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Panel: The Cosmopolitan Peirce: The Impact of his European Experience

Scientific Community and Cooperation in Peirce's European Letters

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1. Introduction: Charles S. Peirce, a scientist philosopher

My contribution aspires to describe —with some documental support from Peirce's correspondence of his first and second European trips— Peirce's conception of science as a collective and co-operative activity of all those whose lives are animated by the desire to find out the truth, whose lives are animated by "an impulse to penetrate into the reason of things" (*CP* 1.44, c.1896; *MS* 615, p. 14, 1908).

Although Peirce was a philosopher and a logician, he was first and foremost a real practitioner of science. Not only was he trained as a chemist at Harvard, but for thirty years (1861-91) he worked regularly and strenuously for the U. S. Coast Survey as a metrologist and as an observer in astronomy and geodesy. His reports to the Coast Survey are an outstanding testimony to his personal experience in the hard work of measuring and obtaining empirical evidence. A glance at his *Photometric Researches* produced in the years 1872-75 immediately confirms this impression of a man involved in solid scientific work (*W* 3, 382-493). I agree with Victor Lenzen —whose serious studies about Peirce's scientific work are nowadays almost completely forgotten— that "Peirce's scientific work is relevant to his philosophy, for his philosophical doctrines indicate the influence of his reflective thought upon the methods of science" (Lenzen 1964, 33), and with Ketner's judgment, "Peirce was not a dilettante in science, but a master scientist" (Ketner 2009, 42). To summarize this in Fisch's words, "Peirce was not merely a philosopher or a logician who had read up on science. He was a full-fledged professional scientist, who carried into all his work the concerns of the philosopher and logician" (Fisch 1993, *W* 3, xxviii-xxix).

Having done research in astronomy, mathematics, logic and philosophy and in the history of all these sciences, Peirce tried all his life to disclose the logic of scientific inquiry. Peirce insisted that the popular image of science as something finished and complete is totally opposed to what science really is, at least in its original practical intent. What constitutes science "is not so much correct conclusions, as it is a correct method. But the method of science is itself a scientific result. It did not spring out of the brain of a beginner: it was a historic attainment and a scientific achievement" (CP 6.428, 1893). Science is for Peirce "a living historic entity" (CP 1.44, c.1896), "a living and growing body of truth" (CP 6.428, 1893), and above all a *communicative mode of life*:

> I do not call the solitary studies of a single man a science. It is only when a group of men, more or less in intercommunication, are aiding and stimulating one another by their understanding of a particular group of studies as outsiders cannot understand them, that I call their life a science" (MS 1334, 12-13, 1905).

Probably there is nothing more alien to the present competitive style of science than the Peircean conception of scientists working together like *brethren*, but it seems to me that we can learn a lot from him on this issue. I will deal with that in two sections, first, on Peirce

as an inventor and builder of research instruments around which scientific communities are built, and, second, on Peirce's experience of cooperation in science.

2. Charles S. Peirce, a builder of instruments of observation

According to Peirce each community of scientists grows up around specific ways of perceiving, certain special methods of research, around particular instruments of observation. Each science corresponds to a special kind of observation, which distinguishes the mode of thought of the students of each special branch (*CP* 1.100, c.1896). The scientists are

> men who spend their lives in finding out similar kinds of truth about similar things understand what one another are about better than outsiders do. They are all familiar with words which others do not know the exact meaning of, they appreciate each other's difficulties and consult one another about them. They love the same sort of things. They consort together and consider one another as brethren. They are said to pursue the same *branch* of science (*HP* 804-5, 1904).

The main branches of research in which Peirce was deeply involved for years were astronomy, geodesy and metrology. I am not going to summarize now his accomplishments in those fields, since they are faithfully referred by Victor Lenzen in his papers and by Max Fisch in his superb: "Peirce as a Scientist, Mathematician, Historian, Logician, and Philosopher", and Carolin Eysele's: "Charles S. Peirce Nineteenth Century Man of Science", available all of them in the same page in our web [http://www.unav.es/gep/DescripcionSegundoViaje.html]. What I want to stress is Peirce's personal involvement in the making and improvement of instruments of measurement. This is particularly evident in the attention that is paid to the instrument builders in his European letters. In London [Browning, Casella], Hamburg [Repsold], and Paris [Brunner, Breguet, Gautier] were the best instrument makers of the time. In this sense, the interesting MS 1560a in which Peirce is suggesting the route through Europe for some colleague of the U. S. Coast Survey, besides hotels and food, places to visit and so, adds for instance, in relation to Switzerland:

> *Basel: Instrument makers here*
>
> *Berne: Instruments makers here. Hipp*

In his letter to Patterson on March 2, 1876 he writes about Paris:

> I have been greatly impressed with the instrument-making establishments here of every kind, and of the immense advantage Paris has over every other place on that account for the prosecution of all physical researches.

Peirce was put in charge of determinations of gravity for the Coast Survey on November 30, 1872. After conducting observations of relative gravity in 1873 at Hoosac mountains with an *invariable* pendulum, he ordered from the firm A. Repsold und Sohne of Hamburg an apparatus with a Bessel *reversible* pendulum (copy of that of the Prussian Geodetical Institute) for determining absolute values of gravity. The main goal of his second European trip was to receive this new pendulum in order to compare the European determinations with the American ones. In his report of May 31, 1875, after his first stay in Kew Observatory, near London, Peirce describes to Patterson with great detail the pendulums that were used there. I will only quote the conclusion: "Decidedly, I must invent some way of making

experiments on the friction of knife-edges, as the subject is very obscure." In fact, on the 27th of May Peirce arrived in Hamburg and went at once to Repsold's where they had his instrument all set up ready for his inspection: "I occupied three days in the examination of all its parts and today —Peirce writes in his report of May 31st— I went and accepted it and paid for it. [...] There are a number of features of it, I confess, which my judgment cannot altogether approve."

In the next monthly report Peirce writes:

> On the 1st day of June I left Hamburg and came to Berlin. I at once went to see His Excellency Herr General-Lieutenant Dr. Baeyer, the director of the Prussian Geodetical Institute. I found him a very courteous and interesting old gentleman with opinions of his own upon pendulums. He has come to the conclusion that he will not use the reversible pendulum and will endeavor to persuade his colleagues of the council of the European Gradmessung to give it up. He thinks invariable pendulums swung in vacuo are the thing; if one could only invent a vacuum apparatus.

The dissatisfaction of General Baeyer with the results obtained with the Prussian pendulum put in a difficult situation his whole project. The story follows with a turn since soon Peirce started to think that the flexure of the stand of the pendulum was affecting for the value of the determinations. This will be a central issue of Peirce's scientific career that he recalls in a lot of different places (*W*3, 217, *W*4, 83 and 516-17, HP, 608-09, *CP* 7.6-10, 1881; *W*6, 26-27). Although it is a bit long, it seems to me that it is worthwhile to quote Peirce's description of that event in the draft of the letter to J. H. Kehler of June 22, 1911 (L 231, NEM3: 207-09):

> I got leave to go abroad to study European methods of investigating gravity. While I was in Paris, there happened to be a conference of all the European Surveys. It was held in the *Palais des affaires étrangeres*; and I received an invitation to attend the meetings. At the first I attended, the subject of gravity was discussed; and I was taken completely by surprise when the president, Gen. Ibañez, called upon me for my opinion of the work they had been doing. Of course, I was obliged to express my real opinion. They thought they were measuring gravity with error not exceeding 1 or at most 2 millionths of itself. But the pendulum was swung from a brass tripod and I expressed the opinion very decidedly from an examination I had made of that tripod in Geneva that it swayed under the pendulum to an extent which though not directly observable, I had been able to get a notion of the amount of, by measuring how much the part where the pendulum rested would be moored by a horizontal pull of 1 kilo's weight. Whence I concluded that all the values of gravity which they had been publishing during the past ten years were too small by about 1/10000 of themselves, or a hundred times the error they thought they were excluding.

Peirce's view was initially accepted by the International Association of Geodesy in Paris, but later in a meeting in Brussels that Peirce was not able to attend, it was rejected. The issue was finally settled in the Stuttgart assembly of 1877. I copy from Peirce's remembrance more than thirty years later:

> I was landed at Plymouth and travelled right through night and day to Stuttgart where was the meeting. I got to the hotel in the evening during dinner. I knew there were 2 men who believed in me, —or rather 1 1/3—. The one was Gen. Baeyer the leader of European geodesy. The 1/3 was a fraction of Mr. Emile Plantamour, who had seen me at work in Geneva. I met Genl. Baeyer and his daughter in the corridor of the hotel as I was being shown to my room and the old General who had been fighting for me all day but really did not *know* much about the

> subject was so delighted to see me that he threw both arms round me and kissed me on both cheeks! The next morning I went into the meeting which was a particularly distinguished gathering, (...) I began with the mathematical theory (...) Then I described the instrument by which I had automatically registered the instants of the passage of the pendulum over the vertical, while it was swinging on the brass tripod and when it was on a properly stiff support. I had the chronograph sheets with me, and the whole demonstration was complete, and when I sat down each of my three antagonists at Brussels [Oppolzer, Plantamour and Cellérier] got up one after another and very handsomely admitted that I was entirely right. And from that time I was acknowledged as the head of that small branch or twig of science.

We may summarize this section showing the images of the pendulums and the stand built by Peirce during the eighties.

3. European Journeys: the community of science

The main goal of Peirce's first trip to Europe (June 1870 - March 1871) was to identify possible locations suitable for establishing observatories in order to study the total solar eclipse that was to take place at noon on December 22nd, 1870 over the Mediterranean Sea. Moreover, his father Benjamin Peirce wanted to introduce his son to several prominent European scientists (De Morgan, Jevons, Clifford, Lockyer, etc.). Peirce pointed out locations in Greece, Italy, and Spain, and contributed to the success of the scientific expedition under the command of his father. Eventually, he observed the eclipse, together with one of the American teams, from Catania, in Sicily. As Joseph Brent wrote, "this expedition was Charles's first experience of large-scale international scientific cooperation, and it illustrated for him the importance of the community of science in reevaluating and validating its hypotheses" (Brent 1993: 80; *W* 2: xxxiv).

I want to bring your attention to a text of his, almost forty years after the event, that we have chosen as a motto for the project we are developing right now on Peirce's European correspondence:

> Philosophy is a study which needs a very protracted concentrated study before one [...] begins to be at all expert in the handling of it, if one is to be precise, systematic, and scientific. I gave ten years to it before I ventured to offer half a dozen brief contributions of my own. Three years later [1870], when I had produced something more elaborated, I went abroad and in England, Germany, Italy, Spain, learned from their own mouths what certain students at once of science and of philosophy were turning in their minds. (C. S. Peirce, Letter to *The Sun*, *MS* 325, p. 4, c.1907).

Let's now turn to Peirce's second assignment to Europe (April 1875 - August 1876) — in which we have been working during the last three years— related with his extensive work with pendulums for gravimetric determinations in what were called the initial stations (Geneve, Paris, Berlin and London) to be compared with the determinations of the gravity in Hoboken, New York. I will pick up several instances of the correspondence that may illustrate well Peirce's sense of scientific activity as a cooperative work, as a communal mode of life.

In England, Peirce was able to meet in the Cavendish Laboratory in Cambridge with the great James Clerk Maxwell to discuss his projected experiments (letters of April 24 and

30, 1875). I copy two paragraphs of his letter of April, 30, 1875 to the Superintendent Carlile P. Patterson:

> I have had an interesting interview with Professor James Clark Maxwell who is a pendulum-swinger and a writer upon the mathematical theory of the resistance of the atmosphere and upon other subjects connected with Attraction. (...) I have still to see several distinguished gentlemen connected with pendulums, especially Professor Stokes who has investigated the resistance of the Atmosphere and who was largely consulted in regard to the methods of making pendulum experiments now used in the British work, General Sir Edward Sabine whom you know as a great swinger of pendulums, and Sir George Airy who swung at the top & bottom of a mine. (...)
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> *I feel the immense advantage of talking with all these people.* For example, in all I ever saw in relation to the effect of the resistance of the atmosphere on pendulums it has been assumed that the resistance was proportional to the density of the air while the temperature has been left out of account altogether, but from considering the matter in the light of the mechanical theory of heat I was led to believe that the largest term of the resistance was independent of the density and also of the surface of resistance and was proportional to the absolute temperature. I was happy to find that Professor Maxwell who is one of the greatest authorities on the viscosity of air, and the best experimenter upon it, entirely agreed with me in this view.

In London Peirce was able to meet other respected scientists. He had a very good relation with John Lockyer and William K. Clifford, who had been both with Peirce in the observation of the eclipse in Sicily in 1870; Herbert Spencer, who introduced him in the Athenaeum; William Spottiswoode, of the Royal Society; the mathematician James Joseph Sylvester (letter of April 2 and 4, 1875); the physicist George Gabriel Stokes, expert on the problem of the friction caused in the pendulums by the viscosity of the air (letter of May 31, 1875). Peirce's attitude was one of learning from everybody with experience on the subject of his research, crediting each one of them with the intellectual or technical debts, without a particular leaning towards priority or originality.

In Berlin he had a close and friendly relation, as we have seen from the kisses, with General Baeyer. Peirce writes in his Report of 1878 that there he "enjoyed the inestimable advantage of the counsel of the Nestor of geodesy, General Baeyer, and also of the great interest in the experiments and the attention to everything which could affect the success of them on the part of Professor Förster" (W4: 83), the head of the Berlin *Sternwarte*.

In Geneve the cooperation with Emile Plantamour —"whose advice in regard to the conduct of the experiments was invaluable" Peirce writes in the same Report of 1878 (W4: 82)— was essential to develop the experiments that detected the until then unnoticed flexure of the stand that General Baeyer had suspected. In Paris it was not easy the relation with Le Verrier, but Peirce got along very well with Yvon Villarceau and Charles Wolf, "astronomer at the observatory, to whose politeness throughout the occupation of the station the writer is much indebted" (W4: 82). Back in London, "the director of the observatory [Kew], Mr. Whipple, thoroughly understands the art of oscillating the pendulum, and was most obliging in furthering the investigation in many ways" (W4: 83).

The references to scientists and quotations could be multiplied. My main point is that the study of Peirce's correspondence —in particular his monthly reports to Superintendent Patterson— of his second and longest trip provides ample evidence of the high quality of Peirce's scientific work, his personal involvement in the improvement of the instruments of

observation and also of his convinced defense of the "brotherhood of Science". As he writes years later (*MS* 1343, pp. 6-7, 1902):

> Science is to mean for us a mode of life whose single animating purpose is to find out the real truth, which pursues this purpose by a well-considered method, founded on thorough acquaintance with such scientific results already ascertained by others as may be available, and which seeks cooperation in the hope that the truth may be found, if not by any of the actual inquirers, yet ultimately by those who come after them and who shall make use of their results (*CP* 7.55, 1902).

The study of Peirce's European correspondence is a wonderful testimony that his image as an isolated researcher is at least historically inaccurate. In his work there was a permanent cooperation with the scientists of his branch and a constant attitude of learning from his colleagues.

4. Conclusion

As I said before, probably there is nothing more alien to the present competitive individualistic style of science than Peircean conception of scientists working together like *brethren*, but it seems to me that in order to invigorate philosophy for the 21st century is our task to try to teach this *mode of life* through the defense of communication and cooperation between scientists in a Peircean spirit of agapastic reasonableness.

Thanks a lot for your attention.

Bibliographic references

References to Peirce's texts are given with the following abbreviations followed by the volume number, the paragraph number and the year of the text:

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HP Eisele, Carolyn (ed.). *Historical Perspectives on Peirce's Logic of Science: A History of Science,* vols. 1-2. Berlin: Mouton 1985.

MS *The Charles S. Peirce Papers* 32 microfilm reels of the manuscripts kept in the Houghton Library. Cambridge: Harvard University Library, Photographic Service 1966.

W Peirce, Charles S. *Writings of Charles S. Peirce: A Chronological Edition*, vols. 1-8. Eds. M. H. Fisch et al. Bloomington: Indiana University Press 1982-.

The Charles S. Peirce letters are quoted in the text by date. All of them are available at [<http://www.unav.es/gep/PrimerViaje.html> and <http://www.unav.es/gep/SegundoViaje.html>]

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