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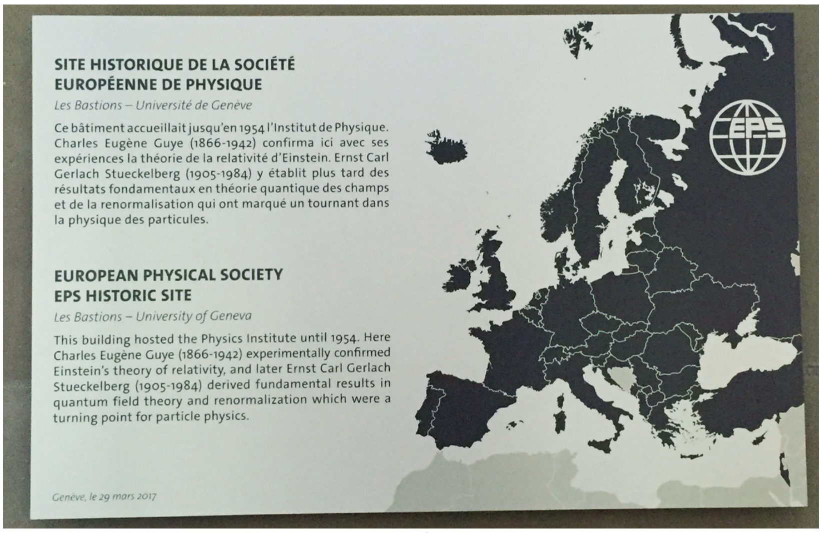
History of Physics (18)

An EPS Historic Sites Award celebrating two Genevan physicists

Jan Lacki, Uni Genève



The Plaque at the new EPS Historic site "Bastions".

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An EPS Historic Sites Award celebrating two Genevan physicists

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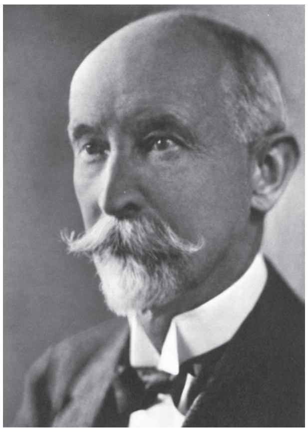
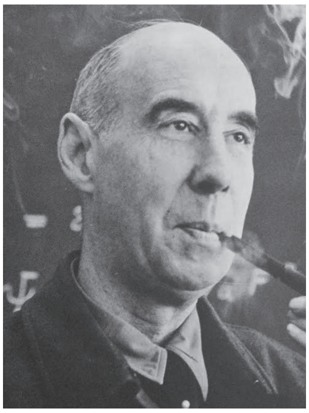
"*The EPS Historic Sites Award commemorates places in Europe important for the development and the history of physics*" [1]. This initiative is a genuine success: until today (May 2017) 33 places have been distinguished and so many commemorative plaques affixed. On March 29, 2017, the *Bastions* building of the Geneva University hosted the unveiling of its Historic Sites plate celebrating the activity of two distinguished Genevan professors, Ch.-E. Guye and E. C. G. Stueckelberg. The ceremony was a success: University officials, SPS and EPS boards members, together with some noteworthy guests (most of them Stueckelberg's former students, collaborators or just admirers), happily enjoyed the speeches and admired the plate. Before I describe the respective achievements of the two Genevan scholars and how they shaped local physics, let me briefly comment on the *Bastions* building itself. Indeed, it is intimately related to the history of the Geneva University and, since its erection till the middle of 20th century, of Geneva physics. From present-day perspective this latter fact may come as a surprise, as the *Bastions* building is today known for hosting mostly the activities of the Humanities and hence is definitely not charged with scientific connotations. But the investigations which made Genevan academic physics enter resolutely in the era of relativity and quantum revolutions all happened in the *Bastions* premises.



Charles-Eugene Guye



E. C. G. Stueckelberg

To start with, let us recall that the University of Geneva, in the form we know today, is a rather late creation. Its origin is Calvin's Academy founded in 1559 by the protestant leader as an institution aiming at educating future city's elite, predicators, lawyers and civil officers. The introduction of science teaching came much later and went stabilized only during the 18th century. In spite of some efforts, throughout its history, to expand its scope, one had to wait until 1872 and a substantial revision of Geneva's higher education system to see it finally granted a full university status [2]. For centuries, Calvin Academy did not have at its disposal any own premises and one had to wait until the 1870's to see inaugurated the very first (and for some time almost unique) building specifically dedicated to academic activities. What is called today the *Bastions* building, together with its adjacent wings, was initiated in 1868 in the perspective of the transformation of the Academy into a University, and was achieved four years later. It was aimed at hosting most of the academic activities, and in particular physics. To the central building were added two wings, one dedicated to host the scientific and naturalistic collections of the City, to constitute what was to be called the *Musée académique*, and the other designed to host the Library. The physics collections, the *Cabinet de Physique* as it was then called (mostly the collections of scientific instruments donated to or acquired by the academic authorities), were instead moved to the basement of the central building. Its first floor hosted the physics laboratories. These premises proved soon too exiguous and when chemistry moved out, in 1880, for a building of its own, the laboratory of experimental physics took possession of the vacant space in the basement.

How was Genevan physics at the time? As much physics was done outside than inside the university. The end of the 19th century witnesses indeed the very last decades when front-rank research is, in substantial proportion, still done outside the academic institutions. Let me remind that our contemporary characterization of universities as institutions which take care of front-rank research and simultaneously dispense most up-to-date education is the outcome of a transition which took place over decades of the 19th century. Until then, universities were not really promoters and/or places of genuine scientific progress. The later was achieved elsewhere and owed substantially to the activities of individuals who were acting on an independent basis and/or were supported to various extents by scientific societies (such as the *Royal Society* in London, the Paris *Academie des Sciences*, or the *Königlich-Preussische Akademie der Wissenschaften* in Berlin, to name a few best known). These societies remained for long the main promoters of research. Universities, on the other hand, until roughly the mid 19th century, were rather dedicated to teaching and even then, they were usually not dispensing the most up to date knowledge. When university professors were involved in research, they were doing it mostly because of personal interests, rather than as a regular part of their academic duties. In the last decades of 19th century, the case of Geneva is more or less illustrative. In 1848, Elie-François Wartmann obtained the chair of experimental physics after Auguste de la Rive, a specialist of electricity, resigned his position for political reasons. Wartmann took care of physics teaching until 1886 and investigated optical phenomena, vision physiology and electricity. Then father Louis Soret (1827-1890) and son Charles Soret (1854-1904) took over. Louis, from 1866 on, was in charge of a complementary teaching in physics before he was promoted full professor in medical physics in 1876. He was interested in various physical topics, none

1 Quote from the EPS Historic Sites Award web page, http://www.eps.org/?page=distinction_sites as of June 1st, 2017.

2 For details, as well as for other historical facts concerning Genevan physics, its sites and its actors, see J. Lacki, The Physical Tourist. Geneva: From the Science of the Enlightenment to CERN, *Physics in Perspective*, vol. 9 (2007), p. 231.

of them really innovative. His son Charles, lecturer at the department of crystallography and mineralogy in 1879, then full professor in 1881, is better remembered for his investigations of thermodiffusion (the so-called *Soret effect*, with a law named after him). An interesting and symptomatic case is Raoul Pictet (1846 - 1929), well known for his research in low temperature physics and in particular for his liquefaction of Oxygen (1877, obtained independently by the French Cailletet). It is only after this achievement that Raoul Pictet was proposed a chair in "industrial physics" (1878) created especially for him. Unfortunately, he did not hesitate, following an entrepreneurial disagreement over the commercial use of his discovery, to resign and to leave Geneva without any negative impact on his research, fully independent of his academic position. Equally, maybe even more innovative, and closer to fundamental issues was the activity of Lucien de la Rive, the son of Auguste. Soon after Hertz discovery of electromagnetic waves, Lucien de la Rive, assisted by his friend Edouard Sarasin, produced and observed the latter in the facilities of the Genevan Société pour la construction des instruments de physique, known later as *Société des instruments de physique* (SIP) [3]. De la Rive's and Sarasin's experiments, which occasionally refined and completed Hertz's own, extended over the period 1889 - 1893 [4]. Later on, Lucien published studies in gravitation, electron theory, Maxwell equations and relativity of which he was an early promoter in spite of his already considerable age. He did it without any genuine academic support: in spite of his front rank scientific activity, Lucien was never appointed by Geneva university. The latter, however, eventually granted him a doctorate *honoris causa* in 1909, the same year it honored the young Albert Einstein.

The mention of Einstein's links with Geneva University brings me now to Charles-Eugène Guye (1866 - 1942). Using this brief survey of Genevan physics at the turn of the century as background, I intend to show that Guye can be considered as the man who brought a systematic tradition of front rank research to Geneva University. He made the Physics Institute the center of ambitious experimental and, to a lesser extent, theoretical investigations and, from this point of view, he is the genuine founder of Genevan modern academic physics. Now, I already had the opportunity to tell the story of Charles - Eugène Guye's famous relativity experiments in the *SPG Mitteilungen* some years ago [5], so let me just remind the essential facts. Guye obtained his Ph. D. in Geneva under the guidance of Charles Soret with a study of the refraction properties of quartz. He moved then to Zürich where he started his academic career, ending up as lecturer at the Federal Institute of Technology. He specialized in various problems related to electrical technology, studying in particular problems of electric line transmission.

When the University of Geneva called his former pupil to the chair of experimental physics, he was not its first choice. After the resignation of Charles Soret in 1900, the vacant chair needed a successor and after laborious discussions with the renowned Pierre Curie from Paris, the latter agreed to come to Geneva, where he was offered very advantageous conditions. However, at the very last moment, Curie changed his mind, for reasons which remain today still unclear [6], causing much disappointment and even some resentment.

Be it as it may, the choice of Guye proved a clever one. Aside from studies on magnetic hysteresis, electrical arcs, internal friction of matter and gas discharges at high pressure, Guye moved resolutely into the frontier domain of electrodynamics, specifically into the field of research related then to the revolutionary hypothesis of an electromagnetic foundation of physics. Between 1907 and 1915, Guye performed, with the help of his students, a series of experiments aimed at studying the effects of variation of mass with velocity. The experiments were located in the *Bastions* building (probably in the basement). Guye's motivation was to compare two rival theories, that of Max Abraham, and the one of Lorentz and Einstein. Taking part in an ongoing controversy over the pioneering experiments of Walter Kaufmann who concluded to the validity of Abraham's theory of the rigid electron, Guye vindicated in the most precise way the relativistic theory of Lorentz and Einstein. Thanks to this success, Guye achieved a considerable fame, sitting in numerous commissions and committees [7]. Before relativity theory became widely accepted and Einstein its undisputed hero, Guye was among its foremost promoters. Indeed, Guye early recognized Einstein's merits. In 1909, when the University celebrated its 350th anniversary, and the City the fourth centennial of Calvin's birth, he managed to nominate Einstein among the numerous scientists who were to be honored as special doctors *honoris causa* of the celebration.

During Guye's times, the faculty of sciences kept growing. The teaching of physics gained a new chair of mathematical physics in 1930 which was offered to Guye's protégé, Arthur Schidlof (1877 - 1934). The latter is remembered for his early atomic model before Bohr's 1913 breakthrough [8], but it is his successor, E. C. G. Stueckelberg von Breidenbach (1905 - 1984) who definitely initiated a tradition of most advanced theoretical physics in Geneva. He was initially appointed temporarily to take over Schidlof's courses left orphan following the latter's untimely death (1934) but just a year later he was promoted full professor. His office which saw much of his best science was in the *Bastions* building until Geneva physics moved eventually to a dedicated building two decades later [9].

Definitely one of the most illustrious Swiss physicists of the 20th century, on par with the best minds of his time, Stueck-

3 Founded in 1862, the SIP became in the following decades a world-renowned constructor of scientific instrumentation, measuring devices and also various machines (in particular its famous "machine à pointer").

4 It is an interesting story for its own sake; for the details see the captivating account by D. Pestre and M. Atten, *Heinrich Hertz. L'Administration de la prevue*, PUF, 2002.

5 *SPG Mitteilungen* Nr. 25, pp. 14-17.

6 As it appears, Pierre Curie did not receive a really convincing counter-offer from Paris; rather, it could be that the cosmopolitism of Paris with respect to a more provincial Geneva had a part in his brisk change of mind. Some biographers point to the determinant role of Pierre's wife, Marie Curie (see Françoise Giroud's biographic study of Marie Curie, *Une femme honorable*, Fayard, 1981, pp. 127 - 128).

7 Today, people remember Guye probably best from the famous picture of the participants to the fifth Solvay congress in 1927. Guye served on the scientific committee of the Solvay Institute three times in a row, from 1927 to 1933, but this is the only time he appeared on the commemorative photography, see my paper on Guye in the *SPG Mitteilungen*, Nr. 25, pp. 14 - 17.

8 See my article on Schidlof in the *SPG Mitteilungen*, Nr. 34, pp. 48 - 51.

9 In 1953, the physics laboratories moved from the old building of the *Bastions* into a new dedicated edifice.

elberg was an eccentric character. Beset by an recurrent mental disorder, he managed nevertheless to achieve in the 1930's - 1950's some most important discoveries in the pioneering field of theoretical high-energy physics, among other in his study of relativistic quantum field theory (covariant perturbation theory), of unitary theories of interactions (the mesic theory of strong interactions, the Stueckelberg's B-field), and of the causal S-matrix theory (the causal propagator and the renormalization group). In 1949, during an episode of his illness, he unexpectedly resigned from his position and it took a decade before he was restored to his professorial status [10].

In his paper published in the *SPG Mitteilungen* [11], Gerard Wanders covered Stueckelberg's initial steps in science, so I pass directly to discussing some of his most remarkable scientific achievements. During the years (1927 - 1932) that he spent in Princeton after obtaining his Ph.D. in Basel, Stueckelberg was mainly busy with molecular physics. He considered problems of molecular collisions, of interpretation of continuum emission and absorption spectra and of calculations of the energy levels of molecules. His work, together with Winans, on the continuous discharge spectrum of the hydrogen molecule, and then with Morse on the ionized Hydrogen molecule were genuine achievements but it is his contribution to the theory of collisions that best characterizes Stueckelberg's creativity in this period. Indeed, in his study of the inelastic atomic collisions, Stueckelberg introduced a formalism still commonly used and known today as the Landau-Zener-Stueckelberg (LZS) theory. It is fundamental in collision situations where there are non-adiabatic state changes associated to the existence of an "avoided crossing" at the level of the potential energy curves [12]. While Lev Landau and Clarence Zener gave first versions of the LZS mechanisms at the turn of 1931, Stueckelberg provided, almost a year later, a much more sophisticated analysis [13].

Just before he was appointed in Geneva, and while still privatdozent at Zürich University (1933), Stueckelberg turned to a new field of interest, quantum electrodynamics and more broadly the theory of elementary particles, at the time a most prominent and rapidly developing topic. It did not take him long to make a remarkable contribution. One of the problems at the time was the lack of a formalism that would enable to do relativistic quantum calculations in an explicitly covariant way. Indeed, in the conventional scheme for time-dependent perturbation theory, because of the distinguished role played by time, the perturbative terms contain denominators involving differences in energy, and so *explicit* covariance is lost. In his approach, Stueckelberg obtained the perturbative corrections, in Pauli's words: "by eliminating time and space [variables] completely from the theory and examining directly the coefficients of the *four*-dimensional Fourier expansion of the wave function" [14] (the method is also characterized by a smart use of complex contour integration). This was, in 1934, the first fully and manifestly covariant perturbative formalism in quantum electrodynamics [15] and it could be used to investigate as well other interactions. Stueckelberg used it successfully over the next years to study Compton scattering, Bremsstrahlung, and most importantly, creation and annihilation of particle-antiparticle pairs [16]. It should be noted that in 1941, Stueckelberg studied the problem of pair creation and annihilation in three papers where he stated the revolutionary interpretation of positrons as electrons travelling back in time. Much later (1949) Feynman came up with the same idea (apparently suggested to him by Wheeler); after initially mentioning Stueckelberg's prior work, Feynman gave up doing so later. This certainly contributed to the fact that Stueckelberg's pioneering works on (classical and) quantum electrodynamics remained rather unknown to the later generations [17].

Stueckelberg could also apply his perturbation theory to the study of beta-decay and of nuclear forces, two research fields which, in the years 1930 - 1935, witnessed an impressive progress. The postulation of the neutrino by Pauli (1930) and the discovery of the neutron by Chadwick (1932) opened new avenues for solving old problems (violation of energy conservation in beta-decay, the structure of the nucleus) and suggested the first unified models of electromagnetic, weak and nuclear processes. Inspired by Fermi's theory of beta decay, by its application to nuclear interactions viewed as an exchange of electron-neutrino pairs between nucleons, and by de Broglie's idea of a photon as a compound particle made of two neutrinos, Stueckelberg proposed in 1936 an ambitious theory of all interactions based exclusively on spinor fields. Because of some fatal problems, he modified his first attempt introducing, in 1937, instead of spinor current couplings, a (non-spinor) field-mediated coupling. The latter, in the case of nuclear forces, involved a charged scalar field that Stueckelberg readily identified with the "heavy electron" found in cosmic rays a year earlier. Doing so, Stueckelberg hit independently on an idea that the Japanese Yukawa had already proposed in 1934 to explain nuclear force as an exchange of a new particle, roughly 200 times heavier than the electron, the meson. Hence, one can consider Yukawa and Stueckelberg as independent proponents of a meson theory of nuclear forces [18]. Yukawa's clear priority and his analysis closer than Stueckelberg's to concrete phenomenology may explain why Stueckelberg is usually not considered as a co-discoverer. As is well known, the new cosmic rays particle turned

10 For an account of this troubled decade, see my article entitled "1946-1960: Une période difficile pour la physique genevoise", in *SPG Mitteilungen*, Nr. 43, pp. 50 - 54, also Nr. 44, pp. 44 - 48.

11 *SPG Mitteilungen* Nr. 32, p. 20, see also chapter 1 in *E.C.G. Stueckelberg; an unconventional figure of twentieth century physics*, J. Lacki, H. Ruegg and G. Wanders (eds), Birkhäuser, 2009 (further quoted as LRW).

12 For details, see my article on Stueckelberg and molecular physics in LRW , chapter 3, p. 13 - 23.

13 See also the interesting historical study of the non-adiabatic transitions by Nikitin and Di Giacomo, "The Majorana formula and the Landau-Zener-Stueckelberg treatment of the avoided crossing problem", Physics *Uspekhi*, vol. 48 (2005) p. 515 - 517.

14 Quoted from a letter to Heisenberg dated 5 February 1937.

15 See my contribution in LRW, chapter 4, p. 25 - 51 and references therein.

16 Ibid, see also Olivier Darrigol's analysis in chapter 5 of LRW, p. 54 - 72.

17 See the final analysis in Lacki, Ruegg and Telegdi, The road to Stueckelberg's covariant perturbation theory as illustrated by successive treatments of Compton scattering, *Studies in History and Philosophy of Modern Physics*, vol. 30 (1999), pp. 457 - 518, also the contribution of Ruegg and Ruiz-Altaba in LRW, chapter 6, pp. 73 - 87.

18 For details, see Olivier Darrigol's contribution to LRW, op. cit.

not to be a meson but instead a muon. Nevertheless, Yukawa obtained in 1949 the Nobel prize for his theory of meson-exchange forces. Because of the reasons above, any talk of Stueckelberg missing the Nobel prize [19] is idle, but this episode clearly illustrates that he was on par with the best.

Stueckelberg's originality and priority is instead undisputable in his work on S-matrix theory. Taking up in 1944 Heisenberg's original idea of an S-matrix (1943), Stueckelberg produced many papers alone or with students until the final version of his formalism in 1950. The theory was meant to present an alternative to the theory of interacting quantized fields which was then plagued with difficulties, most notably divergences no one knew how to get rid of. Heisenberg proposed the S-matrix approach as a theory based on general physical principles and a formalism directly based on transition probabilities in collisions as these are the only observables in (scattering) experiments [20]. The perturbative versions of this program were finally obtained in the 50's. Stueckelberg, who caught on quickly on Heisenberg's proposal, worked out his own original approach where he early recognized the key idea that causality is crucial to the construction of the S-matrix: the features of the space-time evolution in collision processes must be taken into account so that relying only on observable quantities is not sufficient. He also anticipated again some important aspects of Feynman's well-known formalism (1949) for quantum electrodynamics, most notably the causal propagator [21] which was a logical follow-up of the idea of positrons being electrons moving back in time, obtained as we saw in 1941, before the emergence of S-matrix program.

The S-matrix approach had, in Stueckelberg's and students' hands, an unexpected but very important spin-off. As it turns out, the perturbative terms of the S-matrix are not exempt of divergences (contrary to Heisenberg's hopes) but Stueckelberg and Rivier were able to eliminate the latter (1949). This was however at the price of introducing arbitrary quantities appearing because of the necessity to give mathematical meaning to notoriously singular products of propagators such as typically the square of the causal propagator. After the proper handling of the square (mathematically the square of a distribution, hence not uniquely defined), it turns out that it is defined only up to an additive term proportional to a delta function, with the proportionality constant an arbitrary real coefficient. This arbitrariness transpires up to the pertubative terms of the S-matrix. Stueckelberg, with Green, and then with Petermann were eventually able to show that the elimination of the divergences was possible (reviving thus Heisenberg's expectations), at the price of the perturbative terms being defined up to finite linear combinations of delta-functions and their derivatives. Stueckelberg and Petermann, using normalization prescriptions, were in turn able to fix the constants. These prescriptions are not unique, but the final result should not depend on their choice. Studying the dependence of the constants on the prescriptions, Stueckelberg and Petermann eventually hit on the renormalization group which is today widely applied in many fields much beyond the original context of its discovery [22].

This already impressive list of achievements of Stueckelberg does not stop here. Because of lack of space, I shall not comment on the very clever mechanism that Stueckelberg imagined to circumvent the difficult problem of the quantization of massive vector fields: the introduction of an auxiliary scalar field, the so-called B-field, enables to obtain a massive gauge field while preserving gauge invariance [23]. I shall also not comment on Stueckelberg's interest in (relativistic) thermodynamics that kept him busy in the final years of his activity [24].

Let's reach a conclusion. Looking back at the span of years from Guye's appointment to Stueckelberg's odd resignation, one can grasp how much Geneva physics owes to both men whose activity amply justifies the EPS Historic Sites Award. As much a brilliant experimentalist as an able science manager, Guye firmly anchored Genevan physics in modernity, making of the Physics Institute the prime place for physical research. His successor, the experimentalist Jean Weigle could lean on this tradition to push it further and open new fields, most notably by initiating Genevan biophysics. On the other hand, Stueckelberg and his theoretical activity firmly legitimated and established a tradition of advanced theoretical physics (he did the same in Lausanne). It is true, Genevan physics met a difficult decade after Weigle's leave for the USA (1948) and Stueckelberg's resignation in 1949 [25]. But when the theoretician Josef Maria Jauch (1914 - 1974) was finally appointed in 1958 as Stueckelberg's successor and became effectively the boss of all of Geneva's physics, he revived a lost momentum and pushed things further [26]. As much as Stueckelberg was not really interested in administration and science management, Jauch did excel in these matters while simultaneously keeping a very high standard of research [27]. Until recently, much of Geneva physics and its organization still reflected the touch Jauch gave it from the 60's on. So, one can say that the plaque inaugurated last March in Geneva not only honors outstanding scientific achievements which had an impact at international level. From a more local perspective, it pays a tribute to the founders of Genevan 20th century physics.

19 This is often to be found in the folklore surrounding Stueckelberg's science.

20 Heisenberg's S-matrix philosophy was thus again based on the methodological requirement to stick only to observable quantities, already erected as guiding principle in Heisenberg's formulation of matrix mechanics, see for instance Max Jammer's *The conceptual development of quantum mechanics*, 1966.

21 It is used in an unpublished manuscript from 1947 and is explicitly mentioned in 1948 in a *Letter to Physical Review*, details, see Gerard Wanders contribution to LRW, chapter 7, pp. 88 - 91.

22 Ibid, pp. 93 - 99.

23 For an extensive discussion, see H. Ruegg and M. Ruiz-Altaba, The Stueckelberg Field, *Int. J. Mod. Phys A*, vol. 19 (2004), pp. 3265 - 3347, also chapter 6 of LRW, *op. cit.*

24 See the account of Werner Israel in chapter 8 of LRW, pp. 101 - 113.

25 See my account in the SPG Mitteilungen, Nr. 43, pp. 50 - 54, also Nr. 44, pp. 44 - 48.

26 The prestige of Stueckelberg's chair might have played a role in Jauch's acceptance to leave his position in Iowa but the then newly founded CERN must have been an even more powerful source of attraction.

27 He is best remembered as making Geneva a famous place for studies in the foundations of quantum theory.