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# My Swiss Visits of 1906, 1926, and 1930

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Switzerland is one of my favorite countries and I have visited it some twenty times. Perhaps some of my affection for it comes from first having been there at the age of seven, when my father was on sabbatical. I can say that I began my education in Paris and continued it in Göttingen, which sounds very impressive; I was placed in kindergarten at both places, but I cannot claim any corresponding Swiss education. Instead the occasion of my Swiss sojourn in 1906 was a different one. Southern Europe was not as hygienic as now. In early 1906 mother and I were recuperating from what I presume would now be diagnosed as respectively typhoid and diphtheria, or something closely akin thereto. In those days medical therapy for recuperation was strangely correlated with altitude, and the doctor in Florence prescribed a moderately high altitude for one of us, and a low one for the other. Father struck a compromise by selecting a small hotel at Trois Arbres, about $^3/_4$ of the way up the Salève, a mountain near Geneva. We had previously visited Geneva earlier, and no doubt father had taken a liking to the region.

I select 1926 as a particular year for describing a visit because it was the time of the quantum-mechanical revolution, and so I can do some reminiscing about that historic period. I was then an assistant professor at the University of Minnesota. During the winter of 1925–1926, information about the new matrix mechanics reached me in fragmentary form, mainly through the Zeitschrift für Physik, as it was long before the days of Xerox and the preprint mania. I did go to Madison, Wisconsin, to hear some lectures by Max Born. I eagerly waited to see if some one would show that the hydrogen atom would come out with the same energy levels as in Bohr's original theory, for otherwise the new theory would be a delusion. Finally Pauli's paper appeared which dispelled my worries.

When I left Minnesota in 1926 for a summer in Europe I was oblivious of the existence of wave in distinction from matrix mechanics. Schrödinger's first paper where he actually deduced the energy levels of the hydrogen atom from a partial differential equation may possibly have reached Minneapolis before my departure, but if so I might have dismissed it as a fluke, for I was so imbued with the correspondence-principle-matrix approach that anything else seemed on the wrong track. Certainly the later paper (or Eckart's corresponding one) in which he demonstrated the identity of the wave and matrix versions of quantum mechanics could not have reached Minneapolis before my departure in June, as it was submitted to the editor in late March and the Annalen der Physik was notoriously slow in publication. In fact I remember Schrödinger remarking to me a few years later that he liked to publish in the Annalen because one could keep the proofs a long time and decide if the contents were really right. One sometimes wishes that modern writers were equally self-critical, and less avid to get in print rapidly. However, the delays made it difficult for any one in America to keep abreast of developments.

Just before sailing for Europe in 1926 I did see an early paper by Dirac [1] in the Proceedings of the Royal Society. It looked important and so I took with me either a reprint or notes I made from the library copy (I forget which). While on the ship I realized that the 'q-number' technique of this article (essentially the quantum-mechanical version of angle and action vaiiables) furnished a method of the calculating the mean values of $1/r^2$ and $1/r^3$ needed for determining the relativity and spin corrections to the energy levels of the hydrogen atom, which had been calculated without these corrections by Pauli. I was delighted to find that, as surmised by Uhlenbeck and Goudsmit and by Slater, the combined relativity and spin corrections made the energy levels come out identical (at least to the first approximation in $1/c^2$) with those which Sommerfeld calculated relativistically in the old quantum theory without spin. On my portable typewriter I typed up what would be a paper for publication and went with it to Bohr's Institute in Copenhagen only to be told by him that the same calculation had just been published by Heisenberg and Jordan [2]. (Their method was slightly different from mine in that it used ordinary matrices rather than Dirac's q-numbers.) So a day or two later I again appeared at Bohr's laboratory with a calculation of $\langle 1/r^4\rangle_{AV}$ which was needed to determine the quantum defect $\Delta$ in the Rydberg formula $-$ $chRZ^2/(n-\Delta)^2$ arising from polarization of inner shells by a valence electron that does not penetrate them. This time I was told that the calculation of $\langle 1/r^4\rangle_{AV}$ had already been made by Waller with the (to me) mysterious Schrödinger wave mechanics and was in process of publication [3]. A few years later I mentioned to Heisenberg that Dirac's q-numbers could be used to calculate the mean values of negative powers of $r$ (except for $1/r$), and he remarked to me that I should publish this method as the computation of high negative powers of $r$ was quite difficult by wave mechanics. So in 1933 Dirac, by then a Fellow of the Royal Society, communicated to its Proceedings a short article [4] in which I finally used his techniques to calculate $\langle 1/r^5\rangle_{AV}$ and $\langle 1/r^6\rangle_{AV}$, as well of course the lower powers of $1/r$.

While in Copenhagen in 1962 I had another disappointment. I received a letter from the editors of Nature saying that I must shorten my paper on the calculation of the dielectric constant of a diatomic polar molecule. The resulting delay meant that my article appeared about a fortnight after one with a similar calculation by Mensing and Pauli, and practically simultaneously with corresponding ones by Kronig and by Manneback [5]. The delay of a month or so in publication time seems trivial to me now, but was rather distressing to me as a young man. My original version was, I still believe, not excessively long in view of the fact that the restitution of the factor $^1/_3$ in the Debye formula $(\varepsilon-1)/4\pi = (^1/_3)\,N\mu^2/kT$, as compared with the chaotic coefficients in the old quantum theory, was a signal advance coming out of quantum mechanics. When I mentioned all this to Bohr he said 'you should have had me endorse the original version – then it would probably have gone through alright'. Apparently it is a tradition that the editors of Nature are rather wary of publications by comparitively unknown authors. At least I was amused to read in the recent book 'Double-Helix' by James Watson that when he and his collaborators made their epoch-making discovery of the structure of the DNA molecule they were careful to have the manuscript which they submitted to Nature endorsed by Sir Lawrence Bragg to insure prompt publication.

From Copenhagen I went to the meeting of the British Association for Advancement of Science in Oxford. I met Hartree and learned how he was trying to transcribe into quantum mechanics the self-consistent-field procedure he had used in the old quantum theory. I particularly remember hearing a paper by Rutherford at which a youngish and rather bored-looking man chaired the session. When the latter left the platform every one stood up. Later I learned he was Edward VIII, then Prince of Wales and President of the Association.

After Oxford I spent about ten days in Cambridge. I called on Dirac at his rooms in St. John's College and he explained how he was writing on antisymmetric wave functions. This sounded completely mysterious to me. Even more so was a remark that a young Harvard graduate, J. R. Oppenheimer, made to me when he took me punting on the Cam. He said that a book on partial differential equations by Courant and Hilbert was helpful for quantum mechanics. Not till I returned to Minnesota did I have access to the literature that showed how matrix elements could be computed by knowing the solutions of the Schrödinger equation. One question which Dirac asked me was most welcome, as I have always felt that it was more important to have manuscripts clearly marked for the printer, than impeccably typed ones immaculate in appearance. He asked 'do you often use scissors and paste in writing a paper'?

How does Switzerland enter in my 1926 trip? The answer is that in some American universities sabbaticals are defined as for 'research and recuperation', and I presume the same applies to summer vacations. After the scientifically strenuous and rather trying sojourns in Denmark and England I went to Switzerland to recuperate, and make some new trips such as walking over the Joch Pass. Science was involved only in a 'border incident'. On leaving Switzerland at Delle my eye somehow fell on the passport which was being inspected of someone sitting next to me in the compartment. The name on it was 'Linus Pauling', and his wife Ava Helen thought I looked most impertinent to stare at it so hard. Linus and I had previously only corresponded, but on the train a friendship started of over forty years standing. I fear, however, that the trip to Paris was rather boring to Ava Helen while Linus and I talked of the new developments in theoretical physics.

My Swiss visit of 1930, was my longest one (except perhaps that of 1906) and, unlike the others was entirely for 'research' rather than 'recuperation'. I was on a Guggenheim Fellowship and arranged to spend six weeks in Zurich, because of its galaxy of outstanding physicists, while my parents took my wife Abigail on a leisurely tour of the artistic sights of Italy, leaving me for uninterrupted work and 'talking physics' in Zurich. However, when I reached there in late March I discovered that it was the beginning of spring vacation and all the physics faculty were away. The janitor at the ETH, fortunately, was very friendly and arranged for me to have the use of the library. I lived comfortably at the Hotel Waldhaus Dolder, and with a portable typewriter and no distractions by colloquia, social life or sight-seeing, I probably wrote more pages of my 'Theory of Electric and Magnetic Susceptibilities' in my first month at Zurich than in any other comparable time interval. I talked to practically no one except a bright young physicist by the name of L. Rosenfeld with whom I was able to converse in French.

After about a month, school started and I met Pauli. He showed me a manuscript by Landau [6] which claimed that free electrons gave diamagnetism and asked me, without chance to examine the manuscript, if I thought this was possible. I had been so indoctrinated by Bohr on the fallacy of this existing in classical theory that I opined 'no'. A young assistant of his, whose name was Peierls, thought there could be. After reading the manuscript I still wanted to see the Landau diamagnetism calculated by explicit examination of the orbits rather than differentiation of the partition functions. When I later told Teller at Leipzig of this desire, it led him to make and publish the requisite calculation [7].

When I informed Pauli that I was writing a volume on 'Electric and Magnetic Susceptibilities' he remarked 'I don't republish my papers as a book'. Perhaps this remark made me extra careful that my volume had more in it than my earlier papers in the Physical Review.

Professor Scherrer invited me to give a colloquium, but my poor knowledge of German, the universal scientific language of the time, posed a problem. However, it was arranged that I would employ a graduate student, none other than Hans H. Staub, to translate my talk (on the susceptibilities of the rare earths) into German, and I committed it to memory. Fortunately German pronounciation is not as difficult for an American as is the French. I do not know whether Busch was in the audience when I delivered my lecture – I am told he had entered the ETH about three years previously. It was only in later years that I had the pleasure of becoming really acquainted with him, and I feel honored in being asked to contribute a paper to this 'Festschrift' dedicated to him. I subsequently gave the same talk at Göttingen, Leipzig and Munich. At Leipzig apparently there were no questions asked after my talk, and Heisenberg remarked to me 'I had no idea you knew German so well, – you had the cases and genders much more correct than most of the Americans'. Staub's tutoring had evidently been successful. However, at Munich, Sommerfeld told me that whereas I seemed to deliver my talk fairly fluently, my German was quite inadequate for answering the questions. How right he was!

**Acknowledgement**

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