PETER GALISON/LORRAINE DASTON

Scientific Coordination as Ethos and Epistemology

Introduction: The Many-Headed Knower

It is just possible that the young René Descartes once dreamed of a natural philosophy deduced entirely from clear and distinct ideas by a solitary thinker, as Euclid's *Elements* were deduced entirely from axioms, definitions, and postulates. But even Descartes had abandoned the hope of a one-man science by the time his *Discours de la méthode* appeared in 1637: from first principles "I discovered skies, stars, an earth, and even, on the earth, water, air, fire, minerals, and several other things which are the commonest of all," but once he descended to further particulars, he realized that it would be necessary "to make use of many experiments" – experiments that he realized far exceeded his own time and resources, "were they a thousand times greater than they are."[1] He hoped that wealthy patrons might finance a legion of paid experimenters under his direction. Thereafter, no one, even the most resolute rationalist, ever imagined the science of nature as a single-handed operation. Opinions in the seventeenth century differed as to how much manpower would be needed for how long, and how it should be best organized, from Leibniz's wildly optimistic view that a team of scholars would need a mere five years in order to translate all human concepts into an arithmetic "characteristica universalis"[2] to Francis Bacon's vision in the *New Atlantis* of a whole society permanently organized

1 "... et il me semble que, par la, i'ay trouué des Cieux, des Astres, vne Terre, & mesme, sur la terre, de l'Eau, de l'Air, du Feu, des Minéraux, & quelques autres telles choses, qui sont les plus communes de toutes & les plus simples ...;" / "... en si grand nombre, que ny mes mains, ny mon reuenu, bien que i'en ieusse mille fois plus que ie n'en ay, ne sçauroient suffire pour toutes ..." René Descartes. "Discours de la méthode" [1637]. *Œuvres de Descartes*. Ed. Charles Adam and Paul Tannery. Paris: Léopold Cerf, 1902. Vol. 6, 64-65 and 72-73.

2 Gottfried Wilhelm Leibniz. Untitled fragment [1677]. *Die philosophischen Schriften von Gottfried Wilhelm Leibniz*. Ed. Carl Immanuel Gerhardt. Berlin: Weidmann, 1875-90. Vol. 7, 184-89.

around a well-staffed and hierarchically organized research institute known as the House of Solomon.[3] And since the seventeenth century, the most diverse models of scientific collectivities have been invented and institutionalized, from the Enlightenment Republic of Letters to the late twentieth-century research laboratory in high energy physics or genetics. Yet however various these collectivities, real and imagined, may be, they all address the problem of how knowledge about nature can be, indeed must be, the product of many hands and heads.

This is a problem of the division of labor and the multiplication of laborers, akin to any other such problem in the organization of work: how to analyze a complex inquiry into modular parts, find willing and able hands to undertake each part, and efficiently integrate the results. But it is more than a problem in political economy transferred to science. Even if legions of well-trained, specialized scientific workers were to fan out over the length and breadth of nature and send their data to some updated successors of Bacon's "Interpreters of Nature" to tabulate and synthesize, the challenges facing every scientific collectivity would not be solved. Gathering and sifting data is not enough, even if armies of researchers are enlisted. Some means must be found to identify fields worthy of inquiry, to standardize and coordinate tools of inquiry (be these the eye of the botanist, the thermometers and barometers of the meteorologist, or the tables of the astronomer), and, above all, to plot a further course of investigation: what next? The prize questions of the eighteenth-century academies, the magisterial review articles of the late nineteenth century, the gigantic grant proposals of the early twenty-first are all means to these ends, albeit within quite different scientific collectivities: the Enlightenment Republic of Letters, Victorian Men of Science, Wartime Collaborations, Post-World War II Scientific Communities.

Scientific collectivities are historically situated. Unsurprisingly, they borrow and bricolage forms of labor in their ambient societies: early modern scientific endeavors drew heavily on the organization of work in the household, the workshop, and the court;[4] mid-nineteenth-century

---

3 Francis Bacon. "New Atlantis" [1627]. *Lord Bacon's Works*. Ed. Basil Montagu. London: William Pickering, 1825-34. Vol. 2, 348-50 and 361-79.

4 Owen Hannaway. "Laboratory Design and the Aim of Science. Andreas Libavius versus Tycho Brahe." *Isis* 77 (1986): 585-610; Steven Shapin. "The House of Experiment in Seventeenth-Century England." *Isis* 79 (1988): 373-404; Londa Schiebinger. *The Mind Has No Sex? Women in the Origins of Modern Science*. Cambridge and London: Harvard University Press, 1989. 66-96; Bruce T. Moran. *The Alchemical World of the German Court. Occult Philosophy and Chemical Medi-*

science self-consciously likened itself to the factory;[5] mechanization and Taylorism featured prominently in mid-twentieth-century science as well as in coeval industry.[6] Somewhat less obviously, they channel forms of cultural authority as well, as in the case of the earliest scientific academies, which sought royal patronage and actively recruited aristocratic members.[7] Conversely, scientific collectivities are occasionally held up as models for society at large, as when John Stuart Mill looked to the physical sciences for a peaceable solution to modern conflicts over legitimate authority or when Jacob Bronowski praised the scientific community as an ideal democracy.[8] Yet scientific collectivities are seldom simply microcosms of their economic and social macrocosms; they invent new ways of working, communicating, adjudicating, and authorizing. They are obliged to innovate because their aims are, in contrast to ordinary polities, epistemological, as well as economic, social, and political. Hence every scientific collectivity turns the tools of its age to ends other than those for which they were originally forged: the Renaissance court was not primarily designed to probe the secrets of nature, and yet it was ingeniously adapted to those ends;[9] the

---

cine in the Circle of Moritz of Hessen, 1572-1632. Stuttgart: F. Steiner Verlag, 1991; Mario Biagioli. *Galileo, Courtier. The Practice of Science in the Culture of Absolutism*. Chicago and London: University of Chicago Press, 1995; Paula Findlen. "Masculine Prerogatives. Gender, Space, and Knowledge in the Early Modern Museum." *The Architecture of Science*. Ed. Peter Galison and Emily Thompson. Cambridge and London: MIT Press, 1999. 29-58; Alix Cooper. "The Household." *The Cambridge History of Early Modern Science*. Ed. Katharine Park and Lorraine Daston. Cambridge: Cambridge University Press, 2006. 224-37.

5 David Cahan. *An Institute for Empire. The Physikalisch-Technische Reichsanstalt, 1871-1918*. Cambridge: Cambridge University Press, 1989; Norton Wise and Crosbie Smith. *Energy for Empire. A Biographical Study of Lord Kelvin*. Cambridge: Cambridge University Press, 1989; Timothy Lenoir. *Instituting Science. The Cultural Production of Scientific Disciplines*. Stanford: Stanford University Press, 1997.

6 Peter Galison. *Image and Logic. A Material Culture of Microphysics*. Chicago and London: University of Chicago Press, 1997.

7 Roger Hahn. *The Anatomy of a Scientific Institution. The Paris Academy of Sciences, 1666-1803*. Berkeley and Los Angeles: University of California Press, 1971; Michael Hunter. *The Royal Society and Its Fellows 1660-1700. Morphology of an Early Scientific Institution*. Chalfont St. Giles: British Society for the History of Science, 1982.

8 John Stuart Mill. "The Spirit of the Age" [1831]. *Collected Works of John Stuart Mill*. Ed. John M. Robson et al. Toronto: University of Toronto Press, 1981-91. Vol. 32, 227-316; Jacob Bronowski. *Science and Human Values* [1956]. New York: Harper & Row, 1975.

9 William Eamon. *Science and the Secrets of Nature. Books of Secrets in Medieval and Early Modern Culture*. Princeton: Princeton University Press, 1994.

epistolary networks of learned humanists only gradually came to accommodate the exchange of scientific information and specimens;[10] the international scientific congresses of the latter half of the nineteenth and early twentieth centuries were patterned on but not identical to the international diplomatic conferences of the late eighteenth and early nineteenth centuries.[11]

This essay explores how historical specificity and epistemological mission intersected in the international scientific collaborations of the late nineteenth century, including the Carte du Ciel, the Internationale Gradmessung, and the Transits of Venus expeditions, concluding with the reflections of philosopher-scientist Charles Sanders Peirce on the intertwined epistemology and morality of scientific coordination. We are less interested in providing a detailed account of any one of these collaborations, which have been well-treated elsewhere,[12] than in using them together to exemplify a late nineteenth-century solution to the problem of constituting a scientific collectivity, a many-headed knower. In particular, we are interested in the ways in which the epistemological goals of this kind of collectivity demanded an ethos on the part of its members. It was not enough to subscribe officially to the objectives and procedures of an international collaboration; values had to be internalized and sometimes sacrifices made when these values – which were at once moral and epistemological – conflicted with those that informed research within a different scientific collectivity. It is part of the historical location of the international collaboration as one kind of scientific collectivity that it did not always dovetail smoothly with other kinds – no more than the values and mores of different subcultures always harmonize with one another. Prima facie evidence that the values at

---

10 Paula Findlen. *Possessing Nature. Museums, Collecting, and Scientific Culture in Early Modern Italy*. Berkeley and Los Angeles: University of California Press, 1994; Justin Stagl. *A History of Curiosity. The Theory of Travel, 1550-1800*. Chur: Harwood, 1995.

11 Éric Brian. "Transactions statistiques au XIXe siècle. Mouvements internationaux de capitaux symboliques." *Actes de la recherche en sciences sociales* 145 (décembre 2002): 34-46; Aant Elzinga and Catharina Landström, eds. *Internationalism in Science*. London: Taylor Graham, 1996.

12 Charlotte Bigg. "Photography and Labour History of Astrometry. The Carte du Ciel." *The Role of Visual Representations in Astronomy. History and Research Practice*. Ed. Klaus Hentschel. Thun and Frankfurt a.M.: Deutsch, 2000. 90-106; Ulrich Völter. *Geschichte und Bedeutung der internationalen Erdmessung*. Munich: Verlag der Bayerischen Akademie der Wissenschaften, 1963; John Lankford. "Photography and the Nineteenth-Century Transits of Venus." *Technology and Culture* 28 (1987): 648-57; Jimena Canales. "Photogenic Venus. The 'Cinematographic Turn' and Its Alternatives in Nineteenth-Century France." *Isis* 93 (2002): 585-613.

stake in these clashes were at least in part moral is that breaches occasioned not only disappointment but also indignation, a sign that what was at stake was more than a diplomatic compromise among delegates of individual nation states.

The morality in question derived from the epistemological challenge such sustained international collaborations were meant to meet: how to recognize and map phenomena that dwarfed merely human scales of time and space. The shape and path of a storm, the contour of an isotherm, the physiognomy of a landscape, the geographic distribution of a species – these were phenomena, all new objects of inquiry in the nineteenth century, whose regularities or very existence was hidden to the observer located too firmly in the here and now, whose field of vision was circumscribed by the humanly meaningful co-ordinates of a locale or lifetime.[13] Even those phenomena that had been studied earlier by international cooperations, such as the transits of Venus,[14] were investigated differently when made the objects of sustained rather than occasional collaborations that strove to make protocols, instruments, and observers commensurable with one another. Improved means of communication and transportation were the preconditions for but not the causes of these ever wider and more densely woven networks of observers. As the several abortive eighteenth-century attempts to build observer networks in meteorology bear witness, it was not enough to recruit participants or even distribute standardized instruments; the observers themselves must be socialized and standardized in order for the networks to cohere and endure.[15] This is the juncture at which ethos met epistemology.

---

13 Susan Faye Cannon. "Humboldtian Science." *Science in Culture. The Early Victorian Period.* New York: Science History Publications, 1978. 73-110; Michael Dettelbach. "Global Physics and Aesthetic Empire. Humboldt's Physical Portrait of the Tropics." *Visions of Empire. Voyages, Botany, and Representations of Nature.* Ed. Peter H. Reill and David Philip Miller. Cambridge: Cambridge University Press, 1996. 258-92; Marie-Noëlle Bourguet. "Landscape with Numbers. Natural History, Travel and Instruments in the Late Eighteenth and Early Nineteenth Centuries." *Instruments, Travel and Science. Itineraries of Precision from the Seventeenth to the Twentieth Century.* Ed. idem, Christian Licoppe, and H. Otto Sibum. London and New York: Routledge, 2002. 96-125.

14 Harry Woolf. *The Transits of Venus. A Study in Eighteenth-Century Science.* Princeton: Princeton University Press, 1959.

15 Gustav Hellmann. "Die Entwicklung der meteorologischen Beobachtungen in Deutschland von den ersten Anfängen bis zur Einrichtung staatlicher Beobachtungsnetze." *Abhandlungen der Preussischen Akademie der Wissenschaften, Physikalisch-Mathematische Klasse* 1 (1926): 1-25; Robert Marc Friedman. *Appropriating the Weather. Vilhelm Bjerknes and the Construction of a Modern Meteorol-*

It was also the birthplace of new scientific objects. Many-headed knowers not only study nature in ways different than solitary knowers do; they also come to know things that located individuals, no matter how brilliant and diligent, cannot. The parallax of Venus during a transit, the exact shape of the earth, the distribution of stars in the sky and the slow changes in this configuration over millennia – these are phenomena that demand not only collaboration but exquisite coordination among scattered observers. Units of measure, instruments, clocks, as well as reaction times, note-taking habits, perceptual judgments, and certain values must all be synchronized. Otherwise the parts of the puzzle will prove incommensurable; the new scientific object will remain invisible. This is not a claim about metaphysical realism or its antonyms: we take for granted that storm systems and the shape of the earth exist whether or not they are successfully observed.

Rather than beating the dead horse of metaphysical realism, we are concerned here with the intrinsically collective conditions of knowability. There are several kinds of such epistemological collectives, each specific to its age. The late nineteenth century offered peculiarly propitious conditions for a certain variety of planet-spanning scientific collaboration: colonial and commercial enterprises cast an ever more thickly woven net of connections over ever more of the globe. The first successful Atlantic telegraph cables began operation in 1866;[16] the General (later Universal) Postal Union was created in 1874;[17] a flurry of international meetings (themselves made possible by faster and more reliable communication and transportation) met to standardize units of measurement for science and commerce.[18] Equally important were the administrative

---

ogy. Ithaca: Cornell University Press, 1989; James Rodger Fleming. *Meteorology in America, 1800-1870*. Baltimore and London: Johns Hopkins University Press, 1990.

16 Tom Standage. *The Victorian Internet. The Remarkable Story of the Telegraph and the Nineteenth-Century's On-Line Pioneers*. New York: Walker, 1998.

17 James D. Cotreau. *The Historical Development of the Universal Postal Union and the Question of Membership*. Boston: n. publ., 1975.

18 Simon Schaffer. "Late Victorian Metrology and Its Instrumentation. A Manufactory of Ohms." *Invisible Connections. Instruments, Institutions, and Science*. Ed. Robert Bud and Susan E. Cozzens. Washington: Spie Optical Engineering Press, 1992. 23-56; idem. "Metrology, Metrification and Victorian Values." *Victorian Science in Context*. Ed. Bernard Lightman. Chicago and London: University of Chicago Press, 1997. 438-74; Christophe Bonneuil. "The Manufacture of Species. Kew Gardens, the Empire, and the Standardisation of Taxonomic Practices in Late Nineteenth-Century Botany." *Instruments, Travel and Science*. 189-215; Peter Galison. *Einstein's Clocks, Poincaré's Maps. Empires of Time*. New York: W.W. Norton, 2003.

skills of coordination acquired by expanding empires, orchestrating the activities of far-flung envoys and officials from a remote capital. Industrial materials and methods provided a model for the efficient organization of large laboratories and research expeditions. These technologies of steam, wire, and paper did not cause scientific collaborations, nor even make them possible. Long before the telegraph and the steamship, savants had been in communication with one another, by letter and in person. But the expansive ambitions of late nineteenth-century empire and commerce did enable and inspire scientists to think on a different scale, both geographical and chronological. European colonial outposts in the southern hemisphere made mapping, terrestrial and celestial, more comprehensive and sustained than eighteenth-century scientific travelers could ever have imagined. Diplomatic experience in negotiating international treaties was transferred, along with the rhetoric and reality of conflicting national interests, to international scientific congresses – themselves an innovation in this period. The resources of colonial administrations were put at the disposal of traveling scientists. Global reach recreated international science in its own image and enlisted it to serve its own commercial, military, and ideological purposes.

Our examples of this new species of scientific collectivity and its ethical and epistemological implications are, in order of exposition, the Carte du Ciel, the Internationale Gradmessung, and the Transits of Venus (the polyglot titles of the projects already signal their international scope). This list is meant to be exemplary, not exhaustive; many more such late nineteenth-century scientific collaborations might be added. We have chosen these three to make a point about what many-headed knowers can know and how. The Carte du Ciel (begun in 1892) aimed to map the entire heavens, presenting earth-bound astronomers with their first complete picture of the distribution of all heavenly bodies down to the fourteenth magnitude (with a catalogue of stars down to the eleventh magnitude), and to supply future astronomers thousands of years hence with the means for detecting changes in the heavens since ca. 1900. Both spatial distribution and temporal development were new objects of scientific inquiry, which depended essentially on a scientific collectivity extended across continents and centuries. The ambitions of the Internationale Gradmessung (begun in 1886) were purely synchronic, but also vast: the determination of the shape of the earth by measuring variations in the gravitational constant all over its surface. These projects delineated objects of inquiry that previously could have been vaguely circumscribed ("all the stars in the sky"), but that had not yet been made scientifically knowable. In contrast, the Transits of Venus expeditions (1874, 1882) sought to specify an entity already well defined since Copernicus: the distance from the earth to the sun. Known as the astronomical unit, it is the basis for establishing all absolute distances within the solar system. Yet as in the case of the Carte du Ciel and the Internationale Gradmessung, only the most meticulous coordination could make this object of inquiry precisely measurable.

In the context of large-scale scientific collaborations, both literal and figurative standpoints are at issue. The literal standpoint of an observer in space and time can make certain entities invisible: storm systems, isotherms, the emergence and disappearance of stars, the distribution of an organic species. Hence networks of dispersed observers are formed to track these scientific objects that transcend human scales of the here and now. Many nineteenth-century scientific collaborations in meteorology, geodesy, statistics, biology, and astronomy aimed at tracking these global or cosmic objects by uniting the efforts of local observers into a kind of Leviathan or super-observer. This is where the figurative sense of standpoint enters. In order to co-ordinate the local observers into a global network, other kinds of peculiarities must be planed away. These might inhere in a group – e.g. national traditions of instrumentation and data reduction techniques – or in the individual – e.g. personal equation, greater or lesser zeal for the project in question. All of these forms of being situated could and did interfere with the communication and commensurability of observations. International commissions formed to steer the Carte du Ciel, the Gradmessung, and other scientific collaborations wrangled at length over how best to erase the influence of these figurative standpoints, in the service of overcoming literal ones. Only the many-headed knowers created by painstaking coordination – not erasing – of perspectives could detect scientific objects on a planetary or even cosmic scale.

Mapping the Heavens

From 16-25 April 1887 fifty-eight astronomers from sixteen countries plus three colonies met in Paris at the invitation of Admiral E.B. Mouchez, director of the Paris Observatory and the Paris Academy of Sciences, to plan what one contemporary called "the greatest venture yet undertaken in astronomy,"[19] namely a complete photographic map of the

---

[19] Julius Scheiner. *Die Photographie der Gestirne*. Leipzig: Wilhelm Engelmann, 1897. 311.

sky, including all stars to the fourteenth magnitude.[20] Only the combined and prolonged efforts of almost a score of observatories in both the northern and southern hemispheres could produce what promoters hailed as an "imperishable monument," a photographic record of "the authentic state of the universe visible from the earth at the close of the nineteenth century."[21] The proportions of the project were indeed monumental in every sense: eighteen observatories around the world, from Helsingfors at +60.9 degrees latitude to Melbourne at -37.5[22] labored for decades – publication of the catalogue was not completed until 1964[23] – to amass charts projected in 1912 to stack 32 feet high and weigh about 4,000 lbs.[24] Armed with this snapshot of the sky circa 1900, future astronomers would be able, it was hoped, to detect changes in the heavens which unfolded on too long a time scale to be perceptible within a short human lifetime: appearance of new stars, nebulae, and comets, the telltale motion of as yet undiscovered planets, the extended periods of variable stars, the incremental proper motions of the so-called fixed stars.[25] By

20 A star catalogue down to the eleventh magnitude was also planned as part of the Carte du Ciel project. On the history of nineteenth-century astrophotography in general, cf. Daniel Norman. "The Development of Astrophotography." *Osiris* 5 (1938): 560-94; Dorrit Hoffleit. *Some Firsts in Astronomical Photography*. Cambridge: Harvard College Observatory, 1950; John Lankford. "The Impact of Photography on Astronomy." *Astrophysics and Twentieth-Century Astronomy to 1950*. Ed. Owen Gingerich. Cambridge and New York: Cambridge University Press, 1984. 16-39. On the Carte du Ciel project in particular, cf. Institut de France-Académie des Sciences. *Congrès astrophotographique international tenu à l'Observatoire de Paris pour le levé de la Carte du Ciel*. Paris: Gauthier-Villars, 1887; Albert G. Winterhalter. *The International Astrophotographical Congress and A Visit to Certain European Observatories and other Institutions. Report to the Superintendent*. Washington: Government Printing Office, 1889; Ernest B. Mouchez. *La Photographie astronomique à l'Observatoire de Paris et la Carte du Ciel*. Paris: Gauthier-Villars, 1887; Herbert Hall Turner. *The Great Star Map*. New York: E.P. Dutton, 1912; Suzanne Débarbat et al., eds. *Mapping the Sky. Past Heritage and Future Directions. Proceedings of the 133rd Symposium of the International Astrophysical Union*. Dordrecht, Boston, and London: Kluwer, 1988.

21 Camille Flammarion. "La photographie céleste à l'Observatoire de Paris." *Revue d'Astronomie Populaire* 5 (1886): 55.

22 For the zone assignments of individual observatories, cf. Nathy P. O'Hora. "Astrographic Catalogues of British Observatories." *Mapping the Sky*. 136. For a tabulation of the final contributions, cf. Lankford. "Impact." 30.

23 Commission 23 of the International Astronomical Union, established in 1919 to oversee the Carte du Ciel, was dissolved in 1970. Théo Weimer. "Naissance et développement de la Carte du Ciel." *Mapping the Sky*. 30.

24 Turner. *Great Star Map*. 145. The project ultimately produced some 22,000 plates. Lankford. "Impact." 30.

25 On the expected advantages of the map, cf. Mouchez. *Photographie*. Chapters 3-4.

uniting astronomers around the world and across generations, the Carte du Ciel aspired to nature's own Brobdingnagian scale.

Although photography made this immense project conceivable, and although its supporters at times invoked the ideals of mechanical objectivity,[26] it was the ethos of the map constructed by many hands that guided the makers of the Carte du Ciel. Astrophotography promised speed, permanence, and authenticity, but not globality and uniformity. As the deliberations of the 1887 International Congress and of subsequent meetings (1889, 1891, 1896, 1900, 1909) of the Permanent Committee make clear,[27] the intricate co-ordination of telescopes, photographic plates, micrometric measurements, and myriad other details to ensure that the parts of the map would be commensurable required that participants relinquish control not only over instruments and methods, but also over the choice of research area for years to come.

The debate over the kind of telescope to be used in the photographic work of the Carte du Ciel shows how dearly uniformity and globality were sometimes purchased. Although British astronomers such as Andrew Ainslie Common and Isaac Roberts had pioneered stellar photography using reflecting telescopes,[28] and although, unlike refractors, reflectors could be used for both visual and photographic observations, it was Common and Roberts who recommended that "the reflector should yield to the refractor in a work to be undertaken in concert." Common defended the reflector as "the best instrument in all respects for celestial photography," but its proper use required "long and careful experiments," in contrast to the refractor, "whose manipulation was easily learned." Roberts seconded the point that the reflector "required the exercise of great care and patience, and a thorough personal interest on the part of the observer using it. In the hands of such a person it yielded

26 On mechanical objectivity, cf. Lorraine Daston and Peter Galison. "The Image of Objectivity." *Representations* 40 (1992): 81-128. There were references to a map made "by photography alone and without the intervention of any human errors" and to replacing "the personality [i.e., the personal equation] of the observer by the impersonality of the plates." Camille Flammarion. "Le Congrès astronomique pour la photographie du ciel." *Astronomie* 6 (1887): 161-69, on 163; T.N. Thiele quoted in Winterhalter. *The International Astrophotographical Congress*. 59.

27 Cf. Winterhalter. *The International Astrophotographical Congress*; Institut de France-Académie des Sciences. *Congrès*; also the irregularly published *Bulletin du Comité Permanent International pour l'Exécution Photographique de la Carte du Ciel.*

28 On Common's and Roberts's photographic work with reflectors, cf. John Lankford. "Amateurs and Astrophysics. A Neglected Aspect in the Development of a Scientific Specialty." *Social Studies of Science* 11 (1981): 275-303.

excellent results, but in other hands it might be a bad instrument." Neither Common nor Roberts was confident that all observers in so large and international an undertaking would command the necessary skill and patience; therefore, "for the sake of securing uniformity in the operations of a large number of astronomers," they urged the adoption of a refractor like the one used by the brothers Prosper and Paul Henry at the Paris Observatory.[29]

What was being sacrificed here was not simply the expense of building a new telescope to the specifications laid down by the international congress,[30] nor British pride in their illustrious tradition of reflectors, the telescope invented by Newton. At stake was also skill, and the accuracy vouchsafed by skill, for skill in the operation of reflectors was too local a peculiarity to be safely standardized. Where nearly a score of observatories and hundreds of observers had to mesh their methods and results into a seamless whole, there was no room for any idiosyncrasy, including the idiosyncrasy of superior accuracy.[31]

The flattening pressure of uniformity was also exerted on superior precision in the measurement of the plates to determine stellar positions. By resorting to the labor of poorly paid schoolboys and legions of volunteers, Oxford astronomer H.H. Turner was able to complete at least that part of the astrographic catalogue assigned by the International Congress to his observatory by 1911, although the Oxford contribution to the photographic map was never completed.[32] Impatient with those observatories still in arrears in delivering their portion of the Carte du Ciel, Turner chided his colleagues elsewhere for indulging in

29 Winterhalter. *The International Astrophotographical Congress*. 18.

30 The price of each telescope was estimated at FF 40,000, plus an additional FF 21,000 for the dome and measuring apparatus. Flammarion. "Le Congrès astronomique." 167-68. Most of the refractors used in the Carte du Ciel were made either by Grubb in Dublin (seven) or Gautier in Paris (nine). O'Hara. "Catalogues." 136; Patrick A. Wayman. "The Grubb Astrographic Telescopes." *Mapping the Sky*. 139-42.

31 Throughout the 1887 Congress there were tensions between those who demanded "perfect identity" and those who defended superior instruments or methods. For example, Folie, director of the Brussels Observatory, and Janssen, President of the Paris Academy of Sciences, pleaded that more powerful but non-standard instruments like the Meudon telescope be permitted to take part in the mapping. Janssen also noted that it would violate long-standing practice to dictate standards to the best instrument-makers in the various countries participating in the Carte du Ciel: "lorsque l'on s'adresse à des artistes de talent, on leur laisse habituellement une grande latitude pour les détails de la construction." Institut de France-Académie des Sciences. *Congrès*. 26, 46.

32 O'Hora. "Catalogues." 137.

what he deemed an excess of precision in the measurement of their plates. The whole Carte du Ciel was in jeopardy as a result; work that under the best of conditions would have taken twenty years now threatened to stretch out for at least forty:

> The fact is that the necessity for strenuous economy has not been sufficiently realised: some of the larger observatories strained at an accuracy scarcely possible even for them; and their weaker brethren, in attempting to copy their example, have been left far behind. Moderation and self-denial are just as necessary in astronomical work as in other walks of life.[33]

Turner's stern appeal to "self-denial" in the context of getting on with the Carte du Ciel project was of a qualitatively different kind than the self-restraint preached by the advocates of mechanical objectivity, albeit no less moralized in tone. Individual scientists ought to exercise *self-restraint* in judgment and interpretation, lest their voices drown out nature's own.[34] In contrast, individuals or, more often, research groups were admonished to practice *self-denial* in their choice of equipment and methods, lest local peculiarities jeopardize the joint effort to grasp nature as a whole. The levels of sacrifice demanded by the scientific collectivity were several: the cost in time and money of new instrumentation and training, the relinquishing not only of tried and true but also sometimes of more accurate methods, the substitution of efficiency for painstaking precision, the monopolization of resources and personnel for long periods of routinized labor, the steadfast resistance to the temptation to neglect old collaborative commitments in pursuit of an exciting new discovery. These sacrifices were particularly grave for the smaller observatories participating in the Carte du Ciel project: for example, the Australian observatories of Sydney, Perth, Melbourne, and Adelaide took 80 years to complete their three assigned zones of the sky (18% of the entire sky), at the price of limiting other investigations, particularly in the enormously fruitful fields of astrophysics and spectroscopy.[35]

Some astronomers found the sacrifices required by the Carte du Ciel too great. T.N. Thiele, director of the Copenhagen Observatory, declined to take responsibility for a zone, for he foresaw that it would conscript the observatory into years of monotonous labor and jeopard-

33 Turner. *Great Star Map*. 75.

34 Claude Bernard. *Introduction to Experimental Medicine* [1865]. Trans. Henry Copley Greene. New York: Dover, 1957. 22f.

35 Graeme L. White. "The Carte du Ciel – The Australian Connection." *Mapping the Sky*. 48; cf. Lankford. "Impact." 32, on the converse advantages to American observatories which did not participate in the Carte du Ciel.

ize the precision measurements that were his chief interest.[36] But even those who refused to compromise their autonomy by participating in the Carte du Ciel acknowledged the moral authority of the collaboration. Edward Pickering, director of the Harvard Observatory, preferred to conduct his own astrophotographic survey when the 1887 International Congress did not adopt his recommendations concerning instruments and techniques.[37] Yet Pickering felt obliged to defend his defection to Admiral Mouchez in 1889, opposing scientific individualism and diversity to the Congress's call for collaboration and standardization:

> I desired to avoid any appearance of dissatisfaction with the decisions of the International Congress, as I was, as I still am, unwilling to criticize the plans, prepared with such care by so large a number of representative astronomers. But I cannot think it best for the promotion of science to abstain on this account from the attempt to carry into execution a series of observations which has seemed desirable to me for many years . . . In the present state of astronomical photography, it seems to me there is no danger of any serious waste of labor in the repetition of photography of the same regions taken by different methods, each of which may prove to have special merits.[38]

Given that at this time astrophotographic techniques were being refined yearly, Pickering's plea for flexibility and experimentation was reasonable from the standpoint of securing the most accurate star charts possible. Yet for those astronomers who had volunteered the rest of their working lives to the Carte du Ciel, his breezy dismissal of commensurability must have seemed the height of egotism.

Coordinating Coordinates

Mapping the skies was a suitably ethereal undertaking; the determination of the earth – its dimensions and shape – however followed at least in part altogether practical demands. By the early 1860s, ocean navigation was in full-ahead expansion, the rigorous establishment of political boundaries was increasingly vexed, and re-worked maps for Europe and beyond were a major concern. Railroads and miners wanted excellent maps; so too did the colonizers as they began carving up Africa,

---

36 Leif Kahl Kristensen. "T.N. Thiele and the Carte du Ciel." *Mapping the Sky*. 59-63.

37 Pickering did however serve on the photometric commission of the Permanent International Committee of the Carte du Ciel. Lankford. "Impact." 38. For the full text of Pickering's recommendations to the International Congress of 1887, cf. Winterhalter. *The International Astrophotographical Congress*. 55-58.

38 Letter of Pickering to Mouchez, 14 August 1889. Pickering Papers, Harvard University Archives, UAV 630.14, ser. A-9, 3f.

South America, and Southeast Asia. Beyond the practical, measuring the figure of the earth mattered for an understanding of the law of gravity and the basic constants of solar system distances. Peirce, working for the Coast and Geodetic Survey, insisted on the point:

> Although in laying out the plan of a geodetical survey the relative utility of the knowledge of different quantities ought to be taken into account, and such account must be favorable to pendulum work, yet it is also true that nothing appertaining to such a survey ought to be neglected, and that too great a stress ought not to be put on the demands of the practically useful. The knowledge of the force of gravity is not a mere matter of utility alone, it is also one of the fundamental kinds of quantity which it is the business of a geodetical survey to measure.[39]

Peirce was by no means alone in his attention to the abstract as well as the concrete. For centuries leading lights of natural philosophy had been concerned with the shape of the earth. Newton had famously used the measurement between Paris and Amiens in order to determine the shape of the earth - the earth, he argued, should be flatter near the poles because of the earth's spin on its axis. Despite all this interest, as local or national measurements proceeded in the nineteenth century, it had become ever clearer that there were discrepancies and gaps between these partial efforts. For example, various longitude surveys concluded that the earth was more or less flattened at the poles. Astronomical measurements clashed with geodetic ones – in one case they clashed over the longitudinal difference between Milan and Turin by 31.29". And when different observers swung pendula to determine the strength of gravity (a quantity that varied with latitude because of the spinning of the earth) their results differed by as much as 3%. For all these reasons – as well as for the more abstract mathematical-physical desire to know the detailed shape of the earth – scientists from many countries had made measurement of the earth a matter of international concern.[40]

Among other advocates, C.F. Gauss had hoped for a vast chain of interlocking trigonometric measurements to map the world. Johann Jacob Baeyer had begun this task first for the Prussian military, then as part of a European Gradmessung, and with no small amount of difficulty helped form a General Conference that, from 1864 through 1912, met some seventeen times.[41] This coordination would not be trivial, as Baeyer's "General Report" made clear already in 1862:

---

39 Charles S. Peirce. "Six Reasons for the Prosecution of Pendulum Experiments" [1882]. *Writings of Charles S. Peirce. A Chronological Edition*. Ed. Christian J.W. Kloesel et al. Bloomington: Indiana University Press, 1986. Vol. 4, 359f.

40 Völter. *Geschichte*. 7-9.

41 Ibid. 7-9.

> Since the greatest possible similarity in form must be desired, the question arises as to whether, in order to achieve this goal, it would not already be best to design a general working plan and to agree upon this at a general conference. As advisable as this may seem at first glance, on closer inspection one comes up against difficulties which, if agreed upon too soon, in all probability could not be cleared up. For the state of measurement in the various countries is no less variable than the available means and personnel, so that one is indeed everywhere obliged to adjust to particular relations and conditions, of which one cannot assume that they may be treated in the same way.[42]

There were difficulties coordinating procedures, standards, and equipment. Even the individual observers found it hard to execute the work because of an amalgam of physiological, meteorological, and instrumental difficulties. In 1865 Baeyer reported that their original intention to conduct night observations had been thwarted first because his eyes were tired after working all day and they needed rest in order to be fresh for the next set of measures. Second, the lighting equipment for the cross hairs differentially warmed the parts of the instrument leading to errors.[43] Problems multiplied when inter- as well as intra-individual observations had to be coordinated, especially among various national scientific équipes.

Thirteen years later, in 1875, friction among the participating nationalities had only increased. At a meeting called to standardize the form for recording data, tension surfaced the moment these differences became apparent. French-German relations were already severely strained by the Franco-Prussian war in which the Prussians had soundly pummeled the French. Peirce was present for the assembly, so too were many of the great astronomers of the day, including Swiss astronomer Adolph Hirsch and his French colleague Hervé Faye. The Frenchman Yvon

---

42 "Da hierbei eine grösstmögliche Gleichformigkeit wünschenswerth sein muss, so entsteht die Frage, ob nicht, zur Erzielung derselben, schon jetzt ein allgemeiner Arbeitsplan zu entwerfen und auf einer allgemeinen Conferenz zu vereinbaren wäre. Wie zweckmässig dies auch auf den ersten Blick erscheinen mag, so stösst man doch bei näherem Eingehen auf die Sache, auf Schwierigkeiten, welche bei einer zu frühzeitigen Vereinbarung aller Wahrscheinlichkeit nach nicht aus dem Wege geräumt werden können. Denn der Stand der Vermessungen ist in den verschiedenen Ländern nicht minder verschieden als die disponiblen Mittel und Kräfte, so dass man genöthigt sein wird sich thatsächlich überall nach besonderen Verhältnissen und Umständen zu richten, von denen man nicht annehmen kann, dass sie sich gleichartig behandeln lassen." *General-Bericht über die mitteleuropäische Gradmessung für das Jahr 1862*. Berlin: Reimer, 1863. 5.

43 J.J. Baeyer. "Bericht über den allgemeinen Standpunkt der Preussischen Vermessungen in Bezug auf die mitteleuropäische Gradmessung, und im Besonderen über die im Jahre 1864 ausgeführten Arbeiten." *General-Bericht über die mitteleuropäische Gradmessung für das Jahr 1864*. Berlin: Reimer, 1865. 33

Villarceau launched one donnybrook when he dismissed the recording of probabilistic errors as "useless" in the registration of astronomical coordinates. At this, Hirsch reacted with hard-edged diplomacy: of course the French were not obliged to fill out every column of the agreed-upon forms. The Commission obliged no one to do so. But if the French did not calculate the probable errors – which Hirsch himself judged to be a very regrettable state of affairs – one would not be able to say what would happen in other countries.[44]

Their probabilistic honor besmirched, Astronomer Faye responded indignantly, protesting that not only had some of the most beautiful work on the subject been done by French mathematicians, but that the French astronomers prized the work highly indeed, and taught probability in their lectures. Counter-protested Hirsch: no possible offense was meant to M. Faye; Hirsch's concerns touched only M. Villarceau. Hirsch at *no* time would have dreamt of sullying all French scholars. Perish the thought. The idea that his illustrious colleagues would have neglected probability calculus or the theory of least squares would not ever have crossed his mind.[45]

Sometimes the issue was not so much the universality of skill, but the validity of a particular kind of instrument. When one 1875 delegate enthusiastically backed the accuracy of a slightly modified Repsold Pendulum for the determination of mass ("it meets all the scientific demands"), Peirce rose to protest. He had discovered that the swinging of the pendulum gave rise to a distortion of the three-legged stand, so one would find a falsely shortened length of the pendulum.[46] But whether one used the Repsold Pendulum or one of its competitors, swinging pendula, measuring time intervals, setting up stations at far-flung sites in exceedingly demanding conditions was difficult work. The temptation to use automatic, self-recording devices was great – there were even some moderately successful trial attempts. After all, around the mid-1870s self-recording instruments were becoming more and more common in domains from medicine to long-distance timing signals for longitude determination. But the delegates were not persuaded. Automatic registration promised to eliminate – at least in principle – the variation among observers. Peirce, Hirsch, and their colleagues remained attached to the observer-run measurement.[47]

---

44 *Verhandlungen der vom 20. bis 29. September 1875 in Paris vereinigten Permanenten Commission der Europäische Gradmessung*. Berlin: Reimer, 1875. 56f.

45 Ibid.

46 Ibid. 59.

47 Ibid. 60.

While they agreed to abandon automatic registration devices, the delegates unanimously concluded that for the Gradmessung to succeed they would need to provide an international apparatus. It was not that one needed a new, more accurate device. Those in current use were, in and of themselves, accurate to a very high degree. No, the problem was not that the current instruments were, taken separately, inaccurate. Instead, this international device would assist at the boundaries between the already-existing national networks of geodetic triangulation measurements – where it had become all-too noticeable that agreement was very difficult. This had sadly been the case when the Italian and Austrian officers met to establish a baseline at Udine. Furthermore, an international apparatus would offer an unambiguous choice for geodecists from the smaller European countries. At long last it would be possible to bring the European Net into a unified form, and to have all lengths expressed in identical units. If this were admitted, some of the leading figures argued, then the natural consequence would be the establishment of an international metrical standard mass. That mass should be located in the International Bureau of Weights and Measures, under the authority of observers from the various countries who were setting the base lines for their measurements.[48] As Peirce put it in 1879, "The value of gravity-determinations depends on them being bound together, each with all the others which have been made anywhere on earth."[49]

Many measurements demanded great skill, skills of instrument making, instrument maintenance, calculation, and data reduction. In the Carte du Ciel it is clear that the British astronomers could have proceeded with their reflectors, but given many other countries' inability to build and maintain these finicky devices, the effort as a whole was quite likely to fail. The British ceded – as we have seen – and the refractor became the standard instrument. Villarceau may have exaggerated the difficulty of maintaining error bars, but his general point was quite clear: demand too much skill and you lose commensurability. Some five years after he had protested at the Germans' insistence on the recording of errors, he came back with a similar protest about demands that seemed to exceed the abilities of the participants:

> Mr. *Yvon Villarceau* considers it important to differentiate the relative gravity determinations from the absolute ones, the commission having concerned themselves only with the latter. According to him, the absolute determinations present

---

48 Ibid. 62f.

49 Charles S. Peirce. "Measurements of Gravity at Initial Stations in America and Europe" [1879]. *Writings*. Vol. 4, 81.

> so many difficulties that these should only be made in a certain number of observatories, where all the necessary tools are available.[50]

For once, *les Anglo-Saxons* and the French seemed to agree. Efficiency, and the accompanying pragmatic demands of keeping the results on track, were not always easily compatible with the ultimate in measurement precision. On 5 May 1882, Major (later Colonel) John Herschel of the Trigonometrical Survey of India wrote to Prof. J.E. Hilgard, Superintendent of U.S. Coast and Geodetic Survey. The subject was the conference on gravity observations held in Washington, D.C., but the issue more general:

> I hold it to be a very lamentable thing that men of zeal, eager to advance science, should continue to be misled by the old school of physics into launching upon the difficult and precarious entreprise of absolute determination of gravity, generally in ignorance of the real difficulties of the research, and *always* indifferent to the utility of such determination. The German school is responsible for this.[51]

Herschel then went on to speak of his own, British-supported latitude work in India. He insisted that

> the same arguments and motives . . . point out the urgent need for economy in every detail of installation and observation – in the choice of stations and the buildings to be occupied, in the distribution of time to be taken up by the observations, and by the calculations respectively, so as to get, in short, as many results of a sufficient degree of accuracy, and no more, as possible within the year.[52]

Peirce appreciated Herschel's pragmatic stance. As far as Peirce was concerned, it was not only a matter of the raw difficulty of absolute determinations of gravity, but more generally, whether the Germans' insistence on precise least-squares analysis captured the real physical difficulties in estimating errors. Protocol, Peirce was essentially arguing, did not exhaust error:

> The history of pendulum observations abounds with inexplicable contradictions and anomalies indicative of unknown causes of error; and hardly a single observer

---

50 "Herr *Yvon Villarceau* hält es für wichtig, die relativen Schwerbestimmungen von den absoluten zu unterscheiden, mit welchen letzteren allein die Commission sich beschäftigt hat. Seiner Meinung nach bieten die absoluten Bestimmungen so viel Schwierigkeiten dar, dass man dieselben nur in einer gewissen Anzahl Sternwarten anstellen sollte, wo man alle nöthigen Hülfsmittel besitzt." Centralbureau der Europäischen Gradmessung. *Verhandlungen der vom 13. bis 16. September 1880 zu München abgehaltenen sechsten allgemeinen Conferenz der Europäische Gradmessung*. Berlin: Reimer, 1881. 27.

51 Reply of Major J. Herschel to Prof. J.E. Hilgard, 5 May 1882. Peirce. *Writings*. Vol. 4, 353.

52 Ibid. Vol. 4, 354f.

> has ventured to estimate the probable error of his result. Practically, the question of precision is cut by a variety of circumstantial exigencies; and it would seem best to leave it at the discretion of the observer, or director of the work.[53]

Perhaps there is a kind of national poetic appropriateness to these various responses to error estimation: the Germans wanted a rigorous and universal protocol, the Anglo-Americans took the matter to be one of individual, pragmatic choice, and the French found the whole idea of error estimation to be useless. Instruments that in theory were supposed to work well were in practice found to bend and twist in data-wrecking ways. Accurate national surveys clashed on the boundaries, automatic instruments left the delegates fearful of unanticipated error, and the hope – not often fulfilled – was for a universally-shared apparatus that would finally put paid to local variation.

For the shape of the earth, for the determination of gravity at different points, the world-surveying project required a planet-covering net of observers. All those small-scale, partial measurements of the eighteenth and early nineteenth century had, by their discrepancies and gaps, made their inadequacy all too clear. The conditions for a "collective observer" to come into existence took decades, dozens of conferences, new forms of instrumentation, data reduction, and world-covering expeditions.

Tracking Venus

Mapping the heavens and determining the shape of the earth depended crucially on the commensurability of the results submitted by dispersed observers, which in turn depended on intricate coordination of observers, instruments, measurements, and protocols. Without such coordination, costly and nerve-racking though it might be, there would be no way of piecing the parts of the puzzle into a unified scientific object. In the case of the nineteenth-century Transits of Venus expeditions, the problem of coordinating observers, even those with the same training using the same instruments in the same place, proved almost insoluble, despite heroic efforts.

The Transits of Venus expeditions mounted by France, Great Britain, Germany, the United States, and several other countries in 1874 and 1882 were perhaps the single most elaborate and expensive scientific

---

53 Charles S. Peirce. "General Remarks upon Gravity Determinations, by John Herschel." *Writings*. Vol. 4, 368.

undertaking in a century distinguished by ambitious global scientific projects like the Carte du Ciel and the Internationale Gradmessung. The transits were considered to be the most reliable means of determining solar parallax and therefore the astronomical unit, which was in turn the basis for all other calculations of distances within the solar system. The transits occur only once every 121 years, and then in pairs eight years apart. Although the 1639 transit had been observed at least in England by Jeremiah Horrocks and William Crabtree, the idea of using transit observations from remote points on the earth's surface to ascertain solar parallax was first advanced by Edmond Halley after he had observed a transit of Mercury on the stormy island of St. Helen in 1677.[54] Halley's proposal was put into practice only in 1761 and 1769, when astronomers fanned out to Pondicherry and Siberia, Cape Town and Vera Cruz, Santo Domingo and Lapland to track the path of Venus across the sun's surface. The very nature of the phenomenon required a dispersed network of observers, who traveled vast distances with delicate instruments to make observations that only made sense when coordinated with one another. Bedeviled by war, illness, bad weather, and, above all, inexperience in observing the phenomenon in question, the eighteenth-century transit expeditions had produced maddeningly inconsistent and uncertain results.[55] As the transits of 1874 and 1882 approached, astronomers were determined to improve upon the disappointing measurements of their predecessors. They intended to take full advantage not only of new technologies such as photography and the telegraph, but also of imperial grids of transportation, communication, and military organization. Their expeditions, self-consciously described as the continuation of the heroic efforts of the eighteenth-century observers, would extend the observing network commenced over a century earlier in time as well as space.

Three methods were proposed to measure solar parallax during the transit, each dependent on a different, but equally finicky technology. The method of duration (sometimes called Halley's method) measured the duration of the transit as observed from two widely separated stations; here exquisitely calibrated chronometers and equally well-calibrated observers were required to ascertain exactly when contact between Venus and the sun occurred. The heliometric method adapted an instrument originally used to measure the diameter of the sun's disc to find the exact distance from the edge of Venus to the edge of the sun.

---

54 David Sellers. *The Transit of Venus. The Quest to Find the True Distance to the Sun*. Leeds: Maga Velda Press, 2001. 75-90, 104-18.

55 Harry Woolf. *The Transits of Venus*.

Again, considerable skill was required to get good results: "It is a very troublesome instrument to manipulate, and the corrections due to the influence of temperature are extremely difficult to apply. Yet with great care there is little doubt that very accurate measurements can be made."[56] But many astronomers pinned their hopes on the third method, which took a sequence of photographs at intervals as brief as a second of the planet's movement across the sun's disc, using the "photographic revolver" proposed by French scientist Jules Janssen.[57] Janssen's colleague Faye praised the new photographic methods to the Paris Academy of Sciences: "This type of observation removes the observer, and with him the anxiety, the fatigue, the bedazzlement, the haste, the errors of our senses, in a word, the intervention – always suspect – of our nervous system."[58] The eye and brain of the observer would be replaced by a photographic plate and an electric telegraph; with such an apparatus in place, Faye boasted, even a child could outdo the most experienced astronomer.[59] British, German, and American astronomers were perhaps less enthusiastic than the French about photographic methods, but almost all of the 1874 transit expeditions included some apparatus and personnel for making photographs of the transits: "This [photographic] method is looked forward to with much interest, because it is the first time that photography has been extensively employed in delicate astronomical measurements."[60]

These great expectations were bitterly disappointed in the event. In his report on the results of the 1874 British transit expeditions, Astronomer Royal George Biddell Airy judged the photographic results not even worthy of analysis:

> The apparent uncertainty in the conclusions from the photographic registers has led extensively to the persuasion that it is unnecessary to record the photo-

---

56 George Forbes. *The Transit of Venus*. London and New York: Macmillan, 1874. 35.

57 Jimena Canales. *Sensational Differences. Individuality in Observation, Experimentation, and Representation (France 1853-1895)*. Ph.D. dissertation. Harvard University, 2003. Chapter 3.

58 "Ce genre d'observation supprime l'observateur, et avec lui l'anxiété, la fatigue, l'éblouissement, la précipitation, les erreurs de nos sens, en un mot l'intervention toujours suspect de notre système nerveux." Hervé Faye. "Sur l'observation photographique des passages de Vénus et sur un appareil de M. Laussedat." *Recueil des mémoires, rapports et documents relatifs à l'observation du passage de Vénus sur le soleil*. Paris: Firmin Didot Frères, 1874. 178.

59 Hervé Faye. "Rapport sur le rôle de la photographie dans l'observation du passage de Vénus." *Recueil des mémoires, rapports et documents relatifs à l'observation du passage de Vénus sur le soleil*. Paris: Firmin Didot Frères, 1874. 228, 232.

60 George Forbes. *The Transit of Venus*. 31.

> graphic operations with the utmost detail . . . The ardour of the Observers has been much cooled by the apparent general failure of the photographic principle, and they were unwilling to spend further time on those reductions.[61]

The director of the Berlin Observatory Foerster estimated that the probable error of the distances between the centers of Venus and the sun measured from the photographic plates was about five times as great as that derived from the heliometric results. He traced the problem to the instantaneity of the photographs:

> The reason for this oscillation is that, in almost instantaneous photographic images produced, the current phase of atmospheric oscillation is photographed, while in micrometric measurement a good observer avoids them by fixing a median position for the images.[62]

Here the vaunted virtues of photography were turned into vices: automaticity and instantaneity recorded every minute fluctuation in the air, fluctuations that a seasoned observer knew how to smooth out. Even the French conceded defeat and supported the recommendation of the international conference, convened in Paris in a last-minute attempt to better coordinate the national expeditions of 1882, to minimize photographic efforts in the second, all-important attempt.[63]

This threw the weight of responsibility back upon the human, all-too-human observers. Past experience, both of the eighteenth-century and 1874 expeditions, had shown how difficult the observations were to make under often makeshift conditions in remote locales with dodgy weather. The German mission sent to the Auckland Islands did not glimpse the sun for the entire month of November; on December 9th, "the, for us, so important day," the rain abated only long enough to afford a brief glimpse of the transit.[64] The French mission in Patagonia similarly complained of "the harshness of a detestable climate," which

---

61 George Biddell Airy, ed. *Account of Observations of the Transit of Venus, 1874, December 8, Made under the Authority of the British Government. And of the Reduction of the Observations*. London: Her Majesty's Stationery Office, 1881, Appendix V: "Photographic Observations of the Transit of Venus." 14, 19.

62 "La raison de cette inferiorité est que, dans des images photographiques presque instantanées, la phase actuelle des oscillations atmospheriques est photographiée, tandis que, dans les mesures micrométriques, un bon observateur s'en débarrasse en fixant pour les images une position moyenne." Ministère de l'Instruction Publique et des Beaux-Arts. *Conférence internationale du passage de Vénus. Procès-Verbaux*. Paris: Imprimerie Nationale, 1881. 6.

63 Ministère de l'Instruction Publique et des Beaux-Arts. *Conférence internationale du passage de Vénus*. 24.

64 Arthur Auwers, ed. *Die Venus-Durchgänge 1874 und 1882. Bericht über die Deutschen Beobachtungen*. Berlin, 1887-98. Vol. 1, 175.

impeded their work.[65] The cooperation of the local authorities as well as that of the elements was a further precondition for success. The Mexican government had to lay a telegraph cable, build a road, and station a squadron of soldiers to enable the French mission to get on with their observations undisturbed;[66] the British mission on the Hawaiian island of Waimea insured peace and quiet by posting sentries around the observing station and having "the grounds 'tabooed,' or, as they call it, 'kapu,' for the day."[67]

Yet even when both weather and local inhabitants favored the observers, making the observations was often a vexed business. Captain G.L. Tupman, R.M.A., superintendent of the 1874 British expeditions and himself charged with the mission to Honolulu, reported optimal conditions on the day of the transit, 9 December 1874: the sky was cloudless. Her Majesty Queen Kapiolani of Hawaii had obligingly ordered her subjects (who had climbed trees and rooftops to observe the observers) "that 'Silence' must be maintained;" all the instruments were in readiness. Yet Tupman, bent over his telescope, was greatly distressed nonetheless to miss the all-important first contact of Venus with the sun's surface: "After 20 seconds which I have recorded I am perfectly certain the contact was *passed*, established completely – not 'contact' properly speaking, for that implies some definite instant never observed."[68] The four points of contact between planet and sun's perimeter – internal and external ingress, internal and external egress – so essential for the determination of parallax, were anything but sharply defined visual events. The French observers in Patagonia devised the following Cartesian criteria of contact:

1. The beginning of uncertainty (*Le commencement du doute*);
2. The presumed moment of contact (*L'instant présumé du contact*);
3. The end of uncertainty (*La fin du doute*).[69]

Their colleagues stationed on Chubut cautioned that the values they had obtained, albeit convergent, could not be treated as entirely accurate: "the observation is not in itself susceptible of great precision."[70]

65 "la rudesse d'un climat détestable." Académie des Sciences. *Passage de Vénus du décembre 1882. Rapports préliminaires*. Paris: Gauthier-Villars, 1883. 60.

66 Ibid. 30.

67 Airy, ed. *Account of Observations of the Transit of Venus*. 241.

68 Airy, ed. *Account of Observations of the Transit of Venus*. 44.

69 Académie des Sciences. *Passage de Vénus*. 65.

70 "l'observation en elle-même n'est pas susceptible d'une grande précision." Ibid. 81.

Almost all of the observers had been rigorously trained beforehand using artificial transit machines set up at observatories in Paris, Washington, Greenwich, and Berlin. Although the very nature of the transits of Venus, with luck visible only twice within a human lifetime, prevented the accumulation of observational experience by the usual means, it was hoped that practice with the models would hone the eyesight and judgment of the expedition members. "To make a really good observation of this contact," intoned the official instructions issued to the American observers,

> two conditions are essentially necessary to all which have been described. The observer must have had some previous practice in observing first contacts, and must know exactly where to look for the contact. The first condition can be well fulfilled by the artificial transit of Venus apparatus, of which it is intended to have one or more available for observers.[71]

Astronomers were confident that the egregious errors of the 1761 and 1769 expeditions might, with sufficient advance training, be reduced from fifteen to two seconds.[72] Yet when faced with the real transit, many well-schooled observers were dismayed to discover that what they saw did not resemble the familiar model. It was these divergences that persuaded Tupman he had missed first contact, despite straining every nerve to catch it: "Everything hitherto having so closely resembled the appearances in the model, I felt certain that I had missed the contact while focusing, although I could not understand how it could have occurred so much sooner than I expected."[73] Some of the German observers on the island off the coast of the Chinese port city Chih-fu were confused by the ring of light surrounding Venus caused by the illumination of its atmosphere as it passed in front of the sun:

> Because of these very effects, Reimann found the appearance so completely dissimilar from what he was familiar with from the model, that he was utterly unable to recognize the moment to which the observer's attention was primarily directed . . . Quite the opposite, Valentiner was surprised by the complete similarity of the actual appearance to the representation by the model, and he found the main moment of observation to be precisely determined.[74]

---

71 U.S. Transit of Venus Commission. *Instructions for Observing the Transit of Venus, December 6, 1882*. Washington, D.C.: Government Printing Office, 1882. 40.

72 George Forbes. *The Transit of Venus*. 54.

73 Airy, ed. *Account of Observations of the Transit of Venus*. 43.

74 "Reimann fand wegen der Mitwirkung desselben die Erscheinung so gänzlich unähnlich der ihm vom Modell her bekannten, dass er das Moment, auf welches die Aufmerksamkeit der Beobachter hauptsächlich hingelenkt war, gar nicht zu erkennen vermochte . . . Ganz entgegengesetzt wurde Valentiner durch vollkommene Ähn-

Even observers who had scrupulously practiced with the artificial transit machines found themselves confused by the actual phenomenon, even to the point of disagreement as to whether the observed appearances in situ resembled those of the model or not. All that could be done was to conserve attention, as an athlete husbands strength before a race:

> It is essential that the observer should allow his eye nearly perfect repose for several minutes before the contact. It is quite right and proper that he should take a general view of the phenomenon at short intervals, and note the appearance presented by the outline of the planet, but he should not exercise his eye and attention in endeavoring to make any difficult observation.[75]

Faced with these results, instructions to observers on the second, 1882 expeditions underscored the importance of writing down immediate, detailed, and inalterable descriptions, preferably with drawings, of exactly what each observer had seen. Probable errors might later be noted and corrected, but the original, unvarnished impressions and measurements must be preserved at all costs.[76] These accounts, carefully kept and duly published in their entirety, testify to the bewildering diversity of perceptions among the observers, even among those who had been trained on the same models and who were observing side-by-side at the same station. Neither photography nor electric telegraphy nor artificial transit machines could dispense with the observer or eliminate troubling differences among observers.

Artificial star machines, of which the transit models were the most important variety, had first been introduced in observatories in the 1850s and 1860s to measure and hopefully to eliminate the absolute personal equations of individual observers. Since Bessel's analysis of the personal equations of himself and his colleagues Argelander, Struve, and Walbeck in 1820,[77] several observatories had regularly computed the relative personal equations of pairs of observers. When in 1858 J. Hartmann invented an artificial apparatus to help observers improve their skills, French astronomer Charles Wolf leaped at the opportunity to investi-

---

lichkeit der wirklichen Erscheinung mit der Darstellung am Modell überrascht und fand das zu beobachtende Hauptmoment sehr sicher bestimmt." Auwers, ed. *Die Venus-Durchgänge*. Vol. 1, 159.

75 U.S. Transit of Venus Commission. *Instructions*. 41.

76 Auwers, ed. *Die Venus-Durchgänge*. Vol. 1, 195; U.S. Transit of Venus Commission. *Instructions*. 49; Ministère de l'Instruction Publique et des Beaux-Arts. *Conférence internationale*. 30.

77 Friedrich Wilhelm Bessel. *Abhandlungen*. Ed. Rudolf Engelmann. Leipzig: Verlag von Wilhelm Engelmann, 1875-1876. Vol. 3, 300-04.

gate the causes of personal equations and the means to eradicate them.[78] He conducted a long series of experiments on himself and the staff of the Paris Observatory in 1863-64, using an apparatus of his own construction. At first his results seemed nothing less than spectacular. After three months of practicing with the machine, he was able to reduce his own absolute personal equation from 0.30 to 0.11 seconds. But after that, no further progress seemed possible, despite the most focused efforts of attention:

> In the present conditions, it is totally impossible for me to observe in a different way, and even the strictest attention does not allow me to seize the least dead time from the moment when I hear the second and that when I fix the position of the star.

Wolf drew radical conclusions from his own failure to eliminate error:

> Does this mean that time does not exist? Evidently not. By its very nature, in a good observer, it should be completely imperceptible; and it is even an indispensable condition of the consistency of the mode of observation of an astronomer: astronomy should not be perfectible.[79]

What had begun as a quest for perfection, of observation without error, attained through conscious practice, ended as the acceptance of unconscious and incorrigible lags in perception, so long as these were constant. Errors could not be entirely erased, but they could be made as predictable as the stars themselves.

The role of the artificial transit machines in achieving constancy, if not perfection, was not trivial. Wolf's results contradicted those of the Swiss astronomers Hirsch and Plantamour, who had found significant inter- and intra-observer variations in their absolute and relative personal equations.[80] Central to resolving this issue, and indeed all issues concerning the personal equation, was a determination of the efficacy

---

78 Rudolphe Radau. "Sur les erreurs personnelles." *Moniteur Scientifique-Quesneville* (November 15, 1865): 16-25.

79 "Dans les conditions actuelles, il m'est tout à fait impossible d'observer autrement que je ne le fais, et l'attention la plus sévère ne me permet de saisir le moindre temps mort entre le moment où j'entends la seconde et celui où je pointe la position de l'étoile." – "Est-ce à dire que ce temps n'existe pas? Évidemment non. Par sa nature même, chez un bon observateur, il doit être complètement imperceptible; et c'est même là une condition indispensable de la constance du mode d'observation d'un astronome: l'astronome ne doit pas être perfectible." Charles Wolf. "Recherches sur l'équation personnelle dans les observations de passage." *Annales de l'Observatoire de Paris. Mémoires*, 8 (1866): 188.

80 Rudolphe Radau. "Sur les erreurs personnelles." 22; Adolphe Hirsch. "Sur les corrections et équations personnelles dans les observations chronographiques de passage." *Bulletin de la Société des sciences naturelles de Neuchâtel* 6 (1863): 365-72.

of training: how much could absolute personal equations be reduced by attention and education? To what extent was the irreducible error that remained a physiological fact about the observer's constitution, to what extent a variable dependent on contingent circumstance? In a meticulous review of personal equation experiments to date, and against the background of the two recent transits of Venus, Lyon astronomer François Gonnessiat attempted in 1892 to sort out the contributing elements of the personal equation. The differences between the registration of visual, auditory, and tactile stimuli, the influence of the time of day, the finite speed of transmission of nervous impulses, the internalized time rhythms of seasoned observers – all were submitted to painstaking review and experiment. Gonnessiat's aim was to classify the sources of error, and thereby to distinguish between "*a necessary psychological error, and a different error depending more particularly on attention and capable of being reduced by training*." Errors would be sorted out into conscious and unconscious, voluntary and involuntary, psychological and physiological, subjective and objective. Indeed, the exercise of classification re-affirmed the boundary between the subjective and objective observer, between will on the one hand and nerves and muscles on the other. Kantian antinomies were remapped onto "erreurs rhythmiques" and "erreurs décimales." Yet for all Gonnessiat's care and thoroughness, the exercise failed: one component of the personal equation straddled the fence, precisely poised between subjective and objective, "l'erreur psycho-physiologique." This was "the time that an optical impression needs to provoke the muscle play of the hand"– that is, the precise point of intersection between will and the world.[81]

In contrast to the map of the heavens or the exact shape of the earth, solar parallax was at least in principle a well-defined object. The devil lay in measuring it, since parallax by definition requires at least two observers positioned at either end of a baseline. Since the seventeenth century, astronomers had been convinced that the transits of Venus opened up the royal road for the determination of solar parallax; the scale and expense of the eighteenth and, especially, the nineteenth-century expeditions bear witness to the strength of their conviction. Yet even when the weather co-operated, even when colonial networks smoothed the way of the itinerant astronomers, even when observers practiced for

81 "*. . . une erreur physiologique nécessaire, et une autre erreur dépendent plus particulièrement de l'attention et susceptible de réduction par l'exercice.*" / ". . . le temps qui met une impression reçue par l'œil à déterminer le jeu des muscles de la main". François Gonnessiat. *Recherches sur l'équation personnelle dans les observations astronomiques de passage*. Paris: G. Masson, 1892. 151.

months beforehand on identical artificial transit machines, the difficulties of coordination (even at the intra-individual level) pushed the precise determination of the parallax value out of reach.

Conclusion: Peirce as Philosopher of Scientific Coordination

For Peirce, the coordination of observers and the cancellation of scientific error became both the means toward and the metaphor of the bedrock reality sought by science. Peirce's whole life was intertwined with the alignment of systems of observers. From immediately after his graduation from Harvard in 1859, when he signed on as a temporary aide at the U.S. Coast and Geodetic Society, to his separation from the Survey in 1891, Peirce embarked on a succession of field expeditions, theoretical inquiries into the coordination of instrumentation and statistical procedures, and international conferences designed to bring together measurements by different countries. Throughout his life, and across his several disciplinary bases, Peirce sought knowledge that would transcend what he called the "vagaries" of individualist inquiry. Before the Carte du Ciel was launched, Peirce was deeply engaged in Harvard's own massive sky-mapping project.[82] Between February 1872 and January 1875, he observed at a variety of East Coast sites, recording not only the brightness but also the color of stars.[83] When Pickering protested in 1877 that Peirce's magnitudes had fractional errors as high as 1/10, the philosopher-metrologist shot off an angry letter to his father, the Harvard mathematician Benjamin Peirce:

> *This* figure [of a fractional brightness error equal to 1/10] is not derived from the comparison of my observations among each other, but by the comparison of different observers among each other, so as to include all sources of error.

---

[82] More precisely, Peirce employed a Zoellner astrophotometer, a device which allowed a star to be compared with an adjustable reference light, in order to determine stellar magnitudes.

[83] Victor F. Lenzen. "Charles Peirce as Astronomer." *Studies in the Philosophy of Charles Sanders Peirce*. Ed. Edward C. More and Richard S. Robin. Amherst: University of Massachusetts Press, 1964. 44ff.; also Lenzen. "The Contributions of Charles S. Peirce to Metrology." *Proceedings of the American Philosophical Society* 109 (1965): 29-46; Lenzen. "Charles S. Peirce as Mathematical Geodist." *Transactions of the Charles S. Peirce Society* 8 (1972): 90-105; Lenzen. "The Role of Science in the Philosophy of C.S. Peirce." *Logik, Erkenntnis- und Wissenschaftstheorie, Sprachphilosophie, Ontologie und Metaphysik. Akten des XIV. Internationalen Kongresses für Philosophie, 2-9. September 1968*. Vienna: Herder, 1969. 371-76.

> *2nd* My probable error is smaller than that of any other observer . . . Our friend Pickering doesn't know how to appreciate my work, that's all.[84]

For Peirce, it went without saying that the error had to include an *inter*-observer component that was combined with the *intra*-observer spread of results. His sense that the right result would only emerge from the community's efforts is thus (unlike Pickering's use of purely personal error) built into his expression of the probable error machinery itself.

Peirce's persistent concern, expressed in the details of his scientific work as well as the precepts of his philosophy, was to balance the idiosyncrasies and errors of single measurements and of individuals against one another in a great cancellation of errors. As he put it in 1868,

> The real, then, is that which, sooner or later, information and reasoning would finally result in and which is therefore independent of the vagaries of me and you. Thus the very origin of the conception of reality shows that this conception essentially involves the notion of a COMMUNITY, without definite limits, and capable of an indefinite increase of knowledge.[85]

Peirce's daily activities with the Survey put him dead center within the new-style scientific community of his day, the coordinated activities of observer networks – his first Survey trip to Europe was from June to March 1871. After working at the Harvard Observatory some seven or eight months, Peirce composed a manuscript that built on his earlier concern with eschewing the "vagaries of me and you," but went on to reflect the specific nature of observatory practice.[86] Here he asked after the existence of "something independent of what you or I or any number of men, may think about it." "What," Peirce asked, "is the mode of existence of this reality?" By his lights, all right reasoning proceeds either from reason or from observation. Sometimes similar results issue from different premises, as in the various derivations of the rotation of the earth. But Peirce insisted that in fact all – not some – observations differ in this way as no two observers can *ever* have the "same" observation. "The observations which I made yesterday are not the same which I make today. Nor are simultaneous observations at different observatories the same, however close together the observatories are placed. Every man's senses are his observatory."[87] Observation alone is incom-

84 Letter of Charles S. Peirce to Benjamin O. Peirce, 11 February 1877, MSS. Charles Sanders Peirce L33, Correspondence C.S. Peirce-B.O. Peirce, Houghton Library, Harvard University.

85 Charles S. Peirce. "Consequences of Four Incapacities" [1868]. *Writings*. Vol. 2, 239.

86 Charles S. Peirce, MS 204 (Fall 1872). "Chapter IV of Reality." *Writings*. Vol. 4, 54.

87 Ibid. Vol. 4, 55.

petent to contain the judgment that two observations are similar; that is a task for reason.

By June 1873, Peirce was proposing to his father, then Superintendent of the Coast and Geodetic Survey, that he (Charles) undertake a world tour of measurement.

> The only places at which the length of the pendulum has actually been measured are London, Paris, Berlin & Switzerland, & the connection between these is *nothing* except Paris & London & there it might be better. In order therefore for me to connect the pendulum work on this continent with that all over the globe, I ought to take my apparatus to those places. It is a matter of the highest importance.[88]

Not only would he conduct measurements on the Sandwich Islands and other Pacific atolls, he would take the opportunity of his trip to join a transit of Venus expedition; from Japan and China he would head out across Egypt, swinging a pendulum from atop the great pyramid; from there he would sweep up to Europe, where he would compare instruments with his colleagues at the Europäische Gradmessung.

Following the Paris International Geodetic Society meeting in September 1875, Peirce's grand pendulum tours took him to Geneva, Paris, and Berlin, and culminated in his classic memoir on "Measurements of Gravity at Initial Stations in America and Europe" (1879). Even in this technical scientific text, two essential features of the ethos and epistemology of scientific coordination emerge clearly. First, Peirce argued in his cover letter to his father (then the Survey's consulting geometer) that "the value of gravity-determinations depends upon their being bound together, each with all the others which have been made anywhere upon the earth."[89]

Second, Peirce insisted that not only individuals but also nations must sacrifice their own interests and ambitions for the sake of the scientific community. This self-denial in the service of co-ordination, writ large, was the same moral principle that had led British astronomers to give up their superior reflecting telescopes in favor of the world stan-

---

88 Letter of Charles S. Peirce to Benjamin O. Peirce, 16 June 1873, MSS. C.S. Peirce L33, Houghton Library, Harvard University.

89 Consequently, both Peirce senior and junior had supported making "relative" measurements of pendulum motion rather than attempting absolute measurements both of the pendulum arm and the time of oscillation. The idea behind their method was the following: A pendulum of length l, gravitational acceleration g, and period T obeys the relation $T = \pi\ (l/g)^{1/2}$. Therefore the ratio T_1/T_2 is just $(g_2/g_1)^{1/2}$, since by using the same pendulum at each site, the length l drops out, leaving only the time T to be measured in order to fix g_2/g_1.

dard selected by the Carte du Ciel. National pride and ambition, Peirce argued, must yield to the larger network:

> At the meeting of the International Geodetic Congress in Paris, in 1875, it was resolved, at the suggestion of the writer, that the different states should carry their pendulums to Berlin and swing them in the Eichungsamt there. This has already been done by Switzerland and Austria, and will be done hereafter by every survey which is not willing to sacrifice the solution of a great problem to forms of action based on national exclusiveness. Geodesy is the one science the successful prosecution of which absolutely depends upon international solidarity.[90]

But what if both individuals and nations were incorrigible scientific egotists, à la Pickering? Peirce argued hard against the inevitability of individualism, and in "Grounds of Validity" (1869) explicitly opposed such selfishness to scientific objectivity:

> There is a psychological theory that man cannot act without a view to his own pleasure. This theory is based on a falsely assumed subjectivism. Upon our principles of the *objectivity* of knowledge, it could not be based; and if they are correct, it [the selfishness of men] is reduced to an absurdity.

For Peirce, the primacy of the community was a fact and not merely a pious hope, for he concluded from concern for those left behind after death and habitual identification with the communal destiny "that men do not make their personal interests their only ones, and therefore may, at least, subordinate them to the interests of the community."[91] The objective knower must be many-headed.

It was thus reasonable to hope that humanity might voluntarily embrace the morality of "complete self-identification of one's own interests with those of the community" that all inductive inference – and therefore most of science and life – in Peirce's view presupposed.[92] There is once again evidence that Peirce's own experience in the collective conduct of observation and measurement had taught him that rationality imposed from above was futile. In an 1899 manuscript outlining the guiding principles of the U.S. Office of Weights and Measures, he observed that the full force of the Napoleonic empire had been insufficient to enforce the metric system in its dominions:

> In Prussia, for example, it was for some years a criminal offence to have a Rhenish foot rule in one's possession; and yet I well remember that when I wished to have some carpentry done in one of the halls of the Berlin Eichungs-

90 Charles S. Peirce. "Measurements of Gravity." 81.

91 Charles S. Peirce. "Grounds of Validity" [1869]. *Writings*. Vol. 2, 271; emphasis added.

92 Ibid. Vol. 2, 268-72.

> amt, the mechanic whom I brought in for the purpose demanded that all the data I had to give him should be expressed in Rhine feet and inches.[93]

Peirce's identification of a man's senses with an observatory binds together two problem domains of utterly different scales. Like the establishment of results connecting individuals' sense perceptions, linking astronomical observatories had come to stand for the search for realism, the overcoming of a nominalism that would separate us from our later selves, our selves from others, and one observatory from another. Forced coordination, however, would never work, and the futility of trying to fix belief by despotic means was a long-standing theme in Peirce's more abstract writing. In the practical application, not only was the scientific method better suited to fixing belief than despotism; despotism was also thoroughly incompatible with American values, as Peirce went on to say:

> Americans would never consent to such restrictions upon their liberty. They would make a political issue of them if congress were ever so ill-advised as to enact them, which it never would do. The individual in this country expects to manage his own business in his own way. Our principle is to leave police matters, as much as possible, to the states; and nothing would be gained by an interference on the part of the general government with the police side of the work of metrology.[94]

Beyond expelling police power from science to the federal government, Peirce was arguing, in his technical work and in his philosophy, that a moral training for metrology was a necessary prerequisite to the science.

For Peirce, the word "standard" resonated with a double meaning, at once defining weights and measures and the moral code which created and upheld those units.[95] It was an internalized morality of conscience, rather than of obedience to external authority. In this same 1899 manuscript, Peirce wrote: "The office ought to be so administered as to create or maintain a distinct tradition of scientific and metrological morals within the office . . . Young men have to be trained from boyhood to the reverence for the laws of morals and honor which alone can give the best fruit."[96]

---

93 Charles S. Peirce. "On the Proper Functions of a National Office of Weights and Measures" [1899]. Unpublished MS., MSS. Charles S. Peirce, Houghton Library, Harvard University. We thank Christian Kloesel and the Peirce Edition Project for the date of the manuscript.

94 Charles S. Peirce, "MS 1899." *Writings*. Vol. 5.

95 For similar ambiguities concerning standards, cf. Schaffer. "Late Victorian Metrology and its Instrumentation."

96 Peirce. "Proper Functions."

In his work on the Internationale Gradmessung Peirce had seen at first hand the need for more than a new branch of science; he had come to believe that a new kind of scientist was needed, one whose skills and morals both aimed towards the subjection of excessive willfulness. But if data, measuring missions, and instruments needed to be coordinated, so too did the great scientific and military organizations of the late nineteenth century. Between wars and sometimes straight through them, surveyors, astronomers, and physicists learned to make sacrifices, to hash out their differences until sufficient agreement could be obtained to cover the world with an interlaced grid of observers and data. Some looked down, to correct their maps, steer their boats, and correct their physical theories. Others looked up to measure the solar-system distances. All helped form a new kind of collective science with an internationalism made possible by the coordination of an ever more powerful nationalism. And with that coordination came the framework from which it became possible to bring planetary-scale objects into existence: the distribution of gravitational force, the detailed shape of the earth, the distribution of stars across the whole of the visible sky, and the absolute measure of the solar system.

Works Cited

Académie des Sciences, Paris. *Passage de Vénus du décembre 1882. Rapports préliminaires*. Paris: Gauthier-Villars, 1883.

Airy, George Biddell, ed. *Account of Observations of the Transit of Venus, 1874, December 8, Made under the Authority of the British Government. And of the Reduction of the Observations*. London: Her Majesty's Stationery Office, 1881.

Auwers, Arthur, ed. *Die Venus-Durchgänge 1874 und 1882. Bericht über die Deutschen Beobachtungen*. 6 vols. Berlin, 1887-98.

Bacon, Francis. "New Atlantis" [1627]. *Lord Bacon's Works*. Vol. 2. Ed. Basil Montagu. London: William Pickering, 1825-34.

Baeyer, J.J. "Bericht über den allgemeinen Standpunkt der Preussischen Vermessungen in Bezug auf die mitteleuropäische Gradmessung, und im Besonderen über die im Jahre 1864 ausgeführten Arbeiten." *General-Bericht über die mitteleuropäische Gradmessung für das Jahr 1864*. Berlin: Reimer, 1865.

Bernard, Claude. *Introduction to Experimental Medicine* [1865]. Trans. Henry Copley Greene. New York: Dover, 1957.

Bessel, Friedrich Wilhelm. *Abhandlungen*. 3 vols. Ed. Rudolf Engelmann. Leipzig: Verlag von Wilhelm Engelmann, 1875-1876.

Biagioli, Mario. *Galileo, Courtier. The Practice of Science in the Culture of Absolutism*. Chicago and London: University of Chicago Press, 1995.

Bigg, Charlotte. "Photography and Labour History of Astrometry. The Carte du Ciel." *The Role of Visual Representations in Astronomy. History and Research Practice.* Ed. Klaus Hentschel. Thun and Frankfurt a.M.: Deutsch, 2000. 90-106.

Bonneuil, Christophe. "The Manufacture of Species. Kew Gardens, the Empire, and the Standardisation of Taxonomic Practices in Late Nineteenth-Century Botany." *Instruments, Travel and Science. Itineraries of Precision from the Seventeenth to the Twentieth Century.* Ed. Marie-Noëlle Bourguet, Christian Licoppe, and H. Otto Sibum. London and New York: Routledge, 2002. 189-215.

Bourguet, Marie-Noëlle. "Landscape with Numbers. Natural History, Travel and Instruments in the Late Eighteenth and Early Nineteenth Centuries." *Instruments, Travel and Science. Itineraries of Precision from the Seventeenth to the Twentieth Century.* Ed. idem, Christian Licoppe, and H. Otto Sibum. London and New York: Routledge, 2002. 96-125.

Brian, Éric. "Transactions statistiques au XIXe siècle. Mouvements internationaux de capitaux symboliques." *Actes de la recherche en sciences sociales* 145 (décembre 2002): 34-46.

Bronowski, Jacob. *Science and Human Values* [1956]. New York: Harper & Row, 1975.

Cahan, David. *An Institute for Empire. The Physikalisch-Technische Reichsanstalt, 1871-1918.* Cambridge: Cambridge University Press, 1989.

Canales, Jimena. "Photogenic Venus. The 'Cinematographic Turn' and Its Alternatives in Nineteenth-Century France." *Isis* 93 (2002): 585-613.

Canales, Jimena. *Sensational Differences. Individuality in Observation, Experimentation, and Representation (France 1853-1895).* Ph.D. Harvard University, 2003.

Cannon, Susan Faye. "Humboldtian Science." *Science in Culture. The Early Victorian Period.* New York: Science History Publications, 1978.

Centralbureau der Europäischen Gradmessung. *Verhandlungen der vom 13. bis 16. September 1880 zu München abgehaltenen sechsten allgemeinen Conferenz der Europäischen Gradmessung.* Berlin: Reimer, 1881.

Cooper, Alix. "The Household." *The Cambridge History of Early Modern Science.* Ed. Katharine Park and Lorraine Daston. Cambridge: Cambridge University Press, 2006. 224-37.

Cotreau, James D. *The Historical Development of the Universal Postal Union and the Question of Membership.* Boston: n. publ., 1975.

Daston, Lorraine and Peter Galison. "The Image of Objectivity." *Representations* 40 (1992): 81-128.

Débarbat, Suzanne et al., eds. *Mapping the Sky. Past Heritage and Future Directions. Proceedings of the 133rd Symposium of the International Astrophysical Union.* Dordrecht, Boston, and London: Kluwer, 1988.

Descartes, René. "Discours de la méthode" [1637]. *Œuvres de Descartes.* Vol. 6. Ed. Charles Adam and Paul Tannery. Paris: Léopold Cerf, 1902. 1-78.

Dettelbach, Michael. "Global Physics and Aesthetic Empire. Humboldt's Physical Portrait of the Tropics." *Visions of Empire. Voyages, Botany, and Representations of Nature.* Ed. Peter H. Reill and David Philip Miller. Cambridge: Cambridge University Press, 1996. 258-92.

Eamon, William. *Science and the Secrets of Nature. Books of Secrets in Medieval and Early Modern Culture.* Princeton: Princeton University Press, 1994.

Elzinga, Aant and Catharina Landström, eds. *Internationalism in Science.* London: Taylor Graham, 1996.

Faye, Hervé. "Rapport sur le rôle de la photographie dans l'observation du passage de

Vénus." *Recueil des mémoires, rapports et documents relatifs à l'observation du passage de Vénus sur le soleil*. Paris: Firmin Didot Frères, 1874. 227-36.

Faye, Hervé. "Sur l'observation photographique des passages de Vénus et sur un appareil de M. Laussedat." *Recueil des mémoires, rapports et documents relatifs à l'observation du passage de Vénus sur le soleil*. Paris: Firmin Didot Frères, 1874. 175-85.

Findlen, Paula. *Possessing Nature. Museums, Collecting, and Scientific Culture in Early Modern Italy*. Berkeley and Los Angeles: University of California Press, 1994.

Findlen, Paula. "Masculine Prerogatives. Gender, Space, and Knowledge in the Early Modern Museum." *The Architecture of Science*. Ed. Peter Galison and Emily Thompson. Cambridge and London: MIT Press, 1999. 29-58.

Flammarion, Camille. "La photographie céleste à l'Observatoire de Paris." *Revue d'Astronomie Populaire* 5 (1886): 42-57.

Flammarion, Camille. "Le Congrès astronomique pour la photographie du ciel." *Astronomie* 6 (1887): 161-69.

Fleming, James Rodger. *Meteorology in America, 1800-1870*. Baltimore and London: Johns Hopkins University Press, 1990.

Forbes, George. *The Transit of Venus*. London and New York: Macmillan, 1874.

Friedman, Robert Marc. *Appropriating the Weather. Vilhelm Bjerknes and the Construction of a Modern Meteorology*. Ithaca: Cornell University Press, 1989.

Galison, Peter. *Image and Logic. A Material Culture of Microphysics*. Chicago and London: University of Chicago Press, 1997.

Galison, Peter. *Einstein's Clocks, Poincaré's Maps. Empires of Time*. New York: W.W. Norton, 2003.

General-Bericht über die mitteleuropäische Gradmessung für das Jahr 1862. Berlin: Reimer, 1863.

Gonnessiat, François. *Recherches sur l'équation personnelle dans les observations astronomiques de passage*. Paris: G. Masson, 1892.

Hahn, Roger. *The Anatomy of a Scientific Institution. The Paris Academy of Sciences, 1666-1803*. Berkeley and Los Angeles: University of California Press, 1971.

Hannaway, Owen. "Laboratory Design and the Aim of Science. Andreas Libavius versus Tycho Brahe." *Isis* 77 (1986): 585-610.

Hellmann, Gustav. "Die Entwicklung der meteorologischen Beobachtungen in Deutschland von den ersten Anfängen bis zur Einrichtung staatlicher Beobachtungsnetze." *Abhandlungen der Preussischen Akademie der Wissenschaften, Physikalisch-Mathematische Klasse* 1 (1926): 1-25.

Hirsch, Adolphe. "Sur les corrections et équations personnelles dans les observations chronographiques de passage." *Bulletin de la Société des sciences naturelles de Neuchâtel* 6 (1863): 365-72.

Hoffleit, Dorrit. *Some Firsts in Astronomical Photography*. Cambridge: Harvard College Observatory, 1950.

Hunter, Michael. *The Royal Society and Its Fellows 1660-1700. Morphology of an Early Scientific Institution*. Chalfont St. Giles: British Society for the History of Science, 1982.

Institut de France-Académie des Sciences, ed. *Congrès astrophotographique international tenu à l'Observatoire de Paris pour le levé de la Carte du Ciel*. Paris: Gauthier-Villars, 1887.

Kristensen, Leif Kahl. "T.N. Thiele and the Carte du Ciel." *Mapping the Sky. Past Heritage and Future Directions. Proceedings of the 133rd Symposium of the International Astrophysical Union*. Ed. Suzanne Débarbat et al. Dordrecht, Boston, and London: Kluwer, 1988. 59-63.

Lankford, John. "Amateurs and Astrophysics. A Neglected Aspect in the Development of a Scientific Specialty." *Social Studies of Science* 11 (1981): 275-303.

Lankford, John. "The Impact of Photography on Astronomy." *Astrophysics and Twentieth-Century Astronomy to 1950*. Ed. Owen Gingerich. Cambridge and New York: Cambridge University Press, 1984. 16-39.

Lankford, John. "Photography and the Nineteenth-Century Transits of Venus." *Technology and Culture* 28 (1987): 648-57.

Leibniz, Gottfried Wilhelm. Untitled fragment [1677]. *Die philosophischen Schriften von Gottfried Wilhelm Leibniz*. Ed. Carl Immanuel Gerhardt. Berlin: Weidmann, 1875-90. Vol. 7, 184-89.

Lenoir, Timothy. *Instituting Science. The Cultural Production of Scientific Disciplines*. Stanford: Stanford University Press, 1997.

Lenzen, Victor F. "Charles Peirce as Astronomer." *Studies in the Philosophy of Charles Sanders Peirce*. Ed. Edward C. More and Richard S. Robin. Amherst: University of Massachusetts Press, 1964. 33-50.

Lenzen, Victor F. "The Contributions of Charles S. Peirce to Metrology." *Proceedings of the American Philosophical Society* 109 (1965): 29-46.

Lenzen, Victor F. "The Role of Science in the Philosophy of C.S. Peirce." *Logik, Erkenntnis- und Wissenschaftstheorie, Sprachphilosophie, Ontologie und Metaphysik. Akten des XIV. Internationalen Kongresses für Philosophie, 2-9. September 1968*. Vienna: Herder, 1969. 371-76.

Lenzen, Victor F. "Charles S. Peirce as Mathematical Geodist." *Transactions of the Charles S. Peirce Society* 8 (1972): 90-105.

Mill, John Stuart. "The Spirit of the Age" [1831]. *Collected Works of John Stuart Mill*. Ed. John M. Robson et al. Toronto: University of Toronto Press, 1981-91. Vol. 32, 227-316.

Ministère de l'Instruction Publique et des Beaux-Arts. *Conférence internationale du passage de Vénus. Procès-Verbaux*. Paris: Imprimerie Nationale, 1881.

Moran, Bruce T. *The Alchemical World of the German Court. Occult Philosophy and Chemical Medicine in the Circle of Moritz of Hessen, 1572-1632*. Stuttgart: F. Steiner Verlag, 1991.

Mouchez, Ernest B. *La Photographie astronomique à l'Observatoire de Paris et la Carte du Ciel*. Paris: Gauthier-Villars, 1887.

Norman, Daniel. "The Development of Astrophotography." *Osiris* 5 (1938): 560-94.

O'Hora, Nathy P. "Astrographic Catalogues of British Observatories." *Mapping the Sky. Past Heritage and Future Directions. Proceedings of the 133rd Symposium of the International Astrophysical Union*. Ed. Suzanne Débarbat et al. Dordrecht, Boston, and London: Kluwer, 1988. 135-38.

Peirce, Charles S. "Grounds of Validity" [1869]. *Writings of Charles S. Peirce. A Chronological Edition*. Ed. Christian J.W. Kloesel et al. Bloomington: Indiana University Press, 1986. Vol. 2, 242-72.

Peirce, Charles S. MS 204 [Fall 1872]. "Chapter IV of Reality." *Writings of Charles S. Peirce. A Chronological Edition*. Ed. Christian J.W. Kloesel et al. Bloomington: Indiana University Press, 1986. Vol. 4, 54-59.

Peirce, Charles S. "Measurements of Gravity at Initial Stations in America and Europe." [1879]. *Writings of Charles S. Peirce. A Chronological Edition*. Ed. Christian J.W. Kloesel et al. Bloomington: Indiana University Press, 1986. Vol. 4, 79-144.

Peirce, Charles S. "Six Reasons for the Prosecution of Pendulum Experiments" [1882]. *Writings of Charles S. Peirce. A Chronological Edition*. Ed. Christian J.W. Kloesel et al. Bloomington: Indiana University Press, 1986. Vol. 4, 359-60.

Peirce, Charles S. "General Remarks upon Gravity Determinations, by John Herschel." *Writings of Charles S. Peirce. A Chronological Edition*. Ed. Christian J.W. Kloesel et al. Bloomington: Indiana University Press, 1986. Vol. 4, 365-68.

Peirce, Charles S. "On the Proper Functions of a National Office of Weights and Measures" [1899]. Unpublished MS., MSS. Charles S. Peirce, Houghton Library, Harvard University.

Peirce, Charles S. "Consequences of Four Incapacities" [1868]. *Writings of Charles S. Peirce. A Chronological Edition*. Ed. Christian J.W. Kloesel et al. Bloomington: Indiana University Press, 1986. Vol. 2, 239.

Radau, Rudolphe. "Sur les erreurs personnelles." *Moniteur Scientifique-Quesneville* (15 November 1865): 22.

Schaffer, Simon. "Late Victorian Metrology and Its Instrumentation. A Manufactory of Ohms." *Invisible Connections. Instruments, Institutions, and Science*. Ed. Robert Bud and Susan E. Cozzens. Washington: Spie Optical Engineering Press, 1992. 23-56.

Schaffer, Simon. "Metrology, Metrification and Victorian Values." *Victorian Science in Context*. Ed. Bernard Lightman. Chicago and London: University of Chicago Press, 1997. 438-74.

Scheiner, Julius. *Die Photographie der Gestirne*. Leipzig: Wilhelm Engelmann, 1897.

Schiebinger, Londa. *The Mind Has No Sex? Women in the Origins of Modern Science*. Cambridge and London: Harvard University Press, 1989.

Sellers, David. *The Transit of Venus. The Quest to Find the True Distance to the Sun*. Leeds: Maga Velda Press, 2001.

Shapin, Steven. "The House of Experiment in Seventeenth-Century England." *Isis* 79 (1988): 373-404.

Stagl, Justin. *A History of Curiosity. The Theory of Travel, 1550-1800*. Chur: Harwood, 1995.

Standage, Tom. *The Victorian Internet. The Remarkable Story of the Telegraph and the Nineteenth-Century's On-Line Pioneers*. New York: Walker, 1998.

Turner, Herbert Hall. *The Great Star Map*. New York: E.P. Dutton, 1912.

U.S. Transit of Venus Commission. *Instructions for Observing the Transit of Venus, December 6, 1882*. Washington, D.C.: Government Printing Office, 1882.

Verhandlungen der vom 20. bis 29. September 1875 in Paris vereinigten Permanenten Commission der Europäische Gradmessun. Berlin: Reimer, 1875.

Völter, Ulrich. *Geschichte und Bedeutung der internationalen Erdmessung*. Munich: Verlag der Bayerischen Akademie der Wissenschaften, 1963.

Wayman, Patrick A. "The Grubb Astrographic Telescopes." *Mapping the Sky. Past Heritage and Future Directions. Proceedings of the 133rd Symposium of the International Astrophysical Union*. Ed. Suzanne Débarbat et al. Dordrecht, Boston, and London: Kluwer, 1988. 139-42.

Weimer, Théo. "Naissance et développement de la Carte du Ciel." *Mapping the Sky. Past Heritage and Future Directions. Proceedings of the 133rd Symposium of the International Astrophysical Union*. Ed. Suzanne Débarbat et al. Dordrecht, Boston, and London: Kluwer, 1988. 29-32.

White, Graeme L. "The Carte du Ciel – The Australian Connection." *Mapping the Sky. Past Heritage and Future Directions. Proceedings of the 133rd Symposium of the International Astrophysical Union*. Ed. Suzanne Débarbat et al. Dordrecht, Boston, and London: Kluwer, 1988. 45-51.

Winterhalter, Albert G. *The International Astrophotographical Congress and A Visit to*

Certain European Observatories and other Institutions. Report to the Superintendent. Washington: Government Printing Office, 1889.

Wise, Norton and Crosbie Smith. *Energy for Empire. A Biographical Study of Lord Kelvin.* Cambridge: Cambridge University Press, 1989.

Wolf, Charles. "Recherches sur l'équation personnelle dans les observations de passage." *Annales de l'Observatoire de Paris. Mémoires* 8 (1866): 188.

Woolf, Harry. *The Transits of Venus. A Study in Eighteenth-Century Science.* Princeton: Princeton University Press, 1959.