scientific circles in the years before and after the transit, when most scientists shunned Janssen's cinematographic evidence and explored alternative methodologies. Supported in part by the prevalent disbelief in the results of the transit observations, such criticisms became a powerful and long-lasting justification for a sustained philosophical inquiry into scientific methodology. Not only did Bergson and his students campaign against the facile, cinematographic distinction between the discrete and the continuous in life and in logic, but—despite the government's best efforts—the results of the transits proved to even the most credulous the difficulties—perhaps even the impossibility—of eliminating individual differences and of finding an absolute standard of measurement.

Bergson was not the only one to shun Janssen's methods and address questions of space and time in new ways. In light of the highly contested results of the transits, physics started to play an increased role with regard to precision measurements. As a response to the problem of differing observations and in direct contrast to Janssen's cinematographic approach, new methods for determining the speed of light, advocated by Fizeau, Cornu, and Michelson, came to rival astronomical methods for determining the solar parallax and for finding an absolute measurement standard. In a dramatic reversal of the traditional roles of geometrical astronomy and physics, after the Franco-German war, physical methods were increasingly seen as offering "harder" types of evidence.

---

66 Henri Bergson (1972) "Le parallélisme psycho-physique et la métaphysique positive", *Mélanges* (Paris : P.U.F.), p. 502.

67 Henri Bergson (1991) "L'Évolution créatrice," in *Oeuvres* (Paris: Presses Universitaires de France, p. 750.

68 William James (1909) *A Pluralistic Universe* (Londres : Macmillan), p. 226.

Harvard University,
canales@fas.harvard.edu