DIARY OF TRIP TO EUROPE

AUGUST 1 to SEPTEMBER 24, 1951

By J. J. Stoker

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DIARY OF TRIP TO EUROPE AUG. 1 - SEPT. 24, 1951 J. J. STOKER

Wednesday, August 1: Sailed from New York on the Mauretania.

Wednesday, August 8: Arrived in Southampton at 2:00 p.m. and in London at 6:20 p.m., where I was met by Dr. J. Weyl, and went to Lady MacMahon's to my room. (I had stayed with Lady MacMahon before, in 1946.)

Thursday, August 9: Went to the ONR office at Grosvenor Square and left for Cambridge at 10:00 a.m. Went first to Harold Jeffreys' home, then to Miss Cartwright's in Girton College, where we had sherry and talked for a time. Talked to Jeffreys about meteorology, water waves (in particular my ideas on the non-existence, from a rigorous mathematical point of view, of the solitary wave), my new idea in nonlinear vibrations, and about the coming third edition of his "The Earth". This book has, unfortunately, long been out of print. Had lunch with Miss Cartwright at Girton College, where I sat at what I guess is called the high table, since she appears to be Dean, or something of the sort. There was little opportunity to talk with Miss Cartwright about mathematics. She is still writing up the main papers of herself and Littlewood. After lunch Jeffreys and I strolled about the grounds talking more about applied mathematics. Jeffreys was interested in Dressler's work on roll waves and in Peters' work on water waves. Took the train at 3:10 p.m. for London, went to ONR office and had letters typed, reservations made for Porquerolles, checked reservations for Zürich, etc.

Friday, August 10: In the morning went with Joe Weyl to Deacon's group at the Admiralty Research Lab. in Teddington. Charnock and Tucker seem to be the chief people there now. They continue along the same lines as in 1946 -- studying prediction of waves on shore from meteorological information about storms in the North Atlantic. They also work on microseisms, on seiches, and in the development of a new wave analyzer. These people are in the main non-mathematical. From ARL we were

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taken by Charnock to visit E. T. Eady (of the group of meteorologists around Brunt) at Imperial College in South Kensington, in order to discuss meteorological problems. We had a two-hour long discussion about Charney's approach to the long waves in the prevailing westerly currents of the middle latitudes. Eady thinks it a reasonable approach, and thought our way (by means of a formal development) a good idea. We talked also about atmospheric tides and Pekeris's latest idea. There was much talk also about statistical approaches and about the general philosophy of applied mathematics. Returned to ONR office at 4:30 p.m., very tired.

Saturday, August 11: Slept late, then met Miss Friedrichs (KOF's sister) for lunch. Afterwards to the National Gallery, where we met Joe Weyl and his wife. The National Gallery was very fine, with the pictures well hung; I particularly enjoyed some small and not too studied landscapes of Constable. Afterwards we went to the theatre to see the Laurence Olivier production of Antony and Cleopatra (which was wonderfully staged, but left me rather cold).

Sunday, August 12: Took a plane from London to Zürich, where I was met by Dr. K. Grossmann and Heinz Hopf. We went in Grossmann's car to Stein am Rhein, (a beautiful place), where we swam in the Rhine.

Monday, August 13: Visited Ackeret at the ETH (Eidg. Technische Hochschule) in Zürich. He complained of the lack of communication between scientists, and of the difficulty of keeping abreast with developments even in a restricted branch of science. He had, however, looked at our Communications and seemed to know in a general way what we are doing. We spoke of meteorology and its complexities, and he was interested in Pekeris' newest idea about atmospheric tides. We then spoke about Grad's work, and about a new idea of one of Ackeret's people concerning treatment of shocks from the point of view of the kinetic gas theory. The idea seems to be to assume different statistical distributions from the two sides of the shock.

Ackeret proposed as a mathematical problem to treat the Navier-Stokes equations, assuming that flow takes place smoothly along the boundaries so that the problems are potential problems. He asked whether we had ever studied ship waves, and was amused at Peters's result that the region in which the main disturbance is concentrated is delimited mathematically by lines which figure in a Stokes' phenomenon.

Had lunch with Dr. Sennhauser, who had been our physician years ago.

After lunch returned to the ETH and talked with Ziegler, who is Professor of Mechanics at the ETH. He has found an amusing thing concerning elastic stability. Typical is the case of an elastic rod fixed at one end, free at the other, with an axial couple applied at the free end. This system is completely unstable, since motions in three dimensions are clearly possible which absorb infinite energy: rotating motions of the rod in the sense of the couple. Others (Grammel, for example) have made errors because of failure to consider such possibilities. Ziegler interprets this as due to the difference between conservative and nonconservative external forces, but I think he has found, rather, very good examples in which it is really essential to treat the problem of elastic stability dynamically.

Tuesday, August 14: Up early to keep 9:30 appointment with Strutt. We had a cordial talk, which was useful to me. He is sure that no one has done what I had proposed as a doctor's thesis for Echtman, and Strutt thinks it a good and useful thing to do. The general idea is to extend O. Haupt's discussion of the stability regions for the Mathieu equation to systems of higher order, in which the theory of integral equations must be used instead of the oscillation theorems used by Haupt. Strutt gave me a reference to a relevant Italian paper: Asari, Sulla stabilità ....., Acad. Lincei, 1939. Strutt had not heard of the work of Lubkin and me on the stability of columns under oscillatory tension, nor of my book and its chapter on Hill's equation. Strutt collects a huge mass of material for a new

edition of his useful little book on Hill's and Mathieu's equation, and related topics.

Saw Favre (who is now Rector of the ETH, as well as Professor of Mechanics) and had an hour's conversation. (I had to speak German this time, since he knew no English. My German, oddly enough, is much better than it was twenty years ago, in spite of the fact that I almost never speak it.) He has a method of finding by photoelasticity the moments in a plate by using two layers of different materials. He was interested to know how we conduct the educational side of our activities at the Institute, and was astonished at the large number of students, the evening lectures, the fact that we all feel free to teach any subject in the whole repertoire, etc. Favre and Ziegler teach many hours: about like teachers of undergraduates in the U. S. I shall see Favre again at Porquerolles.

Wednesday, August 15: Made preparations for going first to Brussels, then to Delft and Grenoble, at a travel bureau, and exchanged money. Tomorrow I start my mountain-climbing vacation.

August 16 - 25, inclusive: Vacation in the Alps. I climbed in the Bergell, near Maloja, and in the Dolomites in Italy at the Sella Pass. We made, always with a guide, some quite vertical, nontrivial ascents.

Sunday, August 26: Visited Burckhart in Zürich. Burckhart is at the University, and is interested in group theory and in its applications to crystallography. He is also a good mountaineer. We talked about a German named Haack, who seems to be gifted. (Hopf also spoke well of him.) It seems that he has found major gaps in some parts of Hilbert's theory about systems of elliptic partial differential equations on the sphere and has derived correct proofs. Tried to get in touch with M. Rauscher, whom we knew at M.I.T. but who is now at the ETH in Zürich, but failed. Spent the evening with Hopf talking mathematics. He still would like to write a joint book on Differential Geometry in the Large, and does not mind waiting a few years to start it.

Monday, August 27: Saw Plancherel at the ETH, but again failed to see Rauscher. I had not seen Plancherel for nearly twenty years (since my student days, when he was very kind to me)., Made preparations for the trip to Brussels, Delft, Grenoble,

Tuesday, August 28: Flew from Zürich to Brussels. Went to a hotel and then immediately to meetings of I.U.G.G. (International Union of Geodesy and Geophysics), registered, got programs. Saw H. Jeffreys again, who introduced me to Birch of Harvard. Went with latter to dinner, and talked at some length about the Harvard plans for a development in the applied sciences. Van Vleck has been made a Dean and put at the head of the affair, but it is by no means certain what will finally be done. Brussels is not a very attractive city to me. Itis prosperous (on account of the uranium in the Congo), and it is a very expensive place.

Wednesday, August 29: Spent the morning in the meeting of the Meteorology Section on Micrometeorology. This seems to mean such small scale phenomena as the motion, temperature fluctuations, variations in humidity, etc. in the first hundred meters near the ground; or such things as the spread of smoke from a chimney into the atmosphere, the evaporation from a small lake. It was rather profitable for me to hear, though since turbulence plays a considerable role, I felt not too attracted. There are nice partial differential equation problems in some cases.

Had lunch with Fjeldstad from Norway and Eady. Tried to locate Solberg, Sverdrup, and Rossby with no success. Attended the sessions on Microseisms -- an interesting subject but made quite impossible by bad speakers (a Jugoslav and a deaf man, who spoke in ununderstandable French). This conference is badly organized and very diffuse. One paper was interesting and well presented; it dealt with microseisms observed at Bergen and Uppsala. It seems that the microseisms have two distinct origins: waves breaking on the Norwegian coast, and sources arising from storms at sea. Apparently the theory of Deacon's group (recently discussed at

Monday, August 8th: [illegible]

Tuesday, August 9th: [illegible]

Wednesday, August 10th: [illegible]

Thursday, August 11th: [illegible]

Friday, August 12th: [illegible]

length by Longuet-Higgins) that seeks to account for the microseisms through second order terms in standing waves created by storms in the ocean, may not be the only possible means of accounting for them.

Thursday, August 30: Attended IUGG again, and at last found some of the people I wanted to see: Solberg and Sverdrup in particular. Solberg was very encouraging about our work on long waves in the prevailing westerlies -- says it should by all means go on. He will go to Porquerolles! -- says these things on nonlinear vibrations are of interest to meteorologists. Talked to Schönberg and Schijf, from Holland, who work in hydrodynamics of water waves. (We have recently received a long thesis by the former on this subject.) I met Schijf in Paris in 1946.

Had lunch with MacVittie and Mrs. Harold Jeffries. MacVittie works on meteorological problems too -- apparently he knows through Lowell about the Charney theory and has ideas similar to ours.

Left Brussels at 6:00 p.m. by train for Rotterdam and Delft. Traveled with Frenkiel from the Applied Physics Lab. in Baltimore. We talked mathematics most of the time. Arrived in Delft at 10:00 p.m. and was met by W. Koiter, from the University in Delft, whom I am visiting.

Friday, August 31: Went with Koiter from Delft to Amsterdam and spent the morning with van Wijngarden at the Mathematics Institute. It is rather large and employs a considerable number of people. They are doing quite a lot about asymptotic approximations of integrals, special functions, etc. that can be of interest to us. They plan to write a book under the supervision of van der Corput about such things. Had my first experience of a human calculating machine. This man could do the most uncanny things -- like giving in a few minutes the prime factors of a large number without writing anything down. Met and talked with Schouten -- the tensor and Riemannian geometry man. He cares, oddly enough, very little for differential geometry in the large. The Mathematics Institute

impressed me as a lively place where useful and interesting work is being done.

Had lunch with Koiter and van Wijngarden, strolled about the city, and went through the Rijks Museum. They have wonderful Vermeers and Rembrandts. Amsterdam is one of the most attractive cities I have ever seen.

In the evening Koiter and I went to Biezeno's and had a very pleasant two hours conversation with Biezeno and his wife.

Saturday, September 1: Went to the Technische Hoogeschool in Delft with Koiter and talked first with Broer, who is a physicist by training but works now in aerodynamics with Burgers. At first it seemed that we had few if any scientific interests in common, but then it turned out that he had considered the puzzling matter of why it is that energy in a wave motion propagates with the group velocity, although the latter velocity has a purely kinematic significance, and he gave me a paper in which he shows it true within the accuracy of the stationary phase approximation.

I went over the list of publications of Biezino's group over the years, and Koiter will send me those I want.

Went to stroll through Delft with Koiter. It is a lovely old town. The cathedral (14 - 15th century) is quite fine, and very impressive inside. Delft has beautiful old houses ranged along canals lined with trees.

After lunch Koiter and I went to see van Veen, a number theory man, who nevertheless gave last year a course on nonlinear vibrations. He also works at the Mathematics Institute in Amsterdam on asymptotic approximations, and is interested in the work of Haag and Dorodnitsyn. I have promised to try to get a copy of their papers to send him. (Koiter and van Veen both tell me that they do not receive the Russian literature nowadays.)

In the late afternoon went to the Prinzenhof -- former house of William of Orange -- now a museum. It has

very beautiful rooms, and is a most attractive house.

In the evening Koiter and I talked elasticity. His doctoral thesis is on elastic stability, and his point of view is nonlinear. We should have known of this long ago, and perhaps would have if it were not written in Dutch. He says, for example, that Donnell is wrong and that initial imperfections in the cylindrical shell can account for the lower buckling loads obtained by experiment as compared with the theoretical values furnished by the linear buckling theory. This is an important matter.

Sunday, September 2: Went to Amsterdam to take the bus for the airport, and the plane for Geneva.

Economic conditions in Holland are not good, I am told, and they have worsened during the last 1-1/2 years, unlike Belgium. However, Holland was to me a most attractive country. It has, for example, beautiful skies and one can readily understand why Dutch painters like van Ruinsdael and Hobbema lavished such care and affection on the painting of the clouds in their pictures.

Monday, September 3: Geneva is beautiful, but the weather is bad and I could not see the mountains. I went instead to the art museum to see pictures. I was glad to see the well-known pastel portraits of Liotard, and the murals of Hodler. Met P. Danel in the afternoon and was driven in his car to Grenoble -- very fast, and I was scared at times. Annecy and Aix les Bains (through which we passed) are very beautiful vacation places.

Tuesday, September 4: The object of my trip to Grenoble is to visit the Research Laboratory under P. Danel of the Laboratoire Dauphinois d'Hydraulique, Ets. Neyrpic. In the morning I was taken to the laboratory by P. Danel and introduced to various people, shown the library (which is very large and very well organized). Lunch is a long affair here, and quite elaborate, but it serves as a means to continue business. Present at lunch were F. Biesel, A. Craya, F. Suquet, an engineer from

Australia, P.Daniel, and myself. After lunch Biesel, Suquet, and I talked about water waves for the remainder of the afternoon. They have made many careful experiments confirming the basic theory very well -- e.g. with respect to particle paths, etc. -- as far as the potential theory is concerned. Not much experimentation has been made on shallow water theory. However, the most interesting thing to me was: Biesel is able to deal with the breaking of waves to some approximation by purely linear theory by using Lagrange coordinates!! One can actually calculate forms which curl over as the wave approaches the shore. Such a thing was precluded in our calculations since we used the Euler variables and hence could not obtain the surface elevation except as a single-valued function of x. He did it for beaches of small slope, developing with respect to both slope and amplitude. We ought to check this for ourselves.

Had dinner in the evening with Danel and many other people and up late once more.

Wednesday, September 5: Saw the experiments showing the forms of the limiting progressing and the limiting standing waves. The latter is very striking, since the crest angle is truly 0° and the sharp points on the waves are plainly seen. It is done by suddenly dropping a barrier into a train of progressing waves, with standing waves resulting by reflection. The theoretical difficulties in attempting to calculate such a thing would be immense. Perhaps something could be done with our new methods in nonlinear vibrations applied to the nonlinear shallow water theory. Again a two-hour lunch. In the late afternoon Danel was kind enough to drive me to la Bérarde in the mountains. Since Dr. J.Weyl and Dr. Rees are to arrive in Grenoble on September 10 and 11, I have decided to postpone inspection of the laboratories, and further scientific discussions until they arrive, and do these things with them.

Thursday to Sunday, September 6-9, incl.: Again I took a mountain climbing vacation, this time at la Bérarde in the Dauphiné. This was very fine, and I made three good ascents. The first was a sharp needle: the Aiguille Dibona with a stiff and exposed rock climb at the end - so that my guide could test me out. The second was a famous climb in the Dauphiné: the traverse of les Ecrins (4100 meters) upthe south face, along the narrow ridge for about a mile, then down over the glaciers and steep snow slopes of the north fave. The third was the traverse of les Fétoules, a snow and ice tour. The climbing in the Dauphiné was very much to my taste - better than the Bergell in Switzerland and the Dolomites in Italy. These mountains are savage looking, the valleys are very narrow with steep sides, and the country is lonely and not full of places for tourists. I also learned some French, since no one I was with during this vacation spoke anything but French. My French is adequate, but not very grammatical.

Monday, September 10: Joe Weyl and his wife came with Danel to la Bérarde and drove me back to Grenoble. We had dinner together and arranged a program for the next days at the laboratory.

Tuesday, September 11: Mina Rees arrived in the morning. Went to the laboratory and were shown all through it by Ransford -- a young Australian(with a brilliant red beard) who works permanently at Neyrpic and is very gifted according to Danel. The laboratory makes a very great impression. It is huge, and it contains a great variety of things: spillways, models of dams, of tidal power plants, turbines, pipe fittings, experiments on seepage, channels in profusion to study water waves, water hammer experiments. Danel prides himself on making his research laboratory pay commercially while at the same time doing a great deal of a theoretical nature that

Thursday in court, defended by, then! [illegible] - only a [illegible] [illegible] evening, with time to be [illegible] in the [illegible]. This was very [illegible], and I [illegible] [illegible]. [illegible] [illegible] and a [illegible] [illegible]. The [illegible] [illegible] [illegible] [illegible] [illegible] - [illegible] [illegible] [illegible] [illegible]. The second was a [illegible] [illegible] [illegible] [illegible] [illegible]

seems to illuminate the basic ideas but has no necessary immediate applications. He is one of the very few engineers of this type I know. Neyrpic is probably unique in the world. It is amazing how a bit of rather unsophisticated mathematics and theoretical work can be used to make quite considerable improvements in concrete cases. For example, Danel showed me designs of pipe outlets for dams which he designed using simple conformal mappings to improve flow conditions. It turned out that the volume of flow could be multiplied by a factor of nearly three without increasing the size of the pipes by using such simple notions.

In the afternoon talked with Craya and a pupil of his and Kravchenke (University of Grenoble). The young man has carried through what I have often advanced as a reason for the nonexistence of the solitary wave from the rigorous mathematical point of view. The idea is to show that no such wave can exist as an analytic function of its amplitude, since wellknown uniqueness theorems lead easily to the result that all terms in a development with respect to amplitude would vanish identically once it is assumed that the wave amplitude tends to zero at ∞. In the course of our discussion still another argument against the solitary wave as a <u>steady motion</u> occurred to me. It is that the solitary wave of small amplitude has a spead c near to $\sqrt{gh}$; but one knows that a steady wave with velocity approaching $\sqrt{gh}$ does not exist: it does for $c < \sqrt{gh}$ and $c > \sqrt{gh}$, but not for $c = \sqrt{gh}$. My guess now is that what people observe and call the solitary wave is really an unsteady motion, though perhaps one that departs only slowly from a steady motion. I mentioned that Lavrentieff has claimed to prove the existence of the solitary wave recently, but his paper is long, complicated, and written in Ukrainian so that I know no one who has studied it thoroughly. They will try to do that at Grenoble.

Wednesday, September 12: Went to the laboratory in the morning with Joe Weyl and Mina Rees to talk to Craya. He goes to Columbia apparently in part because of the death of Bakhmetev. Craya has been working on turbulence problems lately, and that had more interest for Joe Weyl and Mina Rees than for me. In the afternoon we drove high up in the hills for lunch and talked to Halbronn about his theory of air entrainment in flows of water in channels. His theory is that air starts being entrained when the boundary layer widens and goes to the surface. In the evening we had a wonderful dinner at Biesel's house in Grenoble.

Thursday, September 13: Talked to Angles about elasticity problems. He has devised a means of finding stresses in complicated branching pipe connections by a combination of elasticity and strength of material methods. We also talked about nonlinear elasticity. Angles has done a number of interesting things in elasticity and the theory of structures.

As usual we had a -- 2 1/2 hour luncheon. Afterwards we visited the wave slap experiment tank. This refers to the slapping of a wave against a breakwater, but so timed that the vertical face of the wave strikes the wall in such a way as to cause a heavy impact. Suggested to Biesel a purely elastic treatment of the problem ignoring gravity, in order to obtain a good estimate for the maximum pressure.

Friday, September 14: In the morning talked with Danel about his ground water potential flow problems. He has had a huge number of such flows computed.

I have now had ample opportunity to form an estimate of the work, facilities, personalities at Grenoble. It is a remarkable place, and almost as good as Danel (who is not one to hide his light under a bushel) says it is. Danel is the force and inspiration behind the success of this laboratory. He is a leader who knows that the essential is to find the

right people for his purposes and then to give them as much freedom and as much opportunity as possible to develop their talents. Danel deserves all the more credit for achieving at least partial success in this aim since his laboratory is a purely commercial enterprise that must make money to survive, and is without subsidies of any kind - indeed, he apparently competes successfully with government-subsidized laboratories. His people work long hours, and some of the best scientists among them - Biesel, for example - must devote a good deal of time and energy to the purely business and commercial aspects of the enterprise. What struck me as much as anything else was the fact that relatively unsophisticated notions in mathematics and mechanics were used with a lot of practical success - at least by comparison with competitors accustomed to operate in quite primitive ways. However, these relatively unsophisticated applications of science were made by rather sophisticated people - and probably it is always necessary that those who do such things should be aware of a great deal more in science than they are likely to be able to apply.

Grenoble would be an excellent place to send a few young people for a year or so, with the idea that they should then come back here and transplant the same general ideas and notions in commercial enterprises here - or perhaps also in government laboratories. According to Danel, he has no serious competitor in the U.S.

Saturday, September 15: Arrived on the Isle de Porquerolles in the early afternoon, having left Grenoble the preceding evening. One goes to Hyères on the coast of southern France (not very far east of Toulon) and takes a small boat to the island. Southern France is beautiful. It is a little like California with respect to climate and vegetation, but it nevertheless makes a different impression - perhaps because one feels that it is an old country that has seen and experienced a lot over centuries.

My main purpose in coming to Europe altogether was to attend an International Symposium on Nonlinear Vibrations, as the head of the American delegation. This symposium was sponsored by the International Union of Theoretical and Applied Mechanics (IUTAM), which was given a subvention by the UNESCO. Somewhat more than half of my expenses are to be paid from this source.

Sunday, September 16: Explored Porquerolles a little. On the south side (facing Africa) there are high cliffs, with coves filled with water in all shades of blue and green. The island has fine trees: pines, oaks, live oaks, and others I did not know, and there are paths along which one can stroll for some miles. The swimming is fine. There are good beaches, and the pines grow right down to the edge of the water so that shade is available if it is wanted. I needed shade, since I found the sun almost ubearably bright and hot. The Weyls and Mina Rees are here also, and it is pleasant to relax with them. Valensi from Marseilles, who is in charge of the arrangements for the meetings here, turned up. He is a lively and good-humored person, who did a lot to make our stay on Porquerolles comfortable and pleasant.

Monday, September 17: Morning was spent (in a rather futile fashion) in a meeting of the heads of the various delegations to discuss a long list of rather trivial matters relating to the scientific meetings, which begin tomorrow. There will be people from the U.S., England, France, Holland, Belgium, Norway, Sweden, Denmark, Germany, Italy, Spain, Turkey, and perhaps from still other places.

Tuesday, September 18, to Friday, September 21 incl.: Scientific meetings were held on each of these days from 9 to 12 in the mornings, and 2 to 5 in the afternoon. Since it was hot, and the majority of the papers (as usual at such affairs) were pretty dull, this was hard work. But these IUTAM people take their work seriously,

and just about every one (including myself) was present at all of the sessions and stuck them out to the bitter end.

The real value of such conferences is, of course, the opportunity afforded to meet people and to talk with them and learn from them what is going on. I met on this occasion for the first time, for example: van der Pol, van den Dungen, Perès, Graffi, Grammel, Metler. I had opportunities to talk with Solberg, Burgers, Biezeno, Southwell, Minorsky, and others. I met Kerim Erim, the Turk who is apparently responsible for the choice of Istanbul as the meeting place for the International Congress of Mechanics next year. We had a jolly evening together on one occasion; he is a lively companion, full of jokes and very witty. We spoke German together, since his French and English are bad. I renewed acquaintance with Amerio, saw more of Craya (from Grenoble), and Koiter, as well as Favre. I heard Haag speak again, but his French is hard to understand. He does good things, though, and I shall be glad to have the papers he promised to send me. Haag speaks only French, which also does not make communication easy for me. - The conference was very profitable for me, but I think that the subject of the conference is somewhat too narrow to warrant a conference lasting for 4 days.

Saturday, September 22: Traveled from Porquerolles to Nice along the Riviera, which is spectacular, but spoiled somewhat by its general air of a resort place. Nice and Cannes are big places made up solely of hotels, one feels. Porquerolles is much pleasanter: it has not even one luxury hotel, and thus preserves a better atmosphere.

Sunday, September 23: Left Nice by plane for home, via Barcelona, Lisbon, the Azores, Boston, and finally New York, where I arrived

Monday, September 24, at Idlewild, at about noon. It took 26 hours from Nice to Idlewild, but the trip was pleasant except over Spain, where it was rough.

Electrons and Positrons in a Time-Independent Electromagnetic Field: A Solution in the Schrödinger Picture.

By H. E. Moses

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