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## Photogenic Venus: The "Cinematographic Turn" and Its Alternatives in Nineteenth-Century France

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# Photogenic Venus

## The “Cinematographic Turn” and Its Alternatives in Nineteenth-Century France

*By Jimena Canales\**

### ABSTRACT

During the late nineteenth century, scientists around the world disagreed as to the types of instruments and methods that should be used for determining the most important constant of celestial mechanics: the solar parallax. Venus’s 1874 transit across the sun was seen as the best opportunity for ending decades of debate. However, a mysterious “black drop” that appeared between Venus and the sun and individual differences in observations of the phenomenon brought traditional methods into disrepute. To combat these difficulties, the astronomer Jules Janssen devised a controversial new instrument, the “photographic revolver,” that photographed Venus at regular intervals. Another solution came from physicists, who rivaled 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. The new space emerging from this debate was characterized by a decline in faith in (nonstandardized, nonreproducible) photography and in (pure) geometry and by the growing realization of the importance of alternative elements needed for establishing scientific truths: power and authority, skill and discipline, standardization, mechanical reproducibility, and theatricality. By examining the “cinematographic turn” in science and its alternatives, this essay brings to light unexplored multidisciplinary connections that contribute to the histories of psychology, philosophy, physics, and film studies.

*Why, in other words, is not everything given at once, as on the film of the cinematograph?*

—Henri Bergson

**D**URING THE SECOND HALF OF THE NINETEENTH CENTURY scientists around the world disagreed about what types of instruments and methods should be used to

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observe the “astronomical event of the century”: Venus’s 1874 transit across the sun.<sup>1</sup> The transit was expected to close a century of debate surrounding the most important constant of celestial mechanics, the solar parallax. A reliable figure for the solar parallax would enable astronomers to determine the distance from the earth to the sun, set the dimensions of the solar system, and, using Newton’s law, deduce the masses of the planets.<sup>2</sup>

Astronomers had long known that observing the transit of Venus, which in 1874 would only be visible far from the European continent, was not the only way to establish a value for the solar parallax. But all known alternatives furnished radically different results. While observations of the previous transit (1769) moved most astronomers to settle on 8.57 seconds of arc, recent research on planetary movements as well as new determinations of the speed of light (1862) had led some to believe that the true value was around 8.86 seconds of arc.<sup>3</sup> In France, the Académie des Sciences, the Bureau des Longitudes, the

<sup>1</sup> Given its importance at the time, the current historiography of the transits of Venus is surprisingly limited. Secondary sources include Peter Hingley and Françoise Launay, “Passages de Vénus, 1874 et 1882,” in *Dans le champ des étoiles: Les photographes et le ciel 1850–2000* (Paris: Réunion des Musées Nationaux, 2000), pp. 119–121; Monique Sicard, “Passage de Vénus: Le revolver photographique de Jules Janssen,” *Études Photographiques*, 1998, 4:45–63; A. Chapman, “The Transits of Venus,” *Endeavour*, 1998, 22:148–151; David H. DeVorkin, “Venus 1882: Public, Parallax, and HNR,” *Sky and Telescope*, 1982, 22:524–526; DeVorkin, *Henry Norris Russell: Dean of American Astronomers* (Princeton, N.J.: Princeton Univ. Press, 2000), pp. 44, 46, 49, 60; Steven J. Dick, Wayne Orchiston, and Tom Love, “Simon Newcomb, William Harkness, and the Nineteenth-Century American Transit of Venus Expeditions,” *Journal for the History of Astronomy*, 1998, 29:221–255; Paul M. Janiczek and L. Houchins, “Transits of Venus and the American Expedition of 1874,” *Sky Telesc.*, 1974, 48:366–371; and Janiczek, “Remarks on the Transit of Venus Expedition of 1874,” in *Sky with Ocean Joined: Proceedings of the Sesquicentennial Symposia of the U.S. Naval Observatory, Dec. 5 and 8*, ed. Dick and LeRoy E. Dogget (Washington, D.C.: U.S. Naval Observatory, 1983), pp. 53–74. For a partial account of photography and the transits during this period that does not even mention Janssen’s photographic revolver see John Lankford, “Photography and the Nineteenth-Century Transits of Venus,” *Technology and Culture*, 1987, 28:648–657. Perhaps the most widely read English-language account of the transits was Richard A. Proctor, *Transits of Venus: A Popular Account of Past and Coming Transits* (New York: Worthington, 1875). For a thorough discussion of problems of evidence in science see the various essays in Arnold I. Davidson, James Chandler, and Harry Harootian, eds., *Questions of Evidence: Proof, Practice, and Persuasion across the Disciplines* (Chicago: Univ. Chicago Press, 1991); and Peter Galison, *How Experiments End* (Chicago: Univ. Chicago Press, 1987), Ch. 4.

<sup>2</sup> “Parallax” generally refers to the angular change of an object when it is observed from two different positions. If the distance between the two observational positions is known, it can be thought of as the base of a triangle that, when combined with measurements of the direction of the object as seen from both points, can be used to determine the distance to the object. The solar parallax can be determined using Halley’s method, which consisted in observing the transit of Venus across the sun. This was done either through the method of durations or the method of De l’Isle. In the method of durations the times of the transit as viewed from two different stations were determined, the lengths of the chords were deduced, and from these the least distance between the centers of the sun and Venus was found. However, since the method of durations required the observation of the whole transit, the method of De l’Isle was proposed. This method required the precise determination of the time of contact between Venus and the sun at two different stations, which were either time-coordinated or whose difference in longitude was accurately known. The precision of this method depended in great part on the accuracy of the longitude determinations.

<sup>3</sup> On the “Black Drop effect” see Bradley E. Schaefer, “The Transit of Venus and the Notorious Black Drop Effect,” *Journal for the History of Astronomy*, 2001, 34:325–336. The Berlin astronomer Franz Encke (1824) assigned the value of 8.57 seconds of arc to the solar parallax from his analysis of the eighteenth-century transits, which ranged from 8.1 to 9.4 seconds of arc. Observations of Mars’s oppositions against the sun convinced many astronomers to adopt a value exceeding 8.9 seconds of arc. Yet another method, using lunar theory, pointed to results around 8.916 (Peter Andreas Hansen) or 8.850 (Edward James Stone) seconds of arc. Using planetary theory and the equation of the moon, Urbain Le Verrier, director of the Paris Observatory, set the solar parallax at 8.859 seconds of arc. By adopting this value he was able to reconcile discrepancies in the theories of Venus, Earth, and Mars. For determining the exact time of contacts some astronomers advocated spectroscopic observations, while others used a double-image micrometer. A micrometer is a telescope accessory for measuring small angles. For descriptions of these methods see George Forbes, *The Transit of Venus* (Nature Series) (London: Macmillan, 1874); Edmond Dubois, “Nouvelle méthode pour déterminer la parallaxe de Vénus sans attendre les passages de 1874 ou 1882,” *Comptes Rendus des Séances de l’Académie des Sciences*, 20 Dec. 1869, 69:1290;

Observatoire de Paris, and the École Polytechnique all sponsored different types of evidence to determine the “true” value. To complicate matters further, proponents of these competing techniques were often split intellectually as well as institutionally. Even when a group of scientists agreed on a certain method, differing observations made with it rendered the results highly discordant. Who was right? Particularly puzzling was a controversial “black drop” that, according to some, mysteriously appeared between Venus and the sun exactly when astronomers had to time the apparent contact. At stake in these arguments was nothing less than the determination of “the scale of the universe” and the problem of other worlds. Still more important, the transit of Venus was connected to philosophical debates about the value of geometric methods in astronomy and the nature of space and time—all lofty issues tied to earthly concerns of governance, national prestige, and military might.

As the century progressed, astronomers increasingly repudiated the geometric methods that had characterized astronomy during the previous century. In the eighteenth century the British astronomer Edmond Halley (1656–1742) had claimed that the solar parallax could be determined with exactness by combining simple Euclidean triangulations with direct observations of Venus’s apparent contact with the sun. But more than a hundred years after Halley’s discovery, astronomers came to doubt the possibility of timing the contact between these two celestial bodies precisely. While some astronomers blamed the nervous systems of observers for discrepancies in results, others thought the instruments were at fault. Still others believed that the problems were due to increasingly unskilled and undisciplined observers in astronomy. And some claimed that problematic nongeometric contacts arose from neither observational nor instrumental errors but were due to actual astronomical phenomena that needed investigation. Perhaps most alarmingly, some mathematicians and philosophers were led by the rift between the ostensibly straightforward geometric methods Halley had proposed and their apparently chaotic results to question the very foundations of mathematics. As geometric certainty became harder to obtain, many “solutions” were devised, including artificial transit machines for training an observer’s responses or for measuring his delayed reactions and new cameras that photographed the event at short intervals. Yet none of these could eliminate insidious doubts as to the claims of scientific evidence to absolute truth.

Even before the Franco-Prussian war (1870–1871), but intensely afterward, the problems facing geometrical astronomy forced a new generation of astronomers to dirty their hands with physics, photography, cinematography, pedagogy, and “mimetic experimentation” that reproduced astronomical phenomena on earth, but on a smaller scale.<sup>4</sup> Astronomers learned to buttress their claims to truth by increasing their authority with the government and the lay public. They advocated international cooperation, standardization, mechanical reproducibility, and even theatricality—sometimes turning to techniques shared by popular culture. None of these efforts, however, proved entirely successful.

Although a unified and official effort to observe the 1874 transit of Venus was organized by the French government and by the most important institution of French science, the Académie des Sciences, divisions both within and without official circles prompted the

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Léon Foucault, “Détermination expérimentale de la vitesse de la lumière: Description des appareils,” *ibid.*, 1862, 55:792–796, Hervé Faye, “Note sur les nouvelles tables des planètes intérieures,” *ibid.*, 1862, 54:630; and Urbain Le Verrier, “Sur les masses des planètes et la parallaxe du soleil,” *ibid.*, 22 July 1872, 75:165–172.

<sup>4</sup> I borrow this term from Peter Galison and Alexi Assmus, “Artificial Clouds, Real Particles,” in *The Uses of Experiment: Studies in the Natural Sciences*, ed. David Gooding, Trevor Pinch, and Simon Schaffer (Cambridge: Cambridge Univ. Press, 1989), pp. 225–274.

emergence of alternative instruments and methods. Most notably, the astronomers Hervé Faye and Jules Janssen, who worked at the Bureau des Longitudes, Urbain Le Verrier, director of the Observatoire de Paris, Alfred Cornu from the École Polytechnique, and the physicist Armand Fizeau came to disagree with the academy's official prescriptions. In the end, no consensus was reached with respect to Venus, and scientists had to wait for the asteroid Eros (1932) to quell the controversy surrounding the value of the solar parallax, at least temporarily.<sup>5</sup>

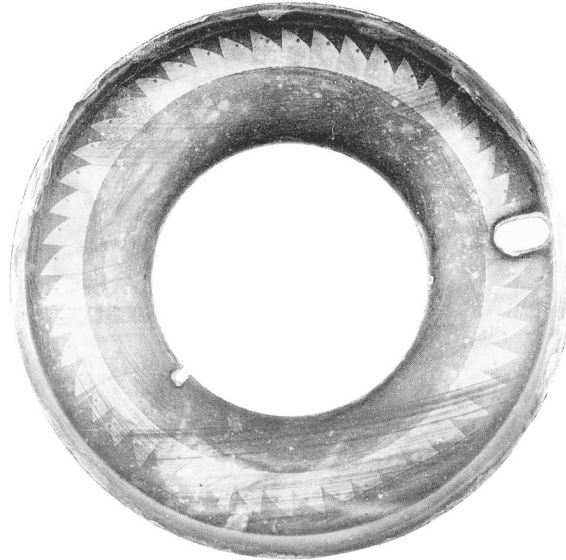
#### THE CINEMATOGRAPHIC TURN

Among the various methods championed by astronomers was a controversial new instrument, intriguingly named the "photographic revolver," that photographed Venus's transit across the sun at intervals of approximately one second. Invented by Jules Janssen to photograph the 1874 transit, it was arguably the most promising device for ending the discord as to the exact time of the planet's apparent contact with the sun.<sup>6</sup> Janssen's apparatus was soon modified and moved into other areas of science and culture, most famously to Étienne Jules Marey's physiological laboratory and then to the studio of the Lumière brothers, where it was gradually transformed into what would soon be called the cinematographic camera. Although the Marey and Lumière instruments differed markedly from Janssen's original, the applications of the "revolver" to the study of living beings as well as its inverse use for synthesizing images (either through projection or by arranging them on a rotating disk) 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. Despite some successes, Janssen did not initially convince everyone of the revolver's merits. In fact, the state-sponsored and official effort for determining the solar parallax, which was organized under the Commission for the Transit of Venus of the Académie des Sciences, did not adopt the device as its main instrument. Even advocates of photography were torn, some arguing on behalf of sequential photography and the advantages of mechanically reproducible methods while others remained fixed on time-tested nonreproducible daguerreotypes.<sup>7</sup>

<sup>5</sup> The value for the parallax commonly adopted after the Eros campaign was 8.790 seconds of arc. This number was soon displaced by the value derived from radar observations of the planets, 8.794148 seconds of arc. See R. d'E. Atkinson, "The Eros Parallax, 1930–31," *J. Hist. Astron.*, 1982, 13:77–83.

<sup>6</sup> In English, the revolver was sometimes called the "Janssen slide" or simply the "Janssen." On Janssen see David Aubin, "Orchestrating Observatory, Laboratory, and Field: Jules Janssen, the Spectroscope, and Travel," *Nuncius*, forthcoming; Françoise Launay, "Jules Janssen's In and Out Correspondence," in *100 Years of Observational Astronomy and Astrophysics: Homage to Miklos Konkoly Thege (1842–1916)*, ed. C. Sterken and J. B. Hearnshaw (Brussels: Sterken, 2001), pp. 159–168; Launay, "Jules Janssen et la photographie," in *Dans le champ des étoiles* (cit. n. 1), pp. 22–31; Aubin, "La métamorphose des éclipses de soleil," *Recherche*, June 1999, 321:78–83; and Marie-Claude Mahias, "Le soleil noir des Nilgiri: L'astronomie, la photographie et l'anthropologie physique en Inde du Sud," *Gradhiva: Revue d'Histoire et d'Archives de l'Anthropologie*, 1998, 24:33–56.

<sup>7</sup> For recent literature on visual practices in astronomy see John Lankford, "The Impact of Photography on Astronomy," in *Astrophysics and Twentieth-Century Astronomy*, ed. Owen Gingerich (General History of Astronomy) (Cambridge: Cambridge Univ. Press, 1984), pp. 16–39; Holly Rothermel, "Images of the Sun: Warren de la Rue, George Biddell Airy, and Celestial Photography," *British Journal for the History of Science*, 1993, 26:137–169; Alex Soojung-Kim Pang, "Victorian Observing Practices, Printing Technologies, and Representations of the Solar Corona, 1: The 1860's and 1870's," *J. Hist. Astron.*, 1994, 25:249–274; Pang, "Victorian Observing Practices, Printing Technologies, and Representations of the Solar Corona, 2: The Age of Photome-



**Figure 1.** Daguerreotype from Janssen's revolver showing Venus's contact with the sun. From Monique Sicard, "Passage de Vénus: Le revolver photographique de Jules Janssen," *Études Photographiques*, 1998, no. 4, pp. 45–63, on p. 63.

The new instruments and techniques employed during the transit of 1874 dramatically altered the exact sciences. More important, these new methods combined with other factors to change the nature of debates about the role of scientific evidence. The influential Nobel Prize winner, philosopher, and critic Henri Bergson 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 modern 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."<sup>8</sup> In a discussion where Bergson sought to emphasize the constructed and artificial 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 that some physical phenomena were highly precise and delimited events. Bergson disagreed, insisting that physical phenomena were never naturally delimited: "It is the astronomer that," with the cinematographic method, "catches the position of the planet from the continuous curve it traverses" (see Figure 1). It was not only Venus's form—even with the aid of Janssen's revolver—that was elusive, but all forms: "there is no form, since form

chanical Reproduction," *ibid.*, 1995, 26:63–75; Pang, "'Stars Should Henceforth Register Themselves': Astrophotography at the Early Lick Observatory," *Brit. J. Hist. Sci.*, 1997, 30:177–202; Simon Schaffer, "On Astronomical Drawing," in *Picturing Science, Producing Art*, ed. Caroline A. Jones and Peter Galison (New York: Routledge, 1998), pp. 441–474; Schaffer, "The Leviathan of Parsontown: Literary Technology and Scientific Representation," in *Inscribing Science*, ed. Timothy Lenoir (Stanford, Calif.: Stanford Univ. Press, 1998), pp. 182–222; and Pang, "Technology, Aesthetics, and the Development of Astrophotography at the Lick Observatory," *ibid.*, pp. 223–248.

<sup>8</sup> Henri Bergson, *Creative Evolution* (Mineola, N.Y.: Dover, 1998), p. 342.

is immobile and reality is movement. What is real is the continual *change of form: form is only a snapshot view of transition.*<sup>9</sup>

This sentiment was prevalent in many scientific circles in the years before and after the transit, when most scientists shunned Janssen's cinematographic type of evidence and explored alternative scientific or philosophical methodologies. Furthermore, the views of Bergson and his disciples 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."<sup>10</sup> Supported in part by the prevalent disbelief in the results of the transit observations, such criticisms became a powerful and long-lasting justification of the need for a sustained philosophical inquiry into scientific methodology. In this essay I will examine the "cinematographic turn" in science and its alternatives, showing how it affected astronomy, modern physics, and mathematics.

#### ORGANIZATION AND HISTORIOGRAPHY

Three interlaced histories run through this essay. All have broad historiographic traditions of their own, but they have not been studied in relation to each other. The topic of the first section, "Individual Differences," has traditionally been considered part of the history of psychology; the implications of the problem of differing observations for the exact sciences have been largely ignored.<sup>11</sup> In this section I focus on how the various solutions proposed for eliminating such differences in the particular case of the transits of Venus were connected to certain ideals of objectivity. Here two dominant strategies are evident, one based on discipline and training and the other on the use of photography.

The second section, "The Photographic Revolver," deals with a third method for eliminating individual differences in observations. This method was also photographic, but it differed dramatically from the daguerreian photography analyzed in the previous section. It was most forcefully advocated by Janssen, who sought to introduce mechanical reproducibility, standardization, and technologies for synthesizing chronophotography that would give sequential images the illusion of movement. His strategy for obtaining assent in visual matters was not based on discipline and training or on the geometrical optics of the *camera obscura* and artisanal daguerreotypes but, rather, on new technologies of mass media that emerged alongside the increased industrialization of the late nineteenth century. The themes of this section have typically been studied from the perspective of art history or film studies; their relation to the exact sciences has not been appreciated.

In "The New Logic" I approach the controversy mainly from the perspective of mathematics and physics. Although many of the events described here have been analyzed

<sup>9</sup> "C'est l'astronome qui *cueille* cette position de l'astre sur la continuité de la courbe qu'il décrit." Henri Bergson, "Le parallélisme psycho-physique et la métaphysique positive," in *Mélanges* (1901; Paris: Presses Univ. France, 1972), pp. 463–502, on p. 502; and Bergson, *Creative Evolution*, p. 302. (Here and throughout this essay, translations into English are mine unless otherwise indicated.)

<sup>10</sup> William James, *A Pluralistic Universe* (London: Macmillan, 1909), p. 226, cited in Paul Douglass, "Bergson and Cinema: Friends or Foes," in *The New Bergson*, ed. John Mullarkey (Manchester: Manchester Univ. Press, 1999), pp. 209–227, on p. 219.

<sup>11</sup> Some exceptions are Simon Schaffer, "Astronomers Mark Time: Discipline and the Personal Equation," *Science in Context*, 1988, 2:115–145; and Jimena Canales, "The Single Eye: Re-evaluating *Ancien Régime* Science," *History of Science*, 2001, 39:71–94. For standard treatments within psychology see Edwin G. Boring, *A History of Experimental Psychology* (New York: Appleton-Century-Crofts, 1929); and Boring, "The Beginning and Growth of Measurement in Psychology," in *Quantification: A History of the Meaning of Measurement in the Natural and Social Sciences*, ed. Harry Woolf (Indianapolis: Bobbs-Merrill, 1961), pp. 108–118.

before in histories of physics, mathematics, and philosophy, their relation to the personal equation has been all but forgotten. In fact, the problem was intimately connected to controversies surrounding the foundations of mathematics in two important ways. First, as geometrical methods in astronomy were increasingly considered impotent, the relation of mathematics to the experimental sciences was questioned. Second, the difficulties surrounding the elimination of individual differences in observational results called into doubt the existence of absolute and natural standards of measurement. Physicists themselves tried to circumvent the problems raised by these differing observations in novel ways. In particular, some physicists and astronomers renewed experiments on the speed of light and its relation to the ether in order to find an alternative solution to the problem of differing observations plaguing science.

#### INDIVIDUAL DIFFERENCES

When astronomers during the late nineteenth century reviewed observations of the 1769 transit of Venus the conclusion was appalling: different people saw differently. Scientists, politicians, and even Napoleon III worriedly debated the nature of these differences, asked whether they were due to the fluctuating conditions of the phenomenon itself, to the different instruments employed, to the visual or mental apparatus of the observers, or, in some rare cases, to outright dishonesty.<sup>12</sup> Quite apart from the larger political and juridical consequences of disagreement, the immediate problems were insurmountable: if the solar parallax remained closer to the value determined by the Berlin astronomer Franz Encke in 1824, astronomers would have to posit the existence of a 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.<sup>13</sup> In

<sup>12</sup> The observations of the Jesuit priest Maximilian Hell (1720–1792) were particularly suspect.

<sup>13</sup> Faye explained the looming crisis: “If we persist in our false evaluation of the parallax, the hypothesis that there exists some other planet which has gone unperceived until now . . . will need to be considered. And, since we cannot see such a probable planet, science finds itself forced into an impasse. . . . All the discordances, all the contradictions which menace the future and, to some degree, the present of astronomy, will disappear if the direct determination of solar parallax . . . gives us 8",9 instead of 8",57.” Hervé Faye, “Association Française pour l’Avancement des Sciences, congrès de Lille, conférences publiques: Le prochain passage de Vénus sur le soleil,” *Revue Scientifique*, 17 Oct. 1874, 14(16):361–369, on p. 367. The value of the solar parallax was directly connected to the values of the masses of the planets and to the problem of Mercury’s perihelion, which would enthrall astronomers until it was explained by Einstein’s theory of general relativity. After fixing a value for the solar parallax and for the mass of Earth from observations of Earth, Mars, and Venus, Le Verrier posited a “missing mass” to explain Mercury’s perihelion. He believed that this mass might be found in the form of either a planet (commonly referred to as Vulcan) or smaller intramercurial planets. If discovered, they would be a second triumph for Le Verrier after his magnificent “discovery” of Neptune. However, their existence remained highly controversial. Building on Le Verrier’s work, Faye claimed that the mass of Earth deduced from current values of the solar parallax could be shown to be erroneous by its long-term effects on the orbits of Mars and Venus. He also based his claim on recent work on the oppositions of Mars (George Biddell Airy), parallactic inequalities of the moon (Peter Andreas Hansen), and the speed of light (Léon Foucault). For histories of the problem of Mercury’s perihelion that discuss Le Verrier’s role see N. T. Roseveare, “Leverrier to Einstein: A Review of the Mercury Problem,” *Vistas in Astronomy*, 1979, 23:165–171; and R. A. Lyttleton, “History of the Mass of Mercury,” *Quarterly Journal of the Royal Astronomical Society*, 1980, 21:400–413. On the relation between the orbit of Mercury, the so-called planet Vulcan, and the implications for Newtonian theory see William Sheehan and Richard Baum, *In Search of Planet Vulcan: The Ghost in Newton’s Clockwork Universe* (New York: Plenum, 1997). The astronomer and popularizer of science Camille Flammarion believed the results of the transits confirmed the existence of a Venusian atmosphere that proved to him that “this planet is a world like ours.” Flammarion was not alone; even Jules Janssen believed that his investigations of water vapor were done to prove “the presence of this aqueous element which plays such a considerable role in the development of life on the surface of a world.” While in 1874 Janssen claimed to have proved the existence of water vapor in the Venusian atmosphere, after 1882 he retracted this claim. See Camille Flammarion, “Le passage de Vénus:



short, scientists and their institutions risked being on the dangerous ground of convention, speculation, and possibility.

Even before the problem of individual differences in observation leaked to the general public, governments across the world became concerned. Napoleon III's positivistic empire was the first in France to preoccupy itself with these strange divergences.<sup>14</sup> In 1869 the minister of public instruction, Victor Duruy, addressed a letter to the academy charging "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 to overcome the obstacles that had voided the observations made in the previous century. The problem, Hervé Faye explained, should be solved "no matter the cost."<sup>15</sup>

In 1866 the astronomer Charles Delaunay, an opponent of Le Verrier who would oust him as director of the Paris Observatory in 1870, 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 Venus and the sun (see Figure 2), 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."<sup>16</sup>

This problem became particularly pertinent after a transit of Mercury on 4 November 1868, visible in Europe, again produced only discordant results. After presenting a brief

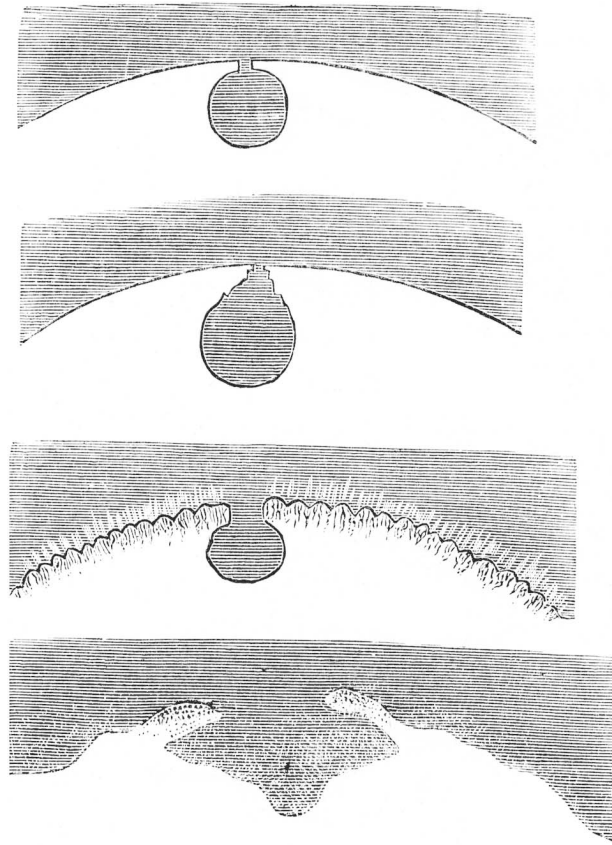
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Résultats des expéditions françaises," *Nature: Revue des Sciences et de Leurs Applications aux Arts et à l'Industrie*, 1875, 3:356–358, on p. 394; Jules Janssen, "Sur l'éclipse totale du 22 décembre prochain," *Comptes Rend. Séances Acad. Sci.*, 24 Oct. 1870, 71:531–533, on p. 531; and Janssen, "Note sur l'observation du passage de la planète Vénus sur le soleil," *ibid.*, 29 Jan. 1883, 96:288–292.

<sup>14</sup> In 1866 a commission presided over by the admiral Jurien de la Gravière was charged by the Ministère de l'Instruction Publique to aid astronomers in their observations of the next transit. On 15 July 1870 a *projet de loi* was sent to the Conseil d'État, which was interrupted by the war. Before the war the commission was composed of the admiral François Paris, Faye, Ernest Laugier, Antoine Yvon-Villargeau, and Victor Puiseux, among others. See A. Gautier, "Travaux scientifiques étrangers: Travaux et faits astronomiques récents," *Rev. Sci.*, 22 Dec. 1871, 8:616–622, on p. 618; and Victor Puiseux, "Note sur la détermination de la parallaxe du soleil, par l'observation du passage de Vénus sur cet astre en 1874," *Comptes Rend. Séances Acad. Sci.*, 8 Feb. 1869, 68:321–324. Puiseux's work was recounted in "Académie des Sciences: Séance du lundi 8 février," *Mondes: Revue Hebdomadaire des Sciences et de Leurs Applications aux Arts et à l'Industrie par M. l'Abbé Moigno*, 1869, 19:231–232. See also *Journal Officiel de la République Française*, 1 Sept. 1872, p. 5796; and "Project de Loi" from Napoleon III, 14 June 1870, and Puiseux, "Rapport," 28 Oct. 1866, Archives Nationales (AN), F17 2928-1, Folder C: Commission de l'Académie des Sciences, Travaux, Préparations, etc., pp. 1–10.

<sup>15</sup> Ministère de l'Instruction Publique, "Lettre," *Comptes Rend. Séances Acad. Sci.*, 1 Feb. 1869, 68:205–206, on p. 205. The meeting at the academy was recounted in "Académie des Sciences: Séance du lundi 1<sup>er</sup> février," *Mondes*, 1869, 19:212–213. The minister's letter was reprinted in Ministère de l'Instruction Publique, "Observation du passage de Vénus," *ibid.*, 18 Feb. 1869, 19:239–240; see also Victor Duruy, *J. Officiel Répub. Française*, 2 Feb. 1869. 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 Faye, "Association Française pour l'Avancement des Sciences, congrès de Lille" (cit. n. 13), p. 361; *J. Officiel Répub. Française*, 27 July 1872, p. 5131; Chambre des Députés, "Chambre des députés—Annexe no. 445: Séance du 3 août 1876," *ibid.*, 14 Sept. 1876, pp. 695–697; and "Loi ouvrant au ministre de l'instruction publique et des beaux-arts sur l'exercice 1876 un crédit supplémentaire de 90,136 fr. 39, applicable à des dépenses relatives à la détermination de la parallaxe du soleil," *ibid.*, 27 Mar. 1877, p. 2465. The term "scientific missionaries" appeared in Jules Janssen, "Présentation de quelques spécimens de photographies solaires obtenues avec un appareil construit pour la mission du Japon," *Comptes Rend. Séances Acad. Sci.*, 22 June 1874, 78:1730–1731, on p. 1731.

<sup>16</sup> Charles Delaunay, "Notice sur la distance du soleil à la terre, extrait de l'Annuaire pour l'an 1866, publié par le Bureau des Longitudes," in *Recueil de mémoires, rapports et documents relatifs à l'observation du passage de Vénus sur le soleil* (Paris: Firmin Didot, 1874), pp. 3–110, on pp. 43, 97–98. The "goutte noire" was alternatively called the "ligament noir."



**Figure 2.** Illustration of Venus's contact with the sun according to the observations of 1769. From George Forbes, *The Transit of Venus (Nature Series)* (London: Macmillan, 1874), p. 49.

history of the differences plaguing previous transit observations, Charles Wolf, an astronomer employed at the Paris Observatory who had an important role in the preparations for the transits, categorically remarked: "Astronomy has not progressed since 1769." In particular, Wolf blamed telescopic aberration and took a jab at his superior, Le Verrier, who for observing Mercury's transit had "placed himself in the same conditions as the astronomers of the previous century, . . . saw the same phenomenon they saw, and obtained a number in accord to those of their observations."<sup>17</sup>

In an article that appeared in the widely read journal *La Nature* Wilfrid de Fonvielle, an important popularizer of science, described "that which was seen" in the transits of 1761 and 1769. He found discordant results even when astronomers observed side-by-side

<sup>17</sup> Charles Wolf, "Le passage de Vénus sur le soleil en 1874," *Rev. Sci.*, 20 Apr. 1872, 9:1006–1011, on p. 1008. Clearly, eliminating differences in observation was seen not only as central to astronomy but as its defining measure of progress. Le Verrier observed the transit of Mercury from Marseille. He did not believe that telescopic aberration was the main problem, pointing out that astronomers had long differed in their measurements of the sun's diameter. He claimed the different measurements of diameters were in part responsible for the different times estimated during the transit. This theory was first stated in Urbain Le Verrier, "Théorie et tables du mouvement apparent du soleil," *Annales de l'Observatoire Impérial de Paris*, 1858, 4:1–262, on p. 69.

and with the same instruments. He suggested that the discrepancies were due mainly to the “black drop,” which he described as “a mysterious object with very strange variations.” Other astronomers similarly reported 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.”<sup>18</sup>

### *Discipline and Skill*

To address the problem of divergent observations, the full authority of the Commission for the Transit of Venus was thrown behind certain official methods and instruments. The commission, headed by the famed chemist and statesman Jean Baptiste Dumas, opted to rely chiefly on well-trained observers and on specific photographic instruments. In particular, it sponsored the work of Wolf, who had already addressed the problem of individual discrepancies in meridian transit observations.

After a century of disagreements, in 1869 Wolf and his collaborator Charles André reported to the academy on their investigations to find “on which side truth lay.” They concluded that observers saw differently primarily 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, and 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.”<sup>19</sup>

Wolf and André claimed to have solved the “black drop” mystery by using an apparatus that artificially reproduced the transit of Venus across the sun. A few years earlier Wolf had used artificial stars to measure individual errors in the stellar transit observations employed for determining time and longitude. His machine enabled him to measure the different times at which observers reacted to an artificial star crossing the wires of a meridian transit instrument. He would then obtain the observer’s “psychological time” by

<sup>18</sup> Wilfrid de Fonvielle, “Les derniers passages de Vénus,” *Nature*, 1874, 2(43):257–259, on pp. 257, 259; Faye, “Association Française pour l’Avancement des Sciences, congrès de Lille” (cit. n. 13), p. 365; and Hervé Faye, “Sur les passages de Vénus et la parallaxe du soleil,” *Comptes Rend. Séances Acad. Sci.*, 4 Jan. 1869, 68:42–50, on p. 42, rpt. in Faye, “Académie des Sciences: Séance du lundi 4 janvier,” *Mondes*, 1869, 19:42–44.

<sup>19</sup> Charles Wolf and Charles André, “Recherches sur les apparences singulières qui ont souvent accompagné l’observation des contacts de Mercure et de Vénus avec le bord du soleil, Mémoire présenté à l’Académie des sciences, dans sa séance du 1<sup>er</sup> mars 1869,” in *Recueil de mémoires, rapports et documents* (cit. n. 16), pp. 115–172, on pp. 125, 130. Their initial work was reported in “Académie des Sciences: Séance du lundi 25 janvier,” *Mondes*, 1869, 19:174. It was continued in “Académie des Sciences: Séance du lundi 1<sup>er</sup> Mars,” *Mondes*, 1869, 19:361; and later recounted in Gautier, “Travaux scientifiques étrangers” (cit. n. 14), p. 618 n 1; “Académie des sciences de Paris: Séance du 4 décembre 1871,” *Rev. Sci.*, 9 Dec. 1871, 8:575; and Wolf, “Le passage de Vénus sur le soleil en 1874, conférence faite à la société des Amis des Sciences, le 29 mai 1873,” in *Recueil de mémoires, rapports et documents* (cit. n. 16), pp. 377–401. A review appeared in Wolf and André, “Sur le passage de Mercure du 4 novembre 1868, et les conséquences à en déduire relativement à l’observation du prochain passage de Vénus,” *Comptes Rend. Séances Acad. Sci.*, 25 Jan. 1869, 68:181–183. The full report submitted to the academy appeared *ibid.*, 1 Mar. 1869, 68:525; see also a letter describing the continuation of their work with artificial transits: André, 11 Nov. 1876, Archives de l’Académie des Sciences (AAS), Carton 1645, Folder: Passage du Vénus, André.

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.<sup>20</sup>

The artificial transit machine was based on the same principles as Wolf’s artificial star machine for time and longitude determinations. Wolf aimed a telescope from the Paris Observatory at the library at the Senate in the Jardin 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 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 objective-glass free of aberration (such as those made by Léon Foucault) was used and the instrument was aimed properly (see Figure 3). Contradicting those who believed that the “black drop” was an inherent astronomical or physiological phenomenon, Wolf and André 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 so that it could be factored into the final result.

The most important conclusion of their paper was that the “black drop” was not a necessary impediment to observation and that, with the right instruments and training, observers could almost see the geometric contact expected, centuries earlier, by Halley when he claimed that the solar parallax could be unambiguously determined by observing Venus’s apparent contact with the sun. For the moment, Wolf and André had vindicated the observational methods that were being so 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 of science 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.”<sup>21</sup>

While Wolf denied that there was either an astronomical or a physiological source for the “black drop,” one aspect of the problem persisted. In an article published in the popular *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 be compared against each other “in order to determine their personal equations, if they exist.” Le Verrier backed Wolf’s suggestion, insisting that future expeditions should be manned by “only those observers who have been compared amongst themselves.”<sup>22</sup>

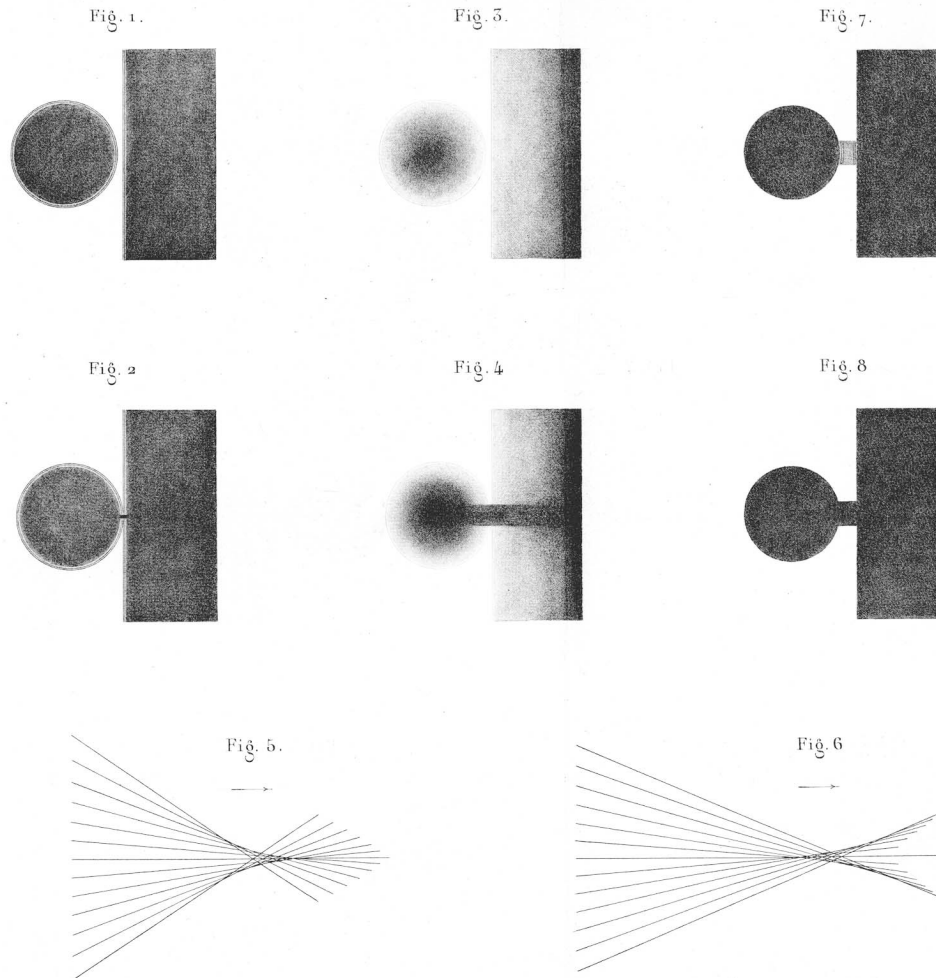
According to Edmond Dubois, who wrote a popular account of the work surrounding the transit of Venus, Wolf and André’s conclusions showed that “almost constant and VERY SIGNIFICANT differences persist between different observers, especially in estimating the time of . . . contacts.” Even the experiments at the venerable Senate could not entirely reconcile the disabling problem of individual differences. Rodolphe Radau, a scientist and

<sup>20</sup> The Russians, Germans, Americans, and English also built artificial transit machines for the practice of observers. On Hirsch see Jimena Canales, “Exit the Frog, Enter the Human: Physiology and Experimental Psychology in Nineteenth-Century Astronomy,” *Brit. J. Hist. Sci.*, 2001, 34:173–197.

<sup>21</sup> Wolf, “Passage de Vénus sur le soleil en 1874” (cit. n. 17), p. 1009.

<sup>22</sup> *Ibid.*, p. 1010; and Urbain Le Verrier, “Sur les passages de Vénus et la parallaxe du soleil,” *Comptes Rend. Séances Acad. Sci.*, 4 Jan. 1869, 68:49–50, on p. 49.

*Recherches sur l'observation des contacts de Mercure et de Vénus, par M. M. C. Wolf et C. André.*



**Figure 3.** Illustration of Wolf and André's experiments on the black drop. From Charles Wolf and Charles André, "Recherches sur les apparences singulières qui ont souvent accompagné l'observation des contacts de Mercure et de Vénus avec le bord du Soleil," in *Recueil de mémoires, rapports et documents relatifs à l'observation du passage de Vénus sur le soleil* (Paris: Firmin Didot, 1874), pp. 115–172, on p. 72.

popularizer, discussed Wolf and André's work in the widely read *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 Wolf and André's 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.<sup>23</sup>

<sup>23</sup> Edmond Dubois, *Les passages de Vénus sur le disque solaire* (Paris: Gauthier-Villars, 1873), p. 154; Rodolphe Radau, "Le passage de Vénus du 9 décembre 1874," *Revue des Deux Mondes*, 15 Jan. 1874, 1:434–449,

### *Photography and the Commission*

Disillusioned by the transitory nature of the “fleeting instants of calm which the English astronomers call a *glimpse*,” the astronomer 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.<sup>24</sup> Interested in both the physical and the physiological aspects of the problem, he suggested that astronomers couple their observations with “a detailed account of the physical phenomena and include drawings.” More importantly, he urged them to use photography, where “everything is automatic.” For the Venusian transits he imagined an apparatus analogous to the one he had previously used for photographing stellar transits and described it in detail. 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.<sup>25</sup>

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 (J. G. Bourbouze, Cornu, Adolphe Martin, Fizeau, Wolf, Yvon-Villarceau) who contributed to the design of the photographic instruments for the transit and ran test measurements on the photographic plates.<sup>26</sup> The overwhelming conclusion at the time was that photographic observations 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.”<sup>27</sup>

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on p. 446, rpt. in Radau, “Le passage de Vénus du 9 décembre 1874,” *Moniteur Scientifique*, 1874, 16:474–484; and Wolf and André, “Passage de Mercure du 4 novembre 1868” (cit. n. 19), pp. 181–183.

<sup>24</sup> Hervé Faye, “Sur l’observation photographique des passages de Vénus et sur un appareil de M. Laussedat,” *Comptes Rend. Séances Acad. Sci.*, 14 Mar. 1870, 70:541–548, on p. 541 (italics in the original); and Faye, “Rapport sur le rôle de la photographie dans l’observation du passage de Vénus, extrait des comptes rendus hebdomadaires des séances de l’Académie des Sciences, séance du 2 septembre 1872,” in *Recueil de mémoires, rapports et documents* (cit. n. 16), pp. 227–236, on p. 228. On “mechanical objectivity” see Peter Galison and Lorraine Daston, “The Image of Objectivity,” *Representations*, 1992, 40:81–128.

<sup>25</sup> Hervé Faye, “Sur les passages de Vénus et la parallaxe du soleil,” *Comptes Rend. Séances Acad. Sci.*, 11 Jan. 1869, 68:69–74, on pp. 71, 72; reported in Faye, “Académie des Sciences: Séance du lundi 11 janvier,” *Mondes*, 1869, 19:85–86; and Faye, “Association Française pour l’Avancement des Sciences, congrès de Lille” (cit. n. 13), p. 366. Other advocates of photography were Warren de la Rue, the American Simon Newcomb, and the German Friedrich Paschen. For a letter describing Faye’s phototelegraphic experiments see Hervé Faye, Paris, 31 Oct. 1861, Preußischer Staatsbibliothek Berlin, Sammlung Darmstädter J 1846(6) Faye, 3.

<sup>26</sup> Faye, “Association Française pour l’Avancement des Sciences, congrès de Lille,” p. 366 (“observer does not intervene,” “nature itself”); and Faye, “Observation photographique des passages de Vénus et sur un appareil de M. Laussedat” (cit. n. 24), p. 543 (“suppressing the observer”). For the commission’s deliberations on photography see Jean Baptiste Dumas and Elie de Beaumont (Secretaires perpetuels de l’Académie) to Ministre de l’Instruction Publique, Paris, 3 Feb. 1873, AN, F17 2928-1, Folder C: Commission de l’Académie des Sciences, Travaux, Préparations, etc., p. 3.

<sup>27</sup> Heliometry uses a divided object-glass micrometer for the accurate measurement of angles. Charles Wolf and Antoine Yvon-Villarceau, “Rapport sur les mesures micrométriques directes à faire pour l’observation du passage de Vénus, présenté à la Commission du passage de Vénus, dans la séance du 8 mars 1873,” in *Recueil de mémoires, rapports et documents* (cit. n. 16), pp. 337–344, on p. 339. Celestial photography was hampered by the difficulty of following an object across the sky with the telescope (in order to keep it focused for the whole exposure time). This difficulty was solved in part by Colonel Aimé de Laussedat, whose expertise on the increasing role played by science in times of war would later make him the ideal choice for directing the Conservatoire des Arts et Métiers. Laussedat’s idea consisted in following the celestial object with a flat, moving

Despite this virtue, the views of the commission and of Faye on photography soon diverged. When Faye presided over the commission before Dumas became its president, 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.”<sup>28</sup> Now headed by Dumas, the commission eventually settled on metallic daguerreotypes instead of using collodion, the process chosen by most other nations and from which paper prints could be made. In the opinion of the commission, the advantage of metal daguerreotypes’ “conditions of inflexibility and invariability, not offered by either paper or glass,” outweighed the fact that they were not easily reproducible and that the images they captured might not be comparable to those of other nations. On this point Faye and Janssen disagreed with the commission’s prescriptions. And here, to understand the controversy, we must delve into the various differences classed under the general (and at times misleading) rubric of “photography,” especially the distinction drawn by the twentieth-century Marxist aesthete Walter Benjamin between daguerreian photography and mechanically reproducible photography.<sup>29</sup> As we will see, there were sharp divisions among those who supported the use of photography.

#### THE PHOTOGRAPHIC REVOLVER

To Janssen’s way of thinking, there was something particularly photogenic about Venus. “For me,” he explained, “it is the observation of the transit of Venus which specifically

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mirror and reflecting the image back to a fixed telescope. The use of mirrors, which were flattened by a process invented by Adolphe Martin, convinced Wolf that “the image of the sky in the mirror is identical to the sky itself.” Since it allegedly rendered the rotating heavens static, Foucault named this new instrumental configuration the “siderostat.” There was considerable debate surrounding its invention. While some attributed it to Laussedat, others credited Foucault or Fizeau. Laussedat, sponsored by the *Ministre de la Guerre*, employed his apparatus to record photographically the moon’s passage over the sun twice in Algeria in 1860 and once in 1867. He was aided by Girard in photographic matters. See Aimé Laussedat, “Réclamation de priorité, au sujet du principe de l’appareil photographique adopté par la Commission du passage de Vénus: Extrait d’une Lettre de M. Laussedat à M. Élie de Beaumont,” *Comptes Rend. Séances Acad. Sci.*, 17 Aug. 1874, 79:455–457; Laussedat, *De l’influence civilisatrice des sciences appliquées aux arts et à l’industrie: Discours prononcé à Oran, le 29 mars 1888, à la séance d’ouverture du congrès de la Association Française pour l’Avancement des Sciences* (Paris: Imprimerie Nationale, 1888); Charles Wolf, “Rapport sur les miroirs plans en verre argenté exécutés par M. Ad. Martin pour la Commission du passage de Vénus, rapport lu à la Commission du passage de Vénus, le 3 janvier 1874,” in *Recueil de mémoires, rapports et documents*, pp. 453–455, on p. 455 (the image of the sky); and Aimé Laussedat to Hervé Faye, 20 Feb. 1870, rpt. in Faye, “Observation photographique des passages de Vénus et sur un appareil de M. Laussedat.”

<sup>28</sup> Jules Janssen to the *Ministre de l’Instruction Publique*, 6 Apr. 1876, p. 5, AN, F17 2928-2, Folder: Commission chargée d’examiner les comptes des dépenses de M. Janssen, nommée le 16 mars 1876, p. 5; and Jean Baptiste Dumas and Elie de Beaumont (Secrétaires perpétuels de l’Académie) to the *Ministre de l’Instruction Publique*, Paris, 3 Feb. 1873, AN, F17 2928-1, Folder C: Commission de l’Académie des Sciences, Travaux, Préparations, etc., p. 2. After Delaunay’s death, Faye became president of the commission on 1 Sept. 1872. He explained the reasons for his departure and countered alternative theories in Faye to the *Director of the Revue*, Passy, 3 Apr. 1873, AAS, Carton 1645, Folder: Faye.

<sup>29</sup> Walter Benjamin, “The Work of Art in the Age of Mechanical Reproduction,” in *Illuminations*, ed. Hannah Arendt (New York: Schocken, 1969), pp. 217–251. Adolphe Martin called attention to the superiority of daguerreotypes, and Yvon-Villarcéau, Wolf, Cornu, and Fizeau all agreed. For a manuscript of Fizeau’s work see Armand Fizeau, “Rapport sur l’appareil photographique,” Paris, 8 Mar. 1873, AAS, Carton 1645, Folder: Passage de Vénus, M. Fizeau. The instructions on the use of the photographic apparatus explain how during critical moments observers should take photographs every 10 seconds. Foerster disputed the superiority of daguerreotypes when compared to collodion. See *Ministère de l’Instruction Publique*, “Première séance: Mercredi 5 Octobre 1881,” in *Conférence internationale du passage de Vénus* (Paris: Imprimerie Nationale, 1881), pp. 1–11, on p. 6.

directed my attention to this fertile area.” 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.<sup>30</sup> His method—which he soon baptized the “photographic revolver”—closely followed Faye’s idea of photographing in a single plate sequences separated by a second.<sup>31</sup> Not only would this apparatus ostensibly suppress the observer, as Faye dreamed, but it also would permit the study of the physical circumstances 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.” But in defending photography, he wanted to do more than simply suppress the observer. He seized on one of the attributes of photography advertised by François Arago in his famed speech to the Chambre des Députés in 1839. For Janssen, photographs not only showed “faithful images, free from the intervention of the human hand,” but in giving concordant results where the eye could not, they showed a “new world” that corresponded much better to the “reality of things.” The difference between ocular and photographic images was not a problem, but a virtue, proving the “advantages of [the photographic] method.”<sup>32</sup> Although Janssen disagreed with the official prescriptions of the commission with respect to photography, “a spirit of discipline” compelled him to follow its prescribed methods during the expedition he was charged to lead to Japan.<sup>33</sup> Nevertheless, in addition to the officially sanctioned instruments, Janssen brought along some unauthorized ones, including his controversial “revolver” (see Figure 4).

### *Controversy, Cooperation, and Photography*

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.<sup>34</sup>

<sup>30</sup> Janssen, “Présentation de quelques spécimens de photographies solaires obtenues avec un appareil construit pour la mission du Japon” (cit. n. 15), p. 1730 (photogenic); Victor Puiseux to Ministre de l’Instruction Publique, Paris, 12 Feb. 1874, AN, F17 2928-2, Folder: M. Janssen Mission dans l’Asie Orientale, Eclipse de Soleil du 6 avril 1875, p. 2 (“missionary”); Hervé Faye, “Sur l’expédition de M. Janssen,” *Comptes Rend. Séances Acad. Sci.*, 12 Dec. 1870, 71:819–822, on p. 821 (“transient phenomena”); and Jules Janssen, “Passage de Vénus: Méthode pour obtenir photographiquement l’instant des contacts, avec les circonstances physiques qu’ils présentent,” *ibid.*, 17 Mar. 1873, 76:677–679. The last note was presented earlier to the Commission for the Transit of Venus; see Janssen, “Méthode pour obtenir photographiquement les circonstances physiques des contacts avec les temps correspondents, communication faite à la Commission du passage de Vénus, dans sa séance du 15 février 1873,” in *Recueil de mémoires, rapports et documents* (cit. n. 16), pp. 295–298.

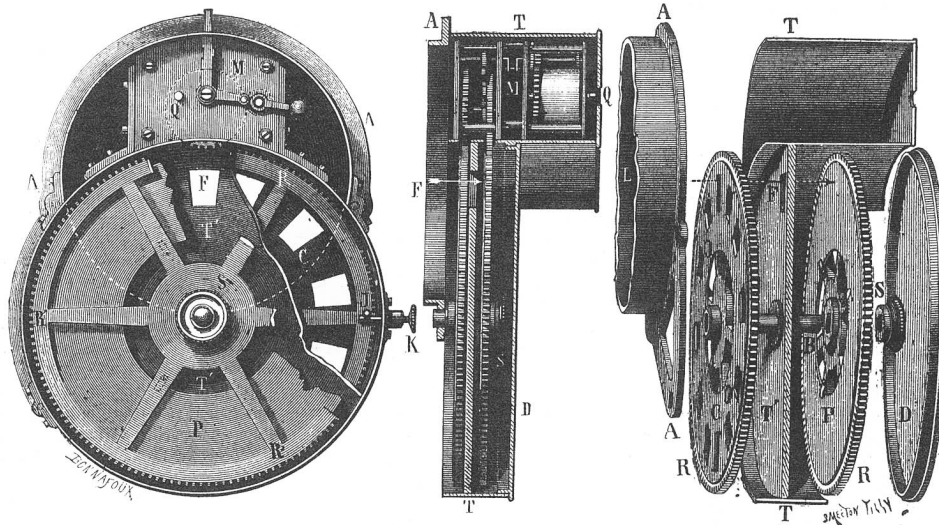
<sup>31</sup> At first Janssen tried a model built by Deschiens, which did not work properly since the instrument shook too much, and later he worked with the Rédier family to build the final prototype. For the transits of 1874 and 1882 Janssen also used a photographic telescope built by Steinheil. For the transit of 1882 Pasteur, a photographer from the Meudon Observatory, aided him. This time he focused on the physical conditions of the phenomenon and not on determining the solar parallax.

<sup>32</sup> Jules Janssen, “Sur la constitution de la surface solaire et sur la photographie envisagée comme moyen de découverte en astronomie physique,” *Comptes Rend. Séances Acad. Sci.*, 31 Dec. 1877, 85:1250–1255, on pp. 1250–1251.

<sup>33</sup> Janssen to the Ministre de l’Instruction Publique, 6 Apr. 1876, AN, F17 2928-2, Folder: Commission chargée d’examiner les comptes des dépenses de M. Janssen, nommée le 16 mars 1876, pp. 5–6.

<sup>34</sup> In Britain Airy at first determined the parallax to be 8.75 seconds of arc from the direct observations of the English expeditions, then settled on 8.82 seconds of arc. Simon Newcomb, in the United States, took as a mean value 8.85 seconds of arc; he then weighted this result against other methods and came up with a total value of 8.80 seconds of arc, which was adopted at the Conference Internationale des Étoiles Fondamentales held in Paris (1896). See Dick *et al.*, “Newcomb, Harkness, and the Nineteenth-Century American Transit of Venus Expeditions” (cit. n. 1), p. 248.





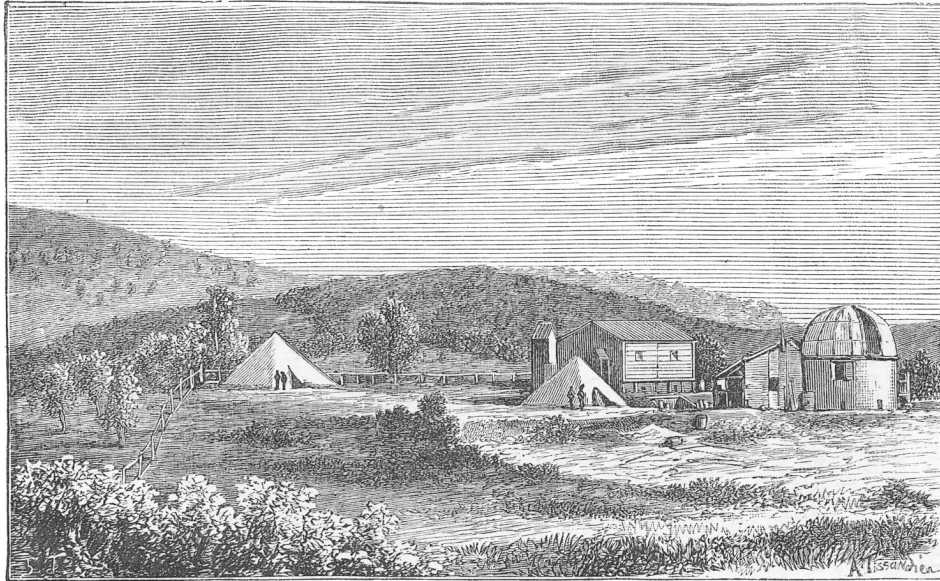
**Figure 4.** Janssen's photographic revolver. From Camille Flammarion, "Le passage de Vénus: Résultats des expéditions françaises," *Nature*, 1875, 3:356–358, on p. 356.

Even before the Navy ships set sail to Saint Paul and Campbell, Beijing (Peking), New Caledonia (Nouméa), Vietnam (Saigon), and Japan, optimism about the commission's methods was fading (see Figure 5). One critic, the *mécanicien* Montagne, who at the last moment refused to participate in the expedition, protested that the astronomers knew that they could not obtain the desired precision and demanded a "public discussion" in order to "eliminate all the doubts and all the errors." Criticisms from more powerful quarters were also silenced. For example, "the *Compte-Rendu* said nothing" of protests made by Fizeau and 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 and which deals with this issue?"<sup>35</sup> 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 it was impossible to compare their results.<sup>36</sup> In short, the same singularities that had plagued the transit of Venus in the previous century had reappeared, and scientists

<sup>35</sup> Montagne fils (*mécanicien*) to Ministre de l'Instruction Publique, St. Etienne, 29 Jan. 1874, 26 Feb. 1874, 9 Apr. 1874, AAS, Carton 645, Folder: Passages de Vénus, demandes de Missions; "Académie des Sciences: Séance du 6 juillet," *Moniteur Sci.*, 1874, 16:774–775; and "Académie des sciences.—6 juillet 1874," *Rev. Sci.*, 1874, 4:45.

<sup>36</sup> De la Rue had unsuccessfully advocated that astronomers use similar instruments in order to have comparable photographs. Airy wanted the telescopes used in the British expeditions to be as similar as possible. For some attempts toward standardization see Dubois, *Passages de Vénus sur le disque solaire* (cit. n. 23), p. 156.



**Figure 5.** Example of a French expedition. Illustration by Albert Tissandier of the mission in New Caledonia to observe the 1874 transit of Venus. From Camille Flammarion, “Résultats des expéditions françaises,” in *Études et lectures sur l’astronomie* (Paris: Gauthier-Villars, 1877), pp. 70–124, on p. 111.

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-Commission was created to deduce the parallax from measurements taken off photographs—again with alarmingly discouraging results. Fizeau, who was in charge of the project, explained that it was going “slowly, but surely.” But Victor Puiseux, a mathematician who had worked at the Bureau de Calculs of the Bureau des Longitudes, challenged the photographic results, considering them inferior to those obtained from direct observations.<sup>37</sup> 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 had reappeared in efforts to measure photographs.<sup>38</sup>

<sup>37</sup> Almost eight hundred photographs had to be measured. A machine to measure the plates was made by Brunner in November 1874 and finished in April 1875. This single machine, however, was not enough, and three others were built by the end of the year. One observer operated each machine. At first these were Cornu, Alfred Angot, Mercadier, and Baille. See Victor Puiseux, “Remarques sur les observations du passage de Vénus du 8 décembre 1874,” 27 Mar. 1880, AAS, Carton 1646, Folder: 1882 Passage de Vénus, pp. 1–11; and Puiseux to Académie des Sciences, Paris, 31 May 1880, AAS, Carton 1647, Folder: 1882 notes diverses.

<sup>38</sup> *Documents relatifs aux mesures des épreuves photographiques*, 3 vols., Vol. 3: *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, 1882). Experiments on taking readings off of photographs had been carried out in preparation for the transit, where Wolf and Yvon-Villarceau concluded that photographs should be measured by the same observer. The sheer number of photographic plates, however, later required that more than one observer be employed. Similarly, de la Rue, Bafour, Balfour Stewart, and Maurice Loewy all carried out investigations on the deformation of photographic plates. Some critics noticed how the exposed photographic plates suffered from some of the same deficiencies as eyes—like irradiation—and similarly concluded that the personal equation of observers reappeared when measurements were taken off photographs. In 1876 Angot warned the Commission of the Transit of Venus that the size of a photographic image varied according to the time of exposure, the intensity of the incoming light, and the aperture. See Wolf and Yvon-Villarceau, “Rapport sur les mesures

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 d’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.<sup>39</sup>

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 1874.”<sup>40</sup> 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 Foerster, 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 Foerster was exceedingly frank when, in 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.” Not everyone agreed on the need for collusion. Antoine d’Abbadie, one of the few defenders of photography present at the conference, opined that each country “should defend their liberty and publish their observations in their own manner,” but the majority remained against him. As a palliative, 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.”<sup>41</sup>

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micrométriques directes à faire pour l’observation du passage de Vénus” (cit. n. 27), p. 339; Alfred Angot, “Bulletin des sociétés savantes, Académie des Sciences de Paris séance du 22 mai 1876: Les photographies obtenues au foyer des lunettes astronomiques,” *Rev. Sci.*, 3 June 1876, 17:549; and Angot, “Bulletin des sociétés savantes, Académie des Sciences de Paris séance du 5 juin 1876: Les images photographiques obtenues au foyer des lunettes astronomiques,” *ibid.*, 17 June 1876, 17:597. See also Janssen, “Constitution de la surface solaire et sur la photographie envisagée comme moyen de découverte en astronomie physique” (cit. n. 32), pp. 1251–1252.

<sup>39</sup> Ministère de l’Instruction Publique, “Première séance” (cit. n. 29), p. 4; Faye, “Association Française pour l’Avancement des Sciences, congrès de Lille” (cit. n. 13), p. 366; and Ministère de l’Instruction Publique, *Conférence internationale du passage de Vénus* (cit. n. 29). The proceedings were reprinted in “La Conférence internationale du passage de Vénus,” *Rev. Sci.*, 14 Jan. 1882, 29:42–46. 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*, 1905, 48:1–453.

<sup>40</sup> Ministère de l’Instruction Publique, “Première séance,” p. 10 (quoting Adolph Hirsch). For Hirsch’s position see letter from Hirsch, Paris, 12 Oct. 1881, p. 1, and a note explaining the need for international cooperation and a single bureau of calculations for the transit of 1882, AAS, Carton 1645, Folder: Passage de Vénus papiers de J. B. Dumas, passage de Vénus 1882, pp. 1–3.

<sup>41</sup> Ministère de l’Instruction Publique, “Deuxième séance: Jeudi 6 Octobre 1881,” in *Conférence internationale du passage de Vénus* (cit. n. 29), pp. 13–18, on pp. 11, 17, 18 (Foerster, Dumas, d’Abbadie); Ministère de l’Instruction Publique, “Cinquième séance: Jeudi 13 Octobre 1881,” *ibid.*, pp. 23–28, on pp. 26–27 (Dumas on cooperation); and Ministère de l’Instruction Publique, “Première séance” (cit. n. 29), p. 4.

With respect to photography the conference attendees asked, “Should we continue to employ photography, and to what degree?” The overwhelming response was that “the photographic trials, taken as a whole, have cast a great incertitude on the value of the solar parallax.” 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” (see Figure 6).<sup>42</sup> By 1882 almost everybody agreed that nonphotographic observations were better. This position was most forcefully articulated by Foerster, 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.”<sup>43</sup>

Foerster found the probable error of photography versus direct micrometer measurements five times as large; he also described photographs that showed pentagonal images of Venus and others in which the planet appeared successively as “bitten” and then complete. Furthermore, the problem of photographic irradiation was added to that of scintillation; there were other “still unexplained” phenomena as well. Foerster and his followers therefore relegated photography to its former pictorial function of providing images of the sun, moon, or star clusters. 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 labor in the observatory.<sup>44</sup>

### *The Revolver’s Survival*

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 Cornu’s work on the speed of light and 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 he returned from his expedition the president of the republic greeted him “warmly,” and shortly thereafter the Meudon Observatory, with Janssen as director, was created by decree.<sup>45</sup>

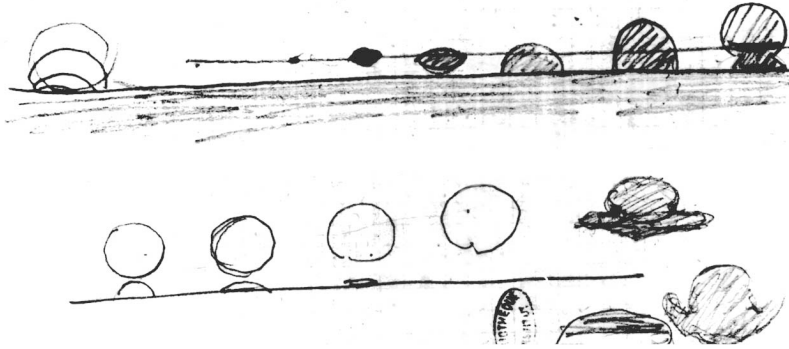
Janssen’s public successes, however, contrasted starkly with his standing in the eyes of the commission. During his expedition to Japan to observe the 1874 transit he took matters into his own hands, ignoring his official instructions. This resulted in the division of his

<sup>42</sup> Ministère de l’Instruction Publique, “Première séance,” pp. 4, 6; and Ministère de l’Instruction Publique, “Instructions pour l’observation des contacts,” in *Conférence internationale du passage de Vénus*, pp. 28–33, on p. 30 (drawings). The instructions for observing the contacts were reprinted in the *Revue Scientifique* of 29 Oct. 1881. Direct observations would be done in eight stations, and only in two would photography be employed.

<sup>43</sup> Dumas acknowledged that some photographs were very good. Emmanuel Liáis was less hopeful because of photographic irradiation. D’Abbadie still threw his weight behind photography, acknowledging that the Americans under David Peck Todd had obtained good results. The British were generally against photography, since it was difficult to ask the government for more money after what had happened in 1874. They placed their hopes instead on the observation of “a considerable number” of contacts. Ministère de l’Instruction Publique, “Première séance” (cit. n. 29), pp. 7–8.

<sup>44</sup> Charlotte Bigg, “Photography and the Labour History of Astrometry: The Carte du Ciel,” in *The Role of Visual Representations in Astronomy*, ed. K. Hentschel and A. Wittman (*Acta Historica Astronomiae*, 9) (Thun: Deutsch, 2000), pp. 90–106.

<sup>45</sup> Stanislas Meunier, “Académie des Sciences,” *Nature*, 1875, 3:78. Janssen’s role in the transit of Venus expeditions was not the only motivation behind the creation of another observatory. Many factors came into play. The debate over the new observatory, for example, was even related to the question of transferring the Paris Observatory to a different location.



**Figure 6.** Detail of drawings of contacts. From Janssen's personal notebook used during his expedition to Japan. Courtesy of the Bibliothèque de l'Institut de France.

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. 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. In despair, Janssen asked, "How can we face our debts, finish our studies and return to France?"<sup>46</sup> He was left with no option but to use his own funds.

Shortly after his return, following a plea from Faye on his behalf, Mac-Mahon, president of the republic, gave him almost 40,000 francs to cover his outstanding expedition expenses and the instruments he had constructed for his personal use, including the revolver. Despite the commission's criticisms of photography, the revolver and Janssen's other photographic methods eventually became powerful means for obtaining assent on visual matters, and the French government—reversing its earlier policy—now paid attention and provided support. The key to understanding the simultaneous failure of the commission's photographic attempts and Janssen's successes lies in their different approaches and goals with respect to photography.

### *Mechanical Reproducibility, Standardization, and Theatricality*

According to Janssen, transforming photography into an evidentiary medium involved working on three related levels: reproducibility, standardization, and theatricality. While the commission voted to use nonreproducible daguerreotypes for the 1874 transit, Janssen eventually saw reproducibility as essential. In a speech given at the annual banquet of the Société Française de Photographie, in 1888, he would explain: "Of all [photography's] beautiful qualities, the most important perhaps is that of the conservation and multiplication of images."<sup>47</sup> Furthermore, while the commission placed no importance on standardizing

<sup>46</sup> Janssen's trip to Japan and his expenses were documented in his journal; see notebook for 1874, Bibliothèque de l'Institut de France (BIF), MS 4128. For a description of his plea for help see Janssen to Ministre de l'Instruction Publique, 6 Apr. 1876, AN, F17 2928-2, Folder: Commission chargée d'examiner les comptes des dépenses de M. Janssen, nommée le 16 mars 1876, p. 5–6. Regarding the reimbursement see Chambre des Députés, "Chambre des députés—Annexe no. 445: Séance du 3 août 1876," *J. Officiel Répub. Française*, 14 Sept. 1876, pp. 695–697.

<sup>47</sup> Jules Janssen, "En l'honneur de la photographie," *Bulletin de la Société Française de Photographie*, June

its photographic methods against those of other nations, Janssen worked hard toward “unifying, simplifying, and standardizing” photography “as much as possible,” in a project that included everything from instruments to terminology. In fact, he believed one advantage of his revolver was that it would also be used by various other expeditions. Faye, following Janssen in this respect, pointed out to the *Ministre d’Instruction Publique* the irony that an instrument invented by a Frenchman was being adopted by other nations but was not authorized by the French commission. This internationalist attitude contrasted with the nationalist position taken during the first transit, when different—incompatible—instruments were seen as an advantage, not as a problem. In 1874 the *Revue Scientifique* noted: “Because of the lack of resources and time, we cannot enter in a competition against the numerous expeditions of England, Russia and the U.S. We need, then, different procedures and different instruments to beat these rival nations.”<sup>48</sup> Despite this initial burst of nationalist optimism, experience would eventually prove to most astronomers that international agreement and standardization with respect to instruments and methods was essential.

The project of standardizing photography was part of Janssen’s drive to eliminate personal—and national—differences in observations. In order to prove the advantages of photographic methods over observational astronomy, Janssen gave a lecture, later republished in the popular *Revue Scientifique*, in which he showed how different astronomers had portrayed sunspots—a particularly controversial topic—at different times. He started with a drawing by Fabricius (1611), moved successively through drawings by Galileo, Christoph Scheiner (1626), William Herschel (1801), and John Herschel (1837), and then after showing the divergent drawings of his contemporaries, concluded: “This short examination is sufficient for showing to us the disaccord that exists even amongst the best observers when observing solar phenomena. It convincingly demonstrates that the true method for observing them is to obtain, firstly, images drawn by the sun itself.”<sup>49</sup> The strategy of presenting differing observations side-by-side had in fact been used by de Fonvielle and by many other commentators on the previous transits, but they had offered no clear resolution to the problem. In contrast, Janssen, by shifting the “authorship” of the sun’s images away from the scientists and to the sun itself, allegedly solved the problem of differing observations. By 1876 he could claim that the revolver “was now definitely introduced in science.”<sup>50</sup>

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1888, rpt. in Janssen, “En l’honneur de la photographie: Discours prononcé au Banquet annuel de la Société Française de la photographie, juin 1888,” in *Oeuvres scientifiques recueillies et publiées par Henri Dehérain* (Paris: Société d’Éditions Géographiques, Maritimes et Coloniales, 1930), pp. 86–90. Janssen’s defense of mechanically reproducible media increased over time. His revolver, for example, had worked with daguerreotypes. Since its initial conception, however, he had become deeply concerned that his photographs be “comparable to those of other nations”: Janssen, “Présentation de quelques spécimens de photographies solaires obtenues avec un appareil construit pour la mission du Japon” (cit. n. 15), p. 1731.

<sup>48</sup> Jules Janssen, “Discours prononcé à la première séance du Congrès internationale de photographie, tenue à Paris, au palais Trocadéro, le 6 août 1889, par M. Janssen, président du Comité d’organisation du Congrès,” in *Procès-verbaux et résolutions, du Congrès international de photographie tenu à Paris du 6 au 17 août 1889* (1889), p. 5; and “Académie des sciences de Paris: Séance du 29 juin 1874,” *Rev. Sci.*, 4 July 1874, 14:23.

<sup>49</sup> Jules Janssen, *La photographie céleste, conférence faite au Congrès de Toulouse* (Paris: Administration de Deux Revues, 1888), rpt. in Janssen, “La photographie céleste,” *Rev. Sci.*, 14 Jan. 1888, p. 33. The debates about sunspots were connected to the controversy over intramercurial planets. See Urbain Le Verrier, “Bulletin des sociétés savantes, Académie des sciences de Paris.—2 Octobre 1876: Les planètes intra-mercurielles,” *ibid.*, 14 Oct. 1876, 18:383; and Janssen, “Bulletin des sociétés savantes, Académie des sciences de Paris.—2 Octobre 1876: Observations sur les passages devant le soleil de corps intra-mercuriels,” *ibid.*

<sup>50</sup> Janssen to *Ministre de l’Instruction Publique*, 6 Apr. 1876, AN, F17 2928-2, Folder: Commission chargée d’examiner les comptes des dépenses de M. Janssen, nommée le 16 mars 1876, p. 8. On the question of scientific authorship I am thinking of Mario Biagioli and Peter Galison, eds., *Scientific Authorship: Credit and Intellectual*

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. Even Wolf and André, whose methods could not have differed more from Janssen's, were well aware that projection techniques were powerful means for creating assent. In their seminal work on contacts, they explained how "they had been able, after the presentation of this work to the Academy, to reproduce *through projection* all the particularities of the black drop."<sup>51</sup> From very early on in his career Janssen studied projection technologies, and in his lessons on general physics to the École d'Architecture he lamented "the suffering of the spectator in our places of spectacle." Referring to inadequacies in the design of theater halls, he sought to use his knowledge of physics for its betterment.<sup>52</sup> Indeed, 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 techniques) ostensibly eliminated personal 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 the use of photography, Janssen started to campaign for the *Carte du Ciel*, a project for cataloguing stars via photography. Ironically, the project was initially led by Mouchez, now director of the Paris Observatory, who had been one of photography's early critics. Some years later Janssen preached his victory to an audience of photographers: "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."<sup>53</sup>

#### THE NEW LOGIC

Criticism of photographic methods not only came from the members of the commission but also were central to problems of philosophy and mathematics. As we have seen, Fizeau and Cornu, who had complained about Janssen's methods, concluded from their attempts to determine the solar parallax from the photographs of the 1874 transit that the problem of the personal equation had merely been shifted from discrepancies between direct observations to differing measurements taken off photographs. And Wolf, for his part, found Faye's dream of "eliminating the observer" absurd. Observers, he claimed, would always

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*Property in Science* (New York: Routledge, 2003); Biagioli, "Aporias of Scientific Authorship: Credit and Responsibility in Contemporary Biomedicine," in *The Science Studies Reader*, ed. Biagioli (New York: Routledge, 1999), pp. 12–30; and Pierre Bourdieu, "The Specificity of the Scientific Field and the Social Conditions of the Progress of Reason," *Social Science Information*, 1975, 14:19–47.

<sup>51</sup> Wolf and André, "Recherches sur les apparences singulières qui ont souvent accompagné l'observation des contacts de Mercure et de Vénus avec le bord du soleil" (cit. n. 19), p. 131 n 1.

<sup>52</sup> Jules Janssen, "Note sur les spectres par projection," *Atti dell' Accademia Pontificia de' Nuovi Lincei*, 1863, 16:482–483; and Janssen, "Discours prononcé à l'ouverture du cours de physique générale à l'École Spéciale d'Architecture, à Paris (Novembre 1865)," in *Oeuvres scientifiques recueillies et publiées par Henri Dehérain* (cit. n. 47), pp. 97–113.

<sup>53</sup> Janssen, "En l'honneur de la photographie" (cit. n. 47). The project of the *Carte du Ciel* lasted until 1970, when the gargantuan effort of mapping stars photographically was finally given up.

be needed for obtaining “absolute and authentic knowledge.” While the difference between the photographic plate and the human retina proved to Janssen the superiority of the former, most astronomers in the early 1880s 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 Foerster, the superiority of direct observation consisted in that, while instantaneous photography recorded only an instant, a good observer did a more valuable job by averaging over all instances.<sup>54</sup> Despite some dissent, the overwhelming conclusion at the Conférence Internationale was that direct observations of Venus were better than photographic ones.

In mathematics and philosophy, the results of the transit opened up debates pertaining to the relationship of geometry to the physical world. Earlier most scientists would have agreed with Flammarion’s remark: “Geometry has justified its name by gaining possession of the terrestrial globe.”<sup>55</sup> But in the case of the transit of Venus, 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 using the objectives made by 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.” In this respect astronomers were aiming their criticisms at Le Verrier, mathematical astronomer *extraordinaire* and widely disdained for his authoritarianism and antirepublicanism. Perhaps Fonvielle was most vocal, objecting that during the Second Empire “geometry has taken up arms” against astronomy. He demanded the elimination of this “radical subversion.”<sup>56</sup>

### *Natural Standards, from the Solar Parallax to the Speed of Light*

The problems raised by inconclusive observations of the transit of Venus were connected to mathematical, philosophical, and scientific debates over absolute standards that had

<sup>54</sup> Charles Wolf, “Sur la comparaison des résultats de l’observation astronomique directe avec ceux de l’inscription photographique,” *Comptes Rend. Séances Acad. Sci.*, 1 May 1886, 102:476–477, on p. 477. Foerster explained: “a good observer overcomes this inferiority by fixing an average position from the images.” D’Abbadie, however, turned this argument around, claiming that this visual averaging was a harmful characteristic, since because of it an observer might “neglect certain [fluctuating appearances] whose great importance might one day be demonstrated by the progress of science.” Van de Sande Bakhuysen, director of the Leiden Observatory and representative of the Low Countries at the conference, remarked that the averaging advantage of the eye could be matched by photography by superimposing various images. Ministère de l’Instruction Publique, “Première séance” (cit. n. 29), pp. 6–7.

<sup>55</sup> Flammarion’s defense of geometry was based on its importance in longitude determinations: Camille Flammarion, “Le prochain passage de Vénus et la mesure des distances inaccessibles,” *Nature*, 1874, 2:386–391, on p. 388. These issues were sometimes explored with explicit reference to the cinematographic camera and the distance between the sun and the earth in the dialogues between Bergson, Édouard Le Roy (a student and follower of Bergson), Henri Poincaré, Louis Couturat, Auguste Calinon, and Jules Andrade.

<sup>56</sup> Faye, “Association Française pour l’Avancement des Sciences, congrès de Lille” (cit. n. 13), p. 365. The criticism of “geometrical conceptions” appeared earlier in Faye, “Sur les passages de Vénus et la parallaxe du soleil” (cit. n. 18), p. 49. See also “Académie des sciences de Paris.—4 décembre 1871,” *Rev. Sci.*, 1871, 8:574–575. For Puiseux’s views see “Académie des sciences de Paris: Séance du 7 mars 1881,” *ibid.*, 19 Mar. 1881, 1:378. Fonvielle’s criticism is in Wilfrid de Fonvielle, *L’astronomie moderne* (Paris: Germer Baillière, 1868), p. xxxvi. Both Le Verrier and Delaunay were often criticized for their excessive mathematical leanings. The tension between astronomy and mathematics, furthermore, was connected to political debates in the Paris Observatory and in France more generally. See Canales, “Single Eye” (cit. n. 11), pp. 84–88.



raged for more than a century. 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 hoped to contribute to this problem of “capital importance,” since the solar parallax would “define the absolute dimensions of the solar system.”<sup>57</sup> 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. 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.”<sup>58</sup>

Fonvielle, who had alerted the general public to the discordances of the previous transits of Venus, mocked the astronomers who had not followed the enlightened road of standardizing measures. At the end of his book *Le mètre international définitif*, he 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.”<sup>59</sup> The attempt to determine an absolute standard of measurement from observations of the transit of Venus suffered a fate similar to the earlier attempt to deduce the meter from measurements of the circumference of the earth, forcing 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 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.<sup>60</sup> In the years before 1874, most scientists had sided with Delaunay in believing that it was only natural to use the distance from the earth to the sun as the standard of measure of the heavens; but what was natural to scientists changed after the transit produced—again—only defective results.

### *The “Inverted” Roles of Physics and Astronomy*

For most scientists Janssen’s successes with his revolver—although spectacular—were illusory. The debate over chronophotography’s claims to truth and the value of the solar parallax quickly moved beyond the confines of astronomy and into the domain of physics.

<sup>57</sup> Flammarion, “Prochain passage de Vénus et la mesure des distances inaccessibles” (cit. n. 55), p. 387; and Alfred Cornu, “Détermination nouvelle de la vitesse de la lumière,” *Journal de l’École Polytechnique*, 1874, 27:133–180, on p. 138. Part of this work was reprinted in Cornu, “Détermination nouvelle de la vitesse de la lumière,” *Comptes Rend. Séances Acad. Sci.*, 1873, 76:338–342. For work on standards see esp. the essays by Ken Alder, Graeme Gooday, and Simon Schaffer in *The Values of Precision*, ed. M. Norton Wise (Princeton, N.J.: Princeton Univ. Press, 1995).

<sup>58</sup> Faye, “Association Française pour l’Avancement des Sciences, congrès de Lille” (cit. n. 13), pp. 367–368. The problem of standards was articulated in Charles Wolf, “La figure de la terre, soirées scientifiques de la Sorbonne,” *Rev. Sci.*, 12 Mar. 1870, 7:226–234. Later, with respect to nominalism, I am thinking of the work of Le Roy and, for conventionalism, Poincaré.

<sup>59</sup> Wilfrid de Fonvielle, *Le mètre international définitif* (Paris: Masson, 1875), p. 140. In this vein Hirsch also complained that “each nation had come up with their own solar parallax”: Ministère de l’Instruction Publique, “Première séance” (cit. n. 29), p. 10.

<sup>60</sup> Fonvielle, *Mètre international définitif*, p. 139. This is not to say that Fonvielle believed the speed of light to be independent of its medium, nor that the Michelson and Morley experiments led to Einstein’s theory of relativity. My point is simply that the idea of using the speed of light as a standard of measurement was directly connected to controversies surrounding the nineteenth-century transits of Venus.

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 the speed of light could be used to determine the solar parallax, for years this method seemed untrustworthy. Only measurements from observations of Venus's transit across the sun, most scientists agreed, could "immediately convince the spirit" of the true parallax value.<sup>61</sup> Traditionally, astronomical methods such as observations of the transit of Venus were used to determine physical constants, such as the speed of light. We need only remember how, 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, rather than from physical determinations. Centuries later, in contrast, physical experiments would be done to determine the precious astronomical constant. In 1875 at the evening lectures of the British Royal Institution, the "inverted" roles of physics and astronomy were described: "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 the sun's parallax, that is to say, the same element which is directly given by the transit of Venus."<sup>62</sup>

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. Seconding Le Verrier, Fizeau—who had collaborated with Foucault—took it upon himself to "demonstrate the possibility of measuring the speed of light on the earth's surface by purely physical means." Cornu, of the École Polytechnique, also believed that determining the speed of light was a simpler, cheaper, and surer way of determining the solar parallax and worked to perfect Fizeau's methods. In the introduction to the "Détermination nouvelle de la vitesse de la lumière," he explained the new order: "Today astronomy reverses those roles and demands from the progress of Optics the value of this constant." Furthermore, 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 through the sun at the price of costly voyages, both difficult and risky."<sup>63</sup> 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 to distant points.

After the 1874 transit once again furnished only discordant results, Le Verrier, who had stood firmly against the whole project since the beginning, finding it an excuse for "all the functionaries who want to profit from it in order travel around the world at the expense of

<sup>61</sup> Delaunay, "Notice sur la distance du soleil à la terre, extrait de l'Annuaire pour l'an 1866" (cit. n. 16), p. 94. Similarly, George Forbes explained how "various methods have been adopted for [determining the solar parallax], but the one which makes use of a transit of Venus has generally been considered to be the most accurate": Forbes, *Transit of Venus* (cit. n. 3), p. 17.

<sup>62</sup> 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 Alfred Cornu, "New Determinations of the Velocity of Light," published by the Royal Institution of Great Britain on 7 May 1875.

<sup>63</sup> Urbain Le Verrier, "Sur les masses des planètes et la parallaxe du soleil," *Comptes Rend. Séances Acad. Sci.*, 22 July 1872, 75:165–172; Armand Fizeau, *ibid.*, p. 172; and Cornu, "Détermination nouvelle de la vitesse de la lumière" (cit. n. 57), p. 133. 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 (*ibid.*, p. 139).

the government,” moved more forcefully into Cornu’s camp. 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 old value for the speed of light, ironically, “found favor among astronomers” but “was not accepted by most physicists.”<sup>64</sup> The new role taken on by physics proved long lasting, spilling rapidly into the search for that elusive linchpin of classical mechanics, the ether that allegedly “filled” empty space—an element investigated in Cornu’s “Détermination nouvelle.”

### *Investigating the Ether-Drag*

One reason the question of the relation between the ether and the speed of light was particularly relevant was how it affected the value for the solar parallax. When the value of the speed of light derived by purely physical means was converted into the astronomical value of the solar parallax, scientists needed to take into account the ether-drag—that is, the resistance encountered by the light as it moved through the ether. The two methods used for undertaking this conversion, one based on the observation of Jupiter’s satellites and the other on the phenomenon of annual aberration, depended on the behavior of light as it moved through the ether. In contrast, current physical determinations of the speed of light eliminated the effect of the ether because the back-and-forth trajectories followed by the ray of light under investigation cancelled its effect.<sup>65</sup> This characteristic, noted by Cornu

<sup>64</sup> Le Verrier to Jurien de la Gravière, Paris, 24 July 1873, AN, F17 3726, Folder: Passage de Vénus (1867–1882, particulièrement 1873–1875), p. 1; and Cornu, “Détermination nouvelle de la vitesse de la lumière,” p. 139. For other accounts commenting on Le Verrier’s opposition see “Académie des Sciences: Séance du 6 juillet,” *Moniteur Sci.*, 1874, 16:774–775; Rodolphe Radau, “Les applications scientifiques de la photographie,” *Rev. Deux Mondes*, 15 Feb. 1878, 25:872–890, on pp. 885–886; and Arthur Schuster, *Biographical Fragments* (London: Macmillan, 1932), pp. 198–201. I thank Simon Schaffer for mentioning this last source to me. After the presentation 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); 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 Dec. 1874”; he also described how his value “conformed to the results of M. Le Verrier” [italics added]); and Cornu to Ministre de l’Instruction Publique, 5 Feb. 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).

<sup>65</sup> The first method depended on the realization that when one of Jupiter’s satellites passed into the shadow of the planet it is seen 480 seconds earlier when Earth is on the same side of the sun as Jupiter than when Earth is on the opposite side. Since the distance between Jupiter and Earth is shorter by a whole diameter of Earth’s orbit when Earth is on the same side of the sun as Jupiter, then, assuming that light travels at around 298,500 kilometers per second, the distance from Earth to the sun would be about 298,500 kilometers per second multiplied by 480 seconds: 143,280,000 kilometers. This was the method used by Rømer for determining the speed of light from current values of the solar parallax in 1676. The second method depended on the realization that the length of Earth’s circumference could be found by multiplying the velocity of Earth’s orbit around the sun by the number of days in a complete cycle (365); once the circumference of Earth’s orbit was known its distance from the sun could be calculated. The velocity of Earth could be found through the phenomenon of stellar aberration, which gave Earth a speed of 1/10,000 times the speed of light. This method, which was used by Cornu, depended on the concept of a stationary ether as developed by Augustin Fresnel and George Stokes. When Michelson undertook his experiments on the “relative motion of the earth and the luminiferous ether” and found no effect from the ether, he concluded that his experiment contradicted “the explanation of the phenomenon of aberration,

at the prompting of the astronomer Yvon-Villarceau (and later pointed out by the physicist and founder of electromagnetism James Clerk Maxwell), proved to many the difficulties and opportunities of investigating the ether-wind through physical experiments on the speed of light. The competition between physicists and astronomers intensified the desire to understand how the ether affected the velocity of light.

Albert A. Michelson, who studied with Cornu in Paris, took on the challenge. He expanded on “Cornu’s elaborate memoir upon the determination of the velocity of light,” spending a mere ten dollars on equipment, and then followed this work with the famous experiment that would later be central to debates surrounding Einstein’s theory of relativity. Even before the second transit approached, Michelson’s work was a strong contender among alternative determinations of the value of the solar parallax. 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.”<sup>66</sup> Furthermore, these experiments were done to eliminate individual differences in observations and in direct response to the photographic and cinematographic methods exemplified by Faye and Janssen.

#### CONCLUSION

The late nineteenth-century transits of Venus saw the emergence of new instruments and methods for determining the value of the solar parallax, and considerable debate surrounded their choice. Particularly pressing was the problem of eliminating individual differences in observation, which had plagued the observations of earlier transits and were threatening to discredit the age-old geometric methods used in astronomy.

While for the 1874 transit the government-sponsored Commission on the Transit of Venus recommended the employment of skilled observers, seconded by photographic daguerreotypes, in 1882 the use of photography was completely discouraged. Yet not everyone agreed. The French government, in particular, eventually moved to photography’s support, especially because Janssen’s chronophotographic methods were becoming immensely powerful means for achieving assent on visual matters. Many held that Janssen’s photographic revolver, which seemed to promise to close centuries of disagreement by taking sequential images of Venus’s transit across the sun, furnished the best type of evidence for determining the solar parallax.

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which has been hitherto generally accepted, and which presupposes that the earth moves through the ether, the latter remaining at rest”: Albert A. Michelson, “The Relative Motion of the Earth and the Luminiferous Ether,” *American Journal of Science*, 1881, 22:120–129, on p. 128. Cornu reflected light signals back and forth from his station at the observatory at the École Polytechnique and Mont-Valérien (a distance of 10,310 meters).

<sup>66</sup> Albert A. Michelson, “Experimental Determination of the Velocity of Light,” *Proceedings of the American Association for the Advancement of Science*, 1880, 28:124–160, on p. 124. For the “unfinished business” see J. B. Gough, “Armand-Hippolyte-Louis Fizeau,” in *Dictionary of Scientific Biography*, ed. Charles Coulston Gillispie, 18 vols. (New York: Scribner’s, 1970–1986), Vol. 5, pp. 18–21, on p. 20. For an account of Michelson’s work and its relation to the solar parallax and the transits of Venus see Octave Callandreaux, “Histoire abrégée des déterminations de la parallaxe solaire,” *Rev. Sci.*, 1881, 2:39–43. For Foucault and Fizeau’s early work on the ether see Paul Acloque, “Hippolyte Fizeau et le mouvement de la Terre: Une tentative méconnue,” *Vie des Sciences*, 1984, 1:145–158, and Pierre Costabel, “L. Foucault et H. Fizeau: Exploitation d’une information nouvelle,” *ibid.*, pp. 235–249.

Although evidentiary techniques located between scientific and popular cultures, such as chronophotography, became more successful than ever after Janssen's transit of Venus expedition, critics remained. From the perspective of philosophy, Henri Bergson criticized the scientific tendency of arranging temporal images sequentially, which he termed "the cinematographic method." Not only did he 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 in observations 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 other ways. In fact, 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-Prussian war physical methods were increasingly seen as offering "harder" types of evidence.

My intention in recounting the debates surrounding the transit of Venus expeditions has been to show the different ways in which scientific evidence was employed. Discursively and technologically, we see a number of shifts in the type of evidence at work in scientists' attempts to deal with the fleeting phenomena of the late nineteenth century. From all these differences, however, certain commonalities emerge. We see, for example, how the French government recognized the political and juridical advantages of eliminating individual differences in observation and eventually moved toward the support of Janssen. From within the scientific community, we see how the power of discipline and training was discovered to be limited, how emphasis was increasingly placed on standardization and international cooperation, and how photography was enlisted as a powerful ally. However, when scientists contested the photographic results, claiming that the feared personal equation reappeared with measurements taken off photographs, the burgeoning distrust of visual methods ultimately led to alternative ones—most importantly, to the determination of the value of the solar parallax from the speed of light. Despite all efforts within scientific and governmental communities to eliminate the differing observations of the transits of Venus, opposing views could not be entirely reconciled.

Clearly, diverse values, traditions, and epistemic commitments were behind the choice of different types of evidence, but what I want to stress is that these different positions were dependent on each other.<sup>67</sup> As we have seen, the photographic and cinematographic methods advocated by Janssen and Faye were created in direct response to the disciplinary and daguerreian methods of the commission. Furthermore, Bergson's and others' questionings of the foundations of mathematics and of the role of philosophy with respect to the exact sciences arose in direct opposition to the methods employed during the transits. New experiments on the speed of light and the ether, as well as the increased role of physics in astronomy, also emerged as alternatives to disciplinary, photographic, or cinematographic methods. But many scientists worked simultaneously in various areas and

<sup>67</sup> On choices, values, and traditions of instrumentation, experimentation, and theory see Peter Galison, *Image and Logic: A Material Culture of Microphysics* (Chicago: Univ. Chicago Press, 1997), Ch. 9.

promoted radically different types of evidence. My intention throughout this essay has been to show the interdependence of different types of evidence and to elucidate a common ground that was *necessarily and at the same time* cinematographic, physical, psychological, and philosophical. In the end, what emerged from this controversy was a conceptual space marked by a complex, enduring debate on scientific evidence and its claim to truth.